Credit Smoothing

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Abstract

Standard economic theory suggests that high-interest, unsecured, short-term borrowing, e.g., via credit cards, 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 due to unemployment. Rather, it appears as if individuals smooth their debt balances. We first use detailed longitudinal information on debit and credit account transactions, balances, and limits from a financial aggregator in Iceland to document that unemployment does not induce a large 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. This absence of a borrowing response occurs even when credit supply is ample and liquidity constraints do not bind (as captured by credit limits). This finding is difficult to reconcile with theories of consumption smoothing, which predict a strictly countercyclical demand for credit. On the contrary, credit rather demand appears to be procyclical, which may deepen business cycle fluctuations.

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1 Introduction

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

In this paper, we seek to investigate and quantify how credit card and overdraft borrowing responds to unemployment shocks. We first use data from a personal finance platform in Iceland (an "aggregator"), containing comprehensive transaction-level information (aggregated to the monthly level) on individual spending, income, account balances, and credit limits, to investigate how expenditures, liquid savings, and consumer debt change upon job loss. The longitudinal nature of our data allows us to include individual fixed effects in our estimations and thereby control for all selection on time-invariant (un)observables. 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 heavily in general and have sufficient liquidity. We argue that these findings are difficult to rationalize with borrowing being a tool for smoothing consumption in the event of adverse transitory income shocks.

Using the financial aggregator data, we find that individuals do not increase high-interest borrowing in response to unemployment. More specifically, we find neither an increase in the amount borrowed nor in the probability of holding an overdraft (even though the baseline probability of holding an overdraft is larger than 50 percent, i.e., the majority of individuals in our sample borrow regularly). The data covers 2011-2017, a period of economic expansion and over which unemployment was low and generally short-lasting. Unemployment in such circumstances is a quintessential kind of transitory income shock. These findings thus suggest that transitory income shocks are not the main reason for individuals to roll over consumer debt.

We report result both with and without controlling for individual fixed effects. We find that our regression estimates are much smaller when including individual fixed effects and this can be explained by the presence problems associated with selection and omitted variables bias. This can explain why we conclude that borrowing is not used to smooth consumption in contrast to some existing papers in the literature (Browning and Crossley, 2009; Gruber, 1997; Keys, 2010; Sullivan, 2008). To the best of our knowledge, fixed-effects analysis of the effect of unemployment on the use of consumer credit using longitudinal, high-frequency, and accurate individual-level data has not been undertaken before.[1]

We then turn to U.S. credit card data from the Federal Reserve Bank of New York and the Equifax Consumer Credit Panel (CCP), covering the universe of accounts nationwide from 2000 to the present, to confirm and replicate our findings. Constructing county-quarter measures of credit outcomes and employing a Bartik-style shift-share instrument as an exogenous source of variation in county-level employment, we produce estimates of the elasticity of equilibrium credit card account balances, limits, inquiries, and utilization.[2] We replicate our initial finding using the financial aggregator data

[1] Our findings are consistent with those in Ganong and Noel (2016), who use transaction-level bank account data of one U.S. bank and show that, in their sample, 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. As the authors observe only checking and credit card accounts from one bank, they do not explore this lack of a borrowing response further.

[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).

in that we find no clear and significant changes in borrowing in response to unemployment shocks. More specifically, we find that individuals in counties with adverse unemployment shocks neither appear to increase their overall outstanding revolving balances (even with respect to available limits) , nor do they appear to increase their inquiries for new credit relative to counties with less adverse unemployment shocks. Our findings are in contrast to Keys et al. (2017) who, using a similar but larger sample of unmatched (to the individual) credit card data and the same source of variation in employment with focus on the cross-sectional variation provided by the Bartik shock in the first quarter of 2008, conclude that consumer demand for credit card borrowing was countercyclical. In contrast, we find that credit card borrowing does not typically increase in the event of unemployment shocks, even for those with access to credit.

Because we do not observe individuals' income in the U.S. data, we complement our aggregate 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, i.e., the biggest quarterly borrowings do not appear to respond to unemployment. The largest increases in borrowing appear to occur regardless of the size of unemployment shock (at least as captured using county-level Bartik variation). We thus conclude that, while average credit card balances in the U.S. seem high—over $15,000—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 (for instance, during the financial crisis).

The 2015 American Household Credit Card Debt Study estimates the total credit card debt owed by an average U.S. household to be $15,762, which amounts to a total of $733 billion and the per-capita amount of borrowing that we observe in the Icelandic data is of similar magnitude. Such large high-interest debt holdings over longer periods of time are very hard to rationalize in standard economic models. As argued by, e.g., Laibson et al. (2003), these 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, even when they have locked up part of their wealth in illiquid assets. Furthermore, Laibson et al. (2007) show that a model with hyperbolic discounting and illiquid assets rationalizes the amount of borrowing we see in U.S. data. However, for the calibration to work, the hyperbolic-discounting parameter has to be half of what is commonly estimated in other domains (refer to, for instance, Kahneman et al., 1990) and agents have to be fully naive, i.e., they must believe that they will not have any hyperbolic discounting problems but are perfectly rational in all future periods. 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, they document that agents in the model bunch at zero borrowing when interest rates are high, such as the rates on credit cards or overdraft facilities, or they borrow but then up to their credit limits when interest rates are low, such as the rates observed on home equity lines of credit.

Most economic models would suggest that credit demand should be countercyclical while credit supply is procyclical. We show this theoretically holds in the influential model by Laibson et al. (2007), which explains the amount of credit card borrowing in the U.S. via hyperbolic discounting and illiquid savings. However, empirically we conclude that households allow consumption to adjust while smoothing their debt balances. Such credit smoothing will amplify business cycles compared to the countercyclical demand predicted by standard models. While an extensive literature has explored supply amplifiers during the Great Recession, our paper suggests we should also examine demand amplifiers of households during the initial expansionary and then contractionary period. Our findings thus relate to the analysis in Herkenhoff et al. (2013) showing that access to unsecured credit might deepen business cycles. Moreover, 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.

This study complements other work that has focused on the "debt overhang" of secured debt such as mortgages (Mian et al., 2013), creating credit-driven business cycles that operate through household demand, by showing that demand for unsecured credit is procyclical rather than countercyclical. Agarwal et al. (2015) use an identification approach based on discontinuities in credit card offer algorithms to show that credit supply was restricted during the post-crisis recovery period. In comparison, our paper focuses on unemployed individuals in particular and their demand for borrowing as well as the available supply of funds. Sullivan (2008) finds 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 11 percent. The author argues that low asset households are credit constrained, which we can directly address in this study because we observe credit limits. Hundtofte (2017) finds evidence of self-imposed financial constraints in field data by observing voluntary credit card closures, and finds that these increase in response to negative economic news such as house price declines or unemployment.

More generally, our findings underscore concerns regarding the value of finance to society. High cost, unsecured lines of credit such as credit cards do not appear to be used to smooth consumption as many economists would believe. The self-insurance benefits of these forms of credit are limited if individuals misunderstand the high costs in normal times and then do not tap these lines in bad times. As such, credit demand could amplify business-cycle consumption volatility rather than mitigating it through consumption smoothing. Furthermore, government policy or education may have a role to play in affecting the demand as well as the supply of credit.

2 Theoretical background

We consider the same model as in Laibson et al. (2007) 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). 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. Laibson et al. (2007) 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. 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.

The model in Laibson et al. (2007) is the following.[3] The agent lives for $t = \{1, ..., T\}$ periods. Each period the agent optimally decides how much to consume C_t. Additionally, he decides how much to save in the liquid and illiquid assets. X_t represents the

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

beginning of period t liquid asset holdings 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 λ times average income at age t. If the agent saves instead of borrows, he earns an interest R. $Z_t \geq 0$ 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 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. (2007) and the model is solved by numerical backward induction. Laibson et al. (2007) estimate the environmental and preference parameters of this model to match the patters of wealth accumulation and credit card borrowing over the life-cycle and we adopt the parameters of their best fit for the hyperbolic agents. 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.,

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

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 $I_{i,t}^{15}$ is an indicator variable if agent i's realization of income at age t is below the 15th percentile relative to all other agents at age t. 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. 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. (2007) 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 a combination of the ones displayed.

As we can see in Table 1, having an income realization in the lowest 15th percentile implies a 315% increase in the amount borrowed and a 39% increase in the likelihood to borrow in the hyperbolic discounting model. We find that present-biased agents in the model are consumption smoothing as standard agents and use borrowing as a tool to smooth transitory income shocks. For the standard agent, the borrowing response is less pronounced as the standard agent almost never borrows at the level of interest rates considered in this model.

[Insert Table 1 about here]

3 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 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 and the financial aggregator data, we believe that the replication of our findings in U.S. data bolsters the credibility of our findings and ameliorates concerns regarding external validity. In the following sections we will describe our data sources in detail.

3.1 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, to uncover the effect of unemployment on consumption and the use of consumer credit. 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 2011-2017. We aggregate our data to the monthly level and restrict the analysis to individuals for which we have full records. The app also collects some demographic information such as age, gender, marital status, and postal code. Furthermore, we infer employment status from salary and unemployment benefit payments we see in the data. This data has been proven useful in studying, e.g., 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 individuals' attention to their 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 (see, e.g., Gelman et al., 2014). 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 (and we have 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). That detailed information on consumption is a rarity in this literature that 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.

[Insert Table 2 and Figures 1 and 2 about here]

As can be seen, the vast majority of unemployment spells are relatively short. The mean length of an unemployment spell is 5 months while the average length is 6.5 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 and unemployment spells are generally short-lasting. In 2017 only 2.4 percent of individuals were unemployed. In comparison, the average unemployment in the Organization for Economic Co-operation and Development (OECD) countries in 2017 was 6 percent, and 8 percent in the European Union while unemployment in the U.S. was 4.5 percent.

Historically, the Icelandic labor market has been characterized by a very low and stable rate of unemployment with unemployment generally fluctuating below 3 percent. Even during the financial crisis unemployment peaked at only around 8%. High level of economic growth in Iceland in the years after the crisis[4] have helped reduce the level of unemployment down to its "normal" level. In that sense we are not too worried that the financial crisis shaking Iceland in 2008 can explain 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

[4]According to OECD figures the Icelandic economy grew by 7.2 percent in 2016, which was the second highest growth in the OECD in 2016.

during our entire sample period.[5] 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 more of a transitory income shocks in our setting than in countries where unemployment is 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. As can be seen in Table 2, individuals hold approximately \$2,000 in overdrafts[6] (in Iceland, individuals typically pay off their credit card in full and use overdrafts to roll-over debt, more details can be found in the next section). Nevertheless, they still enjoy substantial liquidity, i.e., they have substantial liquid savings or borrowing capacity before they hit their liquidity constraints, \$10,000 on average. 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 UK, 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 current 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

[5]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.).

[6]if we condition on individuals having an overdraft the average overdraft amount is approximately \$6,000

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 reality this is a rare occurrence as the overdrafts are profitable for the bank and expensive for the customer. If you are a wage earner in Iceland or a self-employed individual and lose your job, you may be entitled to unemployment benefits. Wage earners and self-employed individuals may be entitled to the 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 70 percent of their average income during a six-month reference period beginning two months before the loss of employment.[7] The income-linked benefits of self-employed individuals can be up to 70 percent of their average income during the preceding income year in which the individual became unemployed. The amount is capped though, i.e., there is a certain 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 percent of undiminished basic benefits for each child.[8]

After being in a job for six months individuals are entitled to a three months notice of unemployment and can be entitled to up to a six months notice. To include periods of time in which unemployment would have been unexpected at least for some of the

[7]This reference period can never be shorter than four months.

[8]Source:

sample, we begin four months prior to the beginning of unemployment for any event study illustrations.

Definitions of variables

Total discretionary spending - Spending is categorized 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 accessories, 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.

Necessary spending - Necessary spending is the sum of spending in grocery stores, gas stations and pharmacies.

Unnecessary spending - Unnecessary spending is the sum of spending 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.

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

Overdraft interest payments - Overdraft interest is interest paid on the amount of overdraft individuals have and the overdraft interest rate is varies with the Central Bank policy rate and is more or less the same as the interest on rolled over credit card

[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 in Iceland.

debt.. Individuals typically pay off their credit card in full and use overdrafts to roll-over debt. Overdraft interest payments should therefore be thought of as the costs of rolling over consumer debt. Figure 3 depicts the time series of overdraft interest and the short-term interest rate over our sample period.

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

Income - We observe the following regular income categories: child support, benefits, child benefits, interest income, invalidity 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.

3.2 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 percent random sample of all individuals who have a credit report with Equifax for the period between 1999 and 2017.[10] The data is described in detail in Lee and Van der Klaauw (2010). We use a 0.01 percent sample for purposes of the current analysis, which includes information on approximately 250,000 randomly selected individuals each quarter, and 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

[10] The quarterly sample starts in 1999 quarter 1 and currently ends in 2017 quarter 3.

(i.e., the very young and any others without reported debts are excluded). This panel data set allows us to track all 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.

[Insert Table 3 about here]

The main benefit of the U.S. data is that it provides an out-of-sample 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 borrowing responses because it links all borrowing/credit cards to an individual. Moreover, the sample is large and has sufficient statistical power to perform, e.g., quantile regressions. 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.

4 Methodology

4.1 Individual-level analysis using Icelandic data

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

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

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 coefficients thus measure by how much the individual outcome deviates when the individual is unemployed. The individual fixed effects control for all (un)observable individual characteristics. It is important to note that when we estimate the responses to unemployment in the months after losing a job, we exclude observations in the last three months prior to the onset of unemployment so that the interpretation of an unemployment estimate is relative to an outcome prior to receiving their notice.

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

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

where $liquidity_{c-4}$ 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 reason for using liquidity four months before the onset of unemployment is that workers typically have a three months notice period.

4.1.1 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:

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

where the $\beta_{1,k}$'s are the main coefficient of interest. Each $\beta_{1,k}$ represents the effect of unemployment on the outcome under investigation in month $t + k$. The cumulative response is obtained by summing the $\beta_{1,k}$s. 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 the month when they lost their job so as to capture any potential announcement effects.

4.2 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 predicted employment. Additional analysis pursues individual-level analysis, while the main source of variation is always county-level. As a source of 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 exogenous changes in employment due its industrial composition. Our exclusion restriction is that the pre-period industrial mix interacted with the national industry trend does not directly affect local credit card variables outside of its affect on employment.

More formally, our first stage of predicting employment outcomes using a Bartik instrument is as follows:

$$\Delta PredictedUnemployment_{c,t} = \sum_i (\frac{Employment_{i,t}}{Employment_{i,t-1}} - 1) EmploymentShare_{i,t-1,c} + \epsilon_{c,t}$$

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 $EmploymentShare_{itc}$ 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 percent changes in employed). We generate the above shocks using quarterly census data (QCEW). In turn, we estimate the following regressions of credit outcomes in a second stage:

$$y_{ct} = \beta \Delta PredictedUnemployment_{ct} + \gamma X_{ct} + \psi_t + \eta_c + \epsilon_{ct} \tag{4}$$

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:

$$y_{it} = \beta \Delta PredictedEmployment_{ct} + \gamma X_{it} + \psi_t + \eta_i + \epsilon_{it} \tag{5}$$

where $y_{i/ct}$ is the credit outcome of individual i or county c at date t and $\eta_{c/i}$ 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 $X_{i/ct}$ controls for time-varying characteristics such as individuals' (or counties' mean) credit scores. Time fixed effects capture any systemic changes/shocks across counties at each individual point in time.

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 employment 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 2-98, 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 employment 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 if individuals use credit cards for consumption smoothing. While the average borrowing response might be insensitive to unemployment shocks, are some borrowers responding strongly to changes in county-level unemployment rates by borrowing, i.e. are the upper quantiles of changes in credit card borrowing (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.

5 Results

5.1 Individual-level analysis using Icelandic data

Table 4 shows our estimates of the average effect of unemployment, i.e., the β_1 coefficients of Equation 3 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 and restrict our sample to individuals that are unemployed for at least 4 months.[11] The table shows the effect of unemployment on total discretionary spending as well as "necessary" spending (groceries, fuel, and pharmacies) and "unnecessary" spending (alcohol, restaurants, other activities, lottery tickets, gambling, gaming, bookstores, recreational sports, specialty stores, theatres, 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 consumption days. Furthermore, we consider overdraft interest, capturing the amount of overdrafts that individuals hold, late fees, overdraft balances, credit card balances, and credit limits. Table 5 shows results by income reduction terciles.

[Insert Tables 4 and 5 about here]

We find that individuals decrease their spending considerably in response to unemployment (by about 11 percent). Additionally, we see that credit card balances decrease, which is consistent with the decrease in spending. We also see that credit card limits are reduced. This is most likely voluntary since credit card issuers would not lower the limit only because of loss of job and this therefore and supports therefore that

[11] 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.

individuals make a conscious decision to cut down their spending. When we break the results up by income reduction terciles we do find, not surprisingly, that the reduction in spending is largest among individuals whose income is reduced the most. However, we do not find a significant increase in overdraft interest among any of the groups.

Furthermore, we find negative (but statistically insignificant) effect on overdraft borrowing, both when using overdraft interest and overdraft amounts as outcomes. Overdraft interest is a better measure for rolled-over consumer debt for two reasons: 1) it actually measures the cost of short term debt which is what we are seeking to explain while the overdraft amount ignores the interest rate and 2) we have twice the length of a time series for overdraft interest relative to overdraft balances. Interestingly, we see that late fees drop by about 15.5%. As discussed above, late fees are applied when individuals pay their bills after their due date. Given that most individuals are very liquid, this is a completely unnecessary expense that can easily be avoided. It thus seems as individuals become more sensitive to such fees during unemployment or that the opportunity cost of paying bills on time drops by enough to find a significant increase in the probability of paying on time. The latter hypothesis is consistent with the findings of our companion paper (Carlin et al., 2017) where we find that the introduction of a new mobile app that reduces the opportunity cost of paying attention to personal finances lowers the amount of late fees incurred.

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 responds to unemployment. It is interesting to compare these regression results to the results in Table 6 where we show results for individuals who have above and below median liquidity. The results show that the effect of unemployment on spending and borrowing is much more pronounced among individuals with low liquidity. However, even though sorting based on an endogenous variable (liquidity) and running separate regressions for each group is not inconsistent with liquidity playing an important role it does not provide a test for the importance of liquidity in determining how spending and borrowing respond to an adverse transitory income shock, which unemployment is in our setting. However, interacting unemployment with liquidity holdings prior to job loss, does provide such a test and suggests that focus that liquidity has received in the theoretical literature (see, e.g, Kaplan and Violante, 2014) may be unwarranted.

[Insert Table 6 about here]

Whether or not we control for individual fixed effects has a large effect on the size of the coefficients and on R^2, i.e., the inclusion of individual fixed effects suppresses the regression coefficients while greatly increasing R^2. This underscores the importance of the individual characteristics in the amount of borrowing they engage 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 its existing papers (Browning and Crossley, 2009; Gruber, 1997; Keys, 2010; Sullivan, 2008).

Table 7 shows the effect of unemployment on the probability of holding an overdraft at any point or on average over a month. We do not find any statistically significant effect on the probability of holding an overdraft at any point, i.e., entering interest-bearing territory. These coefficients suggest therefore that individuals who do not roll-over debt on average or not more likely to roll-over debt when unemployed. However, we are also interested in individuals that do use their overdraft facility during the month and whether they use it more than on average. The results show that individuals who borrow on average are more likely to borrow when they get unemployed, i.e., we do not find an effect of having an overdraft on average in a month of unemployment. It thus appears as if transitory income shock, of which unemployment is a quintessential kind, does not seem to be the main reason for individuals to roll-over consumer debt.

[Insert Table 7 about here]

In Figures 4 and 5 we show the impulse response of unemployment on the ratio of spending to average spending and average income. Clearly, individuals cut their consumption considerably at the onset of unemployment and then increase it gradually, suggesting that consumption commitments do not play a huge role in determining individual decisions to take on debt or not.

[Insert Figures 4 and 5 about here]

In Figure 6 we look at the impulse response of the overdraft amount 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 as can be seen in Figure 6.

[Insert Figure 6 about here]

The lack of a borrowing response could be explained if individuals were hit by permanent income shocks. However, when we compare the incomes of individuals who were unemployed before and after their unemployment spell, we find that their income (both regular and irregular) is not lower after unemployment. The same is true for spending. The comparison can be found in Table 8 and all numbers are inflation adjusted. Furthermore, Figure 7 plots the evolution of labor income prior to and after the onset of unemployment, independent of the duration of the unemployment spell.

The share of unemployed thus decreases as we move further away from the onset of unemployment and the average labor income increases. Around 15 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 further bolsters the credibility of our claim that unemployment is a transitory income shock in the setting of our paper. As discussed, throughout the sample period, the Icelandic economy was growing substantially and unemployment was declining throughout and unemployment shocks are more likely to be transitory in such settings.

[Insert Table 8 and Figure 7 about here]

To summarize, our findings based on the financial aggregator data support that individuals smooth their consumer credit usage during adverse transitory income shocks while they let consumption adjust. Furthermore, our findings also suggest that the increased time that unemployment gives reduces the opportunity cost of time which allows individuals to reduce unnecessary expenses like late fees.

5.2 Analysis using the U.S. credit panel

Figure 8 shows the mean changes in credit card balances by Bartik employment variation quantiles. We see the largest average changes in total revolving credit card balance (of around \$50 to \$100) are in the lowest deciles of unemployment shocks (i.e. positive employment shocks). Overall, better county employment outcomes appear to be 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, though even unconstrained individuals with an unfavorable employment shock do not appear to increase their borrowing. Thus, more constrained individuals decrease their borrowing when negative employment shocks occur. We do not observe a negative relationship between employment and borrowing in these figures either.

[Insert 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 and the first stage F-statistics do not indicate weak identification (F-statistic $>$ 30).

We estimate small and mostly statistically insignificant average borrowing responses to unemployment. The range in estimates varies from a $28 to $115 increase in credit card balances for a one standard deviation (approximately 1 percentage point) increase in the total unemployment rate. Looking at the mean borrowing responses to employment shocks tends to reject consumption smoothing as a consistent driver of unsecured borrowing. Analysis using alternative measures of employment (percent changes in employed, change in logs) provide similar statistically insignificant results.

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 utilisation ratios in Table 10, we find a positive coefficient estimate that (under some reasonable conditions and 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, which further supports our argument that supply-side constraints do not seem to be driving our results. We also find the size of a borrowing response is negatively related to baseline income in the area, i.e., lower income individuals may tend to respond with increases in borrowing to employment, which would also appear to argue against supply-side constraints. The lack of a robust 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 (i.e., applications for new credit) that is also inconsistent with an access-to-credit story.

[Insert 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. Most point estimates fall inside of this range and are statistically insignificant.

[Insert Table 11 about here]

Our results are thus different than the ones in Keys et al. (2017), who exploit a similar source of instrumental variation in employment. Based on efforts to replicate their findings as closely as possible, these differences can be reconciled by two main differences in analysis. First, our results are based on individual-level observations while theirs is based on card-level observations aggregated up to the county-level and weighted by the number of 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. Secondly, Keys et al. (2017) focus on the cross-sectional variation of the Bartik employment shock in 2008Q1, 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 2000-2016.[12]

Table 12 collapses the results of various additional regressions examining additional other credit outcomes. As can be seen there, it does not appear that the supply of credit responds 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 economically insignificant relationships, some which we would describe as precise zeros (for utilisation at the end of the same quarter as a shock, coefficients of less than 0.3 percent). For inquiries, 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.

The picture of little average change in borrowing is similar 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 instru-

[12] 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 percent 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 also run slightly different regression specifications, e.g., examining dollar changes in credit card balances.

mented by Bartik shocks. The coefficients are all small (less than $15), with standard errors of approximatily the same magnitude.

[Insert 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 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 (to a smoothing motivation) 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 percent).[13]

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 was primarily for consumption smoothing, we would expect to see the upper tail of the distribution meaningfully and negatively respond to employment variation. The fact

[13] Out of practical necessity, currently 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.

that we do not suggests that the largest changes in credit card borrowing are for non-consumption smoothing purposes. Instead of a negative relationship to employment, we find consistent positive estimates across the distribution. We thus conclude that the primary usage of credit cards is not in response to unemployment.

[Insert Table 13 about here]

To verify the robustness of our results, we conduct two robustness 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 tend to move closer to zero and 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 unemployment rate is less than 1 percent. The estimates also do not greatly vary depending on the choice of time period (the entire sample available, versus 2008-2014 only).

6 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 data sets, however, we conclude that individuals do not appear to use such credit to smooth consumption. In contrast, it appears as if individuals smooth their debt balances rather than their consumption through transitory income shocks.

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.

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 what would be observed otherwise.

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Figures and tables

Table 1: The effect of low income on borrowing and consumption in the model of Laibson et al. (2007)

	(1) Log of total borrowing	(2) Indicator for borrowing	(3) Log of total spending
Hyperbolic-discounting agent:			
income below 15th percentile	3.1533***	0.3936***	-0.6220***
	(0.0049)	(0.0006)	(0.0016)
Standard agent:			
income below 15th percentile	0.1070***	0.0134***	-0.2119***
	(0.0012)	(0.0001)	(0.0009)
#obs	71,000	71,000	71,000
Age fixed effects	✓	✓	✓

This is the estimated effect of an income realization below the 15th percentile in the simulated data of the model in Laibson et al. (2007) featuring an illiquid asset, credit card borrowing, liquidity constraints, and stochastic labor income. Standard errors are within parentheses. Each entry is a separate regression.

Table 2: Summary statistics

	employed		unemployed	
	mean	standard deviation	mean	standard deviation
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:				
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 in average consumption days:				
cash in consumption days	65.9	223.4	61.1	218.9
liquidity in consumption days	162.5	352.5	152.2	247.0
Overdrafts and credit card balances:				
overdraft amount	222,447	640,011	197,862	366,313
overdraft indicator	0.47	0.50	0.46	0.50
credit card balance	119,171	164,836	112,316	162,241
credit card limit	370,765	532,030	331,515	457,043
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 information on individuals that are unemployed at some point during the sample period. Notes: All numbers are in Icelandic krona. 1 USD $\approx$ 100 ISK.

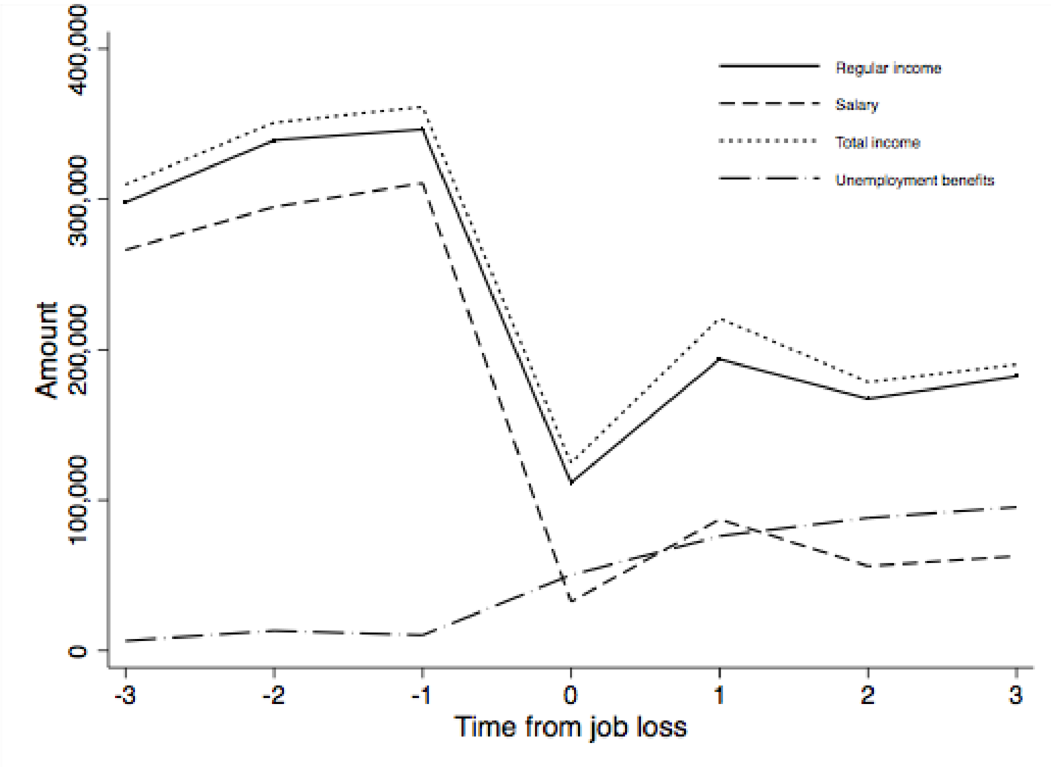

Figure 1: Income and unemployment benefits in the months around job loss

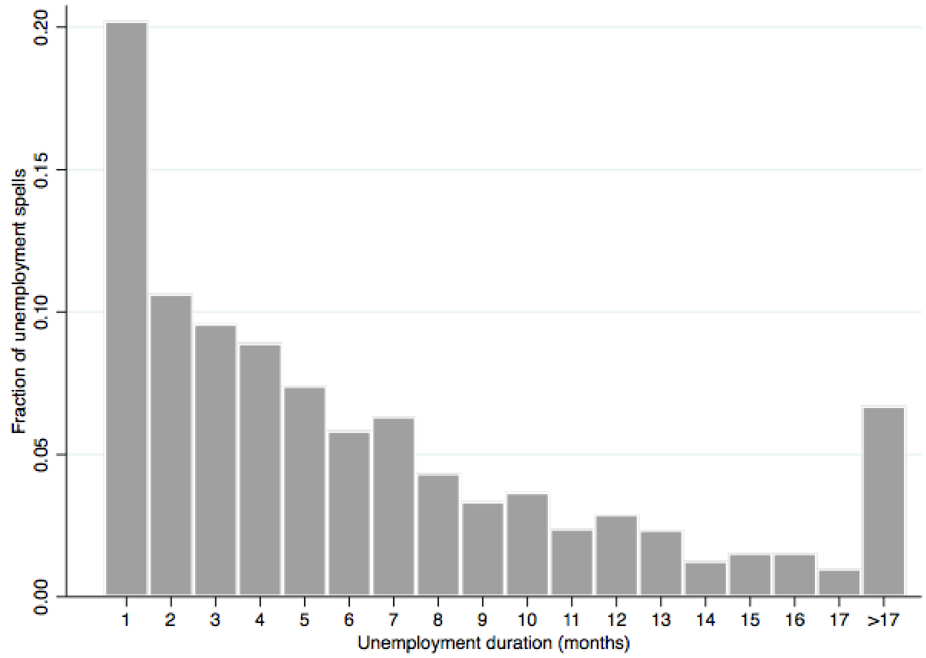

Figure 2: Duration of unemployment

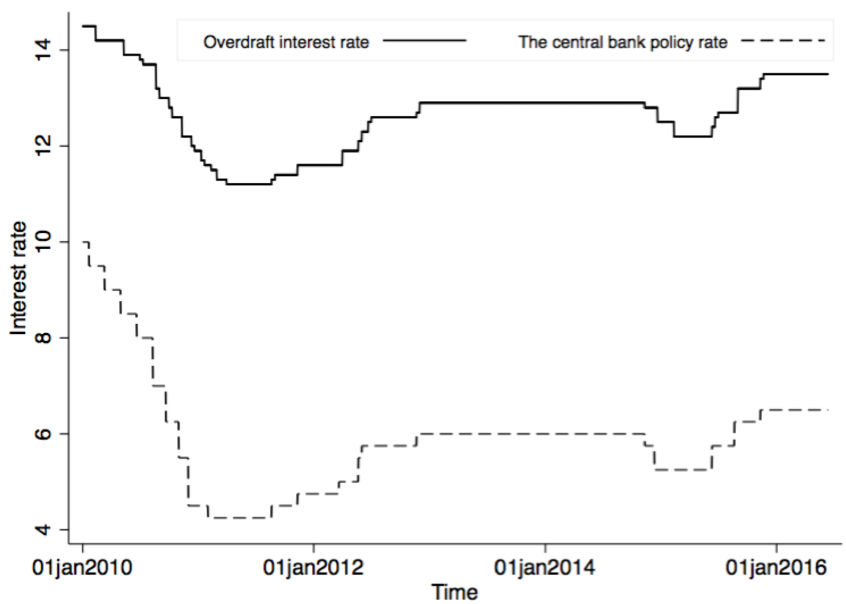

Figure 3: Trends of the Icelandic central bank policy interest rate and overdraft interest rate throughout the sample period

Notes: Data source, Central Bank of Iceland https://www.cb.is/.

Table 3: Summary statistics for U.S. credit panel

Variable	Count	Mean	Standard Deviation	Min	Max
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 utilisation ratio	10.3m	0.0026	0.1638	-0.69	0.62
number of inquires within 3 months	15.4m	0.4106	0.8315	0	4
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
change total debt balance ($)	12.7m	693.2	24,352.4	-119,967	151,625
change in non-current balances ($)	11.7m	53.4	2,752.9	-15,002	18,129
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
credit score	14.8m	690.9	105.2	416	828
age	12.7m	50.8	18.1	20	93
per-capita income	16m	30,543.3	8,532.3	16,659	60,755
year	16.4m	2,008.2	4.9	2,000	2,016
utilisation ratio (credit card)	10.4m	0.4548	13.2874	0	20,083
utilisation ratio (all revolving)	11.6m	0.4818	59.6042	0	83,550
total debt balance ($)	13.1m	77,777.2	154,683.3	0	9,999,999

Notes: All statistics generated using individual-quarter observations in the FRBNY/Equifax Consumer Credit Panel (CCP), other than county-level Bartik Employment Shock (Quarterly Census data) and per-capita income (BEA).

Table 4: The effect of unemployment on consumption, cash holdings, and consumer credit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Log of:	total spending	necessary spending	unnecessary spending	cash	liquidity	overdraft interest	late fees	overdraft amount	current account limit	credit card balance	credit card limit
					With individual fixed effects:						
unemp	-0.118***	-0.157***	-0.139***	0.012	0.015	-0.081	-0.162**	-0.013	-0.044	-0.101*	-0.089*
	(0.021)	(0.034)	(0.035)	(0.035)	(0.021)	(0.061)	(0.077)	(0.101)	(0.093)	(0.061)	(0.054)
R-sqr	0.032	0.013	0.023	0.018	0.034	0.001	0.011	0.012	0.019	0.012	0.020
#obs	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942
					Including liquidity interactions						
unemp	-0.156***	-0.233***	-0.091	-0.445***	-0.578***	-0.328**	-0.160	-0.536**	-0.705***	-0.208	-0.350***
	(0.048)	(0.076)	(0.079)	(0.078)	(0.048)	(0.137)	(0.172)	(0.226)	(0.208)	(0.137)	(0.121)
unemp*	0.012	0.031*	0.002	0.093***	0.114***	0.040	-0.012	0.119**	0.177***	0.017	0.035
liquidity$_{t-4}$	(0.010)	(0.016)	(0.017)	(0.016)	(0.010)	(0.029)	(0.036)	(0.048)	(0.044)	(0.029)	(0.026)
liquidity$_{t-4}$	-0.005	-0.020***	-0.019***	0.015**	0.029***	0.023**	0.017	-0.001	-0.040**	0.010	0.034***
	(0.004)	(0.006)	(0.007)	(0.007)	(0.004)	(0.012)	(0.014)	(0.019)	(0.017)	(0.012)	(0.010)
R-sqr	0.032	0.013	0.023	0.018	0.035	0.001	0.011	0.013	0.019	0.012	0.020
#obs	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942
					Without individual fixed effects:						
unemp	-0.287***	-0.351***	-0.286***	-0.480***	-0.310***	0.064	0.589***	0.515***	-0.516***	-1.397***	-1.441***
	(0.032)	(0.050)	(0.045)	(0.056)	(0.037)	(0.103)	(0.091)	(0.155)	(0.157)	(0.138)	(0.141)
R-sqr	0.021	0.008	0.016	0.005	0.011	0.000	0.006	0.004	0.005	0.002	0.003
#obs	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942	308,942
#groups	10,855	10,855	10,855	10,855	10,855	10,855	10,855	10,855	10,855	10,855	10,855
month-by-year fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

[a] This is 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 current account limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals.

Table 5: The effect of unemployment on consumption, cash holdings, and consumer credit by income reduction terciles

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Log of:	total spending	necessary spending	unnecessary spending	cash	liquidity	overdraft interest	late fees	overdraft amount	current account limit	credit card balance	credit card limit
					With individual fixed effects:						
1st income reduction tercile:											
Unemp.	-0.027	-0.099*	-0.072	0.187***	-0.054	-0.366***	0.001	-0.564***	-0.470***	0.131	0.128
	(0.038)	(0.056)	(0.060)	(0.057)	(0.042)	(0.094)	(0.144)	(0.163)	(0.153)	(0.111)	(0.106)
R-sqr	0.029	0.016	0.022	0.009	0.035	0.006	0.012	0.029	0.035	0.018	0.025
#obs	7,735	7,735	7,735	7,735	7,735	7,735	7,735	7,735	7,735	7,735	7,735
#groups	346	346	346	346	346	346	346	346	346	346	346
2nd income reduction tercile:											
Unemp.	-0.023	-0.006	-0.027	-0.099*	0.004	0.061	-0.217	0.451***	0.015	-0.138	-0.352***
	(0.032)	(0.053)	(0.057)	(0.057)	(0.038)	(0.097)	(0.134)	(0.171)	(0.160)	(0.108)	(0.095)
R-sqr	0.026	0.009	0.023	0.022	0.047	0.007	0.015	0.036	0.054	0.008	0.010
#obs	8,107	8,107	8,107	8,107	8,107	8,107	8,107	8,107	8,107	8,107	8,107
#groups	346	346	346	346	346	346	346	346	346	346	346
3rd income reduction tercile:											
Unemp.	-0.350***	-0.430***	-0.362***	-0.103	0.068	0.143	-0.291*	0.222	0.629***	-0.380***	-0.029
	(0.045)	(0.079)	(0.074)	(0.069)	(0.044)	(0.124)	(0.154)	(0.195)	(0.191)	(0.129)	(0.117)
R-sqr	0.048	0.024	0.039	0.038	0.042	0.002	0.016	0.015	0.024	0.007	0.015
#obs	7,791	7,791	7,791	7,791	7,791	7,791	7,791	7,791	7,791	7,791	7,791
#groups	346	346	346	346	346	346	346	346	346	346	346
month-by-year fixed effect	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

[a] This is 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 current account limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals.

Table 6: The effect of unemployment on consumption, cash holdings, and consumer credit by initial liquidity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Log of:	total spending	necessary spending	unnecessary spending	cash	liquidity	overdraft interest	late fees	overdraft amount	current account limit	credit card balance	credit card limit
					With individual fixed effects:						
below median:											
Unemp.	-0.451***	-0.441***	-0.575***	-0.218**	-0.300***	0.022	-0.517*	-0.222	0.069	-0.506***	-0.645***
	(0.070)	(0.112)	(0.114)	(0.102)	(0.081)	(0.168)	(0.271)	(0.289)	(0.275)	(0.187)	(0.179)
R-sqr	0.044	0.027	0.038	0.027	0.054	0.028	0.035	0.041	0.052	0.022	0.033
#obs	1,865	1,865	1,865	1,865	1,865	1,865	1,865	1,865	1,865	1,865	1,865
#groups	238	238	238	238	238	238	238	238	238	238	238
above median:											
Unemp.	0.103	0.182	0.283	0.256**	0.125*	-0.643***	-0.380	-0.775**	-0.118	0.125	0.012
	(0.119)	(0.163)	(0.174)	(0.113)	(0.064)	(0.185)	(0.293)	(0.328)	(0.303)	(0.201)	(0.176)
R-sqr	0.031	0.026	0.039	0.037	0.037	0.035	0.030	0.082	0.112	0.028	0.038
#obs	1,825	1,825	1,825	1,825	1,825	1,825	1,825	1,825	1,825	1,825	1,825
#groups	228	228	228	228	228	228	228	228	228	228	228
year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
month FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

[a] This is 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 current account limits and credit limits minus credit card balances. Cash and liquidity are measured in days of average spending by individuals.

Table 7: The effect of unemployment on the probability of holding an overdraft

	(1)	(2)	(3)
Panel A:			
unemployment	-0.016	-0.123	0.109
	(0.093)	(0.196)	(0.139)
unemployment* liquidity$_{t-1}$		0.025	
		(0.041)	
unemployment* liquidity$_{t-4}$			-0.041
			(0.034)
R-sqr			
#obs	189,609	189,609	189,609
#groups	6,612	6,612	6,612
Panel B:			
unemployment	-0.000	-0.311	0.153
	(0.099)	(0.222)	(0.152)
unemployment* liquidity$_{t-1}$		0.072	
		(0.046)	
unemployment* liquidity$_{t-4}$			-0.048
			(0.036)
R-sqr			
#obs	153,832	153,832	153,832
#groups	5,388	5,388	5,388
month-by-year fixed effect	✓	✓	✓
individual fixed effect	✓	✓	✓

[a] The outcome variable in Panel A is an indicator for having, at least once during the month, a negative current account balance. The outcome variable in Panel B is an indicator for having, on average during a month, a negative current account balance.
[b] We consider individuals unemployed for at least 4 months.
[c] Standard errors are clustered at the individual level and are within parentheses. Each entry is a separate regression. The first panel uses a dummy for having an overdraft at some point during the month as an outcome and the second penal uses a dummy for having, on average, a negative current account balance during the month.

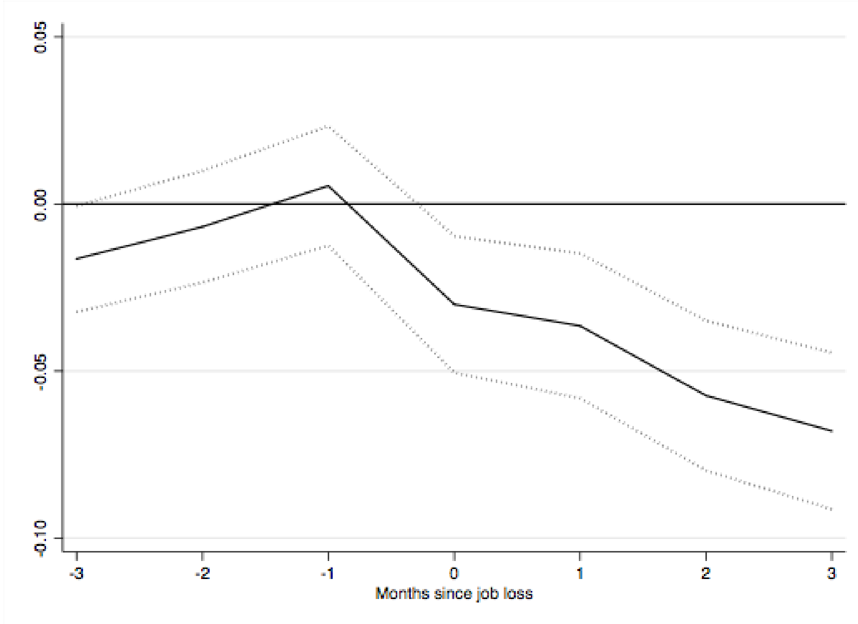

Figure 4: The impulse response of the expenditure to averages expenditure ratio to unemployment
Notes: Regression coefficients for each month before and after unemployment with controls for individual, month, and year fixed effects, estimated with respect to four months prior to job loss (period -4).

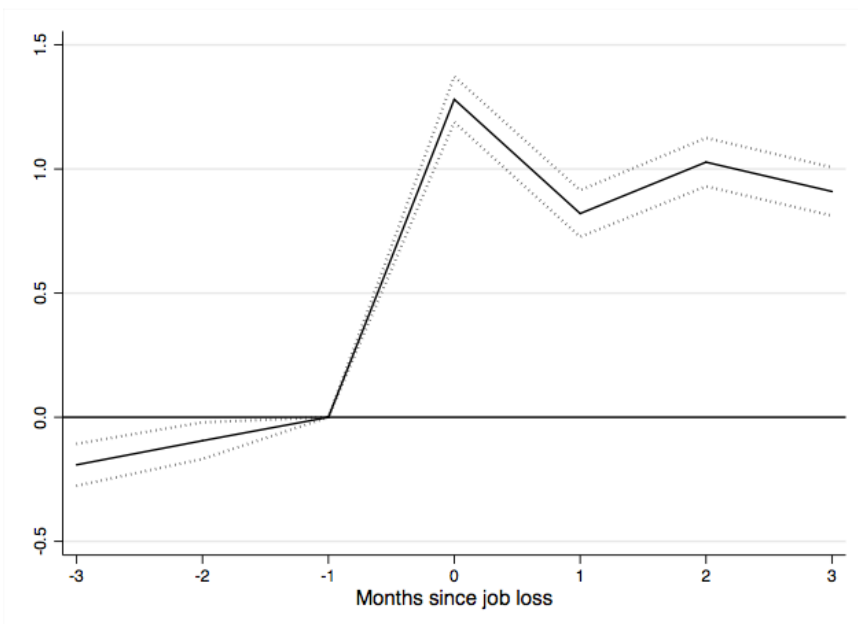

Figure 5: The impulse response of the expenditure to income ratio to unemployment
Notes: Regression coefficients for each month before and after unemployment with controls for individual, month, and year fixed effects, estimated with respect to four months prior to job loss (period -4).

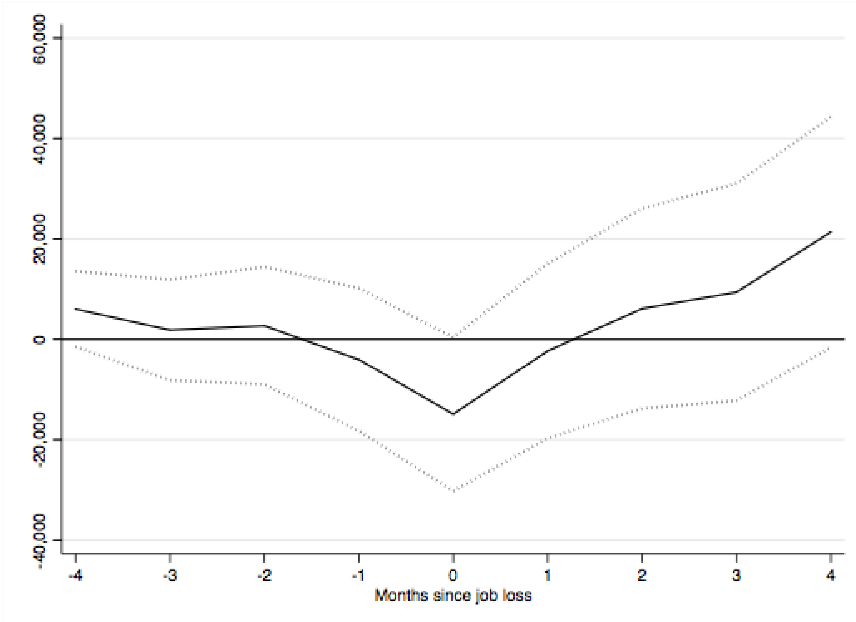

Figure 6: The impulse response of overdrafts to unemployment

Notes: Regression coefficients for each month before and after unemployment with controls for individual, month, and year fixed effects. Estimated values are with respect to four months prior to job loss (period -4).

Table 8: Comparison of income and spending before and after unemployment

	Before unemployment	After unemployment
total income	315,334	355,145
regular income	302,054	340,354
irregular income	13,279	14,791
total spending	139,626	178,173
necessary spending	61,710	76,972
unnecessery spending	39,094	50,302

[a] All numbers are in Icelandic kronas and inflation adjusted.

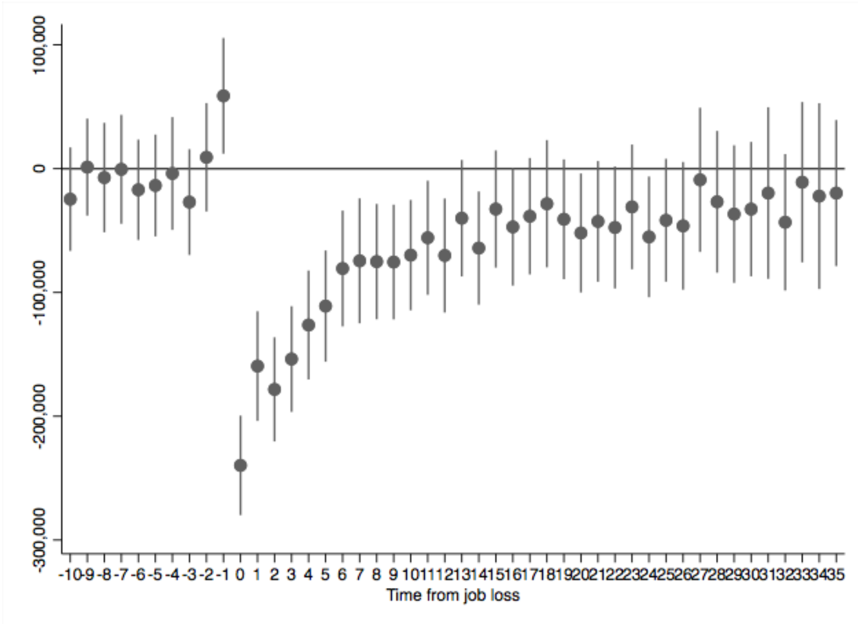

Figure 7: Labor income prior to and after onset of unemployment

Notes: We control for individual and month-by-year effects.

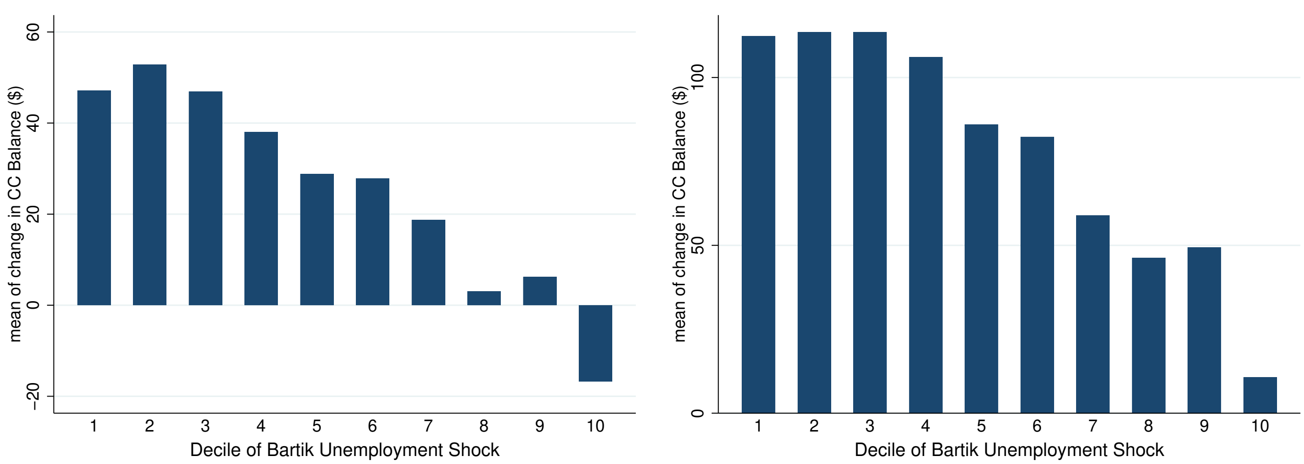

Figure 8: Mean changes in credit card balances by employment shock quantile in U.S. Data.

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 employment shock experienced. 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 riskscore	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 riskscore			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 FEs	✓	✓	✓	✓
Time period	2000-2016	2008-2014	2000-2016	2008-2014
#obs	178,114	84,485	178,057	84,439
R^2	0.025	0.058	0.002	0.064

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses 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.

Table 10: Change in county-level credit card balances by ex-ante borrowing constraints

	Δ credit card balance		Δ credit card bala (if slack in utilizatio
Δ unemployment	18.9	116.8***	10.9
	(38.0)	(35.2)	
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 riskscore	1.17***	2.08***	2.89***
	(0.13)	(0.13)	(0.20)
time and county FEs	✓	✓	✓
#obs	176,141	174,939	178,114

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses   Notes: This Table regresses mean changes i card borrowing at the county-level on the (lagged) county unemployment rate, as instrumented by the Bartik employmen interacted with the average lagged utilization ratio (balances divided by credit limit) and Income (BEA, 2000) for that with controlling time and county fixed effects as well as age and lagged risk scores. Standard errors are clustered at the level.

Table 11: Longer-horizon changes in credit card borrowing

	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 FEs	✓	✓	✓	✓	✓	✓	✓
#*obs*	178,000	177,956	177,919	174,843	171,774	168,713	165,648

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses

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.

Table 12: Other credit outcomes on unemployment shocks

	Estimated coefficients on Δ predicted unemployment rat			
time period:	2000 to 2016	2008 to 2014	2000 to 2016	2008 to 2014
Δ unemployment:	same quarter		previous quarter	
Outcome				
Δ credit limits	3.54	15.1	-5.01	-24.5
	(20.5)	(22.7)	(89.0)	(98.0)
Δ utilisation ratio on credit cards	0.0034	-0.0057	-0.00076	-0.010
	(0.0063)	(0.0075)	(0.0062)	(0.0072)
Δ total revolving acct. balances	8.7	-6.1	9.7	15.0
	(13)	(15)	(13)	(16)
credit inquiries	-0.011	-0.020	0.0051	-0.0062
	(0.0093)	(0.012)	(0.0093)	(0.012)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses Notes: This table summarizes the result of separate regressions of various credit outcomes on the (lagged) county unemployment rate instrumented by the Bartil 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.

Table 13: Quantile regressions of changes in credit card balances on Bartik unemployment shock

	2nd	5th	10th	25th	50th	75th	90th	95th	
				Δ credit card balance					
Bartik	-38.5	-26.3	-17.1	-720.7***	-3.52 ***	-16.0*	-888.8	-2,522.2	-
Unemployment	(7,376.9)	(2,382.3)	(978.4)	(196.8)	(0.31)	(6.96)	(922.7)	(2,767.7)	(
time, county FEs	✓	✓	✓	✓	✓	✓	✓	✓	
#obs					13,866,844				

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses
Notes: this table presents estimates of quantile regressions using individual-quarter observations, with Bartik employme (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

	(1)	(2)
	Δ credit card balance	
Δ unemployment	31.9***	
	(8.98)	
age	-3.95***	
	(0.30)	
lagged riskscore	1.73***	
	(0.056)	
lagged Δ unemployment		28.3**
		(8.61)
lagged age		-3.53***
		(0.31)
2×lagged riskscore		1.53***
		(0.058)
time and county FEs	✓	✓
$\#obs$	178,114	178,057
R^2	0.115	0.119

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses 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.

Table A.2: Changes in credit card borrowing on unemployment shocks - Individual-level

	Δ credit card balance			
	2000 to 2016	2008 to 2014	2000 to 2016	2008 to 2014
Δ unemployment	31.9**	20.3		
	(11.0)	(12.9)		
age	-25.0***	-15.7***		
	(1.43)	(2.34)		
lagged riskscore	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 riskscore			4.27***	5.13***
			(0.037)	(0.057)
individual and time fes	✓	✓	✓	✓
#*obs*	10,629,462	4,834,367	10,598,148	4,811,285

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ Standard errors are in parentheses 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.