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

Deniz Aydin*

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Abstract

I design and implement a large scale field experiment in an economy that had been experiencing a decade-long debt-driven consumption boom, in which I construct a randomized credit line extension that isolates selection and interest rate effects, and track impulse responses using comprehensive data on spending and consumer balance sheets. I document substantial indirect effects of credit, and that the propensity to borrow and spend out of credit remains quantitatively large for those with substantial slack in borrowing capacity, as well as those with a sizable buffer of assets. I use data on the dynamics of the response and expenditure composition to distinguish two classes of potential explanations: one embracing precautionary savings in response to a credit constraint that may bind in the future; and the other invoking behavioral explanations based on myopia or dynamically inconsistent behavior. The reduced form findings provide strong support for a buffer-stock model emphasizing the importance of precautionary savings. (JEL D12, D15, E21, E51)

1 Introduction

The effect of rapid expansions in credit on household consumption expenditures and aggregate fluctuations has been the focal point of discussions between researchers and policymakers in both developed and emerging markets, inspired by the salient and dramatic credit cycle of the last decade. Despite a large literature that uses naturally occurring variation to document that consumer behavior is affected

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1See Schularick and Taylor [2012], Korinek and Simsek [2015], and Mian et al. [2017].
to some extent by credit availability, there is little consensus on how broadly across the population this
effect is observed, or the relative contributions of commonly invoked ingredients—binding constraints
or precautionary behavior versus behavioral factors—that generates the sensitivity.

To shed light on these questions, I design and implement a large-scale controlled trial in an economy
experiencing a decade-long debt-driven consumption boom. I construct a randomized credit line ex-
tension and track the impulse responses using comprehensive data on consumer balance sheets and
spending patterns. The intervention deliberately pauses the internal credit line underwriting process
for a randomly selected subset of 45,307 preexisting consumer credit line customers of a large Euro-
pean retail bank who are preapproved for a credit line increase. I select at random 32,624 of this group
as the treatment, and the consumers in this group are pushed for credit line extensions, whereas the
control group is withheld from lender-initiated credit line increases for 9 months.

The large-scale experiment is a unique opportunity to study the effects of a truly exogenous credit
shock, as randomized assignment of credit—akin to rebate checks—are rare. The increases in limits
are salient changes initiated and pushed by the issuer, are not preannounced, and are difficult to antic-
ipate. Importantly, other features of the contract, such as the interest rate, remain unchanged. The na-
ture of the shock is in a similar spirit to Parker et al. (2013)'s tax rebate study, and the experiment could
be interpreted as randomizing who gets limit increases over the experimental timeframe, however, it
also creates long-run differences in credit availability. The magnitude of the credit shock is also signif-
ically larger than what is often considered in consumption studies—about 140% of monthly income
for the typical consumer—hence the utility loss from nonoptimizing behavior is not trivial. Therefore,
the intervention can be classified as a quantitatively large, unexpected and exogenous shock to only
credit availability, which creates a sharp counterfactual isolating selection, interest rate, and anticipa-
tion effects.

I use the credit market approach to complement prior empirical studies estimating the consumption
and borrowing responses to disposable resources using naturally occurring variation (for example, see
First, the novel features of the experimental design allows me to perform sample splits, examine high-
frequency leveraging dynamics around the constraints, and analyze long-run responses, which similar
studies were often under-powered to address. Second, the comprehensive nature of the spending and
balance sheet data overcomes many deficiencies of studies that rely on credit bureau variables only. It
allows me to estimate a spending response in addition to borrowing, but also to study its composition,
as well as heterogeneity by assets. The size of the indirect effects of credit for asset holders, as well
as the categorical composition of the response by sector—where consumers spend the credit—are
important questions related to understanding credit expansions for which there is a lack of systematic
evidence or consensus.

I begin my analysis by documenting that a pure shock to credit has a precisely measured and eco-
nomically large effect on spending and the use of credit. The increase in credit availability leads to a

2 Also see Hall and Mishkin (1982), Johnson et al. (2006), Agarwal et al. (2007), Baker (2013), Kaplan et al. (2014), Agarwal
for comprehensive surveys.
sharp increase in leverage, with a 9-month cumulative marginal propensity to consume out of credit limits, $MPC^{DL}$, averaged across the treatment group, of 15 cents on the dollar, factoring in balance shifting. Long-run results indicate that consumers continue spending and monotonically leveraging up beyond the experimental timeframe, with statistically significant effects extending to the second and third years, with about one-third and two-thirds of the 3-year cumulative response coming after the first quarter and first year, respectively.

The consumers whose spending behavior is sensitive to credit is not confined to a small set of low asset, hand-to-mouth consumers, or those with binding constraints whom are up against their limits, pointing to substantial indirect effects of credit. The response is highly heterogeneous, and factors like low income, low assets, and proximity to the limit is positively correlated with the $MPC^{DL}$. However, perhaps surprisingly, consumption appears depressed and the propensity to spend out of credit remains quantitatively large for those who are far from the limit with substantial slack in their borrowing capacity, as well as those holding up to 10-months of average income worth of liquid assets to finance a marginal increase in spending.

It is well known that in the benchmark permanent income model a credit shock could affect consumer behavior through a change in the interest rate as in Di Maggio et al. (2017), so that both the liquidity and present-value effects are operating. However, as the experimental intervention is designed to entail no present-value effects, according to this model it should have no effect on consumption behavior. Similarly, two-agent spender-saver models with simple heterogeneity, as in Campbell and Mankiw (1989) and Eggertsson and Krugman (2012), or models that generate endogenous illiquidity in which consumers hold few liquid assets — such as those that feature consumption commitments as in Chetty and Szeidl (2010), or second generation spender-saver models featuring illiquid assets with high returns, as in Kaplan and Violante (2014) — although can explain why consumers hold few liquid assets and frequently face binding constraints, can not explain why unconstrained consumers with assets would spend out of credit.

The classical explanation to this finding embraces precautionary savings in response to a credit constraint that may bind in the future — the most frequently put-forward explanation for the empirical violations of the permanent income hypothesis. The other class of explanations invokes behavioral ingredients to explain aggressive borrowing during booms, focusing on narratives based on myopic or dynamically inconsistent behavior. The empirical literature outlined above remains mostly silent about why the $MPC^{DL}$ would be positive for consumers who are not constrained in the conventional sense, partly because both of these explanations make the similar prediction of high $MPC$s. However, two useful features of the current study are detailed information on debt contract choice and expenditure composition, and a sufficiently sharp counterfactual to closely inspect the dynamics of binding constraints. In the second part of the paper, I distinguish these two competing classes of explanations.

First, I analyze the behavior of unconstrained consumers shifting consumption forward in time. I find that these consumers use credit primarily to finance purchases with investment features in non-lumpy increments using installment-type loans, in which the additional debt is paid down over time according to a preplanned schedule. Detailed analysis of spending categories shows that discretionary
non-durable spending (e.g., restaurants, recreation, hobbies, cosmetics) accounts for an economically and statistically insignificant fraction of the response, and direct expenditures on non-perishable goods with some degree of durability account for up to 60% of the increases in borrowing. In contrast to models featuring myopic behavior, the spending responses show that consumers do not appear to fritter away credit on enticing non-necessities or instantaneously gratifying goods for immediate consumption, but borrowing appears to primarily finance expenditures for future consumption. Moreover, in contrast to models featuring dynamic inconsistencies with respect to repayment behavior—as in Heidhues and Kőszegi (2010), in which consumers underestimate the true cost of credit and their likelihood of revolving after using float benefits—consumers only to a small extent revolve balances, but lever up primarily using loans with restricted and commitment-like contract features.

Second, I turn to analyzing the dynamics of binding constraints and the behavior of high MPC participants who are trying to avoid close-to-zero consumption. 25% of the participants were approximately constrained —i.e., utilizing more than three-quarters of their total available credit— at one point in the year prior to the intervention, and 38% find themselves in a similar situation in the three-year period following the onset of the experiment. Strikingly, I find that in contrast to the response of the unconstrained consumers — which is to increase the pace at which they lever up — the estimated effect for ex-ante constrained consumers with high sensitivity to credit is, to a considerable extent, due to the control group to delever. Most consumers appear to be forward looking and have a tendency to quickly save their way out of the constraints, and the defining tension appears to be between a desire to create a buffer versus the desire to shift consumption forward in time.

These findings are what one might expect from a forward-looking buffer-stock model as in Deaton (1991), Carroll (1997), Guerrieri and Lorenzoni (2015). In this model consumers keep a buffer of credit to insure themselves against low consumption, and credit constraints that are currently not binding but may potentially bind in the future effect consumption dynamics. In line with this model, the empirical findings show that, although a significant sensitivity to credit is estimated broadly across the population, there is little evidence of it in the form of persistently binding constraints.

How plausible are alternative explanations? Attributing the findings to mistakes or near-rational behavior, as in Cochrane (1989) and Kueng (2015), is difficult, as the shock to disposable resources is substantially larger than those considered in previous studies, and the utility loss from non-optimizing behavior are not trivial. It is also possible for an excess sensitivity of borrowing to an increase in credit availability to reflect a high propensity to fall into delinquent status. In contrast, I find no economically or statistically significant differences between the two groups with respect to delinquencies or post-delinquency restructurings. Finally, in line with expectations-based models of credit cycles, one might also ask whether credit line extensions have informational value as evidence that the economic futures of participants are rosier than they previously thought. However, in contrast with this hypothesis, longitudinal features of the data show no association between credit line extensions and the conditional distribution of future income growth.

Two important caveats to the analysis here are, first, the analysis is based on the behavior of a group of preapproved participants with low preexisting limit-to-income ratios; and, second, the study is con-
ducted in a recently industrialized upper-middle-income country—which shares many characteristics with other emerging market economies, such as high GDP growth and high consumption volatility. Hence the typical participant is potentially more likely to face a credit constraint compared to households in the US. Despite the broad similarity of the participant population to the typical cardholder with respect to levels of income and debt growth, the estimates may be biased against models that do not predict a sensitivity to credit. Due to these concerns, I focus my analysis on key mechanisms that are likely to be portable: I examine behavioral heterogeneity across those that are constrained versus unconstrained, but I also explore heterogeneous with respect to aspects in which the samples may not representative.

In addition to distinguishing alternative models, the findings also shed light on normative issues with regard to credit expansions. Several well-known studies have recently pointed out that credit expansions—in particular a rise in household debt—are robust and significant predictors of financial crises and subsequent declines in macroeconomic activity (e.g., Schularick and Taylor (2012) and Mian et al. (2017)). This lead analysts to question the efficiency of credit booms and argue for ex-ante macroprudential policies, such as limit-to-income ratios, in order to curb excess leverage that may be societally suboptimal; for example, see Korinek and Simsek (2015). I show that relaxed credit constraints allow consumers to create a buffer to guard against and avoid low consumption states, and finance spending with potentially high marginal benefits, rather than accumulating debt that must be repaid by squandering credit in a dysfunctional way or using revolving balances in a dynamically inconsistent manner out of which they are unable to save out of. My estimates are obtained toward the end of the credit expansion, and offer guidance on whether and how much a restriction in lending ex-ante would affect aggregate activity during this stage. However, such policies could potentially cause large drops in aggregate demand and consumer welfare during the boom phase, as its effects are observed across the board and not on a small set of low-income borrowers up against their limits.

Layout. Section 2 briefly describes the macroeconomic environment and key institutional features of the credit market under consideration. Section 3 details the experimental timeline, randomization, and implementation. Section 4 presents the event study and the empirical estimates of the \( MPC_{DL} \). The discussion of the heterogeneity of the response by disposable resources is in Section 5. Section 6 studies alternate mechanisms that could potentially generate the sensitivity, focusing on compositional results in 6.2 and the dynamics of binding constraints in 6.3. Section 7 concludes.

2 Environment and Institutional Details

Macroeconomic environment. I conduct my study in Turkey, whose economy had been experiencing a discernible credit-driven consumption boom since the early 2000s. Figure 1 displays the household debt to GDP ratio, which rose from about 3% in 2000 to a peak of 19.6% in 2013. Similar growth patterns were also observed in narrow and broad monetary aggregates, and the rise in household debt was associated with a large current account deficit. A set of macro-prudential policies, including caps on credit limit-to-income ratios, attempted to contain this household debt growth and coupled with tighter monetary policy, led to a reversal of the trend in household indebtedness in 2014. The nominal
GDP per capita based on PPP in 2014 was roughly $20,000, or 53% of EU28 average. As of September 2014, the annual rate of inflation (CPI) was 8.9%, the unemployment rate 9.9% and the exchange rate was 2.21 TRY-$; see Appendix D.2 for details.

Credit card market. The credit lines considered here are very similar to credit cards in the United States along principal dimensions. A single credit limit applies to all in-store purchases. Consumers use the credit lines as a means of payment and for liquidity within pay periods, as well as to transfer resources across pay periods. Those that pay off the end-of-billing-cycle balance in full and on time get a float. Consumers that do not pay their balances in full accumulate interest-bearing revolving debt equivalent to only the unpaid component of the end-of-month balances.

As of 2015, an annual volume equivalent to 24% of GDP flows through credit cards as in-store transactions, and it is estimated that 40% of the working-age population have credit cards. Importantly, credit cards are the predominant payment method for in-store transactions, with debit cards only accounting for 6.5% of the total in-store transaction volume; see Appendix D.1 for details. For the participants with information on labor income, I find that the typical participant spends 35% of their post-tax monthly labor income using credit cards at bank, and has total credit card debt equaling 110% of monthly post-tax labor income.

There are two distinguishing features of the market I conduct my analysis in. First, the maximum interest rate that can be charged on any credit card and checking-linked overdraft account is capped by the regulatory authority at 24% APR, and this upper limit is binding for virtually all customers. This cap allows me to ignore any pecking order across credit cards with potentially different rates, and focus instead on the notion of credit constraints as quantity constraints. Second, the credit card market features wide spread financing of consumer goods and services with installment credit in addition to conventional end-of-month revolving balances. Similar to Affirm, Amazon, or iPhone installment

3 Although the de jure interest rate on the credit card statement is 24% APR, the effective rate paid by the customer could be lower, due to float benefits, special financing, and other forms of promotional in-store credit.
Table 1: Summary Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>s.d.</th>
<th>p10</th>
<th>p50</th>
<th>p90</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age</td>
<td>37</td>
<td>10</td>
<td>26</td>
<td>35</td>
<td>50</td>
</tr>
<tr>
<td>Wage</td>
<td>2,587</td>
<td>2,661</td>
<td>968</td>
<td>1,713</td>
<td>5,384</td>
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<tr>
<td>Credit lines</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Limit (Total)</td>
<td>10,005</td>
<td>16,116</td>
<td>1,600</td>
<td>5,000</td>
<td>22,800</td>
</tr>
<tr>
<td>Debt (Total)</td>
<td>3,318</td>
<td>8,236</td>
<td>82</td>
<td>1,327</td>
<td>6,591</td>
</tr>
<tr>
<td>Limit</td>
<td>5,177</td>
<td>5,682</td>
<td>800</td>
<td>3,200</td>
<td>12,000</td>
</tr>
<tr>
<td>Debt</td>
<td>1,364</td>
<td>2,129</td>
<td>0</td>
<td>0</td>
<td>3,243</td>
</tr>
<tr>
<td>Revolving</td>
<td>448</td>
<td>1,083</td>
<td>0</td>
<td>0</td>
<td>1,327</td>
</tr>
<tr>
<td>Installment</td>
<td>916</td>
<td>1,659</td>
<td>0</td>
<td>0</td>
<td>2,325</td>
</tr>
<tr>
<td>Spending</td>
<td>936</td>
<td>1,083</td>
<td>0</td>
<td>0</td>
<td>2,287</td>
</tr>
<tr>
<td>Balance sheet</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Debt (Total)</td>
<td>19,748</td>
<td>77,954</td>
<td>358</td>
<td>7,276</td>
<td>52,548</td>
</tr>
<tr>
<td>Checking (Bank)</td>
<td>723</td>
<td>2,774</td>
<td>0</td>
<td>0</td>
<td>1250</td>
</tr>
<tr>
<td>Credit score</td>
<td>1,353</td>
<td>236</td>
<td>1,049</td>
<td>1,357</td>
<td>1,653</td>
</tr>
</tbody>
</table>

Note. Summary statistics based on 45,307 experiment participants using data from September 2014. Nominal variables expressed in local currency TRY. The exchange rate was 2.21 TRY$. Unit of analysis an individual. Labor income information is for the subset of customers with direct deposit.

plans, the consumer borrows a fixed sum of money for a predetermined term (typically 3 to 12 months) to finance in-store expenditures and agrees to make amortizing monthly payments of a set amount until the loan is paid off. These types of installment loans were also widespread within the United States during the 1920s. They are often used to finance smaller consumer durables (e.g., electronics, clothing, appliances, furniture) and services (e.g., health and education expenses). They cannot be used, however, to finance expenditures of strictly nondurable goods, such as food and gas. The credit card yields a single consolidated statement, in which the total credit card debt carried across pay periods is the sum of the installment and revolving components of credit line debt. See Appendix D for a detailed discussion of installment plans.

3 Experimental Design

For the purposes of the field experiment, I collaborate with a large retail bank. The controlled trial was conducted by the financial institution as a pilot in order to better understand customer credit usage. The exact nature of the intervention is to deliberately pause the internal credit line underwriting process for 9 months for a group of randomly selected preexisting consumers that otherwise satisfy the

Persons (1930) and Olney (1999) argue that installment buying accounted for much of the 1920s expansion in household debt; the collapse of consumption in 1930 followed soon after the decade of explosion in household use of installment credit. Annual installment sales were about $6 billion; the total debt outstanding at a given time was about half that sum. They find that over 40% of the 506 families of federal employees the BLS surveyed in 1928 used installments to finance furniture, clothing, radios, automobiles, pianos, and appliances.

The financial institution has more than 10% of market share in the local credit card market and the customer base is representative of the local banked population. In addition to credit cards, it offers a multitude of financial products, including credit cards, checking and overdraft accounts, and payment services.
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 Appendix A.

- **How participants are selected.** The participants are not randomly selected from the universe of preexisting customers, but are identified by processing active cardholders through the bank’s underwriting decision rule outlined and discussed in Appendix Table A.1. The decision rule filters high-risk borrowers primarily using the in-house risk scores, and has built-in timing rules that avoid increases to cardholders that have recently opened their accounts or have recently experienced credit line increases. Different divisions within the bank (e.g., affluent, new customer, small business owner) have different decision rules and frequently adjust underwriting parameter cutoffs at different times; however, they all draw on similar fundamental information on the account and borrowers. The key component of the decision rule is a value-added model, which calculates the potential increase in revenue from the limit increase and trades this off with the increased risk of default under the new limit, and pushes credit to cardholders who have low preexisting limit-to-income ratios.

- **How participants compare to the typical cardholder.** Table 1 displays summary statistics on the 45,307 participants, and Table 2 compares the 45,307 participants to a random subsample 10,000 of (non-participant) credit-card holders. Similarly, Figure 2 displays kernel densities to highlight key economic differences of the participants from the typical cardholder. The preapproved sample is, on average, 4 years younger and earn 7% more labor income, have broadly similar credit scores, but have higher credit score growth in the year prior to the intervention. As the kernel densities highlight, the key economic distinction of the participants from the typical cardholder is their preexisting low limits —the approved sample, on average, have total credit lines across all banks that are only half as large as the typical cardholder. In terms of debt growth, participants have lower total credit line debt growth in the year prior to the intervention, however have slightly larger debt growth over the experimental timeframe. Therefore, the participants appear to be younger cardholders that have low ex-ante limits but are catching up with the typical cardholder in terms of credit line magnitude.

- **Randomization.** Assignment of subjects to the control group is done after customers have been preapproved for a limit extension, but before the limits have been pushed. Participants are first stratified into nonoverlapping and exhaustive bins with respect to their end-of-billing cycle balances over limits. A random subsample, totaling up to 32,624 cardholders, is then drawn from each bin using a random number generator and these consumers are assigned to the treatment group —i.e., $Z_i$ is set to 1. The treatment group is then pushed downstream in the underwriting process for limit increases, while the control group is withheld from lender-initiated credit line increases for 9 months starting in September 2014.

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6Despite the broad similarity of the participant population to the typical cardholder with respect to levels of income and debt growth, in the following analysis I fully embrace the notion participants are younger borrowers with lower preexisting limit-to-income ratios; hence the estimates are biased against models that do not predict a sensitivity to credit. It is also possible that a policy that extends credit to those who weren’t qualified for the experiment might have a different response. My analysis focuses on key mechanisms that are likely to be portable, focusing on behavioral heterogeneity across those that are constrained versus unconstrained, but I also explore heterogeneities with respect to aspects in which the samples may not representative.
Table 2: Selection

<table>
<thead>
<tr>
<th></th>
<th>(1) Age</th>
<th>(2) Income</th>
<th>(3) Score</th>
<th>(4) $\Delta t$ Score</th>
<th>(5) $L_{CC}$</th>
<th>(6) $D_{CC}$</th>
<th>(7) $\Delta t D_{CC}$</th>
<th>(8) $\Delta t$ Util.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Participant</td>
<td>-3.8</td>
<td>187.9</td>
<td>-7.6</td>
<td>42.8</td>
<td>-10,591.2</td>
<td>-2,854.4</td>
<td>-325.3</td>
<td>-0.029</td>
</tr>
<tr>
<td></td>
<td>(0.15)</td>
<td>(108.6)</td>
<td>(3.1)</td>
<td>(2.8)</td>
<td>(356.0)</td>
<td>(157.3)</td>
<td>(98.0)</td>
<td>(0.0035)</td>
</tr>
<tr>
<td>Constant</td>
<td>40.9</td>
<td>2,467.0</td>
<td>1,361.8</td>
<td>-11.5</td>
<td>20,585.4</td>
<td>6,172.7</td>
<td>324.4</td>
<td>0.040</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(102.0)</td>
<td>(2.4)</td>
<td>(2.2)</td>
<td>(330.0)</td>
<td>(147.9)</td>
<td>(89.1)</td>
<td>(0.0028)</td>
</tr>
</tbody>
</table>


$p$ < 0.001 0.083 0.014 < 0.001 < 0.001 < 0.001 < 0.001 < 0.001

Note. This table reports the results of simple cross-sectional regressions $Y_i = \alpha + \beta X_i + \epsilon_i$ that compare the 45,307 participants to the 10,000 random subsample of cardholders. It uses data on $N = 55,307$ consumers from the month prior to the onset of the experiment, totaling to $N \times T = 55,307$ observations. $X_i$ is a dummy variable that is 1 for the 45,307 participants and 0 for the 10,000 random subsample. Columns (1) and (2) stand for age and monthly post-tax labor income. Columns (3) and (4) stand for the current level and 12-month change of credit score. Columns (5) and (6) stand for total credit line limits and total credit line debt. Columns (7) and (8) stand for 12-month changes in total credit line debt and total credit line utilization. The bottom row displays $p$-values for the null hypothesis $H_0: \beta = 0$.

Figure 2: Selection: Kernel Densities

Note. These figures plot kernel densities that compare the 45,307 participants to the 10,000 random subsample of cardholders, using data on $N = 55,307$ consumers for September 2014 only. Densities censored at the 10th and 90th percentiles. The figures on the top plot the densities for monthly post-tax labor income and total credit lines across all banks. The figures on the bottom plot the levels and 12-month changes for total credit line debt. I perform Kolmogorov-Smirnov tests and reject the null of the equality of the distributions for all variables except labor earnings.
Figure 3: Pre-trends

Note. Figures plot the levels of covariates for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal zero at the onset of the experiment in September 2014.
Figure 4: Experiment Timeline

Note. Figure on the left plots group averages for fraction of participants that see their limit increase since the offset. Figure on the right plots average credit lines at bank. The x-axis is calendar date. For the figure on the right, the y-axis is normalized to display the cumulative increase in limits relative to the onset of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean. The red dashed and black dash-dot lines indicate the start and end dates of the experimental timeframe.

- **Covariate balance.** This randomization procedure makes the key instrument an indicator for being in the treatment group, \( Z_i \). Figure 3 displays the pre-trends of variables that could potentially be correlated with consumption and debt growth. Similarly, Appendix Table A.2 performs statistical tests on these lags, and finds no statistically significant differences in the levels of and changes in pre-trend variables.

- **Timeline of limit increases.** Figure 4 displays the timeline of limit increases. In what follows, I take September 2014 as the onset of the experiment —i.e., when the first set of participants see the limit increases on their end-of-billing-cycle statements. These extended limits are available for use in the first month of the experiment, in October 2014. There are three impediments to perfect and immediate compliance. First, only 85% of participants in the treatment group see their limits increased —primarily because of the regulatory cap on limit-to-income— and most of this is staggered in the first two calendar months of the experiment. Second, 3% of participants in the control group —whom are excluded from limit increases initiated by the bank— request, and are granted, a limit increase. Finally, starting in month 6 of the experiment, consumers in the treatment group may be reevaluated and have their credit lines increased a second time.

- **Magnitude of the credit shock.** One important strength of the experiment is that the credit shocks under study are quantitatively large —about 140% of monthly income for the typical consumer. However, the magnitude of the limit change conditional on a limit increase, \( \Delta L_i \mid \Delta L_i > 0 \), is

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7The primary determinant of who gets limits first in these 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 second limit increase for the treatment group after month 6 reflects the expiration of the timing rules built into the bank’s decision rule. With regard to the limit increases requested by the control group, we could assume that a similar percentage in the treatment group would request and be granted credit line extensions, given that the assignment to treatment is randomized. I interpret those that request manual increases as always-takers and those that would bounce back from downstream underwriting processes as never-takers, and the rest as compliers.
not random, and could potentially be correlated with consumer characteristics, such as income and preexisting limit. Due to this concern, as well as the potential endogeneity in the timing of the limit increases, the analysis restricts the amount of variation used to the assignment of a participant to control versus treatment group \( Z_i \) —which is fixed at the onset— only.

- \textbf{Duration of experiment.} The experimental timeframe concludes after 9 months in June 2015 when the control group is allowed to proceed downstream in the underwriting process. At this point, the control group is processed by potentially different underwriting parameters, and credit may be extended to some but not necessarily to all. Therefore, similar to \cite{Parker2013}'s tax rebate study, the experimental intervention could be interpreted as randomizing the timing of the limit increases; however, in contrast to that study, not everyone in the control group receives a limit increase after the conclusion of the experimental timeframe, therefore the experiment also creates long-run differences in credit availability. I base my analysis primarily on the 9 month experimental timeframe between October 2014 and June 2015, but also discuss long-run estimates.

- \textbf{Information and salience.} The line extensions are automatic, therefore initiated and pushed by the issuer, not requested by the customer. Subjects in the treatment group are notified through their preferred method of notification (e.g., phone call or text messages), as is typical in all limit increases, but can also learn about the limit extension through their credit card statement, or through purchase receipts after they’ve used their card in-store. Importantly, 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.

- \textbf{Predictability and anticipation.} The typical automatic line increase is a relatively infrequent and idiosyncratic event with no public or bank-intermediated signal preannouncing its arrival, in contrast to dividend payments or publicly announced fiscal programs in which the payments timing and size could be known in advance. However, although randomization ensures treatment and control should have similar expectations, if the limit increases are predictable, then participants may partially respond prior to the limit increase once they anticipate the changes, the control group may think a limit increase is more (or less) likely after not receiving one for 9 months, which could later affect their behavior as well.

To address these concerns, I study in detail the predictability of the limit increases in Figure 5 and Table 3. The bank increases about 10% of the limits in any given quarter; however, it is difficult to accurately predict when a limit increase will occur using fundamental account information: a comprehensive kitchen sink logit model with time fixed effects, quadratics in account and borrower characteristics, and the timing rules built into the bank’s decision rule have an in-sample precision (ratio of true positive to predicted positive) of less than 20%. Due to the infrequent nature of limit increases and low precision of forecasts, I assume that limit increases come as a surprise, and the control group not receiving a limit increase for 9 months does not have a material effect on their behavior.
Figure 5: Anticipation of Limit Increases

**Note.** This figure plots the estimated coefficients from a linear probability model in which the left hand side variable is a dummy variable for limit increase at bank, and the explanatory variables are 10 indicators for quarters since the last limit increase. The regression also includes a separate dummy for the customers that have not received a limit increase in the last 10 quarters. I use data on a $N = 10,000$ random subsample of the universe of cardholders (excluding participants) for 16 quarters in 2012 to 2015 totaling to $N \times T = 10,000 \times 16$ individual-quarter observations. The timing rules capture the consideration by the issuer to not change a cardholders credit line if it has been less than a threshold value, making the probability of a line change is much higher in certain months than in others. A close inspection of the timing rules show that relative to the baseline limit increase frequency of about 10% per quarter, the conditional likelihood of a limit increase is significantly elevated two quarters after the last limit increase to more than 15%, but declines quickly and converges to a long-run level of about 5%.

Table 3: Predictability of Limit Increases

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<td>0.148</td>
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<td>0.640</td>
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**Note.** This table reports the results of quarterly kitchen-sink linear probability and logistic regressions where the left hand side variable is a dummy variable for limit increase at bank. In these regressions, I use data on a $N = 10,000$ random subsample of the universe of cardholders (excluding participants) for 16 quarters in 2012 to 2015 totaling to $N \times T = 10,000 \times 16$ individual-quarter observations. As explanatory variables, I use (i) time fixed effects to control for aggregate factors; (ii) basic account characteristics like the level and changes in credit line utilization, as well as indicators for carrying revolving and installment debt balances across pay periods; (iii) the level and the change in the external credit score; (iv) borrower labor income information, e.g., quarterly change in post-tax labor income and the limit-to-income ratio; (v) timing rules built into the bank’s decision rule, i.e. indicators for quarters since last limit increase. Columns (1) to (3) report the results from the linear probability models, and Columns (4) to (6) report the results of the logit models. Columns (1) uses only a constant as a regressor. Columns (4) uses only indicators for months since last limit increase as a regressor. Columns (2) and (5) uses time fixed effects, indicators for months since last limit increase, and basic account characteristics. Columns (3) and (6) uses the same regressors plus variables based on income. As post-tax labor income information is only available for a small fraction of the 10,000 subsample, the number of observations in these regressions are smaller. For the linear probability models I display the estimated in-sample $R^2$, and for the logit models I display the true positive rate as well as the precision to summarize classification accuracy. The true positive rate (TPR, or sensitivity) is defined as the ratio of true positive to condition positive, and precision is defined as the ratio of true positive to predicted positive. The predicted values and classification accuracy are calculated in-sample using a cutoff equal to the empirical limit increase frequency of 9.8% per quarter.
• **Effects on other margins.** The experiment is designed to isolate other confounding factors and additional potential causal pathways. For example, a significant strength of the environment is that it allows us to abstract away from interest rate differences across time as well as across cards. This is due to the 24% APR regulatory cap on the interest rate for revolving balances discussed in Section 2, and shown by Gross and Souleles (2002) and Di Maggio et al. (2017) to have large effects on borrowing and spending. Importantly, the credit cards do not feature teaser revolving rates, and other features of the credit contract, such as non-interest perks, remain unchanged. Moreover, the credit line extensions are not associated with higher income growth one to three years after the limit increase, a pattern I discuss in Appendix Section 3.2. Therefore the shock provides a unique opportunity to focus on the notion of credit constraints as quantity constraints.

### 3.1 Data

To track the impulse responses I use four types of data, the details of which are given in Appendix D.5. Information on credit lines at the bank are taken from end-of-billing cycle statements and include limits $L_{\text{Bank}}$, within-cycle total expenditures $C_{\text{Bank}}$, and debt carried across statement periods $D_{\text{CC, Bank}}$. Credit line debt at the bank can be decomposed to installment credit card debt $D_{\text{Inst, Bank}}$ and revolving credit card debt $D_{\text{Rev, Bank}}$.

Unlike credit bureau data, these variables are defined as balances carried across pay periods, and do not include balances incurred during the credit card cycle. Therefore, they are tied to net spending, defined as total spending expenditures including interest, minus payments made toward end-of-month balances. Revolving debt represents conventional credit line borrowing, i.e. end-of-month balances that are not paid off in full; in comparison, installment balances are directly incurred in-store at the time of purchase to finance expenditures, such as purchases of electronics and appliances, akin to an iPhone installment plan. Consumption expenditures could also be disaggregated to sectoral spending in 18 categories $C_{s, \text{Bank}}$ (e.g., groceries, appliances, health), which are mapped using a unique retailer point-of-sale machine identifier.

I supplement this information with balance sheet and credit bureau variables, which contain information on credit limits across all cards outside the bank and debt owed both inside and outside the bank. I construct four measures to quantify the total debt response: total credit line limits $L_{\text{CC}}$, total credit line debt $D_{\text{CC}}$, total liquid debt $D_{\text{Liq}}$, and total debt $D_{\text{Tot}}$. Liquid debt includes overdraft debt and total liquid debt measures total debt across all types and banks; all are available on a quarterly basis.

Data also contain limited information on the asset side of the balance sheet — in particular, checking account balances at bank $A_{\text{Liq, Bank}}$ and coarse indicators based on the total of liquid assets, including those at the in-house brokerage. Finally, administrative data on monthly post-tax labor income $Y$ are available for a subset of customers whose employers have a direct deposit relationship with the financial institution.\(^8\)

\(^8\)All this information is consolidated for all the accounts a customer has at the bank, matched using a unique citizenship number, and verified using a customer identification number; this ensures perfect match quality. Information regarding credit card variables are end-of-billing-cycle calculations; checking account balances are end-of-calendar-month values; and credit bureau variables are from the date of the query.
Figure 6: The Event Study

Note. The figures display average credit card debt for the treatment and control groups. The x-axis is calendar date. The y-axis is normalized to display the cumulative increase in debt stock relative to the onset of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean. The red dashed and black dash-dot lines indicate the start and end dates of the experimental timeframe.

4 Event Study

I start my empirical analysis by plotting average credit card debt at the bank for the 45,307 participants, by treatment and control groups, in Figure 6. In these event studies, the x-axis is calendar date and the dashed lines indicate 95% confidence intervals for the estimate of the mean.

The top left panel displays the stock of debt on bank credit lines. The y-axis is normalized to display the increase in levels since the onset of the experiment. This additional borrowing variable measures balances carried across pay periods, and the cumulative change relative to the onset is tied to net cumulative spending. The figure shows stable levels for the treatment and control groups prior to the intervention, but a sharp and significant increase in leverage in credit card borrowing by the treatment group after the intervention. This difference is the causal effect of credit availability.

I first focus on the short-run 9-month experimental timeframe. To better understand the magnitudes, Columns (A) to (D) in Table 4 report simple cross-sectional regressions of the form

\[ Y_i = \alpha + \psi X_i + \varepsilon_i \]  

(1)

Columns (A) and (B) in Table 4 report an intent-to-treat specification, where the explanatory variable is an indicator for assignment to the treatment group \( Z_i \). The error \( \varepsilon_i \) accounts for debt growth due to other factors, such as consumption or income shocks. As discussed in Section 3, sampling and randomization ensures orthogonality between assignment to the control group \( Z_i \) and all other variables—in particular, the residual \( \varepsilon_i \)—and allows me to avoid potential omitted variable bias. Therefore, I do not include controls in this baseline specification. The object of interest is then \( \psi^{ITT} \), which is equal to the difference in debt growth between the treatment and control.
### Table 4: Baseline Estimates

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<th>(B)</th>
<th>(C)</th>
<th>(D)</th>
<th>(E)</th>
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<td>$\sum_{j=1}^{3} \phi_{j}^{MP}$</td>
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<td>3,793.6</td>
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<td></td>
<td></td>
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<td>(23.9)</td>
<td>(33.5)</td>
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<td>(33.5)</td>
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<td>0.151</td>
<td>309.4</td>
<td>570.9</td>
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<td>(47.8)</td>
<td>(87.7)</td>
<td>(0.018)</td>
<td>(0.029)</td>
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*Note.* Columns (A) to (D) report the estimated coefficients $\psi$ from the cross-sectional regression $Y_i = a + \psi X_i + \epsilon_i$ using data on $N = 45,307$ participants and $N \times T = 45,307$ observations, with $Z_i$ the instrument. For columns (A) and (B), the explanatory variable is $Z_i$ and for columns (C) and (D), the outcome variable is either the 1-quarter or 3-quarter change in credit lines relative to the onset of the experiment, $\Delta L_i$. Row (1) and (3) uses as the left-hand side variable the change in credit lines. Row (2) and (4) uses as the left-hand side variable the change in credit card debt. Rows (1) to (2) focus on limits and debt on bank lines, and Rows (3) and (4) use limits and debt across all cards, including outside the bank. Columns (E) to (I) report the estimated coefficients from a quarterly panel distributed lag regression $Y_{it} = \sum_{j=1}^{T} \phi_{j} X_{it-j} + f_t + \epsilon_{it}$ using data on $N = 45,307$ participants and $T=3$ quarters, totaling $N \times T = 135,921$ individual-quarter observations, with three instruments $Z_{it}$, constructed by interacting $Z_i$ with quarter dummies. Robust standard errors are corrected for clustering at the individual level. For columns (E) and (F), the explanatory variable is $Z_{it}$ and for columns (G) and (I), the explanatory variable is $\Delta L_{it}$. These columns report the cumulative marginal propensity over a timeframe of $t$ quarters, $\Phi_t = \sum_{j=1}^{T} \phi_j$, except for Column (I), which reports the total response in quarters 2 and 3.

Row (1) uses as the left-hand side variable the change in bank credit lines $\Delta L_{Bank}^{Bank}$ over either a 1-quarter or 3-quarter period, and could be interpreted as the first stage. Row (2) uses as the left-hand side variable the change in bank credit card debt $\Delta D_{CC, Bank}^{Bank}$. Focusing on Row (2), the 9-month difference in credit lines at bank between the treatment and control groups is 3,793 TRY, and the 9-month difference in credit line debt at the bank is 570 TRY. The latter number corresponds to roughly $250, or about one-fifth of average monthly post-tax labor income for participants. A naive, nonparametric $MPC^{\Delta L}$ on bank cards could then be calculated by taking the ratio of the average difference in debt for treatment relative to control with the average increase in credit lines for the treatment on top of the control, which equals 0.15 after 9 months.\footnote{The naive marginal propensity is calculated, $MPC^{\Delta L, Naive} = \frac{E[\Delta D_i | Z_i = 1] - E[\Delta D_i | Z_i = 0]}{E[\Delta L_i | Z_i = 1] - E[\Delta L_i | Z_i = 0]}$.}

To ensure that the results do not represent mere balance shifting, rows (3) and (4) in Table 4 measure the effect on total credit lines and total credit card debt across all banks respectively. At the onset of the
experiment, 56% of participants had credit cards at other banks, and 47% were carrying balances on other bank cards. Column (B) reports intent-to-treat estimates after 9 months, pointing to a difference in limits of 3551 TRY, and a difference in credit card debt of 518 TRY, implying that 7% of the difference in limits and 10% of the increase in credit card debt is offset by adjustments on non-bank cards. For the rest of the paper, I drop the total superscript when I describe total debt $D_{CC}$ and limits $L_{CC}$ across cards at all banks.

To estimate a treatment effect and obtain a value interpretable as marginal propensity, I use as the explanatory variable in Equation (1) the change in credit lines $\Delta L_{CC}^i$, again over either a 1-quarter or a 3-quarter period. The key instrument $Z_i$ limits the amount of variation used to the assignment of control versus treatment group only. In contrast, estimating Equation (2) by ordinary least squares identifies the effect of credit availability from variation in both assignment to treatment group and the timing and magnitude of the limit increase. However, although the assignment of an individual to the treatment $Z_i$ is random, the variation in the magnitude of the limit change $\Delta L_i | \Delta L_i > 0$, as well as the timing of the limit increase (i.e., when a particular consumer is pushed the limit increases), although possibly uncorrelated with error $\epsilon_{it}$, is not random, and could be correlated with consumer characteristics that determine spending and borrowing decisions.

The object of interest is then $\psi^{MP}$, the instrumental variables estimate of the local average treatment effect —i.e., the effect of an additional unit of credit on participants for whom the credit limit changes— reported in columns (C) and (D). Similar to above, rows (3) and (4) use total debt and limits across all banks. The results show that a unit increase in total credit lines across all cards during the experimental timeframe is accompanied by an increase in total credit card debt by 10.8 cents after 1 quarter and by 14.6 cents after 3 quarters, and these responses are highly statistically significant.

Finally, it is also possible that some participants in the treatment group receive multiple limit increases during the experimental timeframe. To account for dynamic effects and obtain cumulative impulse responses, I also report the results of a quarterly panel distributed lag regression in columns (E) to (H).

$$Y_{it} = \sum_{j=1}^{T} \phi_j X_{it-j-1} + f_t + \epsilon_{it}$$

(2)

In this panel specification, the explanatory variable is the distributed lag of the quarterly change in credit lines $\Delta L_{it}$, and $f_t$ stands for time fixed effects. Instruments $Z_{it}$ are constructed by interacting $Z_i$ with quarter dummies, and similar to above, restrict the variation used to the ex-ante assignment to control versus treatment group at a particular quarter. Consistent with the intent-to-treat specification, the coefficient $\phi_1$ measures the effect on the dependent variable during the first quarter of the limit increase, and the partial autocorrelation coefficients $\phi_2, \ldots, \phi_T$ measure the additional responses. This regression uses data on 45,307 consumer cardholders for the experimental timeframe of $T=3$ quarters.

---

1023% of the participants see their limits increase over the 3-quarter experimental timeframe, and this number is 3.5% lower for the treatment group. The fraction of cardholders carrying balances on cards outside the bank decreases by 0.3% for the treatment group after 3 quarters, however this difference is not statistically significant. Appendix Table A.3 restricts the sample to cardholders that do not have a banking relationship with any other institution at the onset of the experiment and documents that the response is quantitatively consistent and statistically significant for this group.
totaling to \( NT = 135,921 \) participant-quarter observations. Robust standard errors are corrected for clustering at the individual level. The object of interest is the marginal propensity \( \Phi_t = \sum_{j=1}^{T} \phi_j \), which is defined as the cumulative impulse response of the treatment on top of the control over a time frame of \( T \) quarters.

Columns (G) to (H) in Table 4 display the results. For brevity, I report the cumulative response \( \Phi_t \), which gives the impulse response to a unit increase in credit availability. Broadly compatible with the cross-sectional marginal propensity specification, the results show that a unit increase in total credit lines increases total credit card debt by 10.6 cents after the first quarter and by 15.9 cents after 3 quarters. Debt rises sharply and significantly in the first quarter following a credit limit increase and subsequent marginal coefficients decline in significance; however, there is still evidence that debt changes beyond the quarter of the increase in limits.

4.1 Balance sheet effects and long-run estimates

In order to understand the effects on the broad balance sheet and analyze long-run responses, Table 5 reports the effects on additional liabilities and assets, both in the short-run and the long-run. Similar to the baseline specification, in this table Columns (A) to (D) report simple cross-sectional intent-to-treat estimates \( \psi^{ITT} \). Columns (E) to (J) report the results from a panel distributed lag regression, as in Equation (2), using data on \( N = 45,307 \) participants and \( T=12 \) quarters, totaling to \( N \times T = 543,684 \) individual-quarter observations. The event studies for these additional variables are displayed in Appendix Table 7.

Similar to what is reported in Table 4, Rows (1) and (2) use as the left-hand-side variable the change in limits and change in debt across credit lines across all banks. First focusing on the long-run responses, I find that the cumulative marginal propensity is 24.5 cents on the dollar after 2 years and 28.1 cents on the dollar after 3 years. This impulse response is displayed in Figure 8. When the credit line is relaxed, consumers continue spending and monotonically leveraging up beyond the experimental timeframe, with statistically significant effects extending to the second and third years, with about one-third and two-thirds of the 3-year cumulative response coming after the first quarter and first year, respectively.

The 3-quarter response in this long-run specification that also use the variation in the second experiment is broadly consistent with the estimates reported in the short-run specification in Table 4. Importantly, for the 3-quarter timeframe of the experiment, as well as on a 3-year horizon, the cumulative response of the treatment group does not exhibit a reversal and does not exhibit hump-shaped patterns, implying that credit does not shift the timing of the spending, but has long-run effects.

Row (3) uses as left-hand-side variable the change in bank installment debt, which is directly tied to cumulative net in-store installment expenditures. Row (4) uses as left-hand variable net liquid debt, defined as the sum of credit card debt and overdraft debt both inside and outside the bank, minus liquid assets at the bank, and finds broadly compatible results. As the credit shock may trigger

\[11^{32\%} \text{ of the participants carry overdraft debt and } 41\% \text{ have balances in checking accounts at the onset. I find that the increase in credit card debt is not offset by a reduction in overdraft debt or an accumulation of assets at the bank. In Appendix Table 5, I report results that use as left-hand-side variable total debt, which includes illiquid debt such as personal loans,} \]
Figure 7: The Event Study: Balance Sheet Effects

Note. Figures plot the levels of covariates for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal zero at the onset of the experiment in September 2014.
Table 5: Balance Sheet Effects and Long-run Estimates

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<th>(H)</th>
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<td>$\gamma_{\text{ITT}}^{\text{T} \times \text{q}}$</td>
<td>$\gamma_{\text{ITT}}^{\text{ITT}}$</td>
<td>$\gamma_{\text{ITT}}^{\text{T} \times \text{12} \times \text{q}}$</td>
<td>$\Phi^{\text{ITT}}_{\text{ST}}$</td>
<td>$\Phi^{\text{ITT}}_{\text{T} \times \text{q}}$</td>
<td>$\Phi^{\text{ITT}}_{\text{T} \times \text{12} \times \text{q}}$</td>
<td>$\sum_{\text{q}}^{\text{q}} \Phi^{\text{ITT}}_{\text{ST}}$</td>
<td>$\sum_{\text{q}}^{12} \Phi^{\text{ITT}}_{\text{T} \times \text{q}}$</td>
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<td>712.1 (167.4)</td>
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<tr>
<td>(2)</td>
<td>$\Delta D^{\text{CC}}$</td>
<td>518.4 (69.6)</td>
<td>386.5 (77.4)</td>
<td>376.3 (80.1)</td>
<td>286.2 (92.6)</td>
<td>0.158 (0.023)</td>
<td>0.183 (0.031)</td>
<td>0.245 (0.045)</td>
<td>0.281 (0.060)</td>
</tr>
<tr>
<td>(3)</td>
<td>$\Delta D^{\text{Inst, Bank}}$</td>
<td>359.5 (30.5)</td>
<td>268.0 (39.6)</td>
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<td>0.111 (0.011)</td>
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<tr>
<td>(4)</td>
<td>$\Delta D^{\text{Liq, Net}}$</td>
<td>501.9 (89.2)</td>
<td>374.2 (98.2)</td>
<td>404.5 (114.0)</td>
<td>362.5 (128.4)</td>
<td>0.147 (0.030)</td>
<td>0.192 (0.040)</td>
<td>0.250 (0.064)</td>
<td>0.276 (0.081)</td>
</tr>
<tr>
<td>(5)</td>
<td>$\Delta I { D^{\text{illiq}} }$</td>
<td>0.018 (0.007)</td>
<td>0.009 (0.007)</td>
<td>0.009 (0.009)</td>
<td>0.013 (0.009)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(6)</td>
<td>$\Delta NPL^{\text{Bank}}$</td>
<td>0.0007 (0.0013)</td>
<td>-0.0004 (0.0017)</td>
<td>0.0009 (0.0026)</td>
<td>0.0018 (0.0031)</td>
<td></td>
<td></td>
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</tr>
</tbody>
</table>


Note. Columns (A) to (D) report the estimated coefficients $\gamma$ from the cross-sectional intent-to-treat regression $Y_i = \alpha + \gamma X_i + \epsilon_i$, using data on $N = 45,307$ participants and $N \times T = 45,307$ observations, where the explanatory is $Z_i$. Columns (E) to (J) report the estimated coefficients from a panel distributed lag regression $Y_{it} = \Phi_{T}^{\text{T} \times \text{q} = 1} f_j X_{it} j + f_t + \epsilon_{it}$, using data on $N = 45,307$ participants and 12 quarters, totaling to $N \times T = 543,684$ individual-quarter observations. In this regression, the explanatory variable is $\Delta L_{it}$, with 12 instruments $Z_{it}$ constructed by interacting $Z_i$ with quarter dummies. Robust standard errors are corrected for clustering at the individual level. Columns (E) to (H) report the cumulative marginal propensity over a timeframe of $T$ quarters, $\Phi_T = \sum_{j=1}^{T} \phi_j$, and Columns (I) and (J) report the total response in years 2 and 3. Rows (1) and (2) use as the left-hand-side variable the change in limits and change in debt across credit lines across all banks. Row (3) uses as left-hand variable the change in bank installment debt, which is directly tied to cumulative net in-store installment expenditures. Row (4) uses as left-hand variable net liquid debt, defined as the sum of credit card debt and overdraft debt both inside and outside the bank, minus checking assets at the bank. Row (5) uses an indicator for having illiquid debt, $\Delta I \{ D^{\text{illiq}} \}$. Row (6) uses an indicator for falling into 90+ status across all of the accounts a participant has at the bank.

adjustment on the extensive margin in illiquid loans, I make simple comparisons between treatment and control with respect to an indicator for having illiquid debt, $\Delta I \{ D^{\text{illiq}} \}$, reported in Row (5). I find that the treatment group is about 1% more likely to have illiquid debt at the end of the experimental timeframe; however, I am not able to reject the null of no balance sheet effects with respect to this aspect in the long-run.

As the credit line contract contains the option to default, an excess sensitivity of borrowing to credit availability may reflect a high propensity to fall into delinquent status. A credit account is classified by the bank as nonperforming if payments of outstanding balances are past due by 90 days or more. Row (6) uses an indicator for falling into 90+ status across all of the accounts a participant has at the auto loans, and mortgages, both inside and outside the bank. Although the estimated coefficient for total debt is significantly larger than that of liquid debt only, the estimates are fairly imprecise and the standard errors are too large to make an inference.
Figure 8: Cumulative Marginal Propensity

Note. This figure reports the cumulative marginal propensity over a timeframe of $\tau$ quarters, $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Coefficients are estimated using a panel distributed lag regression $Y_{it} = \sum_{j=1}^{T} \phi_j X_{it-j-1} + f_t + \epsilon_{it}$ using data on $N = 45,307$ participants and 12 quarters, totaling to $N \times T = 543,684$ individual-quarter observations. The left-hand-side variable is the quarterly change in credit line debt. The explanatory variable is $\Delta L_{it}$, with 12 instruments $Z_{it}$ constructed by interacting $Z_i$ with quarter dummies. Robust standard errors are corrected for clustering at the individual level.

bank. After 36 months, I find that participants are only 0.18% more likely to fall into 90+ status with $p=0.57$, indicating that there exists no economically or statistically significant difference between the two groups with respect to delinquencies. Similar findings apply to late payments, as well as loan restructurings. The lack of a detectable difference in delinquencies is not surprising, however, as the experiment only creates a small long-run difference in credit availability.

5 Heterogeneity by disposable resources

A clear picture emerging from the previous section is that a pure shock to credit availability has substantial effects on the borrowing and spending behavior of participants. However, the average responses mask substantial heterogeneity and do not distinguish whether spending increases across the distance to limit and cash-on-hand distribution, or the response is driven by part of the sample that has low liquidity and binding constraints. As the experimental variation in $Z$ is orthogonal to participant characteristics and ex-ante balance sheet positions, I am able to examine how different consumers respond to the credit shock differently.

I estimate the heterogeneity of the response by disposable resources, with a particular focus on behavioral heterogeneity with respect to consumers who are constrained versus unconstrained in the conventional sense. I use two measures. First, consistent with models in which precautionary mechanisms are in play and distance-to-limit is key, I use total credit line utilization, defined as the ratio of total credit card debt $\Delta D^{CC}$ to the total credit card limit $\Delta L^{CC}$. Second, I use coarse classifications based on the end-of-month balances of total assets at the in-house brokerage. For each of these var-
ables, individuals are sorted by their ex-ante values and placed into $K$ bins. I then estimate a nested variant of the dynamic distributed lag model in Equation (2),

\[
y_{it} = \sum_{k=1}^{K} \sum_{j=1}^{T} \phi_{j}^{k} \times X_{it-j-1} \times B^{k} + f_{kt} + \epsilon_{it}
\]

where $y_{it}$ represents the change in total credit line debt, $f_{kt}$ stands for quarter-bin fixed effects, and $X_{it} = \Delta L_{it}$, the total change in credit lines, with $X_{it} \times B^{k}$ instrumented using $Z_{it} \times B^{k}$. Table 6 then displays the cumulative impulse response of the treatment on top of the control after 3 quarters, $\Phi_{3q}^{k}$. In this table $D_{CC}^{CC}$ stands for total credit line debt across all banks, and $D_{Inst, Bank}^{Inst}$ stands for installment component of credit line debt at bank respectively. For reference, I also display the simple cross-sectional intent-to-treat estimates $\psi_{3q}^{ITT}$ of the change in total limits and total credit line debt for each group, as well as the level of available credit across all cards $L - D_{CC}^{CC}$ they have at the onset.

The panel on the left reports the $MP_{D_{L}}$ heterogeneity by the total credit line utilization measure at the onset. Cardholders who were not carrying any balances across pay periods are in the first bin. Those that were carrying some balances but were using less than 25% is in the second bin $B_{2}$, and so on. This utilization measure directly identifies cardholders for whom the credit constraint is binding and yields sharp results.

Table 6 points to substantial heterogeneity based on a consumers’ disposable resources. Unsurprisingly, I find that the consumers whom are closer to their constraints give larger responses. For example, those with utilization rates above 75% spend roughly 60 cents on the dollar, which corresponds to about four times the average response. Although these consumers appear close to a corner solution to their intertemporal problem, therefore the potential value of borrowing and spending may potentially exceed that of keeping available credit, their response is significantly lower than 1 —the 95% confidence interval for the response of the constrained lies between 0.36 and 0.84, so we can reject the null of strict hand-to-mouth behavior.

The sensitivity to credit is not confined to customers who are close to their constraints, and perhaps surprisingly, the propensity to spend out of credit remains large even for unconstrained consumers far away from the limit. For example, those that carry some balances but use less than 25% of their limits across all cards accumulate 5.8 cents of debt per unit of credit line increase. This utilization group, on average, had available credit of more than 10,000 TRY at the onset and sees an increase in limits of 4,404 TRY on top of the control, which is accompanied by an increase in debt of 178 TRY. Roughly 85% of this group (including the control group) have available credit at the onset that is larger than the new limits they receive. The adjacent bin —those using between 25% and 50% of their total credit lines— accumulate about 16 cents of debt on the dollar, roughly equal to the average response. This group also has substantial available credit relative to the limit increase they receive.

The short-run simple cross-sectional intent-to-treat estimates are obtained using a nested variant of Equation (1), $y_{i} = \sum_{k=1}^{K} \psi^{k} \times X_{i} \times B^{k} + f_{k} + \epsilon_{i}$, where $y_{i}$ represents either the change in credit card limits or total credit line debt, $f_{k}$ stands for bin fixed effects, and $X_{i} = Z_{i}$, the key instrument.

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Table 6: Heterogeneity by Disposable Resources

<table>
<thead>
<tr>
<th></th>
<th>(A)</th>
<th>(B)</th>
<th>(C)</th>
<th>(D)</th>
<th>(E)</th>
<th>(F)</th>
<th>(G)</th>
<th>(H)</th>
<th>(I)</th>
<th>(J)</th>
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<tbody>
<tr>
<td>B_k</td>
<td>p_k</td>
<td>\psi_{3q}</td>
<td>\psi_{3q}</td>
<td>\psi_{3q}</td>
<td>\Phi_{3q}</td>
<td>\Phi_{3q}</td>
<td>B_k</td>
<td>p_k</td>
<td>\psi_{3q}</td>
<td>\psi_{3q}</td>
</tr>
<tr>
<td>D_{LCC} / L</td>
<td>L – D_{CC}</td>
<td>\Delta L_{CC}</td>
<td>\Delta D_{CC}</td>
<td>\Delta D_{Inst}</td>
<td>\Delta D_{CC}</td>
<td>\Delta D_{Inst}</td>
<td>A_{Tot}</td>
<td>L – D_{CC}</td>
<td>\Delta L_{CC}</td>
<td>\Delta D_{CC}</td>
</tr>
<tr>
<td>(1)</td>
<td>0</td>
<td>0.06</td>
<td>4,840.3</td>
<td>(223.9)</td>
<td>3,659.9</td>
<td>(129.3)</td>
<td>324.6</td>
<td>(67.1)</td>
<td>0.099</td>
<td>(0.037)</td>
</tr>
<tr>
<td>(2)</td>
<td>0.25</td>
<td>0.41</td>
<td>10,108.4</td>
<td>(226.7)</td>
<td>4,404.3</td>
<td>(132.3)</td>
<td>178.5</td>
<td>(36.7)</td>
<td>0.058</td>
<td>(0.027)</td>
</tr>
<tr>
<td>(3)</td>
<td>0.50</td>
<td>0.24</td>
<td>6,319.0</td>
<td>(157.7)</td>
<td>3,246.3</td>
<td>(139.0)</td>
<td>478.3</td>
<td>(67.0)</td>
<td>0.158</td>
<td>(0.037)</td>
</tr>
<tr>
<td>(4)</td>
<td>0.75</td>
<td>0.17</td>
<td>3,564.4</td>
<td>(95.2)</td>
<td>2,716.4</td>
<td>(154.7)</td>
<td>843.5</td>
<td>(106.1)</td>
<td>0.332</td>
<td>(0.078)</td>
</tr>
<tr>
<td>(5)</td>
<td>1</td>
<td>0.12</td>
<td>1,161.2</td>
<td>(58.2)</td>
<td>2,412.4</td>
<td>(185.5)</td>
<td>1,425.7</td>
<td>(217.3)</td>
<td>0.603</td>
<td>(0.121)</td>
</tr>
</tbody>
</table>

Note. This table reports the heterogeneity of the response by credit line utilization (left panel) and liquid assets (right panel). Individuals are sorted by their ex-ante values and placed into K bins, where B_k denotes these bins and P_k indicates the fraction of participants in each bin. In this table D_{CC} stands for total credit line debt across all banks; and D_{Inst, Bank} stands for installment component of credit line debt at bank. The panel on the left reports the cross-sectional distribution of the heterogeneity by the total credit line utilization measure at the onset. The panel on the right uses coarse classifications based on the six-month average of the end-of-month balances of total assets. I then estimate a nested variant of the dynamic distributed lag model Equation (2), \[ Y_{it} = \sum_{k=1}^{K} \sum_{j=1}^{T} \phi_k^{ij} \cdot X_{iit} \cdot \Phi_{3q} \cdot B_k + f_{it} + \epsilon_{it}, \] where Y_{it} represents the change in total credit line debt, f_{it} stands for quarter-bin fixed effects, and X_{iit} = A_{iit} the total change in credit lines, with X_{iit} \cdot B_k is instrumented using \( Z_{it} \cdot B_k \). This regression uses data on 45,307 consumer cardholders for the experimental timeframe of T=3 quarters, totaling to N \times T = 135,921 participant-quarter observations. Robust standard errors are corrected for clustering at the individual level. Columns (D) to (E), and (I) to (J) displays the cumulative \( MPC_{DCC} \) after 3 quarters, \( \Phi_{3q} = \sum_{j=1}^{T} \phi_j^{ij} \). For reference, I also display the simple cross-sectional intent-to-treat estimates \( \psi_{3q}^{ITT} \) of the change in total limits, change in total credit line debt, and change in installment debt at bank for each group, as well as the level of available credit across all cards \( L – D_{CC} \) they have at the onset.
These heterogeneous \( MPC^{DL} \) are broadly consistent with research cited in the literature review that document a sensitivity to credit among consumers with low liquidity. Although the \( MPC^{DL} \) is not directly proportional to ex-ante utilization as would be predicted by models featuring simple heuristics, the distance to limit at the onset clearly has very strong predictive power for explaining the magnitude of the response.\(^{13}\) I decisively reject the null hypothesis that the response is equal between the low and the high utilization groups (\( p < 0.001 \)).

Second, I measure the heterogeneity of the response by coarse categories based on the 6-month average of total holdings of liquid assets, which also include the assets at the in-house brokerage that are in liquid form, including checking accounts, certificates of deposits, as well as holdings of stocks, bonds, and investment funds, but excluding holdings in retirement accounts. Additional details are given in Appendix D.5. Customers are placed into five bins. Participants with no active accounts with assets (38\%) are placed in the first bin. Those with assets are grouped by cuts at 250 TRY, 2,500 TRY and 25,000 TRY, which roughly correspond to about one-tenth, one, and ten times the average labor income for the participants. 11\% and 4\% of the participants are in bins (4) and (5), respectively.

The right panel in Table 6 displays the cross-sectional distribution of the cumulative marginal propensity after 3 quarters. Similar to above, the table points to substantial heterogeneity in the responses. For example, participants with an account but next to no assets at the end-of-the month on average spend 26 cents on the dollar. For the adjacent group, those with an average level of assets between 250 TRY and 2,500 TRY, the marginal propensity drops to 12 cents on the dollar. By most definitions, these two groups consist of participants whom are liquidity constrained and are unable to spend out of resources that will accrue to them in the future.

The effect of credit, however, extends even to those with a sizable buffer of assets. Participants in the next group, those with between 2,500 TRY and 25,000 TRY in liquid assets, spend 9 cents on the dollar out of the credit extension. Roughly 30\% of this group have revolving balances (excluding installments) at 24\% APR, so they could have paid some of their existing debt back using assets. The increase in credit card debt for this group is 485 TRY, and in comparison, their average available credit at the onset is 9,804 TRY. As these are participants who also had at least 2,500 TRY of liquid assets, they have the disposable resources ex-ante, on average, in both credit and liquid assets to finance the new increase in debt.

For the participants in bin (5), who have more than 25,000 TRY as total assets, I am unable to reject the null that credit has no effect on their borrowing and spending behavior. The estimated effect for this group is 4 cents on the dollar. This group, on average, had about four times the disposable resources

\(^{13}\)Although the findings are directionally compatible with these models — the response is larger for customers with higher utilization — two direct predictions of these models are rejected in the data. First, this theory predicts that the estimated \( MPC^{DL} \) should equal ex-ante utilization of roughly 0.35 and the cumulative change in utilization should be zero. Second, \( MPC^{DL} \) heterogeneity as a function of utilization should be proportional to ex-ante utilization, lying on the 45-degree line. I decisively reject the hypotheses \( H_0 : \frac{\Delta C}{C_L} = 0.35 \), \( H_0 : \Delta C^{\hat{D}L} = 0 \), and \( H_0 : \frac{\Delta C^{\hat{D}L}}{C_L} = \frac{\Delta C}{C_L} \), all with \( p < 0.001 \). In particular, under the extended limits the utilization rate drops, as the average \( MPC^{DL} \) is significantly smaller than the ex-ante utilization of 0.35, and the heterogeneity of the estimated effect by ex-ante utilization is significantly flatter than the 45-degree line: those at their constraints reduce utilization and those not utilizing at the onset do respond. For example, someone who starts at 75\% utilization or above at the onset of the experiment should have an \( MPC^{DL} \) of 0.75 or higher. Similarly, someone who start at 0\% utilization should exhibit no sensitivity to credit. This is in contrast to what is documented in Table 6.
including available credit at the onset relative to the limit increase they get over the experiment — and only 5% of this group had a binding constraint in the year prior — but 9% still face a binding constraint — use more than three-quarters of their credit line — in the three years down the line after the onset. The latter number is about 43% for the second bin and 20% for the fourth bin. I nevertheless reject the null hypothesis that the response is equal between the second bin and the bottom bin. ($p=0.012$).

6 What is the mechanism that generates the sensitivity?

6.1 Conceptual framework

To put things into perspective, consider the certainty equivalent version of the benchmark permanent income model, in which consumption admits an explicit formula and is an affine function of assets $A_0$ and the discounted present value of permanent income $Y^P$,

$$C_0^\Pi = \frac{R}{1+R} \left[ A_0 + Y_0 + \sum_{t=1}^{\infty} \left( \frac{1}{1+R} \right)^t \mathbb{E}_0^\Pi \left[ Y^P \right] \right]$$

In the permanent income model, consumption is a martingale, debt is a unit root process — i.e., ex-ante debt is the best predictor of future debt — and new borrowing presages income growth as it equals the discounted present value of expected future increases in permanent income $\mathbb{E}_0^\Pi \left[ \Delta Y^P \right]$. Consumption depends only on the first moment of the present value of endowment, and precautionary motives are absent. In this model, although credit shocks could affect consumer behavior through a change in the interest rate $R$ that entails present-value effects, a change to the credit limit that does not entail a wealth effect has no effect on consumption behavior. Hence the $MPC^DL$ is zero: $\frac{\Delta C^0}{\Delta L} = 0$.

In models in which $\frac{\Delta C^0}{\Delta L}$ is positive, it will be related to the concept of $MPC$ via the following identity:

$$\frac{\Delta C^0}{\Delta L} = \frac{\Delta C^0}{\Delta A_0} - \frac{R}{1+R} \frac{\Delta C^0}{\Delta Y^P}$$

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$$\frac{\Delta C^0}{\Delta L} = \frac{\Delta C^0}{\Delta A_0} - \frac{R}{1+R} \frac{\Delta C^0}{\Delta Y^P}$$

Intuitively, extended credit limits could be used to permanently increase assets at a periodic cost proportional to the annuity factor, see [Guerrieri and Lorenzoni (2015)] for a similar proof. Therefore, consumption will be sensitive to a change in credit limits if consumption is sensitive to current resources, but not if consumption is highly sensitive to future resources. As a special case, under the permanent income model, $\frac{\Delta C^0}{\Delta Y^P} = 1$ and $\frac{\Delta C^0}{\Delta A_0} = \frac{R}{1+R}$. Two immediate corollaries of this result are, first, the $MPC^DL$ will bound below $MPC$, hence a high $MPC$ need not imply high $MPC^DL$; and second, the $MPC$ will only equal $MPC^DL$ if the cost of borrowing is zero or if the consumer is unable to respond at all to changes in future income or transfers; $\frac{\Delta C^0}{\Delta L} = \frac{\Delta C^0}{\Delta A_0}$ only if $R = 0$ or if $\frac{\Delta C^0}{\Delta Y^P} = 0$.

The empirical results show that the moment $\frac{\Delta C^0}{\Delta A_0}$ is positive for a broad share of the participant population, including consumers with substantial credit availability or a meaningful buffer of liquid assets.

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14 Notice that $C_0^0(A_0, Y^P; L) = C_0^0(A_0 + \Delta, Y^P - \Delta + \frac{R}{1+R}(L - \Delta))$, as $C + \frac{A'}{1+R} = A + Y^P$ if and only if $C + \frac{A' + \Delta}{1+R} = (A + \Delta) + (Y^P - \frac{R}{1+R}\Delta)$; and $A_0 > -L$ if and only if $A_0 + \Delta > -(L - \Delta)$. 

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25
This finding is also not compatible with two-agent models in which there exists a group of savers with positive liquid wealth and very low consumption responses and another group of hand-to-mouth spenders with no liquid wealth who display strong consumption responses. For example, models featuring consumption commitments as in Chetty and Szeidl (2010), or second generation spender-saver models, as in Kaplan and Violante (2014), can explain why consumers hold few liquid assets and frequently face binding constraints, however can not explain why consumers with assets would spend out of credit.  

However, alternative explanations are possible. One interpretation of this finding embraces precautionary savings due to the possibility of constraints binding in the future, albeit not binding currently. The other class of alternatives invokes behavioral ingredients to explain the increase in borrowing and spending and focuses on explanations based on myopic or dynamically inconsistent behavior. I now bring in novel data on the composition and the dynamics of the response and discuss each of the potential explanations in turn.

6.2 How do consumers spend the credit?

First, I turn to the behavior of unconstrained consumers, who use the additional credit to pull consumption forward. Although an across-the-board increase in spending is also compatible with models featuring precautionary savings, an alternative explanation is myopic behavior. For example, in models in which temptation or instantaneous gratification are in play, consumers who are not at their constraints may exhibit a strong preference for consuming now and deriving utility in the moment, and the increase in borrowing may be directed toward enticing non-necessities and goods for immediate consumption, such as expenditures on restaurants, cosmetics, recreation, and hobbies.

Similarly, even if borrowers do not have a taste for immediate gratification with respect to consumption, they may make suboptimal repayment choices and borrow due to dynamic inconsistencies with respect to repayment behavior— for example, because they naively underestimate the probability that they will revolve their end-of-billing cycle balances, as in Heidhues and Köszegi (2010). This latter model interprets credit line extensions as seemingly cheap credit (e.g., within-month liquidity with a float) that allows for flexible repayment in a way that induces the borrower to unexpectedly change her behavior at will (e.g., revolve rather than pay end-of-month balances in full), however accompanied by a 24% APR interest rate for falling behind schedule.

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15Similarly, in the simple spender-saver framework of Hall and Mishkin (1982) and Campbell and Mankiw (1989), recently used by Eggertsson and Krugman (2012) and Korinek and Simsek (2015) to study household leveraging dynamics, the sensitivity to credit is driven by a small fraction of consumers who are either myopic (e.g., either impatient, as in Krusell and Smith Jr (1998)); or forward looking with a currently binding credit constraint. Therefore a shock to credit availability does not affect the behavior of unconstrained consumers; \( \frac{\Delta C}{\Delta t} \mid A_0 > 0 = 0 \)

16In their notation, suppose the consumer spends \( C \) up to a limit \( L \) at \( t = 0 \) (within the month, prior to seeing the end-of-billing cycle statement) and can either make payments \( q \) in \( t = 1 \) after seeing the end-of-billing cycle balance, or payments \( k \) in \( t = 2 \) after having revoked some of it. From the perspective of the spender, \( u_0 = c - k(q) - k(r) \), but from the perspective of consumer that sees the balance, \( u_1 = -k(q) - \beta k(r) \). Self 0 expects to pay off the balance and get a float with \( \hat{r} = 0 \), hence, trades off \( C \) with \( k(q) \). However, the repayment behavior is determined by self 1’s preferences via \( k'(q) = \beta k'(r) \), and a non-sophisticated borrower whom mis-predicts her own \( \beta \), hence repayment behavior, accepting a cheap and front-loaded repayment schedule, but ends-up choosing an expensive back-loaded schedule.
Figure 9: The Event Study: Spending and Borrowing

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

Figure 10: (Gross) Spending by Category

Note. Figure plots the categorical composition of the spending response to credit. First, I estimate the response of categorical expenditure for each of 18 spending categories using the distributed lag Equation (2) in Section 4 on a sample of 45,307 consumer cardholders. The outcome variable is categorical spending in sector $C_s$. The panel distributed lag regression $Y_{it} = \sum_{j=1}^{T} \phi_j X_{it-j-1} + f_t + \epsilon_{it}$ uses data on $N = 45,307$ participants and $T=3$ quarters, totaling to $N \times T = 135,921$ individual-quarter observations, with three instruments $Z_{it}$, constructed by interacting $Z_i$ with quarter dummies. As spending is a flow variable linked to the change in debt via an accounting identity, I analyze it in levels. Robust standard errors are corrected for clustering at the individual level. The explanatory variable is $\Delta L_{it}^{Bank}$. These columns report the cumulative response after 3 quarters. The upper and lower shadows indicate the 99.8% confidence intervals for the estimate of the mean, accounting for Bonferroni correction. The red bars represent the total share of spending in non-durables, durables and services respectively. See text for discussion.
To investigate this possibility, Appendix Table 9 displays event studies decomposing the spending and borrowing response to installment and regular purchases; and Table 5 and Table 6 decompose the response of installment debt at bank separately for average and unconstrained customers. The increase in installment debt accounts for roughly 60% of the average increase in credit line debt at bank, and most of the response for the unconstrained. Hence the main type of debt contract that accounts for the consumers use of debt and spending has simple predetermined payment schedules (e.g. the same nominal payment every month), akin to Amazon.com or iPhone installment plans. These types of contracts are often used to finance smaller consumer durables (e.g., electronics, clothing, appliances, furniture) and services (e.g., health and education expenses). However, they cannot be used to finance expenditures on strictly nondurable goods, such as food, gas, restaurants, and recreation.

In contrast to the model of Heidhues and Kőszegi (2010), these contracts do not give borrowers an opportunity to exhibit dynamic inconsistencies with the eventuality of the latter information set by revising the choices they made initially; on the contrary, consumers forgo repayment flexibility. Therefore the predominant component of borrowing, and in particular for unconstrained borrowers, is not using the flexible repayment option in a way that induces the borrower to unexpectedly change her behavior at will accompanied by large interest penalties for falling behind schedule. Long-run results corroborate these findings, and shows that consumers lever up primarily using installment type debt contracts with restricted repayment profiles.

To further understand spending patterns, Figure 10 decomposes gross transaction volume into sectoral spending in each of the 18 categories. To estimate the effect on categorical transactions, I use the panel distributed lag marginal propensity specification as in Equation (2), where the left-hand side variable is consumption expenditures in each spending category. As spending is a flow variable linked to the change in debt via an accounting identity, I analyze it in levels. Figure 10 then displays the gross MPC\textsuperscript{DL} for each category, where the red bars correspond to the total increase in spending on nondurables, durables, and services respectively.

Note that these results on transactional spending categories require careful interpretation. Credit cards are used as a means of payment and for within month liquidity float with a benefit of one-to-two per cent per dollar spent. For borrowers who do not carry balances across pay-periods, the gross non-installment transactions are entirely consumption pulled forward from the end-of-the-month. When analyzing gross spending, it is not possible to know which of these transactions correspond to an increase in borrowing. My main empirical analysis is robust to this issue, and uses as left-hand-side variable the increase in credit card debt, which is tied to the net spending after factoring in payments.

The left panel of Figure 10 displays this gross MPC\textsuperscript{DL} which is due to installment spending. The major category in which the increases in installment transactions are reflected is goods for future consumption with durability, such as clothing, furniture, electronics, and appliances. The increase in durables expenditures account for 45% of the total increase in installment transactions. About 30% of the in-

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17There could be many reasons as to why consumers choose installment plans over revolving debt, and use loans with preplanned payment schedules, akin to mortgages and auto loans. First, the loan could be free, as in an iPhone installment program. Second, cognitive limitations or budgeting heuristics may favor repayment schedule with predetermined periodic pay. Third, their inflexible nature could serve as a commitment contract to pay-back debt on time.
crease in spending is on services, such as insurance, tourism, health, and education. Discretionary nondurable spending, such as hobbies, cosmetics, recreation, and stationery, constitutes an economically insignificant fraction of the response.

Similarly, the panel on the right displays the gross $MPC_{D}^{\Delta L}$ that is due to non-installment spending. The participants in the treatment group, compared with the control, increase their revolving debt on average by 211 TRY, and this debt is accumulated by making transactions worth 1,123 TRY. Decomposing these 1,123 TRY worth of transactions into sectoral categories, I find that 36% of the total increase in transactions is for groceries, which is driven primarily by spending in supermarkets and greengrocers. The rest is evenly accounted by increases in vehicle expenses, which primarily consists of gas, but also includes repairs; and durables and services as discussed above.

In summary, the composition of the response by both debt type and transaction categories show that the increases in borrowing, in particular by the unconstrained participants, do not appear to finance discretionary spending on non-necessities or categories associated with short-sighted behavior such as restaurants, cosmetics, hobbies and recreation. Rather, it is accompanied by an increase in goods with durability and for future consumption; and a small increase in spending on nondurable necessities—such as gas and car repairs—for the more constrained.

6.3 Precautionary savings and the dynamics of binding constraints

Second, I turn to analyzing the dynamics of binding constraints and the behavior of high $MPC_{D}^{\Delta L}$ participants who are trying to avoid close-to-zero consumption. As the classical explanation for the empirical violations of the permanent income hypothesis for consumers that are not currently facing a binding constraint embraces precautionary savings in response to a constraint that may bind in the future, this model predicts that forward-looking consumers will depress consumption and keep a buffer of credit availability to insure themselves against uninsurable income fluctuations, and as the precautionary drop is larger at low liquidity, the $MPC_{D}^{\Delta L}$ will be heterogeneous and larger for consumers near the limit, but positive even for consumers with substantial slack, in line with what is documented in Section 5.

The defining tension in this model is a desire to create a buffer versus the desire to shift consumption forward in time, and the smoking gun for precautionary behavior is a tendency to deaccumulate debt at states with low liquidity. As debt growth creates a wedge between consumption and income, the properties of the consumption rule can be indirectly inferred from the dynamics of debt, using longitudinal data, by analyzing how consumers move within the wealth distribution. In this model

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18 These categories of goods purchased are not perishable, so that consumption and expenditure cannot be equated, a point made by Hayashi (1985). Examples of such services are dental services, taking a vacation, and education. I do not find, however, any hump-shaped patterns in net durable expenditures.

19 Could the response of the participants with a low credit line utilization rate be explained by large purchases which was not feasible under the old limit $L_0$ but is feasible under the extended limit $L_0 + \Delta L$? In Appendix Section B.1, to provide an upper bound for the fraction of the response due to lumpy purchases, I identify 991 participants in the treatment group (2.18% of 45,307 participants) who make at least one categorical installment spending larger than 90% of $L_0$ during the experimental timeframe. I find that excluding these cardholders reduces the average response for unconstrained cardholders by no more than about 15%; hence lumpiness can not explain a significant fraction of the installment debt growth for the unconstrained.

20 For example, in the permanent income model, consumption is a martingale and debt is a unit root process: -ex-ante debt
Figure 11: Leveraging Dynamics

Note. These figures plot two normalized measures of debt levels. In both of these figures the x-axis indicate the calendar date. For visual conciseness, I normalize the month prior to the onset of the experiment as 0, and sort participants into 10 equal-width nonoverlapping bins with respect to their total credit card utilization across all banks. The panel on the right then plots credit line utilization, defined as the ratio of total credit card debt $D_{t}^{CC}$ to the total credit card limit $L_{t}^{CC}$. The panel on the left plots a normalized measure of debt, defined as the ratio of total credit line debt level to $L_{0}^{CC}$, the credit limits just before the onset of the experiment.

there would be little evidence of persistently binding constraints, as consumers should keep a buffer of credit to insure themselves against low consumption. This is in contrast to models featuring extremely impatient borrowers or behavioral factors, where consumers at their limits are expected to increase their leverage or face difficulties committing to a repayment plan to delever.

To graphically illustrate ideas and test this key prediction of models that feature precautionary savings, I plot in Figure 11 two normalized measures of debt levels. In both of these figures the x-axis indicate the calendar date. For visual conciseness, I normalize the month prior to the onset of the experiment as 0. I sort participants into 10 equal-width nonoverlapping bins with respect to their total credit card utilization across all banks. The panel on the right then plots credit line utilization, defined as the ratio of total credit card debt $D_{t}^{CC}$ to the total credit card limit $L_{t}^{CC}$. The panel on the left plots a normalized measure of debt, defined as the ratio of total credit line debt level to $L_{0}^{CC}$. These figures overlap prior to the onset and allow me to directly compare how debt levels evolve at various distance-to-limit values separately for treatment and control groups, and study the dynamics of leveraging and binding constraints under increased credit availability.

The first discernible feature in the figure is the significant predictability of debt levels, in the form of mean-reversion. For example, participants right at their constraints reduce their utilization and is the best predictor of future debt. In contrast, if consumption is depressed near the credit constraint in order to provide a buffer, consumers will then have a target for disposable resources and credit line utilization will display predictability toward the target. Similarly, if there was a fraction of highly impatient consumers whom are unable to pay back debt due to self-control problems, or hand-to-mouth consumers that set consumption equal to disposable resources, these consumers would have a unique footprint: they would remain persistently stuck at their constraints. Hence not all calibrations of the discount factor and risk aversion are compatible with the mean-reversion in debt dynamics.
debt levels significantly under tight limits. Despite having next to no available credit on the credit lines, these consumers do not appear to borrow as much as they could and push themselves to a strict corner solution to their intertemporal problem. In contrast, they generally spend very little time at the limit, binding constraints appear to be a transitory event for the typical consumer lasting no more than a quarter, and most consumers act forward looking and have a tendency to leave a buffer of available credit. Appendix Table A.6 displays the transition matrix for credit line utilization, which corroborate these findings—the behavior of no more than 6.6% of the subjects resembles hand-to-mouth behavior, which would imply a unique footprint of remaining persistently stuck near constraints.

Second, the figures show that the estimated treatment effect for unconstrained consumers is due to the treatment group is leveraging up by increasing spending, whereas, strikingly, the estimated quantitatively large response of consumers at their constraints is partly due to the constrained control group to significantly delever. When granted additional credit, borrowers close to their constraints stop deleveraging and marginally increase the absolute level of their debt. The key mechanism that determines $MPC_D$ levels and consumption dynamics around binding constraints appears to be a precautionary savings effect, by which constrained households foresee a potential limitation to their consumption smoothing ability and delever under tight constraints.\footnote{Note that the evidence on treatment effect heterogeneity based on an priori sample split by disposable resources could be evidence in favor of a large precautionary drop due to liquidity, but is also compatible with behavioral factors that affect both the level of liquidity and the magnitude of the documented sensitivity spuriously. Appendix Table A.5 provides additional splits based on credit line utilization, in which I attempt to isolate situational versus persistent factors that determine liquidity at the onset, and Appendix Table A.6 displays the transition matrix for credit line utilization. I split the participants into 4 groups based on their credit line utilization at the onset versus their average credit line utilization in between months -36 to -13 relative to the onset. For the 30% of the participants who were relatively constrained at the onset (i.e., utilizing more than half of their credit lines), and irrespective of their long-run utilization levels, the estimated response is about 40 cents on the dollar. In contrast, focusing on cardholders who were not at their constraints at the onset (i.e., utilizing less than half of their credit lines), the estimated response is 6 versus 13 cents on the dollar for those with low versus high long-run utilization rates. These findings corroborate the view that current liquidity is an immediate driver of behavior, however constraints that are currently not binding also effect consumption dynamics proportional to the frequency that they bind in the long-run.}

These findings provide direct evidence on buffer-stock behavior, exactly in line with what one might expect from a model as in [Deaton (1991) and Carroll (1997)]. This model predicts that there should be little evidence of persistently binding constraints, as consumers should keep a buffer of credit to insure themselves against low consumption, and the response will primarily be driven by consumers whom are not constrained in the conventional sense. Therefore a significant sensitivity to credit will be estimated broadly across the population, including consumers who are far away from their constraints, as constraints that are currently not binding but may potentially bind in the future effect consumption dynamics.

7 Conclusions

Understanding macroeconomic fluctuations associated with credit booms requires an understanding of the expansion phase itself—to provide realistic microfoundations for the household sector to study the dynamics of household leveraging (e.g., [Eggertsson and Krugman (2012), Guerrieri and Lorenzon (2015)]) as well as for the formulation of fiscal, monetary and macroprudential policy to offset these
fluctuations (e.g., Jappelli and Pistaferri (2014), McKay et al. (2016), and Korinek and Simsek (2015)). This paper constructs a randomized, quantitatively large, and difficult to anticipate shock to only credit availability, and uses this novel identified moment, in the spirit of Nakamura and Steinsson (2018), to test the validity of alternative intertemporal consumption models.

The findings show that a pure shock to credit has a precisely measured and economically large effect on spending, and long-run results indicate that consumer leveraging continues beyond the experimental timeframe, with about one-third and two-thirds of the 3-year cumulative response coming after the first quarter and first year, respectively. The consumers whose spending behavior are sensitive to credit is not confined to a small set of low asset, hand-to-mouth consumers, or those with binding constraints whom are up against their limits: I document that credit expansions are borne broadly across the population, credit has substantial indirect effects, and the propensity to spend out of credit remains quantitatively large for consumers with slack in their borrowing capacity, as well as those with a considerable buffer of assets to finance the marginal increase in spending in cash.

As the experiment is a unique opportunity to study a credit shock that entails no wealth effects, the findings can’t be reconciled with a simple model a la Friedman (1957), in which only wealth matters for spending. Moreover, the heterogeneity of the response shows that simple spender-saver models, as in Campbell and Mankiw (1989) and Eggertsson and Krugman (2012), or models that generate endogenous illiquidity in which consumers hold few liquid assets —such as those that feature consumption commitments as in Chetty and Szeidl (2010), or second generation spender-saver models featuring illiquid assets with high returns, as in Kaplan and Violante (2014)— although can explain why consumers hold few liquid assets and frequently face binding constraints, can not explain the broad based response to credit observed in the data.

Although a quantitatively large sensitivity to credit brings up the possibility of short-sighted behavior as a potential explanation —that consumers accumulate debt that must be repaid by squandering credit in a dysfunctional way or using revolving balances in a dynamically inconsistent manner out of which they are unable to save out of— spending composition and debt contract choice rule out models in which temptation or instantaneous gratification is at play, or models where dynamic inconsistencies with respect to repayment behavior, as in Heidhues and Köszegi (2010), are the main driver of the borrowing response. The longitudinal features of the data provide direct support for a classical explanation that embraces precautionary savings in response to a credit constraint that may bind in the future —where relaxed credit constraints allow to finance spending for future consumption with potentially high marginal benefits, while keeping a buffer of credit availability to guard against and avoid low consumption states.

In future work, it would be worthwhile to directly test the relationship between $\text{MPC}$ and $\text{MPC}^{AL}$ discussed in Section 6.1 as well as other behavioral explanations that could in principle rationalize findings that the experiment here is not designed to address, such as those based on cue-triggered spending. For example, a cue such as the limit increase could raise the marginal utility derived from consumption —as in Laibson (2001) and Bernheim and Rangel (2004)— which would lead to a spontaneous increase in spending— but could also contain information about the future income prospects of the
borrower. Understanding the quantitative importance of these behavioral effects is a task I leave for future research.
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