NBER WORKING PAPER SERIES

CREDIT SMOOTHING

Sean Hundtofte
Arna Olafsson
Michaela Pagel

Working Paper 26354
http://www.nber.org/papers/w26354

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2019

We thank Andreas Fuster, Michael Haliassos, Ori Heffetz, Constantine Yannelis, Scott Nelson, Kyle Herkenhoff, Botond Koszegi, Thomasz Piskorski, Jesse Schreger, Michael Weber, and conference and seminar participants at the NY Fed, Toulouse School of Economics (finance), University of Mannheim (finance), Nottingham University (economics), University of St. Gallen (finance), AEA, the 2018 European Household Finance Conference, CFPB Research Conference, EEA Annual Symposium, Colorado Finance Summit in Vail, and the Workshop on New Consumption Data at Copenhagen University, WFA, and the Consumer Finance: Micro and Macro Approaches Conference for valuable comments and advice. We also thank David Laibson, Peter Maxted, Andrea Repetto, and Jeremy Tobacman for sharing the solution code of their life-cycle model in Laibson et al. (2017). This project has benefitted from funding from the Carlsberg Foundation. We are indebted to Meniga and their data analysts for providing and helping with the data. We also thank Fedra De Angelis Effrem and Andrea Marogg for outstanding research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Credit Smoothing
Sean Hundtofte, Arna Olafsson, and Michaela Pagel
NBER Working Paper No. 26354
October 2019
JEL No. D14,D90,G51

ABSTRACT

Standard economic theory says that unsecured, high-interest, short-term debt — such as borrowing via credit cards and bank overdraft facilities — helps individuals smooth consumption in the event of transitory income shocks. This paper shows that — on average — individuals do not use such borrowing to smooth consumption when they experience a typical transitory income shock of unemployment. Instead, individuals smooth their credit card debt and overdrafts by adjusting consumption. We first use detailed longitudinal information on debit and credit card transactions, account balances, and credit lines from a financial aggregator in Iceland to document that unemployment does not induce a borrowing response at the individual level. We then replicate this finding in a representative sample of U.S. credit card holders, instrumenting local changes in employment using a Bartik (1991)-style instrument. The absence of a borrowing response occurs even when credit supply is ample and liquidity constraints, captured by credit limits, do not bind. Standard economic models predict a strictly countercyclical demand for credit; in contrast, the demand for credit appears to be procyclical which may deepen business cycle fluctuations.

Sean Hundtofte
Better.com and NYU Stern
shundtofte@better.com

Arna Olafsson
Copenhagen Business School
Department of Finance
Solbjerg Plads 3
2000 Frederiksberg
Denmark
ao.fi@cbs.dk

Michaela Pagel
Columbia Business School
3022 Broadway
Uris Hall
New York, NY 10027
and NBER
mpagel@columbia.edu

1 Introduction

How does unsecured, high-interest, short-term borrowing respond to income shocks? Standard consumption models make a clear-cut prediction: if such credit is ever used, then it is used to smooth consumption in the event of adverse transitory income shocks.[1] However, empirical evidence for a strong borrowing response to transitory income shocks has remained scarce. This can partly be explained by a lack of data containing accurate, high-frequency information on both individual credit use and income. Furthermore, disentangling the effects of the demand and supply of credit (adverse income shocks may increase demand but decrease supply) and transitory from permanent income shocks (the former should entice a borrowing response while the latter should not) is difficult.

In this paper, we investigate and quantify how credit card and overdraft borrowing responds to a quintessential transitory income shock due to unemployment. We first use monthly data from a personal finance platform in Iceland (a "financial account aggregator"), containing comprehensive transaction-level information on individual spending, income, account balances, and credit limits, to investigate how consumer debt changes upon temporary job loss. The longitudinal nature of our data allows us to include individual fixed effects in our estimations and thereby control for selection on all time-invariant (un)observed characteristics.

Using the financial aggregator data, we find that, over the average spell of unemployment, individuals reduce their spending, but do not increase their consumer debt holdings substantially, even if they borrow regularly and have sufficient liquidity. More specifically, we do not find an increase in the likelihood to have an overdrawn checking account, in the number of overdrawn accounts, or in the overdraft amount as measured

[1] Unsecured debt or non-collateralized debt refers to loans that are not tied to any asset. In the U.S. and Iceland as well as other developed countries, unsecured debt consists primarily of credit card loans and overdrafts from current accounts.

by interest payments. The data covers the period 2011 to 2017, a time of economic expansion in Iceland over which unemployment was low and generally short-lasting. Unemployment in such circumstances is a quintessential kind of transitory income shock, which we confirm in the data by showing that income of individuals is not permanently affected by job loss. These findings thus suggest that large transitory income shocks are not a major driver of the substantial consumer debt holdings we observe (the baseline probability of holding an overdraft is approximately 50%, i.e., individuals in our sample borrow half the time). This finding is inconsistent with a view of unsecured high-interest borrowing as an important tool for smoothing consumption in the event of adverse transitory income shocks.

We then turn to the U.S. using household credit data from the Federal Reserve Bank of New York and the Equifax Consumer Credit Panel (CCP) that covers the universe of credit card accounts nationwide from 2000 to the present. In this data set, we confirm and replicate our findings using the financial aggregator data from Iceland. Constructing county-quarter measures of credit outcomes and employing a Bartik-style shift-share instrument as a plausibly exogenous source of variation in county-level employment, we estimate the responses of credit card account balances, limits, inquiries, utilization, and delinquencies.[2] We replicate our findings in that we do not find large or statistically significant changes in borrowing in response to unemployment shocks. More specifically, individuals in counties with adverse unemployment shocks neither appear to increase their overall outstanding balances (even among those with a slack in their credit utilization ratios), nor do they appear to increase their inquiries for new credit relative to counties with less adverse unemployment shocks.

This paper's analysis using U.S. data faces two challenges: first, in the credit report

[2] The estimation and interpretation of causal effects using a Bartik-style instrument to isolate shocks to labor demand has been employed by a number of authors. An incomplete list of papers includes Blanchard et al. (1992); Gould et al. (2002); Aizer (2010); Nguyen et al. (2015); Chodorow-Reich et al. (2012); Maestas et al. (2016); Keys et al. (2017).

data, we (as well as all existing papers in this space such as Keys et al., 2017; Braxton et al., 2018) use snapshots of credit card balances as a measure of rolled-over consumer debt; and second, we do not observe individual-level incomes in the U.S. data. In contrast, in the Icelandic data, we know exactly how much consumer debt is rolled over and paid interest upon and who became unemployed. To nevertheless address both concerns in the U.S. data to some extent, we complement this analysis by looking at the distribution of individual borrowing outcomes conditional on county-level unemployment shocks. Using quantile regressions, two facts stand out: even amongst the top half of the conditional distribution, responses are economically small and if anything, point estimates are in the "wrong" direction. The largest increases in borrowing occur in counties in which unemployment, as captured by the Bartik instrument, is low. We thus conclude that, while average credit card balances in the U.S. are materially high, most individuals do not increase credit card debt in response to unemployment, even when they could have done so and for unemployment shocks that are arguably transitory (we separate the time periods before and during the financial crisis).

The 2015 American Household Credit Card Debt Study estimates the total credit card debt owed by an average indebted U.S. household to be $5,762 and the average Icelandic household's amount borrowed is of similar magnitude. Such large high-interest debt holdings over longer periods of time are very hard to rationalize in standard economic models. For example, Laibson et al. (2003) argue that such debt holdings constitute a puzzle for standard life-cycle models in which fully rational agents would rather forgo the benefits of consumption smoothing than borrow at such high interest rates. Laibson et al. (2017) show that a model with hyperbolic discounting and illiquid assets rationalizes the amount of borrowing we see in U.S. data. There also exist rational models that generate some borrowing in response to permanent income shocks in the presence of illiquid assets (Kaplan and Violante, 2014). However, Kaplan and

Violante (2014) assume the absence of transitory income shocks, to which any rational agent would respond by holding a small buffer of liquidity. Furthermore, agents in this model bunch at zero borrowing when interest rates are high, such as the rates on credit cards or overdrafts, and only borrow (up to their credit limits) when interest rates are relatively low, such as the rates observed on home equity lines of credit.

The aforementioned models, and, to to best of our knowledge, all other existing economic models predict that credit demand should be countercyclical. We demonstrate this holds theoretically for hyperbolic as well as standard preferences using the model by Laibson et al. (2017). In the event of an income shock calibrated to the Icelandic unemployment replacement rate, the hyperbolic agents in the model, calibrated to match the real-world borrowing on credit cards that we see, increase their likelihood to borrow by 31%, increase their amount borrowed by 319%, and decrease their spending by 7%. While the spending responses matches our empirical results quite well, the strong borrowing response is clearly a prediction that is not borne out in the data. Theoretically it is important to note that any economic model in which individuals have a concave utility function and thus want to smooth consumption will predict a negative correlation between borrowing and income (in the absence of supply-side responses or other features such as varying costs of default). However, empirically, we conclude that households allow consumption to adjust while smoothing their debt balances and thus document an important discrepancy between the theoretical predictions and the empirical findings.

As a broader implication, the credit smoothing we observe in the data will amplify business cycles compared to the countercyclical demand predicted by the economic models. While an extensive literature has explored supply amplifiers during the Great Recession, our paper suggests that we should also examine demand amplifiers during the initial expansionary and then contractionary period. Our results are consistent with household demand driving dynamics in consumer credit markets which relates our paper to the demand-side (Adelino et al., 2016) versus supply-side (Mian and Sufi, 2009; Mian et al., 2013) studies in the mortgage market. In the same manner as for mortgages (Mian et al., 2013), consumer credit may cause "debt overhang", creating credit-driven business cycles that operate through household demand. Furthermore, if unsecured, high-interest lines of credit such as credit cards are not used to smooth consumption, self-insurance benefits from these financial products are limited. In turn, if individuals misunderstand the high costs, then government (through regulatory policy or education) may have a role to play in affecting not only the supply of credit, but also its demand.

2 Literature review

The rapid growth in the use of consumer credit in recent decades has spurred a lot of interest in this area of research while recent improvements in data availability have made it possible to investigate this phenomenon empirically. Müller (2018) documents that over the last 50 years, household credit has risen dramatically. Furthermore, he shows that unsecured consumer credit, rather than mortgage lending, accounts for the majority of this growth. Whether or not consumer credit is used as economic models suggest, i.e., a tool to smooth consumption and is thus countercyclical, is an open empirical question.

Several recent studies document that consumer demand for credit is not countercyclical. Closest to our paper, Baker and Yannelis (2017), Gelman et al. (2015), Ganong and Noel (2018), Olafsson and Pagel (2018a), and Olafsson and Pagel (2018b) show a lack of consumption smoothing in response to predictable positive and negative income shocks using transaction-level income and spending data. Baker and Yannelis (2017) and Gelman et al. (2015) analyze individual responses to a temporary shortfall in income due to the 2013 government shutdown. Strikingly, despite the short nature and perfect certainty of this transitory income shock, Gelman et al. (2015) find insignificant and small coefficients for credit card spending and balances but a brief decrease and delay of credit card repayments. Individuals simply cut spending sufficiently and substitute to home production (as shown in Baker and Yannelis, 2017) to get by. Following these two papers, we show that this lack of borrowing also generalizes to larger and longer-lasting – but still temporary – income shocks due to short spells of unemployment. Our findings are in line with Ganong and Noel (2018) who show that consumption drops in response to unemployment but borrowing increases by merely $23 two months after the onset of unemployment and by merely $45 two months after unemployment benefit exhaustion, even when individuals have substantial credit available.[3] Using survey data, Fuster et al. (2018) find consumption declines in surveys of hypothetical negative shock scenarios, and that these declines are similar even when the scenario includes an interest free loan. Finally, Hundtofte (2017) also finds evidence of self-imposed financial constraints in field data by observing voluntary credit card closures. Furthermore, voluntary closures increase in response to negative economic news such as declines in house prices or increases in unemployment.

However, other recent studies show that the demand for credit is countercyclical and that individuals indeed tap their credit lines when they experience a temporary income decline unless they face liquidity constraints imposed by the supply of credit. Using survey data, Browning and Crossley (2009) and Sullivan (2008), for example, find that very low asset households as well as wealthy households do not increase their debt in response to unemployment, while the average effect for all other households is positive. The authors both argue that low asset households are credit constrained,

[3]As the authors observe only checking and credit card accounts from one bank, they do not explore this lack of a borrowing response further.

which we can directly address in this study by observing credit limits. Our findings are in contrast to Keys et al. (2017) who use a similar, but larger, sample of unmatched (to the individual) credit card data and the same source of variation in employment for the U.S. However, the authors focus on the cross-sectional variation provided by the Bartik shock in the first quarter of 2008 and show that total credit card borrowing is reduced in counties hit by adverse unemployment shocks relative to counties in better standing.[4]

Another recent study is Braxton et al. (2018) who also show that job losers retain access to credit and that the average borrowing response to unemployment as well as the effect on credit limits is small in annual data. However, the authors argue that this zero result is due to unconstrained job losers borrowing (an increase in balances), while constrained job losers delever and default (a decrease in balances). In our quarterly U.S. data, we do not find an increase in borrowing by unconstrained job losers nor a substantial increase in delinquent balances in response to unemployment. In Iceland, where defaulting is very rare, we can proxy delinquencies with late fee payments but do not see an increase there. Additionally, before defaulting, we would expect individuals to use their open credit lines.[5]

[4]Our findings can be reconciled with those in Keys et al. (2017) by noting that Keys et al. (2017) document a relative response: it could be that counties with relatively high unemployment reduce their credit card borrowing by less than those with low unemployment. Efforts to replicate the findings of Keys et al. (2017) as closely as possible have revealed that the different conclusions can be explained by two important differences between the analyses. First, our results are based on individual-level observations while the analysis in Keys et al. (2017) is based on card-level observations aggregated up to the county-level and weighted by the base-period number of accounts in the county. If individuals with different number of cards behave differently, a different relationship should be reflected in different estimates from aggregating individual-level rather than card-level data. Secondly, Keys et al. (2017) focus on the cross-sectional variation of the Bartik employment shock in the beginning of 2008, examining various longer horizon outcomes in response to that shock, while we focus on shorter-run responses to employment shocks, pursuing a panel analysis of shocks over the period 2000 to 2016.

[5]If individuals in Iceland default they are enlisted in the official defaulters' list which prevents them from taking out new loans and opening up new credit cards for up to 4 years among other sanctions. Therefore, defaulting has serious consequences while borrowing using existing credit lines does not. Furthermore, banks would recover almost all delinquent balances via wage garnishment.

3 Theoretical background

We consider the same model as in Laibson et al. (2017) to formally illustrate the standard predictions of how borrowing responds to income shocks in a life-cycle model that successfully explains the extent of credit card borrowing via illiquid savings and naive hyperbolic discounting (see, Laibson, 1997; O'Donoghue and Rabin, 1999; Kuchler and Pagel, 2015). Additionally, the model explains the existing evidence documenting a lack of consumption smoothing by showing that individual marginal propensities to consume out of transitory income shocks are very high (see Shapiro and Slemrod, 1995, among many other studies). Beyond illiquid assets and naive hyperbolic discounting preferences, the model features revolving high-interest credit, liquidity constraints, stochastic labor income, social security, child and adult household dependents, retirement, and mortality. The authors estimate the preference parameters using the method of simulated moments; in particular, the exponential discount function of a standard agent as well as the present-biased discount function of a hyperbolic-discounting agent. The authors show that the standard model of exponential discounting can be formally rejected in favor of hyperbolic discounting. Nevertheless, the hyperbolic discount factor the authors estimate is relatively low in comparison to typical estimates and assumptions in the micro literature (see, for instance DellaVigna, 2009, for a literature survey).

More specifically, Laibson et al. (2017) consider the following model.[6] The agent lives for $t = \{1, ..., T\}$ periods. Each period the agent selects an optimal level of consumption C_t. Additionally, he decides how much to save in both the liquid and illiquid assets. The variable X_t represents liquid asset holdings in the beginning of period t before receipt of period t income Y_t. If $X_t < 0$ then uncollateralized high-interest debt, i.e., credit card debt, was held between t and $t-1$ at an interest rate of R^{CC}. The agent also faces a credit limit in period t of $\lambda > 0$ times average income at age t. If the agent

[6] We thank the authors for kindly sharing their solution code.

saves instead of borrows, he earns an interest R. The stock variable $Z_t \geq 0$ represents illiquid asset holdings at the beginning of period t, earning interest R^Z and providing consumption value. However, illiquid assets can be liquidated only with a proportional transaction cost, which declines with age $\kappa_t = \frac{1/2}{1+e^{t-50/10}}$. Let $I_t^X$and I_t^Z represent net investment each period into the liquid and illiquid assets so that the budget constraint is given by

$$C_t = Y_t - I_t^X - I_t^Z + \kappa_t \min(I_t^Z, 0).$$

The consumer has constant relative risk aversion quasi-hyperbolic preferences and maximizes

$$\max_{I_t^X, I_t^Z} \{ n_t \frac{(\frac{C_t + \gamma Z_t}{n_t})^{1-\rho}}{1-\rho} + \beta E_t [\sum_{\tau=1}^{T-t} \delta^\tau (\prod_{j=1}^{\tau-1} s_{t+j}) (s_{t+\tau} \frac{(\frac{C_{t+\tau} + \gamma Z_{t+\tau}}{n_{t+\tau}})^{1-\rho}}{1-\rho} + (1 - s_{t+\tau}) B(X_{t+\tau}, Z_{t+\tau}))]\}$$

each period t subject to the budget constraint. Here n_t represents family size in period t, ρ is the coefficient of relative risk aversion, β is a hyperbolic discount factor, and δ is an exponential discount factor. The agent is fully naive in the sense that his period t self does not take into account that his period $t+1$ self is present-biased. $B(\cdot)$ incorporates the bequest motive in the death state which is represented by $s_t = 0$ instead of $s_t = 1$ when the agent survives. More details can be found in Laibson et al. (2017) and the model is solved by numerical backward induction. Laibson et al. (2017) estimate the environmental parameters of the model using data from the American Community Survey of the U.S. Census Bureau, the Survey of Consumer Finances, and the Panel Study of Income Dynamics and the preference parameters of this model to match the patterns of wealth accumulation and credit card borrowing over the life-cycle and we adopt the parameters of their best fit for the hyperbolic agent. In turn, we consider a standard agent by setting $\beta = 1$.

We simulate the life-cycle consumption paths of 10,000 agents and then run the equivalent of our empirical specification in the simulated data; i.e.,

$$\log(abs(X_{i,t})|X_{i,t} < 0) = \alpha + \beta I^{15}_{i,t} + age_{i,t} + \epsilon_{i,t}$$

where $\log(abs(X_{i,t})|X_{i,t} \leq 0)$ is the amount borrowed by agent i at age t (set to zero if the agent does not borrow) and $I^{15}_{i,t}$ is an indicator variable if agent i's realization of income at age t is 67% or less than his income at age $t-1$. The income process is calibrated to include social security and unemployment benefits but does not specifically model unemployment which is why we choose a low draw of income to represent a transitory income shock. Here, we take 67% as it represents the Icelandic unemployment insurance replacement rate. The simulation results are robust to modifying this cutoff; the lower the cutoff the more extreme the borrowing response. Furthermore, to eliminate life-cycle effects, $age_{i,t}$ is a set of age or cohort fixed effects. Alternatively, we can use an indicator for whether or not agent i at time t borrows as the outcome variable as well as log consumption. Because all agents are the same in the sample of simulated data, this regression is equivalent to our empirical specification with individual fixed effects. Of course, in reality, there does not only exist one type of agents but agents are heterogenous in their preferences. That is why we report the regression results for two types of agents: a hyperbolic agent, whose preference parameters are estimated by Laibson et al. (2017) using a representative sample of the U.S. population, and also a standard agent who does not have a hyperbolic discounting problem. If one were to observe a mixed group of these two agents, the coefficients would be an average of the ones displayed.

As we can see in Table 1, having an income realization less than 67% the previous income results in a 31% and 319% increase in the likelihood and amount borrowed and a 7% decrease in consumption in the hyperbolic discounting model. After all, present-biased agents in the model are consumption smoothing as standard agents are. Therefore, they both use borrowing as a tool to smooth transitory income shocks. Therefore, when income is low then borrowing increases. For the standard agent, the borrowing response is somewhat less pronounced but still positive and significant. However, the standard agent almost never borrows at the level of interest rates considered in this model (calibrated to match actual interest rates on credit cards). In fact, the standard agent only borrows 0.15% of the time.

[Table 1 about here]

4 Data

In this study, we exploit two complementary data sources. We first use detailed longitudinal information on debit and credit account transactions (providing a detailed measure of spending), balances, and limits from a financial aggregator in Iceland. We then test whether our results based on the financial aggregator data are supported by findings based on credit card data in a representative sample of U.S. credit card holders. Even though the U.S. data is not as detailed as the financial aggregator data, we believe that the replication of our findings in U.S. data ameliorates concerns regarding external validity of the results based on the aggregator data. In the following sections we will describe our data sources in detail.

4.1 Icelandic data: Financial Aggregator Data

The financial aggregator data we use is generated by Meniga, a financial aggregation software provider to European banks and financial institutions. Meniga's account aggregation platform allows bank customers to manage all their bank accounts and credit cards across multiple banks in one place by aggregating data from different financial institutions. We generate a panel of aggregated user-level data for different income and spending categories as well as account balances and credit limits for 2011 to 2017. We aggregate our data to the monthly level and restrict the analysis to individuals for which we have full records, defined by four requirements.

First, we restrict our sample to individuals for whom we see bank account balances and credit lines. Second, we restrict our sample to individuals for whom we observe income arrivals (this not only includes labor market income, but also covers sources such as unemployment benefits, pension payments, invalidity benefits, and student loans). The third requirement is that key demographic information about the user is available (age, gender, marital status, and postal code). The final requirement is that the consumption of each user must be credible, which we ensure by requiring at least 5 food transactions in at least 23 months of a 24 months period.

Furthermore, we infer employment status from salary and unemployment benefit payments we see in the data. This data set has been proven useful in multiple studies, e.g., to analyze individual spending responses to income payments together with individual liquidity constraints (Olafsson and Pagel, 2018a), individual spending, savings, and consumer debt responses to retirement (Olafsson and Pagel, 2018b), and the drivers of individual attention to personal finances (Olafsson and Pagel, 2017).

Because our financial aggregator data is derived from actual transactions and account balances, it overcomes the accuracy, scope, and frequency limitations of the existing data sources of consumption, income, and financial standing. The data we use is exceptionally thorough and accurate with respect to capturing all income and spending because of three reasons: (1) the income and spending data are precategorized (with very few uncategorized transactions), (2) the app is marketed through banks and supplied for their customers (thus covering a fairly representative sample of the population), and (3) the data are basically free of one important shortcoming of all transaction-level data—the absence of cash transactions (in Iceland, consumers almost exclusively use electronic means of payment and less than 1% by volume or number of our transactions are ATM withdrawals). Such detailed information on consumption is rare within this literature, which typically relies on proxies for consumption (e.g., car purchases), noisy survey measures of consumption, or imputed measures of consumption from yearly snapshots of wealth and income.

Description of sample

Table 2 displays summary statistics of employed and unemployed individuals. Furthermore, Figure 1 shows the evolution of income and unemployment benefits in the months around job loss while Figure 2 shows the distribution of the length of unemployment spells.

[Table 2 and Figures 1 and 2 about here]

The evolution of income and unemployment benefits shows that income increases slightly in the months leading up to job loss. This can be explained by the fact that unused holiday payments are paid out together with the last salary payment for individuals asked to leave their jobs. Upon unemployment there is a sharp drop in income which is quite stable and then increases. This can be explained by the fact that not all unemployed individuals claim their unemployment benefits from the very start of their unemployment spell (this does not mean that they forego their benefits, only that they are paid out later).

As can be seen when looking at the distribution of the length of unemployment spells, the vast majority of unemployment spells are relatively short. The median length of an unemployment spell is 4 months while the mean length is 6.3 months. In terms of labor market regulations, Iceland is characterized by relatively flexible labor laws, more similar to the U.S. than continental Europe. Moreover, unemployment is low. In 2017 only 2.4% of individuals were unemployed. In comparison, 2017 average unemployment was 6% in the Organization for Economic Co-operation and Development (OECD) countries, 8% in the European Union, and 4.5% in the U.S.

Historically, the Icelandic labor market has been characterized by a very low and stable rate of unemployment with unemployment generally fluctuating below 3%. Even during the financial crisis, unemployment peaked at only around 8%. High levels of economic growth in Iceland in the years after the 2008 financial crisis have helped reduce the level of unemployment down to a level near its historical average. In that sense, we are not overly concerned that the crisis in Iceland matters for our findings. While the Icelandic financial crisis undoubtedly affected individuals, the country recovered very quickly after the crisis and experienced high economic growth and low unemployment during our entire sample period.[7] Furthermore, the fact that unemployment is and has been low in Iceland makes it unlikely that those without a job fear being unemployed for a long period of time, i.e., unemployment is arguably a transitory income shock in our setting as opposed to countries where unemployment rates and durations are higher.

Furthermore, Iceland is very similar to many other economies, including the U.S., when it comes to usage of high-interest unsecured consumer debt. Individuals hold on average approximately $2,000 in overdrafts, the main source of unsecured borrowing. Furthermore, if we condition on individuals having an overdraft, the average overdraft amount is approximately $6,000. In Iceland, individuals typically pay off their credit

[7] The OECD Economic Survey Iceland from June 2011 states that the economic contraction and rise in unemployment appear to have been stopped by late 2010 with growth under way in mid-2011. The Icelandic government was successfully able to raise $1 billion with a bond issue in June 2011, which indicates that international investors have given the government and the new banking system a clean bill of health. By mid-2012, Iceland was regarded as a recovery success story (Forelle, Charles (19 May 2012) Wall Street Journal.). According to OECD figures, the Icelandic economy grew by 7.2% in 2016, which was the second highest growth in the OECD in 2016.

card in full and use overdrafts to roll-over debt (more details can be found in the next section). Nevertheless, individuals still enjoy substantial liquidity or borrowing capacities. In fact, the average individual could borrow another \$10,000 on average before hitting their credit limits. In comparison, the Survey of Consumer Finances (SCF) shows that the average credit card debt for individuals rolling over is approximately \$4,000 in the U.S. We thus believe that our results can be generalized to the U.S. and other European countries with relatively large consumer debt holdings, e.g., the U.K., Spain, and Turkey.

Institutional background: borrowing and unemployment in Iceland

Individuals in Iceland use overdrafts as their main means of high-interest unsecured consumer debt. An overdraft occurs when withdrawals from a checking account exceed the available balance. This means that the balance is negative and hence that the bank is providing credit to the account holder and interest is charged at the agreed rate. Virtually all current accounts in Iceland offer a pre-agreed overdraft facility, the size of which is based upon affordability and credit history. This overdraft facility can be used at any time without consulting the bank and can be maintained indefinitely (subject to ad hoc reviews). Although an overdraft facility may be authorized, technically the money is repayable on demand by the bank. In practice this is a rare occurrence as the overdrafts are profitable for the bank.

Wage earners in Iceland or self-employed individuals who lose their jobs may be entitled to unemployment benefits. More specifically, wage earners and self-employed individuals may be entitled to basic unemployment benefits for the first half-month (10 working days) after they lose their job. After having been paid basic benefits for the first two weeks after the loss of their jobs, wage earners and self-employed individuals may be entitled to income-linked unemployment benefits for up to three months. The income-linked benefits of wage earners can be up to 67% of their average income during a six-month reference period beginning two months before the loss of employment and this reference period cannot be shorter than four months. The income-linked benefits of self-employed individuals can as well be up to 67% of their average income during the preceding income year in which the individual became unemployed. The income-linked benefits are capped at approximately $3,800, i.e., there is a maximum in the amount of monthly payments of unemployment benefits. Furthermore, after three months of unemployment, the income-linked benefits are canceled, and only basic benefits are paid thereafter. Unemployment benefits are paid for a maximum of thirty months. Moreover, individuals receiving unemployment benefits who have children under the age of 18 to provide for may be entitled to an additional 4% of their average income of undiminished basic benefits for each child. The basic benefits amount is capped at approximately $2,400.[8]

After being in a job for six months, individuals are entitled to a three months notice of unemployment and can be entitled to a notice up to six months in advance (which is not the case for the majority of the population however). To include periods of time in which unemployment would have been unexpected at least for some of the sample, we begin four months prior to the beginning of unemployment for any event study illustrations.

Definitions of variables

Total discretionary spending - Spending is pre-classified into 15 categories and aggregated to generate a monthly panel. The spending categories are groceries, fuel, alcohol,[9] ready made food, home improvement, transportation, clothing and acces-

[8]Further details can be found here: https://vinnumalastofnun.is/en/unemployment-benefits/unemployment-benefit-amounts

[9]We can observe expenditures on alcohol that is not purchased in bars or restaurants because a state-owned company, the State Alcohol and Tobacco Company, has a monopoly on the sale of alcohol

sories, sports and activities, pharmacies, media, bookstores, thermal baths, toy stores, insurances, and various subcategories of recreation (e.g., cinemas, gaming, gambling etc.). Total spending is the sum of the spending in all these categories and excludes all recurring spending, e.g., rent and bills.

Necessities - Expenditures in grocery stores, gas stations and pharmacies.

Discretionary entertainment - Expenditures in the alcohol, restaurants/take-outs, lottery, gambling, gaming, and cinema categories.

Cash - Cash is defined as the sum of checking and savings account balances, normalized by the average discretionary spending per day of individuals, i.e., we measure cash in consumption days. We observe checking and savings account balances for approximately half of the sample period.

Liquidity - Liquidity is defined as cash plus credit and overdraft limits minus credit card and overdraft balances, normalized by the average discretionary spending per day of individuals, i.e., we measure liquidity in consumption days.

Credit lines - Credit lines are defined as borrowing capacity until individuals hit their overdraft and credit card limits, normalized by the average income per month of individuals, i.e., we measure credit lines in month of average income.

Overdraft usage - Individuals typically pay off their credit card in full and use overdrawn checking accounts to roll-over debt. We look at whether individuals hold an overdraft in a given month, i.e., their checking account balance is negative at least once, how many overdrawn checking accounts they have, and how much they pay in overdraft interest.

Late fees - Fees assessed for paying bills after their due date.

Income - Income is pre-classified by the aggregator app and we observe the following regular income categories: child support, benefits, child benefits, interest income, inva-

in Iceland.

lidity benefits, parental leave, pension income, housing benefits, rental benefits, rental income, salaries, student loans, and unemployment benefits. In addition, we observe the following irregular income categories: damages, grants, other income, insurance claims, investment transactions, reimbursements, tax rebates, and travel allowances.

4.2 U.S. data: consumer credit panel

For the U.S. replication, we use the Consumer Credit Panel (CCP) of the Federal Reserve Bank of New York containing detailed information on individual debt and credit (Lee and Van der Klaauw, 2010). The Consumer Credit Panel (CCP) is an anonymous longitudinal panel of individuals, comprising a 5% random sample of all individuals who have a credit report with Equifax for the period between 1999 and 2017. Our quarterly sample starts in 1999 first quarter and ends in 2017 third quarter. The data is described in detail in Lee and Van der Klaauw (2010). We use a 0.01% sample for purposes of the current analysis, which includes information on approximately 250,000 randomly selected individuals each quarter. For our main specifications, we aggregate data to the county level.

The CCP provides credit registry information on all debts monitored by one of the three main credit bureaus, in addition to public records (bankruptcies and deaths) and mobility (address changes) for all individuals that are visible to the credit registry (i.e., the very young and any others without reported debts are excluded). This panel data set allows us to study a number of aspects of individuals' financial liabilities, including their bankruptcies and foreclosures, mortgages, detailed delinquencies, various types of debts, the number of accounts, and balances. Information on location of residence is available at the census block level.

Table 3 provides summary statistics on the average change in revolving credit card debt as well as total debt balances and utilization ratios in the U.S. credit panel. It also reports the number of inquiries as well as individual risk scores, age, and income.

[Table 3 about here]

The main benefit of the U.S. data is that it provides an externally valid test of any of our findings based on the financial aggregator data. The CCP is a representative sample of the U.S. population and we can look at individuals' entire balance sheet responses because it links all borrowing and credit cards to each individual. Moreover, the sample is large and has sufficient statistical power to perform quantile regressions as we do. The main drawback is that we do not have information on income and employment status of the individuals in our sample, so we must restrict ourselves to examining borrowing responses to local unemployment shocks. Furthermore, in the credit panel, we only observe a snapshot of credit card balances which we take as a measure of rolled over consumer debt. Clearly, snapshots of credit card statement balances are an imperfect measure of the amount of credit card debt rolled over each period and on which interest is owed, but are commonly used in the literature (see, e.g. Agarwal et al., 2017; Braxton et al., 2018; Keys et al., 2017). In fact, snapshots of balances may not even be positively correlated with rolled-over consumer debt in the event of unemployment. After all, we know that individuals cut consumption substantially (Ganong and Noel, 2018, and shown in our analysis) which will reduce credit card balances. However, if individuals cut consumption and, at the same time, increase their rolled-over consumer debt, then the outcome that we study is negatively correlated with the outcome that we are interested in. In contrast, the Icelandic data set overcomes both of these challenges: we know what is the amount of rolled over interest-bearing debt and we know who gets unemployed.

5 Methodology

5.1 Individual-level analysis using financial aggregator data

For the analysis using the financial aggregator data, we estimate the effect of unemployment by running the following regression

$$y_{i,t} = \beta_0 + \beta_1 Unemployment_{i,t} + \beta_2 X_{i,t} + \psi_t + \eta_i + \epsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is the outcome under consideration—spending, savings, or use of consumer credit—of individual i at time t, $Unemployment_{i,t}$ is an indicator equal to 1 if i is unemployed at time t and equal to 0 otherwise. $X_{i,t}$ is a vector of controls, ψ_t are month-by-year fixed effects, and η_i is an individual fixed effect. The β_1 coefficient thus measures by how much the individual outcome deviates when the individual is unemployed. The individual fixed effects control for all time invariant (un)observable individual characteristics. It is important to note that when we estimate the responses to unemployment in the months after losing a job and that we exclude observations in the last three months prior to the onset of unemployment. In turn, the interpretation of an unemployment estimate is relative to the individual outcome prior to receiving the unemployment notice.

Because economic models suggest that liquidity holdings are important for individual responses to transitory income shocks, we also modify our benchmark specification and allow for interaction effects between unemployment and liquidity. The specification is as follows:

$$\begin{aligned} y_{i,t} = \beta_0 &+ \beta_1 Unemployment_{i,t} + \beta_2 Unemployment_{i,t} * liquidity_{i,t-4} \\ &+ \beta_3 liquidity_{i,t-4} + \psi_t + \eta_i + \epsilon_{i,t} \end{aligned} \tag{2}$$

where $liquidity_{t-4}$ is the amount of liquidity, measured in number of average consumption days, held by individual i four months prior to unemployment, to avoid problems of reverse causality. The consumption days are individual specific: for instance, if an individual spends \$100 per day has liquidity of \$1,000, he or she has 10 consumption days of liquidity. The reason for using liquidity four months before the onset of unemployment is that the vast majority of workers have a three months notice period and we do not want to pick up liquidity increases in anticipation of unemployment for instance.

5.1.1 Dynamic responses

How strongly spending and borrowing respond to a transitory income shock depends obviously on the time frame under consideration. The results above focus on the average effect within any month of unemployment. Of equal interest is how spending and borrowing respond to unemployment over time. We therefore also estimate impulse responses over the four months after losing a job, conditional on being unemployed for at least four months to make sure that the sample size for each of the coefficient estimates remains the same. We employ the following specification:

$$y_{i,t+k} = \beta_0 + \beta_{1,k} Unemployment_{i,t} + \psi_{t+k} + \eta_i + \epsilon_{i,t+k} \text{ for } k = -3, -2, ..., 3 \tag{3}$$

where the $\beta_{1,k}$s are the main coefficients of interest. Each $\beta_{1,k}$ represents the effect of unemployment on the outcome under investigation in month $t+k$. It is important to note that when we estimate the effect of a unemployment on the outcomes under investigation in the months after the loss of a job we include observations up to 3 months before they lost their job, where most individuals receive their notice of layoff, so as to capture any potential announcement effects. The cumulative effect is given by the sum of the $\beta_{1,k}$s.[10]

5.2 Analysis using the U.S. credit panel

For the analysis using U.S. data, our main specifications regress dollar changes in credit outcomes aggregated at the county level on percentage changes in unemployment rates (results are similar using other measures of changes in employment conditions). While our main analysis is at the county-level, our additional analyzes pursue individual-level variation.

As a source of plausibly exogenous variation in employment, we use a Bartik-style shift-share instrument for employment shocks. More specifically, the change in a county's employment is instrumented using the interaction of the pre-period industry mix of employment in that local labor market with the national change in industry employment (exclusive of the given county). National increases in demand in some sectors therefore result in plausibly exogenous changes in employment at the local level. Our exclusion restriction for interpreting estimates as causal effects of consumer demand is that the pre-period industrial mix interacted with the national industry trend does not directly affect local credit card variables outside of its effect on local employment (income).

More formally, we consider the following regression:

$$y_{c,t} = \beta \Delta \widehat{Unemployment}_{c,t} + \gamma X_{c,t} + \psi_t + \eta_c + \epsilon_{c,t} \tag{4}$$

[10]The differences between the cumulative effects here and the estimated effects in our main specification can be explained by the following: for the impulse responses we make restrictions 1) we use the same individuals to estimate each of the $\beta_{1,k}$s, i.e., we restrict ourselves to individuals that we do observe unemployed for at least 3 months prior to the onset of their unemployment and 2) we only use individuals we observe unemployed for at least 4 consecutive months after that. In our main specification we do not make this restriction and we also exclude the 3 months prior to unemployment because we do not want to confound the anticipation effect of unemployment and the actual effect.

when the credit outcomes studied are aggregated from individual-level outcomes by taking the mean in that county and time period. Before proceeding to quantile regression analysis using individual-level observations, we also confirm our county-level OLS findings using individual observations that allow for individual-level fixed-effects regressions, to confirm the estimates are largely unchanged:

$$y_{i,t} = \beta \Delta \widehat{Unemployment}_{c,t} + \gamma X_{i,t} + \psi_t + \eta_i + \epsilon_{i,t} \tag{5}$$

where $y_{i/c,t}$ is the credit outcome of individual i or county c at date t and $\eta_{i/c}$ is an individual or county fixed effect. The β coefficients thus measure by how much outcomes of individuals that live in counties that experience unemployment shocks deviate. Fixed effects control for all (un)observable individual or county characteristics and $X_{i/c,t}$ controls for time-varying characteristics such as individuals' (or counties' mean) credit scores. Furthermore, ψ_t are time fixed effects that capture any systemic changes or shocks across counties at each individual point in time.

To isolate a source of variation in $\Delta Unemployment_{c,t}$ we instrument it using a measure of predicted unemployment, defined in the following way:

$$\Delta \widehat{Unemployment}_{c,t} = \sum_i \left(\frac{Employment_{i,t}}{Employment_{i,t-1}} - 1\right) EmploymentShare_{i,t-1,c}$$

where $\frac{Employment_{i,t}}{Employment_{i,t-1}} - 1$ is the change in the national employment of industry i from time $t-1$ to t and $EmploymentShare_{i,t,c}$ is the share of employment in industry i at time t in county c. As an employment outcome, for comparability to our Icelandic analysis, we focus on changes in unemployment rates (results are qualitatively similar when examining percentage changes in employment). We generate the above shocks using quarterly census data (QCEW).

The credit outcomes we consider are the change in credit card balances, the change in credit card limits, the sum of changes in all revolving debts (including home equity), the number of credit inquiries (from any lender or application), and the credit utilization ratio. The credit utilization ratio is calculated by dividing the outstanding balance on the category of revolving debt by the total appropriate credit limit. We report results for both contemporaneous and delayed employment. For robustness, we report results where we control for delayed credit score and also where we only consider specific, shorter, time periods surrounding the financial crisis (2008 to 2014). Standard errors are clustered at the county level as the unemployment instrument is a county-level source of variation.

Analysis of conditional distributions using quantile regressions

Whereas the above regressions estimate a conditional mean of an outcome variable given certain values of predictor variables, a quantile regression aims at estimating any point on the conditional distribution, such as the conditional median or other quantiles. Recall that the τ-quantile of a distribution is the point on the support such that the probability of observing values at that point or below is $\tau\%$. Quantile regressions allow us to examine the data under the assumption that a particular quantile changes as a linear function of some variables x.

A quantile regression differs from an ordinary least squares regression in two key respects. First, the quantile regression minimizes the sum of absolute errors, rather than the sum of squared errors. Second, it puts differential weights on the errors depending on whether an error term is above or below a quantile. In a quantile regression of $y_{i,t}$ on $x_{i,t}$ the regression slope β_τ is chosen to minimize the quantile weighted absolute value of errors. More specifically, for a range of quantiles from 2 to 98, instead of $Q(\tau)$ being fixed at $Q(\tau) = a_\tau$, we assume that:

$$Q_{y_{i,t}}(\tau, x_{i,t}) = a_\tau + x'_{i,t}\beta_\tau \tag{6}$$

where $x_{i,t}$ includes any individual-level or time-varying controls in addition to county-level unemployment shocks, our variable of interest.

We use quantile regressions to examine heterogeneity in individual credit responses to unemployment shocks. The quantile regressions allow us, for instance, to investigate whether any portion of the distribution responds as individuals who were using credit cards for consumption smoothing would. While the average borrowing response might be insensitive to unemployment shocks, there may be some borrowers responding strongly to changes in county-level unemployment rates by borrowing. To investigate this we can look at whether the upper quantiles of changes in credit card borrowing are (inversely) sensitive to changes in unemployment. Quantile regressions thus give us a more comprehensive analysis of the relationship between unemployment and borrowing in the U.S. data.

6 Results using financial aggregator data

Table 4 shows our estimates of the average effect of unemployment in Iceland, i.e., the β_1 coefficients of Equation 1 for the outcomes under consideration, both with and without individual fixed effects, and with and without interacting unemployment with liquidity holdings prior to the onset of unemployment. We focus on the first 4 months of unemployment to maximize sample size, and restrict our sample to individuals that are unemployed for at least 4 months. The reason we restrict our analysis to individuals that are unemployed for at least 4 months is that we want to avoid including individuals that are unemployed voluntarily for a short period of time in our analysis. Individuals can leave their job and receive unemployment benefits for a couple of months before starting a new job. For individuals that would like to take some time off between starting a new job this might be an attractive option. However, these individuals are not those of interest to us. Individuals that want to take some time off from work are unlikely to do that for 4 months or longer and we therefore believe that by restricting our analysis to these individuals we exclude individuals that voluntarily leave their jobs.

The table shows the effect of unemployment on total discretionary spending as well as "necessary" spending (groceries, fuel, and pharmacies) and "discretionary" entertainment (alcohol, restaurants, other activities, lottery tickets, gambling, gaming, bookstores, recreational sports, specialty stores, theaters, shows, and toys). It is important to note that we only consider discretionary spending and not consumption commitments, i.e., recurring spending, such as rent, mortgage payments, and utilities. Moreover, the table shows results for cash holdings (checking and savings account balances) as well as liquidity (checking and savings account balances plus credit limits minus credit card balances). We normalize cash holdings and liquidity by the daily average discretionary spending of individuals, i.e., we measure cash and liquidity in individual consumption days. Furthermore, we consider use of overdrafts, late fees, and credit lines. Credit lines are measured as distance to credit limits and normalized by average individual income. Table 5 shows results by income reduction terciles, Table A.4 by initial liquidity, and Table 7 by whether or not an individuals hold an overdraft most of the time.

[Tables 4, 5, A.4, and 7 about here]

We find that individuals decrease their spending considerably in response to unemployment by about 8% per month. Cash holdings are insignificant and average around zero and liquidity and credit lines are also insignificant and small. Furthermore, we find a very tightly estimated zero effect on the likelihood of overdrawing the checking account or the number of overdrawn checking accounts. It appears that individuals make a conscious decision to cut down their spending and try to restrict themselves rather than entering high-interest cost territory to smooth their consumption. Furthermore, we also obtain a tightly estimated insignificant coefficient on overdraft interest. While the likelihood to hold an overdraft is tightly estimated for all subgroups of individuals, however, the overdraft interest is more noisily estimated for some splits. The noisiness is partly due to the fact that interest rates and mark-ups vary over the sample period and within individuals. For that reason, we consider the likelihood to have an overdraft as the better outcome variable. That said, overdraft interest is never significantly negative for any subgroup of individuals split by income reduction, liquidity, or the frequency of overdraft use.

When we break the results up by income reduction terciles we find that the reduction in spending is largest among individuals whose income is reduced the most. However, we do not find a significant increase in the probability of having an overdraft among any of the groups. The coefficients for the probability of an overdraft are tightly estimated around zero and even in the 3rd income reduction tercile we can rule out an increase in the probability of holding an overdraft that is greater than 5%.

When we break the results up by initial liquidity, we find that the reduction in spending is largest among individuals who have less liquidity. However, we do not find a significant increase in the probability of entering or rolling over an overdraft among the two groups. The coefficients for the probability of an overdraft is tightly estimated around zero and we can rule out an increase in the probability of holding an overdraft that is greater than 5%. That said, we do find that individuals in the low liquidity group decrease their cash holdings by using some of their initial savings to consume. This also reduces their liquidity significantly. However, credit lines, i.e., how much borrowing capacity individuals have until they would hit their overdraft and credit card limits, is insignificant and small as before. Thus, individuals use cash that they have to smooth consumption but are not inclined to borrow any more.

Given that on average individuals overdraw their checking accounts about half the time, we thus find that these overdrafts do not seem to be driven by unemployment. To make that more clear, we also split the sample according to whether or not individuals hold an overdraft most of the time in Table 7. For both high-frequency and low-frequency users of overdrafts, we find similarly small and insignificant responses of overdraft frequency to unemployment. Thus, unemployment does not seem to be the main reason to holding overdrafts.

Whether or not we control for individual fixed effects has a large effect on the size of the coefficients and on the explanatory power. More specifically, the inclusion of individual fixed effects suppresses the regression coefficients while greatly increasing R^2. This underscores the importance of individual characteristics in the amount of borrowing the individual engages in, highlighting potential selection problems and the need to control for time-invariant characteristics (observable and unobservable). To the best of our knowledge, the analysis of individual-level data using fixed-effects regressions with transaction-level, high-frequency data has not been undertaken before in this literature and might explain why we conclude that borrowing is not used to smooth consumption in contrast to some existing papers (Browning and Crossley, 2009; Gruber, 1997; Keys, 2010; Sullivan, 2008).

In Figure 3 we show the impulse response of unemployment on spending controlling for individual and month-by-year fixed effects. Estimated values are with respect to 4 months prior to job loss (period -4) since the standard notice period is 3 months. Clearly, individuals cut their consumption considerably at the onset of unemployment and then increase it gradually.

[Figure 3 about here]

In Figure 4 we look at the impulse response of the probability of holding an overdraft to unemployment controlling for individual and month-by-year fixed effects. Estimated values are with respect to 4 months prior to job loss (period -4) since the standard notice period is 3 months. We do not find significant results and all estimates and standard errors are small.

[Figure 4 about here]

We thus document an obvious discrepancy between our theoretical and empirical results. In the event of an income shock calibrated to the Icelandic unemployment replacement rate, the hyperbolic agents in the model, calibrated to match the real-world borrowing on credit cards that we see, increase their amount borrowed by 319%, the probability to borrow by 31%, and decrease their spending by 7%. While the spending responses matches the data quite well, the borrowing response is completely off. Clearly, in the model, the reason to borrow is to smooth consumption in response to transitory income shocks, however, we do not find such behavior in the data.

6.1 Robustness Checks

In this section we examine potential explanations for the spending responses that we observe during unemployment spells. Among the most plausible explanations for the difference between the predictions of the theoretical models and the empirical evidence are binding liquidity constraints, changes in beliefs about permanent income, and shifts from market goods to home production. In the following subsections we will assess the empirical relevance of each of these potential explanations. At the same time, these additional results provide a number of robustness checks for our main findings.

6.1.1 Binding liquidity constraints

As can be seen in Table 4, interacting unemployment with liquidity does not change our main findings. Liquidity holdings do not appear to explain how individual spending and consumer debt respond to unemployment. Furthermore, as can be seen in Table 2, both employed and unemployed individuals have substantial liquidity or borrowing capacity, i.e., cash holdings or space until they hit their credit and overdraft limits. Unemployed individuals have on average more than \$8,000 in liquidity left which translates into more than 150 days or five months of discretionary spending. The liquidity and credit lines that individuals have and therefore their borrowing capacity does not decrease upon getting unemployed. These findings suggest that it is not liquidity constraints or a decrease in credit supply that deter individuals from borrowing more.

It is interesting to compare these regression results to those in Table A.4 where we show results for individuals who have above and below median liquidity. The results in Table A.4 show that the effect of unemployment on spending is much more pronounced among individuals with low liquidity. When we look at liquidity and credit lines in this table, we see a significant negative change in cash for low-liquidity individuals (i.e., a reduction in checking and savings account balances). A reduction in cash holdings translates into a reduction in liquidity but we do not see decreases in credit lines or borrowing capacity (which is insignificant and small). Most importantly, we do not see any significant effect on the likelihood to borrow and the zero effect is tightly estimated for both low and high liquidity households. Directionally, low liquidity households have a positive but insignificant effect in overdraft debt and high liquidity households a negative effect, therefore, if anything it appears as if low-liquidity households respond to unemployment by borrowing more. That said, we observe a relatively noisy coefficient here. That is the reason we prefer the likelihood to borrow as the outcome variable. Our zero coefficient is very tightly estimated here for both high-liquidity and low-liquidity individuals.

It is also important to note here that sorting based on an endogenous variable (liquidity or income) and running separate regressions for each group is not inconsistent with liquidity playing an important role. However, such sorting does not provide a strong test for the importance of liquidity in determining how spending and borrowing respond to an adverse transitory income shock such as unemployment. After all, individuals with high or low income or liquidity are very different on an uncountable number of dimensions. Interacting unemployment with liquidity holdings prior to job loss provides a more powerful within-individual test and suggests that limited liquidity is not the reason for a lack of borrowing in response to unemployment.

6.1.2 Changes in beliefs about permanent income

The lack of a borrowing response could be explained by a permanent income shock instead of a temporary shortfall. However, when we compare the incomes of individuals before and after their job loss, we find that their income is not permanently lower after they lose their jobs. This can be seen in Figure 5 plotting the evolution of labor income prior to and after the onset of unemployment, independent of the duration of the unemployment spell. The share of the unemployed individuals thus decreases as we move further away from the onset of unemployment and the average labor income increases. Around 11 months after the onset of unemployment there is no statistically significant difference in the average income of individuals who at some point lose their job from their average income prior to the onset of unemployment. This shows that unemployment is indeed a transitory income shock in the setting of our paper. As discussed earlier, throughout the sample period, the Icelandic economy was growing substantially and unemployment was consistently low throughout so that unemployment shocks are transitory income shocks. As mentioned earlier, unused holidays are paid out with the last salary payments for individuals who are asked to leave their jobs. This explains why we see an income increase in the months prior to the start of an unemployment spell.

[Figure 5 about here]

It could be the case that individuals are uncertain about whether the unemployment shock is permanent or transitory, or could even hold systematically biased beliefs about the severity of the shock, and therefore be reluctant to borrow more. Rozsypal and Schlafmann (2017) show that there is over-persistence bias in individual income expectations although Druedahl and Jørgensen (2018) show that people seem to be able to distinguish between permanent and transitory income shocks. Furthermore, there is a study by Spinnewijn (2015) on individual beliefs about the duration of unemployment showing that people underestimate the length of unemployment spells. The effects of misguided beliefs about shock persistence is outlined in a general-equilibrium model in Kozlowski et al. (2016). But even in models such as the one in Rozsypal and Schlafmann (2017), any agent with a concave utility function would borrow more in the event of adverse transitory income shocks. As discussed, we argue that the Icelandic situation makes it unlikely that people think their income is permanently affected. But even if some individuals have wrong beliefs about the temporary nature of the unemployment shock, we should see that some individuals respond with borrowing instead of estimating a tight zero. Moreover, almost all individuals regularly hold high-interest consumer debt, they must build up this debt in some instances but unemployment does not appear to be a reason. Thus, in summary, even though unemployment probably constitutes the largest transitory income shocks individuals typically face, we do not find a borrowing response that would explain why our individuals hold so much debt on a regular basis.

6.1.3 Substitution from market goods towards home production and non-separabilities between consumption and leisure

Theory predicts that if preferences over consumption and leisure are non-separable then individuals will smooth their marginal utility from consumption and adjustments in leisure time can therefore lead to changes in consumption. Furthermore, having more time on their hands implies that the opportunity cost of home production is reduced so the unemployed are likely to substitute market goods with home produced goods, causing a decline in spending. Such time allocation response is plausible but is often difficult to verify empirically due to data limitations. Unemployed workers may also be able to forgo work-related expenses like commuting. However, at the same time many workers receive either subsidized or free meals during working hours and are able to combine their work commute with other activities such as bringing children to school or daycare.[11] Other research (see, e.g., Gruber, 1997; Guler and Taskin, 2013) that has focused on this question has found significant declines in actual expenditures among the unemployed but smaller declines in proxies for consumption or utility. However, because there are arguments for both increases (e.g, no more free meals) and decreases (less commuting costs) in expenditure in relation to being unemployed, it is not clear whether the findings of previous studies that are conducted in a particular setting can necessarily be applied to other settings. This is therefore an empirical question that needs to be investigated in each setting.

We use the richness of our data to look for direct evidence of changes in home production and leisure spending. We re-estimate our main specification for different

[11] Daycare in Iceland is highly subsidized and is provided for children from the age of 6 months. However, getting a spot often requires some waiting and if parents take their children out of the daycare facility during unemployment spells they can in general not get the same slot again once they return to work. Taking children out of daycare in order to reduce expenses during periods of unemployment is therefore highly unlikely. Children start elementary school at the age of 6 and schools are free.

spending categories and the results are shown in Figure 6. If unemployed individuals are substituting from market goods towards more home production, this would be evident through relatively greater decreases in spending on items such as restaurants and relatively smaller decreases or even increases in spending in grocery stores for instance. However, as can be seen in 6, the reduction in restaurant and grocery store spending is remarkably similar. Furthermore, other expenditures that serve as proxies for work related expenses, e.g., fuel spending, is reduced by about as much as proxies for leisure expenses, e.g., recreation. These finding suggest that substitution from market goods to home production as an explanation for the drop in spending upon job loss does not seem to be empirically relevant in the setting of this study.

One potential explanation for a drop in spending during a transitory income decline is that individuals simply consume previously-purchased nonperishables while their spending on perishables remains the same. We can test for this directly by comparing spending in consumption categories which can be labeled as durables and nondurables. Figure 6 shows that we we find a statistically significant reduction in all main consumption categories except for alcohol and that the response of some perishable spending categories, e.g, ready-made-food spending, and some nonperishable spending categories, e.g., home improvement, are similar. This suggests that the perishability of goods that spent on cannot explain our findings.

While Figure 6 shows the average coefficients for the entire unemployment spell, Appendix Figures A.1 and A.2 show the responses over time.

To summarize, our findings based on the financial aggregator data suggest that individuals smooth their consumer credit usage during adverse transitory income shocks while they let consumption adjust by spending less in all categories. Furthermore, even if individuals are able to make up for their reduction in salary income by home production, the question remains: why do we observe such large consumer debt positions in the first place? If large transitory income shocks, such as unemployment, are not the reason as our models predict, then what are the reasons?

6.1.4 Other types of high-interest borrowing

We also do not see an effect of unemployment on the use of payday loans.[12] Furthermore, as can be seen in Figure 7, when investigating the use of payday loans more generally, we can see that a substantial fraction of borrowers are able to borrow money less expensively. The figure shows the distribution of individual liquidity, i.e., space that individuals have until they reach their credit limits, net the amount of the payday loan. For 65% of individuals, this amount is negative, but 35% of individuals could have tapped their existing credit instead of borrowing using a payday loan.

Furthermore, Table 8 analyzes the consumption categories on days in which individuals take out a payday loan versus other days. It can be seen that individuals do not only spend on necessities on days with payday loans but seem to increase spending on discreationary goods and services even more. This supports that high-interest unsecured consumer loans are in most cases not used in response to rainy days and suggests that self-control or poor information are proximate causes of payday borrowing, rather than liquidity constraints. The fact that individuals tend to borrow in response to good news rather than bad news is also documented in Olafsson and Pagel (2019). In this paper, it is shown that the average individual borrows more in response to small income windfalls in the form of lottery payments.

[Figure 7 and Table 8 about here]

Overall, however, our individuals do not use payday loans frequently. Overdrafts are the most common way to utilize high-interest unsecured consumer debt.

[12] Payday loans in Iceland are not contingent on income from paychecks and unemployment benefits provide predictable and regular income payments.

7 Results using the U.S. credit panel

Figure 8 shows the mean changes in credit card balances by quantiles of Bartik unemployment variation. We see that in terms of raw correlations, the largest average changes in total revolving credit card balances (of around \$50 to \$100) are in the lowest deciles of unemployment shocks (i.e. positive employment shocks). Overall, better county employment outcomes are correlated with more, rather than less, borrowing. Figure 8 also breaks down the estimation sample by individuals with or without credit card borrowing slack, taking a first step at distinguishing demand from supply. The more constrained sample has a borrowing response that is only half of the less constrained sample. However, even unconstrained individuals in counties with an unfavorable employment shock do not appear to increase their borrowing. We do not observe a positive relationship between unemployment and borrowing in these figures.

[Figure 8 about here]

Table 9 reports our main regression results based on the CCP, with changes in revolving credit card balances as the outcome variable, and focusing on short horizons of the same period and one quarter from the shock. We first run our regressions with outcomes aggregated to the county level.[13] The estimated average borrowing responses to unemployment are inconsistently signed and statistically insignificant. The range in estimates varies from a -\$62 to \$115 increase in credit card balances for a one percentage point (approximately 1 standard deviation) increase in the total unemployment rate. Looking at the mean borrowing response to employment shocks tends to reject consumption smoothing as a consistent driver of unsecured borrowing. Analysis using alternative measures of employment (percentage changes in employment or changes in logs) provide similar statistically insignificant results.

[13] The first stage F-statistics indicate robust identification (F-statistic > 30).

While we have already controlled for what is likely to be the main determinant of supply-side constraints over this period (lagged credit scores), we also find similar results when only looking at changes in credit card balances amongst those individuals with slack in their utilization ratio as can be seen in Table 10. In addition, when interacting employment shocks with lagged utilization ratios in Table 10, we find a positive coefficient estimate that (if it were statistically significant) would be inconsistent with increases in credit card borrowing by individuals with low utilization ratios and far from their borrowing limits. This further supports an argument that supply-side constraints do not seem to be driving results. We also find that the size of the borrowing response is negatively related to baseline income, i.e., lower income individuals tend to respond to unemployment shocks with more positive changes in borrowing, which would also appear to argue against supply-side constraints. In summary, the lack of a robustly positive relationship between borrowing and unemployment in the U.S. data does not appear to be driven by limited access to credit. Later in this section, we will present additional evidence from changes in credit limits and new inquiries (applications for new credit) that is also inconsistent with an access-to-credit story.

[Tables 9 and 10 about here]

We next rerun our main regression with changes in credit card balances at longer horizons. Standard errors are clustered at the county level. The estimates are not majorly affected as can be seen in Table 11. Many estimated coefficients are approaching statistical precision with relatively tightly estimated standard errors, indicating 95% confidence intervals of +/- \$100 in credit card balances for a 1% (approximately one standard deviation) change in unemployment rate. Most point estimates fall inside of this range and are statistically insignificant.

[Table 11 about here]

Our results thus differ from the ones in Keys et al. (2017), who exploit a similar source of instrumental variation in employment and find a consistently positive relationship between unemployment and borrowing. When we try to replicate their analysis as closely as possible, we note two main differences. First, our results are based on individual-level observations rather than card-level observations aggregated up to the county-level and weighted by the number of credit cards in a county. If individuals with different number of cards behave differently (which seems plausible), a different relationship should be reflected in different estimates from aggregating individual-level rather than card-level data. Second, Keys et al. (2017) focus on the cross-sectional variation of the Bartik employment shock in the first quarter of 2008, examining various longer horizon outcomes in response to that shock, while we focus on shorter-run responses to employment shocks, pursuing a panel analysis of shocks over the period 2000 to 2016.[14]

Table 12 collapses the results of various additional regressions examining additional credit outcomes. As can be seen, the supply of credit does not appear to respond to the cross section of Bartik shocks, as we estimate insignificant coefficients close to zero on changes in total credit card limits. Again, the coefficients appear tightly estimated with larger estimates when examining shocks arising in the previous period. Two more credit outcomes speak to the demand versus supply of credit: the credit utilization ratio and new inquiries. For credit utilization we again estimate an economically insignificant relationship, with coefficients which we would describe as precise zeros (for utilization at the end of the same quarter as the shock, coefficients of less than 0.3%). For inquiries,

[14] If we discard these two differences we can reproduce similarly, economically and statistically significant point estimates using a tradeline data set recently available for the CCP sample. This replication does not exclude other potential factors, such as a different sample. The Consumer Financial Protection Bureau (CFPB)-derived sample of credit card tradeline data is a much larger sample of slightly fewer issuers, and has approximately 90% similar issuer coverage to start with for the period 2008-2014. Keys et al. (2017) eliminate cards from any issuers not observed over the entire period to generate a balanced sample for their analysis, which could lead to larger differences between samples.

we estimate small (while sometimes positive and sometimes negative) coefficients for the whole sample period (and the crisis period from 2008 to 2014), with absolute magnitudes less than 0.01, as shown in Table 12. Again, it does not appear as if individuals are being denied access to credit from existing lenders, at least not as captured by applications for new lines of credit.

When we look at total revolving credit, which includes instruments such as home equity loans, we estimate again economically insignificant coefficients for changes in unemployment as instrumented by Bartik shocks. The coefficients are all small (less than \$15 for a one percentage point increase in unemployment rate), with standard errors of approximately the same magnitude. Finally, looking at delinquencies or non-current balances that predict default, it does not appear as if the source of variation in unemployment we use is statistically related to falling behind on payments. The confidence intervals are wider (95% confidence intervals of +/- \$200 for a one percentage point increase in unemployment rate) and the signs of the point estimates are inconsistent when estimating the relationship of delinquency in the same or the next quarter from the unemployment shock.

[Table 12 about here]

Finally, we examine the conditional distribution of borrowing at the individual level with Bartik-unemployment shocks as the forcing variable. What would we expect to see in the data if individuals use credit cards primarily to smooth consumption in response to industry-predicted employment shocks? We would expect to see at least the upper tail of the distribution in individual borrowing respond positively and strongly. Table 13 therefore reports estimation results based on quantile regressions that examine whether the results vary along the conditional distribution. Across the distribution, we estimate either statistically insignificant borrowing responses or statistically significant, but "wrongly" signed (compared to what consumption smoothing would predict) responses of credit card borrowing to unemployment. The upper end of the conditional distribution of changes in credit card borrowing, if anything, appears to be negatively related to employment conditions. Across the distribution, the only large or statistically significant point estimates we see are negative to unemployment. Focusing on the top half of the conditional distribution, the 75th quantile is estimated with statistically significant precision and indicates a less than $1 increase in response to one standard deviation in the Bartik shock (these shocks are approximately mean 0, standard deviation 2.5%).[15]

The largest increases in borrowing (the upper quantiles) appear to occur regardless of the size of unemployment shock. In a world where credit card borrowing is primarily for consumption smoothing, we would expect to see the upper tail of the distribution meaningfully and positively respond to an increase in unemployment. The fact that we do not suggests that the largest changes in credit card borrowing are for non-consumption smoothing purposes. Instead of a positive relationship to unemployment, we find consistent negative estimates across the distribution. We thus conclude that the primary usage of credit cards is not in response to unemployment.

[Table 13 about here]

To verify the robustness of our results, we conduct two additional checks (results in Appendix Tables A.1 and A.2.). First, we rerun our regressions examining changes in borrowing weighting our regressions with the number of people in a county. Second, we rerun our regressions at the individual level with individual fixed effects (see Equation 5) . When using analytical weights by the population in a county, the point estimates

[15]The quantile regressions are estimated in reduced form, without an instrumented second stage, and with non-robust/unclustered standard errors, which we expect to be non-conservative to statistical significance. Their intent is to illustrate the shape and size of conditional distribution to underlying instrumental variation.

tend to move closer to zero but are now statistically significant, with a percentage point increase in the total unemployment rate associated with a \$30 average increase in credit card balances. Controlling for individual and time fixed effects as well as lagged risk scores and age, we estimate some statistically significant, but all economically small, coefficients of \$20-\$30 increases in credit card borrowing for a one percentage change in the county unemployment rate as instrumented for with Bartik shocks, as can be seen in Table A.2. To interpret the magnitudes of these coefficients, note that the standard deviation in the unemployment rate is less than 1%. The estimates also do not greatly vary depending on the choice of time period (the entire sample available, versus 2008 to 2014 only).

8 Conclusion

Economists believe that high-interest, unsecured, short-term borrowing, for instance via credit cards and overdrafts, can help individuals to smooth consumption in the event of transitory income shocks (Browning and Crossley, 2009; Gruber, 1997; Keys, 2010; Sullivan, 2008). After analyzing two very high-quality data sets, however, we conclude that individuals do not appear to use such credit to smooth consumption through large transitory income shocks due to unemployment. In contrast, it appears as if individuals smooth their debt balances rather than their consumption.

We document this lack of borrowing in response to unemployment directly using a longitudinal data set containing detailed information on consumption, income, and account balances as well as limits from a financial aggregator platform in Iceland. We then turn to U.S. credit card data to replicate the lack of a borrowing response to unemployment shocks using a Bartik (1991) style methodology.

By comparing our empirical results to the theoretical predictions of a state-of-theart model to explain the amount of borrowing we see in the data, we document a striking discrepancy: in the model, agents increase their likelihood to borrow by 31% and their amount borrowed by 319% and decrease their consumption by 7% in response to a negative income shock calibrated to an unemployment event. While the spending response in the model is a bit smaller but roughly matches the empirical finding, the borrowing response clearly does not. In the model, the reason to borrow is to smooth consumption in response to transitory income shocks, however, we do not find such behavior in the data.

Our findings are difficult to reconcile with theories of consumption smoothing that predict that credit demand should be countercyclical and lean against changes in credit supply. On the contrary, our findings show that consumers appear to smooth their unsecured debt burdens rather than their consumption responses. Such behavior could lead to greater consumption volatility than would be observed otherwise.

References

Adelino, M., A. Schoar, and F. Severino (2016). Loan originations and defaults in the mortgage crisis: The role of the middle class. *The Review of Financial Studies 29*(7), 1635–1670.

Agarwal, S., S. Chomsisengphet, N. Mahoney, and J. Stroebel (2017). Do banks pass through credit expansions to consumers who want to borrow? *The Quarterly Journal of Economics 133*(1), 129–190.

Aizer, A. (2010). The gender wage gap and domestic violence. *The American economic review 100*(4), 1847.

Baker, S. R. and C. Yannelis (2017). Income changes and consumption: Evidence from the 2013 federal government shutdown. *Review of Economic Dynamics 23*, 99 – 124.

Bartik, T. J. (1991). Boon or boondoggle? the debate over state and local economic development policies.

Blanchard, O. J., L. F. Katz, R. E. Hall, and B. Eichengreen (1992). Regional evolutions. *Brookings papers on economic activity 1992*(1), 1–75.

Braxton, J. C., K. Herkenhoff, and G. Phillips (2018). Can the unemployed borrow? implications for public insurance. Working paper.

Browning, M. and T. F. Crossley (2009, December). Shocks, stocks, and socks: Smoothing consumption over a temporary income loss. *Journal of the European Economic Association 7*(6), 1169–1192.

Chodorow-Reich, G., L. Feiveson, Z. Liscow, and W. G. Woolston (2012). Does state fiscal relief during recessions increase employment? evidence from the american recovery and reinvestment act. *American Economic Journal: Economic Policy 4*(3), 118–145.

DellaVigna, S. (2009). Psychology and economics: Evidence from the field. *Journal of Economic literature 47*(2), 315–372.

Druedahl, J. and T. Jørgensen (2018). Can consumers distinguish persistent from transitory income shocks?

Fuster, A., G. Kaplan, and B. Zafar (2018). What would you do with $500? spending responses to gains, losses, news and loans.

Ganong, P. and P. Noel (2018). Consumer spending during unemployment: Positive and normative implications. *American Economic Review* (forthcoming).

Gelman, M., S. Kariv, M. D. Shapiro, D. Silverman, and S. Tadelis (2015, March). How individuals smooth spending: Evidence from the 2013 government shutdown using account data. Working Paper 21025, National Bureau of Economic Research.

Gould, E. D., B. A. Weinberg, and D. B. Mustard (2002). Crime rates and local labor market opportunities in the united states: 1979–1997. *The Review of Economics and Statistics 84*(1), 45–61.

Gruber, J. (1997). The consumption smoothing benefits of unemployment insurance. *The American Economic Review 87*(1), pp. 192–205.

Guler, B. and T. Taskin (2013). Does unemployment insurance crowd out home production? *European Economic Review 62*(C), 1–16.

Hundtofte, S. (2017). Credit aversion. Working paper.

Kahneman, D., J. Knetsch, and R. Thaler (1990). Experimental Tests of the Endowment Effect and the Coase Theorem. *Journal of Political Economy 98*(6), 1325–1348.

Kaplan, G. and G. Violante (2014). A model of the consumption response to fiscal stimulus payments. *Econometrica 82*, 1199–1239.

Keys, B., J. Tobacman, and J. Wang (2017). Rainy day credit? unsecured credit and local employment shocks. *Working Paper*.

Keys, B. J. (2010). The credit market consequences of job displacement. *The Review of Economics and Statistics 0*(ja), null.

Kozlowski, J., L. Veldkamp, and V. Venkateswaran (2016). The tail that wags the economy: Belief-driven business cycles and persistent stagnation.

Kuchler, T. and M. Pagel (2015). Sticking To Your Plan: Empirical Evidence on the Role of Present Bias for Credit Card Debt Paydown. *Working Paper*.

Laibson, D. (1997). Golden Eggs and Hyperbolic Discounting. *Quarterly Journal of Economics 112*(2), 443–477.

Laibson, D., P. Maxted, A. Repetto, and J. Tobacman (2017). Estimating discount functions with consumption choices over the lifecycle.

Laibson, D., A. Repetto, and J. Tobacman (2003). A Debt Puzzle. *in Philippe Aghion, Roman Frydman, Joseph Stiglitz, and Michael Woodford, eds., Knowledge, Information, and Expectations in Modern Economics: In Honor of Edmund S. Phelps, Princeton: Princeton University Press*.

Lee, D. and W. Van der Klaauw (2010). An introduction to the frbny consumer credit panel.

Maestas, N., K. J. Mullen, and D. Powell (2016). The effect of population aging on economic growth, the labor force and productivity. Technical report, National Bureau of Economic Research.

Mian, A., K. Rao, and A. Sufi (2013). Household balance sheets, consumption, and the economic slump. *The Quarterly Journal of Economics 128*(4), 1687–1726.

Mian, A. and A. Sufi (2009). The consequences of mortgage credit expansion: Evidence from the us mortgage default crisis. *The Quarterly Journal of Economics 124*(4), 1449–1496.

Müller, K. (2018). Credit markets around the world, 1910-2014. *Available at SSRN 3259636*.

Nguyen, H.-L., M. Greenstone, and A. Mas (2015). *Essays on Banking and Local Credit Markets*. Ph. D. thesis, Massachusetts Institute of Technology.

O'Donoghue, T. and M. Rabin (1999). Doing it Now or Later. *American Economic Review 89*(1), 103–124.

Olafsson, A. and M. Pagel (2017, October). The ostrich in us: Selective attention to financial accounts, income, spending, and liquidity. Working Paper 23945, National Bureau of Economic Research.

Olafsson, A. and M. Pagel (2018a). The liquid hand-to-mouth: Evidence from personal finance management software. *Review of Financial Studies*.

Olafsson, A. and M. Pagel (2018b, March). The retirement-consumption puzzle: New evidence from personal finances. Working Paper 24405, National Bureau of Economic Research.

Olafsson, A. and M. Pagel (2019). Borrowing in response to windfalls. *Working Paper*.

Rozsypal, F. and K. Schlafmann (2017). Overpersistence bias in individual income expectations and its aggregate implications.

Shapiro, M. D. and J. Slemrod (1995, March). Consumer response to the timing of income: Evidence from a change in tax withholding. *American Economic Review 85*(1), 274–83.

Spinnewijn, J. (2015). Unemployed but optimistic: Optimal insurance design with biased beliefs. *Journal of the European Economic Association 13*(1), 130–167.

Sullivan, J. X. (2008). Borrowing during unemployment unsecured debt as a safety net. *Journal of human resources 43*(2), 383–412.

Figures and tables

Table 1: The Effect of Low Income on Borrowing and Consumption in the Model of Laibson et al. (2017)

	(1) Log of total borrowing	(2) Indicator for borrowing	(3) Log of total spending
Hyperbolic-discounting agent:			
Income below 67th percentile	3.1926***	0.3126***	-0.0673***
	(0.0061)	(0.0006)	(0.0019)
Standard agent:			
Income below 67th percentile	0.0213***	0.0026***	-0.0301***
	(0.0005)	(0.0001)	(0.0009)
# of Observations	71,000	71,000	71,000
Age Fixed Effects	✓	✓	✓

This table shows the estimated effect of an income realization below the 67th percentile in the simulated data of the model in Laibson et al. (2017), featuring an illiquid asset, credit card borrowing, liquidity constraints, and stochastic labor income. The hyperbolic discounting agent borrows on average 35% of the time and the standard agent 0.15% of the time. Standard errors are within parentheses. Each entry is a separate regression.

Table 2: Summary Statistics for Aggregator Dataset

	Employed		Unemployed	
	Mean	St. Dev.	Mean	St. Dev.
Demographics:				
Age	38.6	11.0	39.2	11.4
Female	0.58	0.49	0.60	0.49
Spending:				
Total Spending	165,066	157,037	150,965	130,038
Income:				
Total Income	348,674	486,943	207,778	323,831
Regular Income	333,980	469,847	191,147	279,576
Irregular Income	14,694	115,388	16,631	161,957
Salary	301,352	446,575	63,692	231,674
Unemployment Benefits	2,346	17,242	97,767	94,925
Balances (ISK):				
Checking Account Balance	191,412	1,051,303	145,159	640,524
Savings Account Balance	199,108	1,093,441	216,563	1,786,336
Cash	390,520	1,521,741	361,722	1,914,925
Liquidity	936,815	1,800,455	831,921	2,030,023
Balances (average consumption days):[a]				
Cash	65.9	223.4	61.1	218.9
Liquidity	162.5	352.5	152.2	247.0
Overdrafts and credit lines:				
Credit Lines[b]	2.253	5.033	2.369	3.081
Overdraft Indicator	0.424	0.494	0.428	0.495
# of Overdrafts	0.486	0.622	0.473	0.589
Other short-term debt:				
Payday Loan	84	2,444	76	2,083
Payday Loan Uptake	0.20%	4.60%	0.20%	4.50%

This table contains simple raw data sample means and standard deviations comparing individuals that are unemployed versus those who are employed. All numbers are in Icelandic krona where applicable. 1 USD $\approx$ 100 ISK.
Notes: [a] Measured in days of average spending. [b] Measured in months of average income.

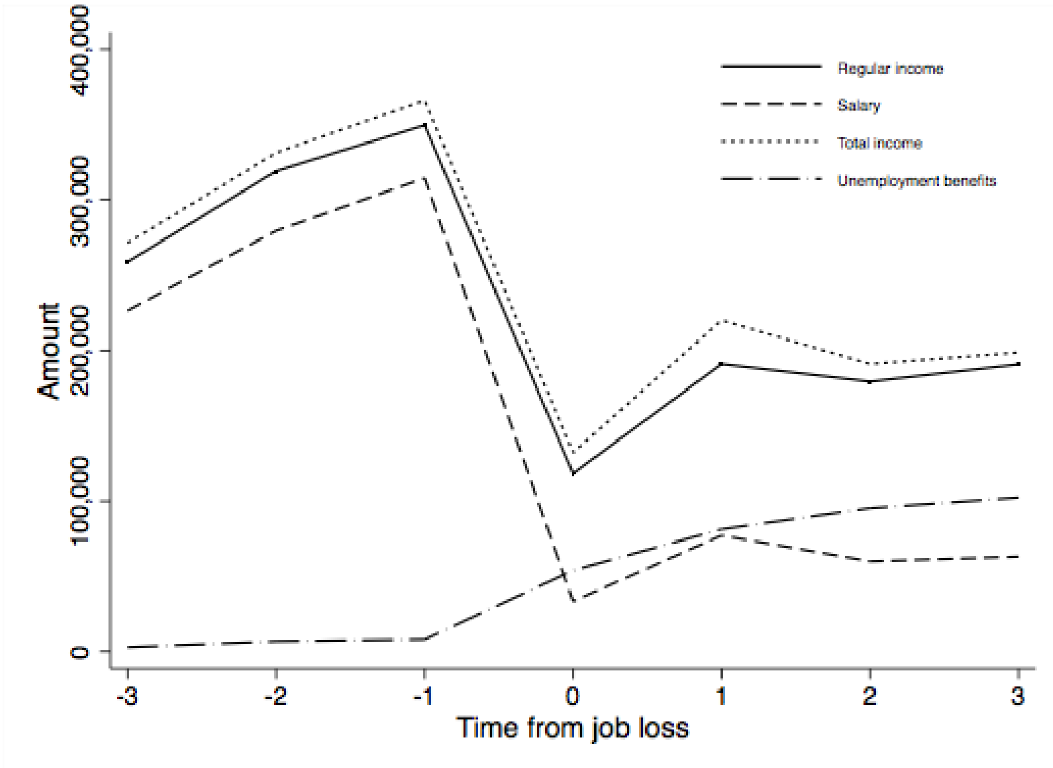


Figure 1: Income and unemployment benefits in the months around job loss, raw data averages.

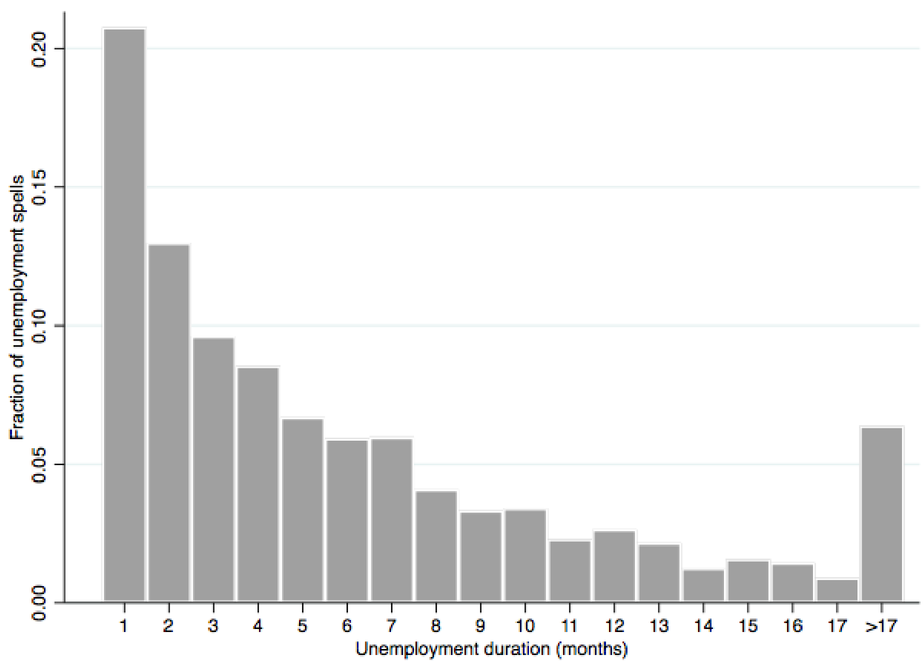


Figure 2: Duration of unemployment in months, raw data.

Table 3: Summary Statistics for U.S. Credit Panel

Variable	Observations	Mean	St. Dev	Min	Max
Balances, limits, utilization ratios, credit score, and inquiries:					
Change in credit card balance	16M	35.4	1,661.4	-8,137	7,318
Change in credit card limits	16M	105.1	3,341.0	-15,700	15,000
Change in credit card utilization ratio	10.3M	0.0026	0.1638	-0.69	0.62
Utilization ratio (credit card)	10.4M	0.4548	13.2874	0	20,083
Credit Score	14.8M	690.9	105.2	416	828
Number of inquires within 3 months	15.4M	0.4106	0.8315	0	4
Revolving balances, limits, and non-current balances:					
Change in any revolving balance	16M	19.4	636.2	-2,695	4,409
Change in revolving limits	16M	21.3	1,108.7	-5,416	6,050
Utilization ratio (all revolving)	11.6M	0.4818	59.6042	0	83,550
Change total debt balance	12.7M	693.2	24,352.4	-119,967	151,625
Total debt balance	13.1M	77,777	154,683	0	9,999,999
Change in non-current balances	11.7M	53.4	2,752.9	-15,002	18,129
Unemployment and demographics:					
Change in unemployment rate	15.7M	0.0003	0.7581	-2.13	2.60
Bartik Employment Shock	15.2M	0.0050	0.0210	-0.17	0.29
Age	12.7M	50.8	18.1	20	93
Per-capita Income	16M	30,543	8,532	16,659	60,755

Notes: This table contains simple raw data sample means and standard deviations. All statistics generated using individual-quarter observations in the FRBNY/Equifax Consumer Credit Panel (CCP) for the period 2000-2016, other than county-level Bartik Employment Shock (Quarterly Census data) and per-capita income (BEA).

Table 4: The Effects of Unemployment on Consumption and Consumer Credit Usage

	(1) Total Spending	(2) Necessities	(3) Discretionary Entertainment	(4) Cash	(5) Liquidity	(6) Overdraft Indicator	(7) # of Overdrafts	(8) Overdraft Interest	(9) Late Fees	(10) Credit Lines
					With individual fixed effects:					
Unemployed	-0.079***	-0.052	-0.118***	-0.015	0.013	0.005	-0.002	0.006	-0.012	-0.017
	(0.018)	(0.042)	(0.038)	(0.066)	(0.048)	(0.010)	(0.012)	(0.083)	(0.066)	(0.022)
R^2	0.084	0.024	0.041	0.019	0.039	0.002	0.002	0.001	0.007	0.026
					Including liquidity interactions:					
Unemployed	-0.070***	-0.024	-0.109**	-0.138	-0.063	0.006	0.000	0.007	-0.068	-0.024
	(0.021)	(0.050)	(0.046)	(0.110)	(0.091)	(0.012)	(0.013)	(0.097)	(0.076)	(0.037)
Unemployed × Liquidity$_{t-4}$	-0.009	-0.029	-0.007	0.042*	0.022	-0.002	-0.003	-0.014	0.025	0.003
	(0.009)	(0.020)	(0.016)	(0.025)	(0.018)	(0.004)	(0.005)	(0.031)	(0.027)	(0.008)
Liquidity$_{t-4}$	0.004	0.010	-0.001	-0.016	0.015	0.002	0.001	0.027	0.041**	-0.001
	(0.005)	(0.012)	(0.011)	(0.016)	(0.013)	(0.003)	(0.004)	(0.026)	(0.019)	(0.006)
R^2	0.084	0.024	0.041	0.019	0.040	0.002	0.002	0.001	0.007	0.026
					Without individual fixed effects:					
Unemployed	-0.274***	-0.259***	-0.310***	-0.487***	-0.308***	-0.002	-0.023	-0.024	0.404***	-0.054
	(0.024)	(0.049)	(0.047)	(0.109)	(0.074)	(0.016)	(0.019)	(0.138)	(0.095)	(0.042)
R^2	0.044	0.013	0.028	0.005	0.012	0.007	0.006	0.005	0.007	0.003
# of Observations	649,372	649,372	649,372	308,110	308,110	621,221	621,221	621,221	621,221	309,039
# of Individuals	10,856	10,856	10,856	10,856	10,856	10,856	10,856	10,856	10,856	10,856
Month-by-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

* p<0.1, ** p<0.05, *** p<0.01 This table shows the estimated effect of being unemployed. Standard errors are clustered at the individual level and are within parentheses. Each entry is a separate regression. Cash holdings are checking and savings account balances and liquidity is checking and savings account balances plus overdraft limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals. Credit lines are measured as distance to credit limits and normalized by average individual income.

Table 5: The Effects of Unemployment by Income Reduction Terciles

	(1) Total Spending	(2) Necessities	(3) Discretionary Entertainment	(4) Cash	(5) Liquidity	(6) Overdraft Indicator	(7) # of Overdrafts	(8) Overdraft Interest	(9) Late Fees	(10) Credit Lines
1st income reduction tercile:										
Unemployed	0.038	0.134**	-0.003	0.090	-0.040	-0.016	-0.034	-0.185	0.034	-0.041
	(0.027)	(0.060)	(0.058)	(0.103)	(0.084)	(0.017)	(0.021)	(0.145)	(0.106)	(0.039)
R^2	0.098	0.035	0.044	0.009	0.039	0.007	0.007	0.009	0.012	0.035
# of Observations	15,873	15,873	15,873	7,845	7,845	15,281	15,281	15,281	15,281	7,863
# of Individuals	345	345	345	345	345	345	345	345	345	345
2nd income reduction tercile:										
Unemployed	-0.064**	-0.033	-0.059	-0.147	-0.020	0.025	0.014	0.157	0.043	-0.006
	(0.030)	(0.074)	(0.063)	(0.112)	(0.078)	(0.016)	(0.019)	(0.132)	(0.115)	(0.033)
R^2	0.093	0.036	0.056	0.024	0.047	0.005	0.004	0.005	0.010	0.036
# of Observations	16,882	16,882	16,882	7,946	7,946	16,313	16,313	16,313	16,313	7,962
# of Individuals	345	345	345	345	345	345	345	345	345	345
3rd income reduction tercile:										
Unemployed	-0.227***	-0.260***	-0.314***	-0.025	0.077	0.021	0.032	0.174	-0.028	-0.007
	(0.040)	(0.093)	(0.087)	(0.129)	(0.093)	(0.019)	(0.021)	(0.156)	(0.121)	(0.041)
R^2	0.103	0.041	0.053	0.036	0.050	0.005	0.006	0.003	0.012	0.024
# of Observations	15,563	15,563	15,563	7,835	7,835	15,121	15,121	15,121	15,121	7,863
# of Individuals	346	346	346	346	346	346	346	346	346	346
Month-by-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

* p<0.1, ** p<0.05, *** p<0.01 This table shows the estimated effect of being unemployed. Standard errors are clustered at the individual level and are within parentheses. Each entry is a separate regression. Cash holdings are checking and savings account balances and liquidity is checking and savings account balances plus overdraft limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals. Credit lines are measured as distance to credit limits and normalized by average individual income.

Table 6: Effects of Unemployment by Liquidity at the Time of Job Loss

	(1) Total Spending	(2) Necessities	(3) Discretionary Entertainment	(4) Cash	(5) Liquidity	(6) Overdraft Indicator	(7) # of Overdrafts	(8) Overdraft Interest	(9) Late Fees	(10) Credit Lines
Below median:										
Unemployed	-0.065**	-0.057	-0.066	-0.188**	-0.066	0.021	0.010	0.134	0.060	-0.005
	(0.033)	(0.080)	(0.062)	(0.092)	(0.068)	(0.024)	(0.026)	(0.191)	(0.189)	(0.027)
R^2	0.046	0.023	0.022	0.022	0.096	0.023	0.023	0.028	0.023	0.108
# of Observations	3,459	3,459	3,459	3,181	3,181	3,451	3,451	3,451	3,451	3,186
# of Individuals	283	283	283	283	283	283	283	283	283	283
Above median:										
Unemployed	-0.072	-0.076	-0.105	0.225*	0.090	-0.021	-0.037	-0.179	-0.093	-0.012
	(0.057)	(0.135)	(0.098)	(0.129)	(0.061)	(0.022)	(0.034)	(0.182)	(0.170)	(0.037)
R^2	0.059	0.020	0.035	0.027	0.029	0.024	0.033	0.029	0.018	0.021
# of Observations	3,440	3,440	3,440	3,169	3,169	3,434	3,434	3,434	3,434	3,174
# of Individuals	279	279	279	279	279	279	279	279	279	279
Month-by-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

* $p<0.1$, ** $p<0.05$, *** $p<0.01$ This table shows the estimated effect of being unemployed. Standard errors are clustered at the individual level and are within parentheses. Each entry is a separate regression. Cash holdings are checking and savings account balances and liquidity is checking and savings account balances plus overdraft limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals. Credit lines are measured as distance to credit limits and normalized by average individual income.

Table 7: The Effects of Unemployment by Low and High Frequency Users of Overdrafts

	(1) Total Spending	(2) Necessities	(3) Discretionary Entertainment	(4) Cash	(5) Liquidity	(6) Overdraft Indicator	(7) # of Overdrafts	(8) Overdraft Interest	(9) Late Fees	(10) Credit Lines
Low frequency users:										
Unemployed	-0.077***	-0.052	-0.094*	0.075	0.040	0.006	0.008	0.048	-0.166*	-0.015
	(0.025)	(0.061)	(0.054)	(0.081)	(0.058)	(0.011)	(0.012)	(0.089)	(0.087)	(0.020)
R^2	0.083	0.030	0.042	0.017	0.028	0.014	0.015	0.013	0.012	0.036
# of Observations	18,971	18,971	18,971	10,420	10,420	18,332	18,332	18,332	18,332	10,450
# of Individuals	647	647	647	647	647	647	647	647	647	647
High frequency users:										
Unemployed	-0.067***	0.003	-0.100*	-0.109	-0.055	0.004	-0.007	-0.023	0.089	-0.016
	(0.025)	(0.062)	(0.052)	(0.099)	(0.073)	(0.013)	(0.017)	(0.115)	(0.089)	(0.035)
R^2	0.075	0.025	0.036	0.022	0.062	0.003	0.004	0.005	0.012	0.047
# of Observations	21,871	21,871	21,871	11,616	11,616	21,937	21,937	21,937	21,937	11,643
# of Individuals	703	703	703	703	703	703	703	703	703	703
Month-by-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

* p<0.1, ** p<0.05, *** p<0.01 This table shows the estimated effect of being unemployed. Standard errors are clustered at the individual level and are within parentheses. Each entry is a separate regression. Cash holdings are checking and savings account balances and liquidity is checking and savings account balances plus overdraft limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals. Credit lines are measured as distance to credit limits and normalized by average individual income.

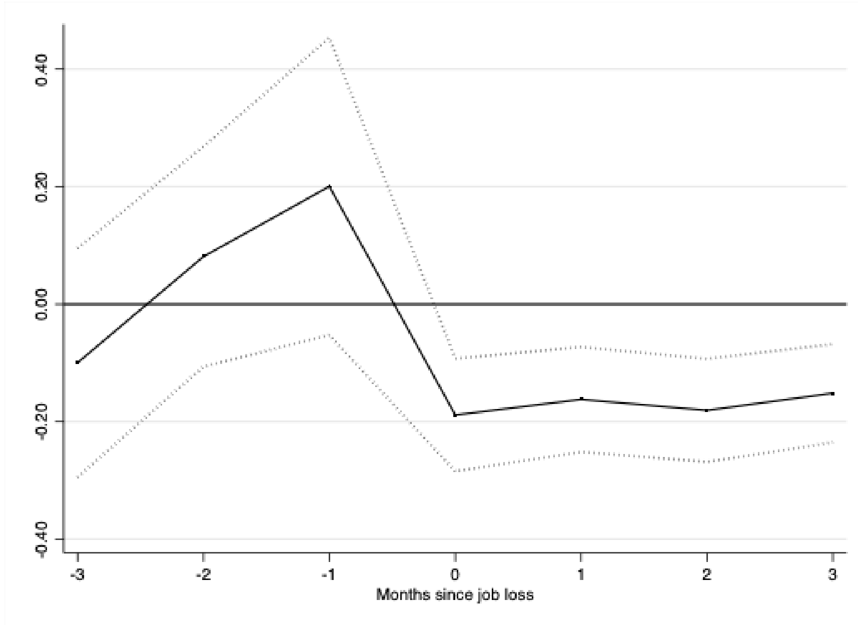


Figure 3: The impulse response of expenditure to unemployment

Notes: Regression coefficients and 95% confidence intervals for each month before and after unemployment with individual and month-by-year fixed effects, estimated with respect to all months prior to 3 months before start of job loss.

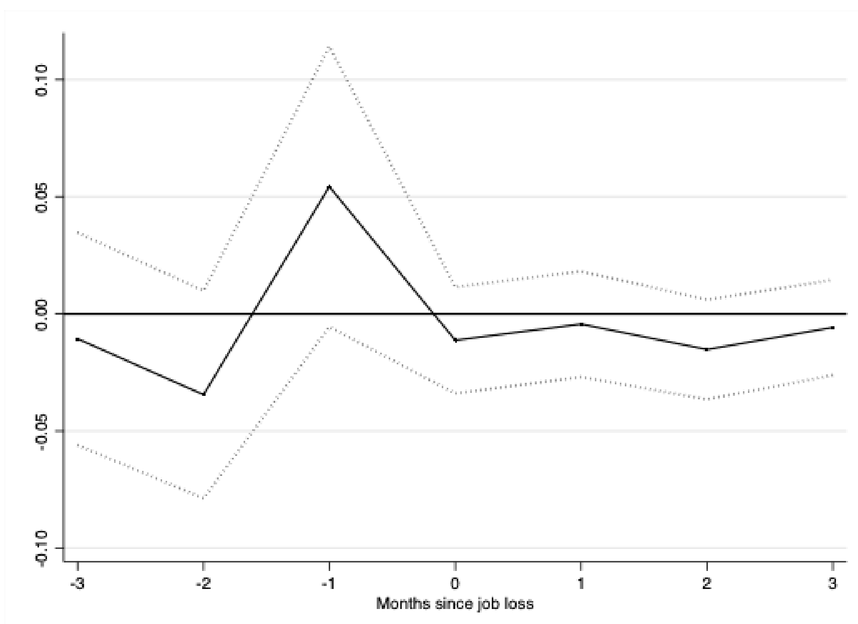


Figure 4: The impulse response of overdraft usage to unemployment

Notes: Regression coefficients and 95% confidence intervals for each month before and after unemployment with individual and month-by-year fixed effects, estimated with respect to all months prior to 3 months before start of job loss.

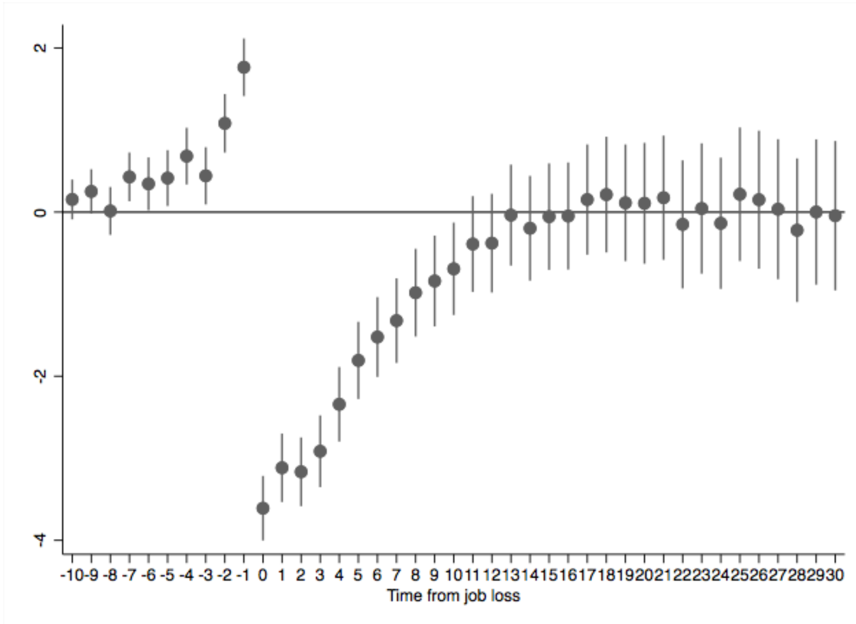


Figure 5: Labor income prior to and after onset of unemployment

Notes: Regression coefficients and standard error bars for labor income calculated in constant prices regressed on each month before and after unemployment controlling for individual and month-by-year fixed effects.

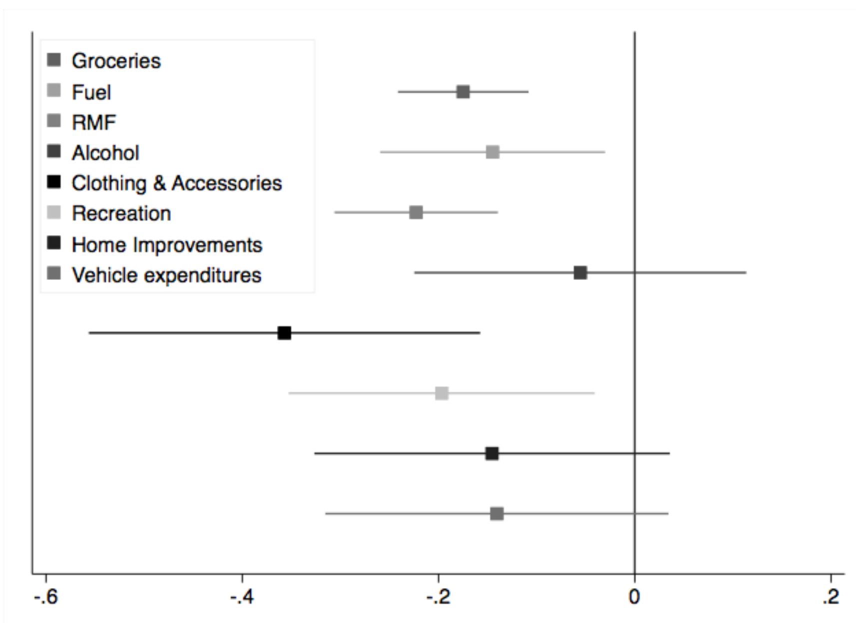


Figure 6: Spending responses by consumption category

Notes: Regression coefficients and standard error bars for each consumption category controlling for individual and month-by-year fixed effects.

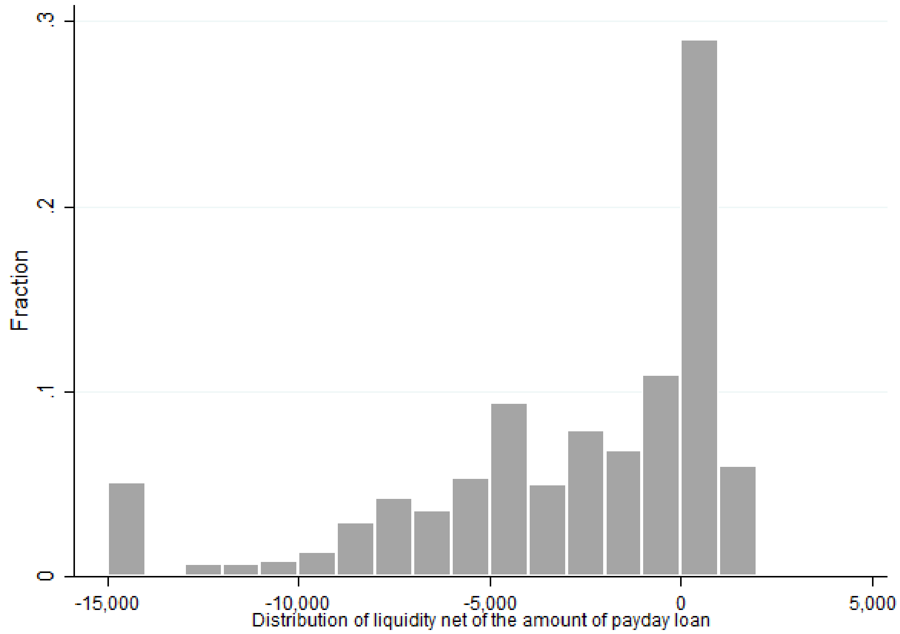


Figure 7: Distribution of liquidity net of the amount of payday loan

Table 8: Daily Expenditures by Payday Loan Users

	Days Receiving Loans		Days Without Loans		% Increase
	Mean	St. Dev.	Mean	St. Dev.	
Total Expenditures	62.3	75.7	29.3	99.5	113%
Groceries	23.2	40.6	10.6	30.3	120%
Fuel	13.0	30.1	5.4	20.6	140%
Alcohol	3.8	14.0	1.4	9.8	162%
Ready-Made-Food	12.3	24.2	5.1	16.5	139%
Home Improvement	2.6	17.3	2.0	27.8	28%
Transportation	2.4	17.6	1.4	73.2	67%
Clothing and Accessories	2.2	18.5	1.4	17.2	55%
Sports and Activities	1.2	12.0	0.8	12.9	46%
Pharmacies	1.5	8.1	0.9	7.2	61%

Note: All numbers are in U.S. dollars.

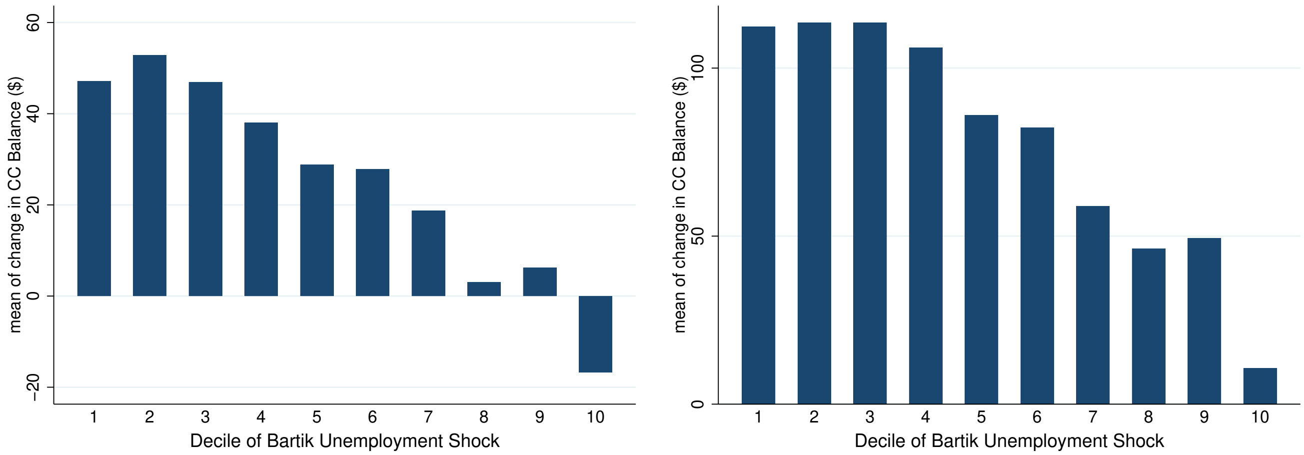


Figure 8: Mean changes in credit card balances by unemployment shock decile in U.S. data.

Notes: The observations underlying this figure are at the individual-quarter level, from a 0.1% representative sample of U.S. credit reporting from the FRBNY/Equifax CCP. Mean changes in credit card balances are arranged by decile of county-level Bartik unemployment shock experienced (decile 10 represents the largest unemployment shock). The figure on the left is generated for the whole sample, while the figure on the right is only for individuals with credit card "slack", where slack is defined as having a utilization ratio on their credit cards in the previous quarter of less than 0.9.

Table 9: Unemployment Shocks and Changes in County-Level Credit Card Balances

	Δ Credit Card Balance			
Δ Unemployment	57.9	-62.4		
	(48.2)	(53.4)		
Age	-4.60***	-5.93***		
	(0.59)	(1.18)		
Lagged Risk Score	2.06***	2.71***		
	(0.13)	(0.24)		
Lagged Δ Unemployment			114.8**	28.6
			(54.2)	(60.8)
Lagged Age			-3.77***	-5.68***
			(0.66)	(1.31)
2×Lagged Risk Score			1.68***	2.25***
			(0.14)	(0.27)
First-stage/Kleibergen-Paap F-stat	31.37	31.37	31.37	31.37
Time and County Fixed Effects	✓	✓	✓	✓
Time Period	2000-2016	2008-2014	2000-2016	2008-2014
# of Observations	178,114	84,485	178,057	84,439
R^2	0.025	0.058	0.002	0.064

* p<0.1, ** p<0.05, *** p<0.01

Notes: This table presents the second stage of 2SLS estimates as detailed in section 3.2, with county-quarter observations of the mean change in credit card balances from the FRBNY/Equifax CCP. "Unemployment" refers to total unemployment rate (in %) at the county-level. "Risk Score" is an Equifax credit score similar to FICO in construction and scale. The change in the (lagged) county unemployment rate is instrumented for with the Bartik employment shock at the county level. Controls for county and time (month-by-year) fixed effects as well as age and lagged risk scores are included. Standard errors are clustered at the county level and are within parentheses.

Table 10: Change in County-Level Credit Card Balances by Ex-Ante Borrowing Constraints

	Δ Credit Card Balance		Δ Credit Card Balance (if slack in utilization ratio)
Δ Unemployment	18.9	116.8***	10.9
	(38.0)	(35.2)	(602.6)
Δ Unemployment x Utilization Ratio	93.7***		
	(29.3)		
Δ Unemployment x Income		-0.000024***	
		(0.0000092)	
Utilization Ratio	-343.8***		
	(18.0)		
Age	-4.40***	-4.60***	-6.88***
	(0.63)	(0.59)	(1.02)
Lagged Risk Score	1.17***	2.08***	2.89***
	(0.13)	(0.13)	(0.20)
Time and County Fixed Effects	✓	✓	✓

* p<0.1, ** p<0.05, *** p<0.01
Notes: This Table regresses mean changes in credit card borrowing at the county-level on the (lagged) county unemployment rate, as instrumented by the Bartik employment shock, interacted with the average lagged utilization ratio (balances divided by credit limit) and income (BEA, 2000) for that county, with controlling time and county fixed effects as well as age and lagged risk scores. Standard errors are clustered at the county level and are within parentheses.

Table 11: Longer-Horizon Changes in Credit Card Borrowing

# of Lags	Δ Credit Card Balance						
Δ 2 × Lagged Unemployment	-70.3						
	(47.0)						
Δ 3 × Lagged Unemployment		-88.3*					
		(52.5)					
Δ 4 × Lagged Unemployment			60.7				
			(48.3)				
Δ 5 × Lagged Unemployment				150.3***			
				(56.6)			
Δ 6 × Lagged Unemployment					45.6		
					(49.2)		
Δ 7 × Lagged Unemployment						-43.8	
						(59.2)	
Δ 8 × Lagged Unemployment							38.2
							(52.2)
Age and Risk Score Same × Lagged	✓	✓	✓	✓	✓	✓	✓
Time and County Fixed Effects	✓	✓	✓	✓	✓	✓	✓
# of Observations	178,000	177,956	177,919	174,843	171,774	168,713	165,648

* p<0.1, ** p<0.05, *** p<0.01

Notes: This table presents results of estimating linear regressions of the change in county-level credit card balances on (lagged) changes in county unemployment rates as instrumented by Bartik employment shocks at the county level, controlling for time fixed effects as well as age and lagged risk scores. Standard errors are clustered at the county level and are within parentheses.

Table 12: Other Credit Outcomes on Unemployment Shocks

Outcome	Estimated Coefficients on Δ Predicted Unemployment Rate			
Δ Credit Limits	-17.1 (105.7)	35.0 (111.8)	84.7 (89.4)	-37.0 (98.1)
Δ Utilization ratio on credit cards	0.0034 (0.0063)	-0.0057 (0.0075)	-0.00076 (0.0062)	-0.010 (0.0072)
Credit Inquiries	-0.011 (0.0093)	-0.020* (0.012)	0.0051 (0.0093)	-0.0062 (0.012)
Δ Total revolving acct. balances	8.7 (13)	-6.1 (15)	9.7 (13)	15.0 (16)
Δ Non-current Balances	59.4 (83.8)	116.2 (98.9)	-12.4 (85.1)	-47.2 (98.6)
Time Period:	2000 to 2016	2008 to 2014	2000 to 2016	2008 to 2014
Δ Unemployment Quarter:	Same	Same	Previous	Previous

* $p<0.10$, ** $p<0.05$, *** $p<0.01$ Notes: This table summarizes the results of separate regressions of various credit outcomes on the (lagged) county unemployment rate instrumented by the Bartik employment shock, with controls for individual and time fixed effects as well as age and individual lagged credit scores. Standard errors are clustered at the county level and are within parentheses.

Table 13: Quantile Regressions of Changes in Credit Card Balances on Bartik Unemployment Shock

Percentile:	2nd	5th	10th	25th	50th	75th	90th	95th	98th
					Δ Credit Card Balance				
Bartik Unemployment	-38.5 (7,376.9)	-26.3 (2,382.3)	-17.1 (978.4)	-720.7*** (196.8)	-3.52 *** (0.31)	-16.0* (6.96)	-888.8 (922.7)	-2,522.2 (2,767.7)	-3,926.7 (5,652.1)
Time and County Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓
# of Observations					13,866,844				

* p<0.10, ** p<0.05, *** p<0.01

Notes: this table presents estimates of quantile regressions using individual-quarter observations, with Bartik employment shocks (reversed in polarity to represent unemployment) at the county-level and with individual and time-period fixed effects.

A Appendix

Table A.1: WLS estimates of Main Borrowing Relationship

	Δ Credit Card Balance	
	(1)	(2)
Δ Unemployment	31.9*** (8.98)	
Age	-3.95*** (0.30)	
Lagged Risk Score	1.73*** (0.056)	
Lagged Δ Unemployment		28.3*** (8.61)
Lagged Age		-3.53*** (0.31)
2×Lagged Risk Score		1.53*** (0.058)
Time and County Fixed Effects	✓	✓
# of Observations	178,114	178,057
R^2	0.115	0.119

* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Notes: This table presents WLS estimates of regressions of the change in county-level credit card balances on (lagged) county unemployment rate as instrumented by the Bartik employment shock. Weights are the number of sampled individuals at the county-level. Controls for time fixed effects as well as age and lagged risk scores. Standard errors are clustered at the county level and are within parentheses.

Table A.2: Changes in Credit Card Borrowing - Individual-Level

Variable	Δ Credit Card Balance			
Δ Unemployment	31.9***	20.3		
	(11.0)	(12.9)		
Age	-25.0***	-15.7***		
	(1.43)	(2.34)		
Lagged Risk Score	4.95***	5.92***		
	(0.043)	(0.059)		
Lagged Δ Unemployment			15.7*	18.9*
			(9.64)	(10.7)
Lagged Age			-20.1***	-3.39
			(1.27)	(2.18)
2× Lagged Risk Score			4.27***	5.13***
			(0.037)	(0.057)
Individual and Time Fixed Effects	✓	✓	✓	✓
Time Period	2000 to 2016	2008 to 2014	2000 to 2016	2008 to 2014
# of Observations	10,629,462	4,834,367	10,598,148	4,811,285

* p<0.10, ** p<0.05, *** p<0.01

Notes: Linear regression of the change in individual total credit card limits on the (lagged) county unemployment rate instrumented by the Bartik employment shock at the county level controlling for individual and time fixed effects as well as age and individual lagged risk scores. Standard errors are clustered at the county level and are within parentheses.

Table A.3: The Effect of Unemployment Interacted With Reduction in Monthly Income

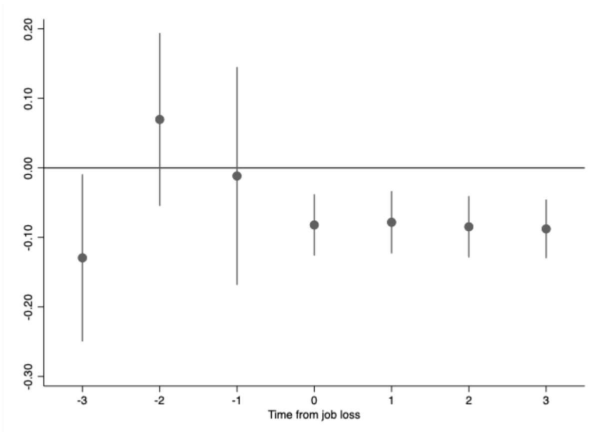
	(1) Total Spending	(2) Necessities	(3) Discretionary Entertainment	(4) Cash	(5) Liquidity	(6) Overdraft Indicator	(7) # of Overdrafts	(8) Overdraft Interest	(9) Late Fees	(10) Credit Lines
Unemployed	0.022	0.090	-0.023	0.171	-0.034	-0.015	-0.028	-0.185	-0.007	-0.053
	(0.027)	(0.064)	(0.061)	(0.110)	(0.089)	(0.017)	(0.021)	(0.144)	(0.110)	(0.040)
Unemployed ×	-0.261***	-0.378***	-0.299***	-0.277*	0.144	0.037	0.056*	0.346	0.014	0.062
H. red.	(0.049)	(0.111)	(0.105)	(0.163)	(0.117)	(0.025)	(0.030)	(0.211)	(0.167)	(0.054)
Unemployed ×	-0.082**	-0.112	-0.042	-0.289*	0.020	0.027	0.028	0.263	-0.024	0.050
M. red.	(0.039)	(0.093)	(0.083)	(0.155)	(0.121)	(0.023)	(0.028)	(0.198)	(0.155)	(0.053)
R^2	0.084	0.024	0.041	0.019	0.040	0.002	0.002	0.001	0.007	0.026
# of Observations	649,372	649,372	649,372	308,110	308,110	621,221	621,221	621,221	621,221	309,039
# of Individuals	11,545	11,545	11,545	11,545	11,545	11,545	11,545	11,545	11,545	11,545
Month-by-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

* p<0.1, ** p<0.05, *** p<0.01
This table shows the estimated effect of being unemployed. Standard errors are within parentheses. Each entry is a separate regression. Cash holdings are checking and savings account balances and liquidity is checking and savings account balances plus overdraft limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals. Credit lines are measured as distance to credit limits and normalized by average individual income.

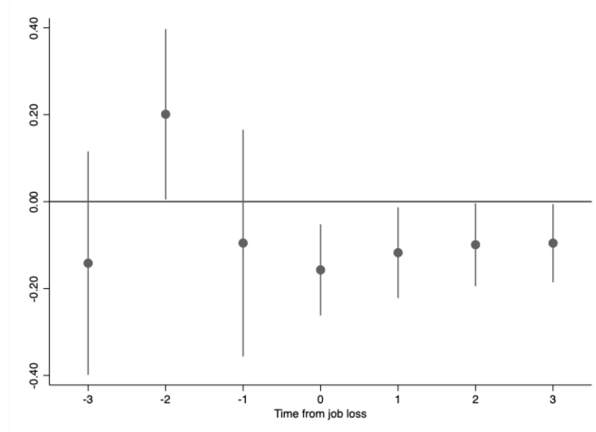
Table A.4: Effects of Unemployment by Cash Holdings at the Time of Job Loss

	(1) Total Spending	(2) Necessities	(3) Discretionary Entertainment	(4) Cash	(5) Liquidity	(6) Overdraft Indicator	(7) # of Overdrafts	(8) Overdraft Interest	(9) Late Fees	(10) Credit Lines
Below median:										
Unemployed	-0.065*	0.011	-0.122*	-0.060	0.063	0.018	0.006	0.149	-0.034	0.024
	(0.036)	(0.083)	(0.070)	(0.102)	(0.060)	(0.019)	(0.031)	(0.176)	(0.178)	(0.035)
R^2	0.048	0.018	0.024	0.069	0.100	0.019	0.024	0.021	0.023	0.047
# of Observations	3,447	3,447	3,447	3,179	3,179	3,449	3,449	3,449	3,449	3,186
# of Individuals	283	283	283	283	283	283	283	283	283	283
Above median:										
Unemployed	-0.076	-0.142	-0.053	0.123	-0.035	-0.016	-0.027	-0.183	0.036	-0.046
	(0.058)	(0.142)	(0.096)	(0.118)	(0.071)	(0.027)	(0.030)	(0.199)	(0.179)	(0.029)
R^2	0.056	0.024	0.030	0.016	0.021	0.013	0.013	0.014	0.014	0.045
# of Observations	3,452	3,452	3,452	3,171	3,171	3,436	3,436	3,435	3,436	3,174
# of Individuals	279	279	279	279	279	279	279	279	279	279
Month-by-Year Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Individual Fixed Effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

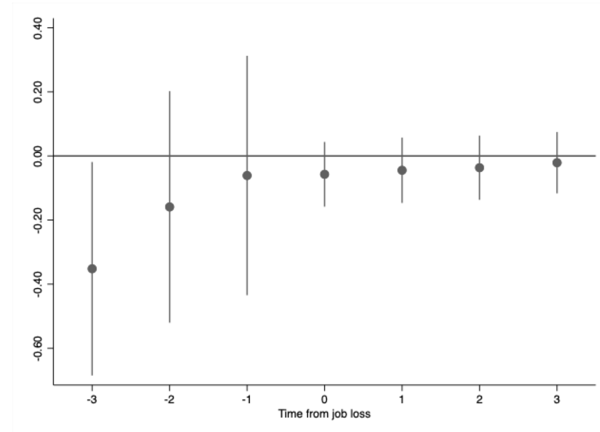
* $p<0.1$, ** $p<0.05$, *** $p<0.01$ This table shows the estimated effect of being unemployed. Standard errors are clustered at the individual level and are within parentheses. Each entry is a separate regression. Cash holdings are checking and savings account balances and liquidity is checking and savings account balances plus overdraft limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals. Credit lines are measured as distance to credit limits and normalized by average individual income.



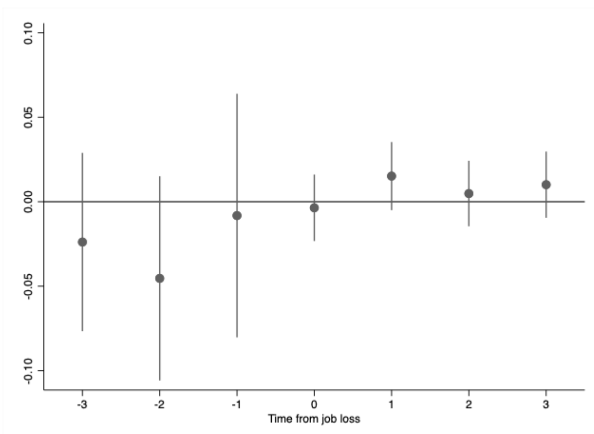
Total Expenditure



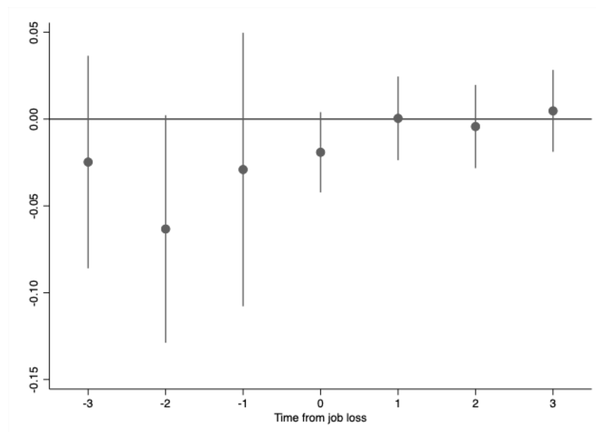
Discreationary Entertainment



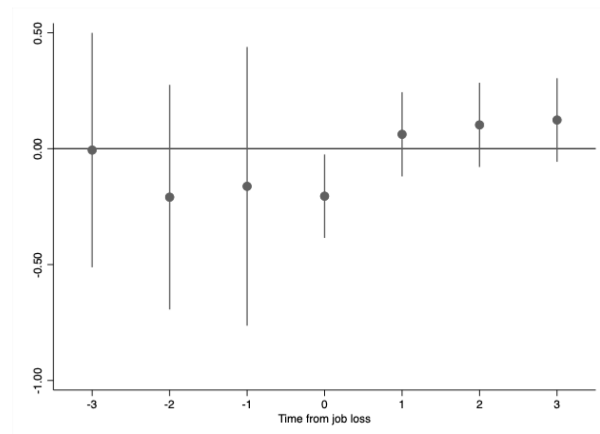
Necessary Expenditures



Overdraft Indicator



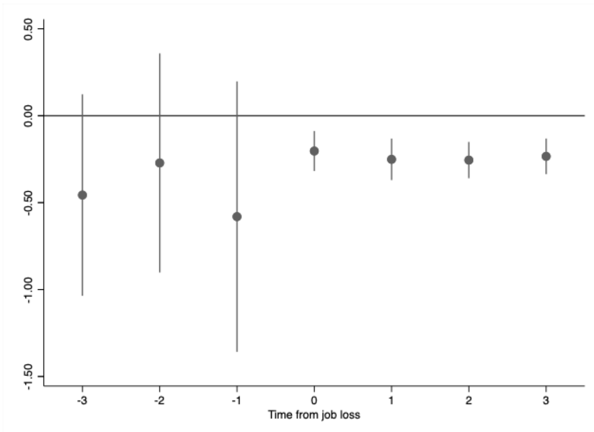
# of Overdrafts



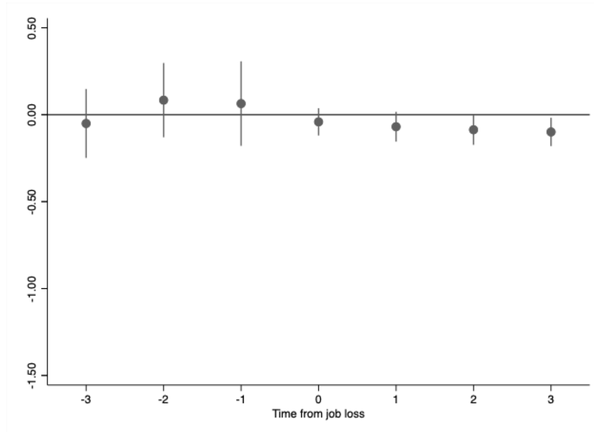
Late Fees

Figure A.1: Dynamic Responses to Unemployment

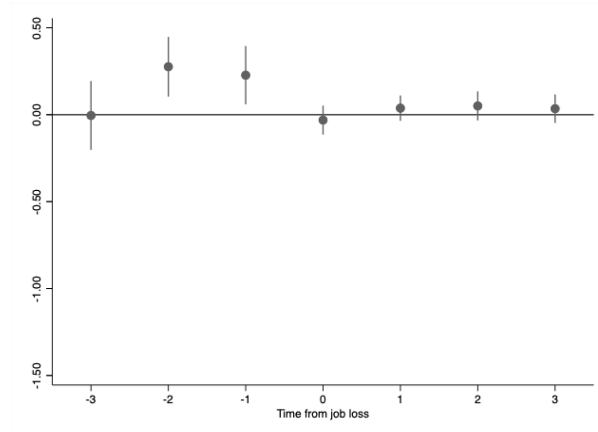
Notes: Controls include time-fixed and individual-fixed effects. The lines represent 95 percent confidence intervals using standard errors clustered at the individual level.



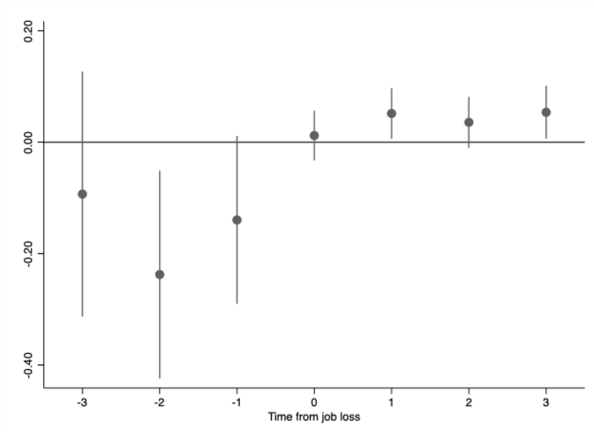
Total Expenditure - High Income Reduction



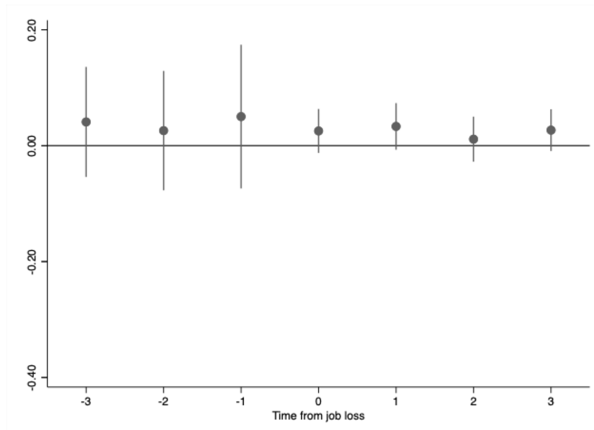
Total Expenditure - Medium Income Reduction



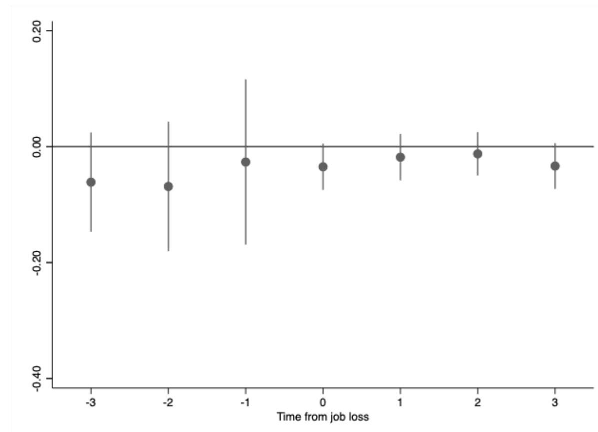
Total Expenditure - Low Income Reduction



Overdraft Indicator- High Income Reduction



Overdraft Indicator- Medium Income Reduction



Overdraft Indicator- Medium Income Reduction

Figure A.2: Expenditure and Borrowing Dynamics by Income Reduction Terciles

Notes: Controls include time-fixed and individual-fixed effects. The lines represent 95 percent confidence intervals using standard errors clustered at the individual level.