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IMPULSIVE CONSUMPTION AND FINANCIAL WELLBEING:
EVIDENCE FROM AN INCREASE IN THE AVAILABILITY OF ALCOHOL

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Impulsive Consumption and Financial Wellbeing: Evidence from an Increase in the Availability of Alcohol

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ABSTRACT

Increased availability of temptation goods might harm individuals if they have time-inconsistent preferences and consume more in the present than planned before. We study this idea by examining the credit behavior of low-income households around the expansion of the opening hours of retail liquor stores during a nationwide experiment in Sweden. Consistent with store closures serving as commitment devices, expanded operating hours led to higher alcohol consumption (Nordström and Skog 2003) and greater consumer credit uptake and default. Thus, our results show that limiting the availability of temptation goods can improve the financial wellbeing of individuals with inconsistent time preferences.

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1 Introduction

Individuals with time-inconsistent preferences (“present bias”) consume in the present more than they planned in the past (e.g., Shefrin and Thaler 1981, Laibson 1997, Hoch and Loewenstein 1991, Loewenstein and Prelec 1992, O’Donoghue and Rabin 1999a, b, 2000). This behavior leads to lower saving and higher borrowing in the future, because overconsumption today comes at the expense of future consumption (Laibson 1997). This self-control problem is especially acute for poor people, since unplanned consumption of temptation goods may constitute a large fraction of their disposable income (Banerjee and Mullainathan 2010, Bernheim, Ray, and Yeltekin 2015). Banerjee and Mullainathan (2010) differentiate between normal goods and temptation goods (e.g., alcohol, sugary and fatty foods). They posit that temptation goods trigger consumption driven by present bias. Our paper is the first study to use an empirical, real-world setting to examine the effect of the availability of a temptation good (alcohol) on the financial wellbeing of low-income consumers.

Researchers have proposed that commitment mechanisms might help individuals stick to their planned consumption path (e.g., Laibson 1997, Thaler and Benartzi 2004). However, prior studies have found conflicting evidence about the effectiveness of restricting consumer access to temptation goods. Currie, DellaVigna, Moretti, and Pathania (2010) report that the obesity rates of children and pregnant women increase when fast food restaurants open nearby. Norström and Skog (2003, 2005) document that alcohol consumption increased by 3.7% in Sweden following the opening of government-controlled liquor stores on Saturdays. In contrast, Bernheim, Meer, and Novarro (2016) find that changes in the opening hours of off-premise liquor stores in the United States on Sundays did not significantly affect alcohol consumption. More generally, the effectiveness of self-enforced pre-commitment devices is questionable. DellaVigna and Malmendier (2006) examine gym memberships, which are precommitment devices for exercising. They show that individuals do not use their gym memberships sufficiently to justify the cost (relative to the cost of single entry passes). Given the conflicting results, it is unclear whether restricting the availability of temptation goods improves the wellbeing of individuals in general, and their financial wellbeing specifically. This question has, of course, both academic and policy implications.

In this paper, we examine the effects of an increase in the availability of alcohol on the financial wellbeing of consumers. We rely on the insights from Banerjee and

Mullainathan (2010), who argue that the consumption of temptation goods exacerbates poverty. They argue that there are two types of goods, normal goods (e.g., broccoli, Swedish meatballs) and temptation goods. Temptation goods are goods where we would spend money on in the moment (e.g., cigarettes, alcohol), but we would like our future selves to not spend money on or at least spend less money on. In their model, temptation goods have an adverse effect on the poor because they spend a disproportionately larger fraction of their income on these goods than wealthier individuals. Furthermore, because of the budget constraint, the consumption of temptation goods must come at the expense of future consumption of normal goods, leading to lower saving and greater borrowing.

Our study examines the outcomes of a nationwide experiment that took place in Sweden in 2000. Retail liquor stores began to open on Saturdays in only some Swedish counties. As with other studies (e.g., Bernheim, Meer, and Novarro 2016, and Hinnosaar 2016), our identifying assumption is that rational individuals can plan their shopping in advance and shop when the store is open; for these customers, operating hours do not affect their consumption patterns much. In contrast, present-biased individuals underestimate their future demand for alcohol and thus do not optimally plan. If the store is open, they purchase alcohol; if it is closed, they cannot. For these individuals, the exogenous change in the availability of alcohol is effectively a relaxation of a commitment device that previously prevented them from purchasing alcohol on Saturdays: They can follow their impulsive consumption behavior on Saturdays.

The Swedish experiment enables us to trace out the demand for a temptation good, alcohol, and test whether the increased consumption affected consumers' financial wellbeing. The results show that, indeed, individuals in the counties with greater access to alcohol accumulated higher debt amounts and had ex post higher default rates. Thus, our findings reveal a causal relationship between impulsive consumption and diminished financial wellbeing, and we show that impulsive consumption is an important factor for the financial wellbeing of individuals who live at the margins of the formal credit market.

In Sweden, the sale of alcohol for off-site consumption is permitted only in government-owned stores. Prior to the experiment, liquor stores were open only on weekdays and were closed on weekends. Following consumer demand, the government initiated an experiment in February 2000 to evaluate the impact of opening the stores on Saturdays. Sweden has a total of 21 counties. The experiment took place in six counties, and stores

remained closed on Saturdays in the other counties. The experiment was set up and evaluated by Swedish social scientists (Nordström and Skog 2003). They found that alcohol consumption in the treated counties increased by 3.7–4% on average (Nordström and Skog 2005, Grönqvist and Niknami 2014). This translates into an average monthly increase per capita of approximately three bottles of wine or 15 beers. Because evaluations of the trial initially did not reveal negative health or crime consequences, the Saturday opening was extended throughout Sweden in July 2001.

Limiting the hours of operation of liquor stores could restrict the consumption of alcohol in two primary ways. First, limited opening hours makes alcohol purchases more troublesome. Second, some consumers might have present-biased preferences. Such a consumer might decide not to buy alcohol for tomorrow because she plans to save more and consume less. However, when tomorrow comes, she alters her plan, preferring consumption today again (O'Donoghue and Rabin 1999). For these consumers, limited opening days function as a commitment device, helping them follow through on their planned behavior to consume less. Conversely, expanded operating hours might allow consumers with self-control problems to give in to their desire for immediate gratification (i.e., spend more money on alcohol), without considering the longer run consequences. Such consumers could end up with too little money to pay the bills in the next period, resorting to short-term credit to make ends meet.

Identifying a causal relationship between impulsive consumption and financial distress is challenging because a positive correlation between the two might be driven by other confounding factors. For example, financial distress might induce consumers to drink more to relieve stress (reverse causality), or consumers who have trouble sticking to their plan might also typically borrow too much (selection bias). Our initial empirical strategy is based on triple differences. If we simply compare consumers in the counties with increased access to alcohol to consumers in another set of counties without increased access (a diff-in-diff analysis), our results could be confounded by unobserved differences between people who choose to live in the various counties. Therefore, we employ a triple-difference specification. We exploit the fact that people under age 20 are not permitted to buy alcohol anywhere in Sweden, but people ages 18–19 can take out credit. To ensure that our treatment group is comparable, we confine our treatment group to young adults between the ages of 20 and 25.

Our main empirical strategy is to compare financial outcomes for individuals who live in the treatment and control counties who were eligible to buy alcohol (20–25 year olds) with those who were not eligible (18–19 year olds). We track how their financial outcomes change after February 2000, when the experiment began. We start by running our triple differences on a county level and then conduct panel regressions to control for individual fixed effects.

In line with our theoretical predictions, we find that the increase in the availability of alcohol led to a rise in the demand for credit and in defaults. An individual who lived in a treatment county and could legally buy alcohol was approximately 25% more likely to take out a pawn loan and 10% more likely to get a credit card, relative to an individual who lived in a control county, and relative to an underage individual. We also document a 12% increase in pawn loan size and a 15% increase in credit card balances. Together, these translate into an increase of 230 SEK (approximately 26 USD) per pawn loan and 695 SEK (approximately 78 USD) per credit card.

Furthermore, we find no evidence that this additional pawn and mainstream credit helped people avoid default in and outside the credit market. Instead, an individual's risk of receiving a delinquency flag on his or her credit record significantly increased by 1.3 percentage points compared to a mean of 5% in the 18 months prior to the experiment. This translates into a 26% increase in the probability of default.

We also examine the differential effect of the increased access to alcohol on males and females. Several studies have found that, in general, males drink about twice as much as females do, a trend also true in Sweden (e.g., World Health Organization reports). Consistent with the literature, we find that the extended opening hours had a materially stronger effect on the financial wellbeing of males than females.

In addition, we examine a prediction about the timing of borrowing. Specifically, impulsive purchases on the weekend are expected to result in an increase in the number of pawn borrowing in the beginning of the following week (pawn shops are closed on weekends) as opposed to later in the week. We test this conjecture exploiting the daily frequency of our pawn borrowing dataset and find that 27% to 32% of the increase in borrowing takes place in Mondays.

Finally, we conduct several robustness tests. First, we test whether convenience shopping, rather than present bias, is driving our results. To do so, we compare populations

that have more time at hand (the unemployed, retirees) than their more time-constrained counterparts (e.g., the employed and people near-retiring age). We find no differential effect between the groups, supporting the idea that the effects are not driven by time constraints, i.e., convenience shopping. Second, we perform a placebo test by moving our empirical strategy one and a half years back in time when there was no difference in opening hours among the monopoly government-owned alcohol stores in Sweden. Running our main regressions (equation 2) using this time period produces no significant differences in credit uptake or default between our treatment and control counties. Lastly, we confirm that our results are not driven by spillover shopping in neighboring countries, the age cut-offs we use, or the level of error clustering.

Overall, our findings indicate that when consumers have increased access to temptation goods, their financial wellbeing suffers. The fact that our study is based on a change in the availability of (rather than the introduction of) temptation goods points to the mechanism at work: by greater access, you reduce the commitment device of closed stores which increases consumption in the present. When someone is on a limited budget, greater consumption leads to borrowing and higher default rates.

Unlike the extant literature, our study provides empirical, real-world evidence that time-inconsistent preferences can affect consumers' financial wellbeing. In general, it is difficult to assess whether time-inconsistent preferences have *any* impact on consumers' financials in the real world. The difficulty with studying these effects lies in the fact that the supply of immediate consumption opportunities is an endogenous response to the demand for these goods. Therefore, there is no easy way to know whether any part of the observed correlation between immediate consumption opportunities and economic and financial health is causal. Many researchers test the economic effects of time-inconsistent preferences in the laboratory (for overviews, see Cohen, Ericson, Laibson, and White 2016, and Frederick Lowenstein, and O'Donoghue 2002). In a laboratory setting, the researcher can control the supply of consumption opportunities and thereby trace out potential demand effects. Furthermore, the laboratory allows the researcher to elicit time preferences, e.g., through computer games and price lists. Our study captures the financial effects of time-inconsistent preferences in a real-world setting.

Our results also shed light on the relation between present bias and financial behavior, where the literature found conflicting results. While empirical studies have found a correlation

between the two, the nature of this correlation is unclear. Meier and Sprenger (2010) and Skiba and Tobacman (2007) document a positive correlation between present-biased time preferences (elicited or estimated) and high interest rate borrowing (credit card debt and payday borrowing). This correlation, however, may be driven by borrower confusion or lack of information: Bertrand and Morse (2011) find that once payday borrowers are forced to think about their future interest payments, their demand for payday loans declines. Mani, Mullainathan, Shafir, and Zhao (2013) and Carvalho, Meier and Wang (2016) document evidence that suggests the causality runs in the opposite direction: They report that a high debt burden and financial stress reduce the cognitive function of borrowers, affecting their financial decision making. We contribute to this literature by providing empirical, real-world evidence that greater access to alcohol led to an increase in the demand for alternative and mainstream credit and defaults.

The remainder of the paper is organized as follows. Section 2 provides a simple framework for understanding how increased access to alcohol may affect credit decisions. Section 3 describes our empirical setting and the baseline identification strategy we use to uncover the effects of inconsistent time preferences on credit decisions. Section 4 describes our data and presents the relevant summary statistics. Section 5 discusses our main results. In section 6 we show additional tests that suggest that impulsive consumption explains our results. In section 7 we perform a set of robustness tests and section 8 concludes.

2 Simple Framework

This section provides a simple framework to demonstrate how limited opening hours may affect consumption and consumers' financial wellbeing.

2.1 Setup

Following the behavioral finance literature that stresses the importance of self-control problems, we assume quasi-hyperbolic preferences as in Laibson (1997),

$$U_0 = c_0 + \beta\delta c_1 + \beta\delta^2 c_2 + \dots \beta\delta^n c_n.$$

This model encompasses two cases, when $\beta = 1$ consumers have exponential discounting while if $\beta < 1$, their preferences are dynamically time-inconsistent (from now on: present-

biased preferences); A consumer with present-biased preferences might plan to consume less and save more in the future. When that future arrives, however, she will have trouble sticking to her initial plan. Put differently, if $\beta < 1$, the marginal rate of substitution (MRS) between today and tomorrow's consumption is not constant over time.

At t_0 , the consumers values c_1 *versus* c_2 as follows:

$$\frac{\partial U_0}{\partial c_1} = \beta\delta \text{ and } \frac{\partial U_0}{\partial c_2} = \beta\delta^2 \Rightarrow MRS_{c_1, c_2} = \frac{\beta\delta^2}{\beta\delta} = \delta,$$

whereas at t_1 she values c_1 versus c_2 in this way:

$$\frac{\partial U_1}{\partial c_1} = 1 \text{ and } \frac{\partial U_1}{\partial c_2} = \beta\delta \Rightarrow MRS_{c_1, c_2} = \frac{\beta\delta}{1} = \beta\delta.$$

Thus, over time the MRS_{c_1, c_2} changes. In other words, when $\beta < 1$, the individual consumes more in the present despite not having planned so in the past, even though there is no new information.

2.2 Present-Biased Preferences and Limited Opening Hours

Alcohol can be stored at home at low cost and people buy alcohol frequently, hence unbiased consumers should be able to adjust their behavior relatively quickly to the operating hours of the store and determine the optimal size of their alcohol stock at home. Thus, limited opening hours should merely shift the timing of their purchases, not the level of their consumption (Bernheim, Meer, and Novarro 2016).

However, when consumers have present-biased preferences, limited opening hours can function as a commitment device that helps consumers stick to their planned consumption. Imagine you plan *not to drink* tomorrow. Whether you are unbiased or have present-biased time preferences, you will not buy additional alcohol today so that you can drink tomorrow. But when tomorrow comes, if you have biased preferences, you will diverge from your plan and value drinking today again more than in the future. Thus, you will be tempted to buy alcohol. A closed store would then function as a commitment device that helps you stick to your plan not to drink. If you have unbiased preferences (consistent over time), however, you

will not change your mind and thus will follow your plan not to drink, independent of whether stores are open or closed.

2.3 Limited Opening Hours and Consumers' Financial Wellbeing

For consumers with time inconsistent preferences, removing a commitment device exposes them to the risk of consuming more than they would have liked to from a prior perspective. Because overconsumption today comes at the expense of future consumption, liquidity constrained consumers might need to borrow to finance everyday expenses, such as their grocery shopping or electricity bill later in the week.

Note that the financial wellbeing of low-income consumers might not only be influenced by the direct cost of the increased alcohol purchases. Alcohol spending can have a multiplier effect on consumer spending. For example, alcohol consumption often goes hand-in-hand with other activities such as dining and socializing. In addition, standards about what one is willing to buy can be lower while under the influence of alcohol. This can play out at home through online shopping and television infomercial purchases as well as outside the home in a café, club, restaurant, shop, and so forth.

Lastly, credit uptake enables consumers to smooth consumption when they are confronted with their overconsumption. This borrowing also bears the risk of beginning a downward spiral into financial distress. This risk is especially high when consumers utilize expensive alternative financial services, as borrowers in these markets tend to refinance their loans for multiple pay cycles.

3 Background: A Swedish Nationwide Experiment

3.1 Swedish Alcohol Market

Alcohol consumption and purchases are strongly regulated in Sweden. Taxes on alcohol are high, and the state has a monopoly on the retail sale of alcoholic beverages that contain more than 3.5% alcohol by volume and are not consumed onsite (i.e., restaurants and bars are not included in the monopoly). In 2000, the state owned 420 stores named Systembolaget, which were located throughout Sweden, with at least one store in each

municipality. In addition to the stores, there were about 520 retail agents in rural areas, where consumers can pre-order alcohol from Systembolaget's network. The minimum legal age to buy alcohol at Systembolaget is 20, and this is strictly enforced. Cashiers are instructed to ask for identification from customers who look younger than 25 (Norström and Skog 2005, Grönqvist and Niknami 2014).

Relative to other countries, the average per capita amount of pure alcohol consumed in the last two decades in Sweden has been low. In 1999, right before the experiment, Swedes consumed an average of 6 liters per capita, compared to 8 in the United States and 15 in France (see Figure 2). However, since then there has been an upward trend in the alcohol consumption in Sweden and a downward trend in France. Alcohol consumption in the United States has remained rather stable.

3.2 Swedish Mainstream Banking Sector

We study the credit decisions made within the Swedish pawn and mainstream credit markets. Mainstream lending to the public in Sweden takes place primarily through banks and mortgage institutions. Banks provide loans with different types of security as well as smaller loans without collateral. Banks, like mortgage institutions, also provide loans secured on homes and other buildings and property. In 2014, the financial industry accounted for 4.8% of the total gross domestic product (GDP) in Sweden. Swedish households account for 28% of total lending to the public, while Swedish businesses and foreign borrowers account for 32% and 33%, respectively.

The interest rates that banks set for their deposits and credits are highly dependent on the interest rates prevailing in the money market. Other factors affecting interest rates include the borrower's creditworthiness, the risk of the undertaking, the bank's financing costs, the competition among credit institutions, and the competition between different savings and loan forms. The banks' average deposit and lending rates have shown a clear downward trend since the early 1990s.¹

¹Source: <http://www.swedishbankers.se>, Banks in Sweden.

3.3 Swedish Pawn Industry

The pawn credit industry and its customer base in Sweden are similar to that of the United States. Pawn credit is a relatively simple transaction: The broker makes a fixed-term loan to a consumer in exchange for collateral. The pawnbroker supplies credit based only on the collateral value and not on the borrower's creditworthiness.

In 2000, Sweden had 25 pawnbroker chains with 56 pawnshops, 14 of which were based in Stockholm. The loan term in a standard contract varies from three to four months. In our data, we observe stable interest rates across pawnbrokers of approximately 3.5% per month. Customers can negotiate their loan-to-value ratio. If the customer repays the loan, the interest, and all required fees, the broker returns the collateral to the customer. However, if the customer does not repay the loan by the maturity of the contract, the collateral is appropriated by the pawnbroker and sold at auction or in store; the customer's debt is then extinguished. The borrower can roll over the debt for an additional three to four months and avoid losing the collateral by paying a fee and the accumulated interest.

3.4 Swedish Nationwide Experiment in Extending Store Hours

Since 1981 to 2000, the state monopoly liquor stores have been closed on weekends. However, due to growing consumer demand for extended opening hours, the Swedish parliament passed a bill to open liquor stores on Saturdays during a trial period (starting from February 2000) in certain parts of the country. It was determined that if the evaluation of the trial did not reveal any negative effects, Saturday opening hours would be extended to the entire country. The government commissioned researchers Thor Norström and Ole-Jørgen Skog (2003) to design and evaluate the experiment. The researchers selected the treatment counties (where the stores would be open on Saturdays) based on size, geographic location, and degree of urbanization to increase the external validity of the experimental findings. The treatment counties were Stockholm, Skåne, Norrbotten, Västerbotten, Västernorrland, and Jämtland. In addition, they selected control counties and designated buffer counties that stayed out of the experiment to prevent spillage across county lines. Following the researchers, we also exclude the buffer counties from our analysis. The map in Figure 1 identifies the treatment, control, and buffer counties. At the time, nearly half of the total

Swedish population lived in the treatment region. No other material alcohol policies were altered during the experiment period.

The initial assessment of the experiment was conducted a few months after its introduction by comparing time-series trends in alcohol sales and various crime and health indicators of both the treatment and control regions. The analysis showed a 3.7% rise in alcohol sales and no statistically significant effect on assaults or health (Norström and Skog 2003). The Swedish parliament, therefore, voted to expand the Saturday opening hours nationwide, a policy implemented in July 2000. In a follow-up study of the combined effects of the initial experiment and the nationwide expansion of Saturday opening hours, Norström and Skog (2005) again found an increase in sales of alcohol by about 3.7% and no statistically significant impact on assaults. Importantly for our study, the researchers found that the alcohol was purchased for immediate consumption. Specifically, they document a dramatic increase in positive alcohol breath analyzer tests that were taken while the stores were open, on Saturdays between 10am and 2pm but no change in tests that were taken when the stores were closed; between 2pm in Saturdays and 2pm on Sundays. Grönqvist and Niknami (2014) evaluated the same experiment by exploiting a much richer dataset with individual-level information for the entire Swedish population. Their findings confirm an overall increase in alcohol sales of 3.7–4%. In contrast to earlier studies, however, they also found that overall crime increased by about 20%.

The extended opening hours of the liquor stores could have affected people's motivation to purchase alcohol in two ways. First, Saturday sales could relax a pre-commitment device, giving present-biased individuals access to a temptation good that they would not have consumed had the liquor stores remained closed. Second, it facilitates access to alcohol for rational consumers who would like to plan their consumption ahead of time but who have time constraints. For example, people who work during the week may have trouble accessing the liquor stores during their weekday operating hours. In our study, we address the different channels through which the relaxation of the operating hours might affect consumption patterns.

3.5 Identification Strategy

We aim to identify the causal effects of impulsive consumption on financial wellbeing. A simple correlation would likely suffer from both reverse causality and omitted variable bias.² An idealized experiment to identify this causal effect would consider two identical groups of individuals. In that experiment, access to alcohol would increase for one group more than the other.

In our empirical setting, we use the variation in alcohol availability induced by the February 2000 policy change in Sweden to approximate this idealized setting. One naïve empirical strategy would be to focus on credit behavior before the policy change, comparing individuals who live in the treated counties to those who live in the control counties. After all, the consumers in the treated counties did experience greater access to alcohol.

However, because individuals who live in different counties are likely to differ in ways that may be correlated with credit market outcomes, a comparison of the credit behavior of individuals who live in treated and control counties is likely to be biased. For this reason, our empirical approach is based on triple-differencing. We take advantage of the fact that individuals in Sweden ages 18 and 19 are allowed to take credit but are not permitted to buy alcohol. This double-difference analysis (20–25 minus 18–19 year olds) is the basis of our identification strategy. We then take a third difference and compare outcomes before and after the policy change in February 2000.

We exclude the buffer counties to mitigate cross-county shopping. We also exclude people who move to avoid capturing strategic behavior focused on having greater access to alcohol.³

The identification assumption we make is that, in the absence of the policy change, the difference in credit market outcomes of individuals in the control and treatment counties who were eligible and ineligible to buy alcohol would have remained constant before and after February 2000. In Section 5.1 we provide pre-trend evidence that is consistent with this assumption.

² For example, individuals' financial distress may causally affect their alcohol consumption (reverse causality). Furthermore, individuals who are more likely to consume temptation goods may also be the types of people who are more likely to get into financial troubles (omitted variables).

³ In fact, only 1.6% of the individuals in our baseline sample move within the time window of our analysis.

4 Data and Summary Statistics

4.1 Data

We use a panel dataset that matches Swedish pawn borrowers and their pawn credit choices with their mainstream credit outcomes. The pawnbrokers' association in Sweden, which covers 99 percent of the total pawn broking market, generously supplied this dataset. This base dataset provides information on the transactions of 332,351 individuals who took out at least one pawn loan any time between 1999 and 2012. In the years of the experiment about 4 to 5% of the Swedish adult population borrowed at a pawnshop every year. This dataset contains information on all borrower transactions on a daily frequency, including loan size, value and type of pledge, and subsequent repayment behavior. Using the data, we construct a bimonthly panel to match the frequency of the mainstream credit bureau data.

By restricting the sample to pawn borrowers, our analysis is focused on the lower socioeconomic tier of the Swedish population. While this sample does not represent the entire Swedish population, it is well-suited to testing the predictions of Banerjee and Mullainathan (2010) about the relation between the consumption of temptation goods and the financial wellbeing of the poor. In the next stage, we match the population in our pawn dataset with records from the mainstream credit data registry. This dataset is supplied by the leading Swedish credit bureau, which is jointly owned by the six largest banks in Sweden and covers approximately 95% of the mainstream credit market. In addition to detailed credit information from the banking sector, the credit bureau also collects data from the Swedish tax authority (income and capital) and other government agencies, including the national enforcement agency (Kronofogden), which administers and executes private claims and all government claims. This dataset contains bimonthly snapshots of individual credit records from 1999 to 2001.

To implement our empirical strategy, we make two necessary restrictions to our sample. First, we include in our sample only individuals between the ages of 18 and 25. Second, we focus on individuals who reside in either the treatment or control counties during our window of analysis, 1999–2001. (Liquor stores began opening on Saturdays in February 2000.) These selection criteria result in a sample of 38,436 individuals whom we follow bimonthly from February 1999 to June 2001.

We use ordinary least squares (OLS) regressions to estimate the effect of alcohol exposure on the population in the treated counties that is eligible to purchase alcohol during the experiment period. The variable $treated_i$ equals one if the individual lives in one of the counties where the liquor stores opened on Saturdays. We interact $treated_i$ with the dummy variable $eligible_{i,t}$, which distinguishes between individuals who were allowed to buy alcohol, i.e., equals one if the individual is younger than 20 at each point in time. Finally, we create the dummy $post_t$, which equals one after February 2000. We include individual fixed effects, $\omega_{individual}$, calendar time fixed effects, ω_{time} , and calendar time $times$ county fixed effects, $\omega_{time*county}$. Finally, we also include all double interactions that are not absorbed by the fixed effects.

Our dataset has the advantage of greater detail than other datasets used in the literature, but it also has a drawback. Because of data and regulatory constraints, our data construction is restricted to people who took at least one pawn loan over the 1999–2012 period. Consequently, our sample covers a limited population and has an embedded look-ahead bias because it includes people who will be pawn borrowers in the future. We do not view this bias as critical because we restrict the data to borrowers who are ages 18 to 25 at any point in the studied period. Since borrowers must be at least 18 years old, we cannot avoid the look ahead if we are interested in having a group of 18–19 year old borrowers as part of our control sample. The look-ahead bias might bias our absolute level estimates of borrowing rates. However, because we are focusing on a short event window and are interested in estimating differences between age groups within counties (triple-difference estimation methodology, as discussed above), there should be no material bias in the main estimations of the analysis.

While we do not have detailed information about the entire Swedish population, we can estimate borrowing activity based on aggregate county-level information. Our county-level regressions use quarterly information purchased from Statistics Sweden on the number of individuals in each age group living in each county. We use these data to scale variables like the number of new loans and number of defaults to the entire population of the county. Using these data, we can make statements about the extent of the aggregate pawn lending activity at the county level, while controlling for the varying number of residents.

4.2 Summary Statistics

We begin the empirical analysis by discussing select summary statistics of our outcome variables. Appendix A contains definitions of both the dependent and independent variables of interest. Table 1 provides the summary statistics of our outcome variables during the period before the experiment started (February 1999 to February 2000).

Our sample is composed of people of relatively low socioeconomic status. Panel A presents summary statistics about pawn borrowing. In our sample period, the average number of new pawn loans is 0.11 and the default rate is 0.7% per month in the pre-period.

Panel B presents the mainstream credit outcome variables for the pawn borrowing population. As we focus on the Swedish population that lives on the margins of formal credit markets, it is no surprise that the percentage of individuals with an arrear is 4% and the average number of arrears is 1.00. Furthermore, a large share of this population does not have a credit card; the mean number of credit cards is 0.13, with a mean revolving credit card balance of 599 SEK (67 USD), which constitutes 10% of their mean monthly total income, registered by the tax authorities at that time.

5 Main Results

5.1 Demand for Pawn Credit: County-Level

We start by exploring the demand for credit after the liquor stores began opening on Saturdays. We run triple-difference regressions at the county level. The unit of observation is the number of credits taken/defaults/etc. per 100,000 individuals living in a specific county and of a certain age (18, 19, 20, ..., 25) for each quarter. Where in the calculation of the percentages of our outcome variables per age group in each county, the ‘ones’ are retrieved from our pawn credit registries and the ‘zeros’; the total number of people in each age group in each county, are retrieved from Statistics Sweden. Our cross-section specification is the following reduced form model:

$$\begin{aligned} \text{Credit UpTake}_{\text{county,age,time}} & \quad (1) \\ &= \beta_1 \text{Treated}_c * \text{Eligible}_{\text{age}} * \text{Post}_t + \beta_2 \text{Treated}_c * \text{Post}_t \end{aligned}$$

$$+\beta_3Eligible_{age} * Post_t + \beta_4Eligible_{age} + \omega_{county} + \omega_{time} + \varepsilon_{county,age,time}.$$

We cluster robust wild bootstrap standard errors with 1,000 replications.⁴

Table 2 presents the regression results for our pawn credit outcome variables. Column (1) shows that the probability of taking out a pawn loan by individuals who are eligible to buy alcohol and live in a county where the retail alcohol stores remained open on Saturdays increased by an average of 73.5 per 100,000 residents. This effect is a 28% increase over the pre-period average credit uptake rate. Column (2) shows that the effect is as large as 44% when the estimation uses the log specification. Column (3) combines the extensive (taking a pawn loan) and intensive margins (size of the loan), and thus is a good estimate for overall pawn borrowing activity in the treated population. The average pawn loan size per 100,000 people increased by 78%. Columns (4) and (5) show that for the number of pawn credit defaults and rollovers, the point estimates are positive but are not statistically significant.

Unfortunately, due to the quarterly frequency of our population statistics, we have insufficient observations in the pre-period to run county-level regressions for our mainstream credit outcomes.⁵

5.2 Demand for Total Credit and Default: Individual-Level

We next turn to our baseline panel regression specification so that we can take into account individual fixed effects and exploit additional mainstream credit outcomes. Our main panel specification is the following reduced form model:

$$\begin{aligned} CreditUpTake_{individual,time} &= \beta_1 Treated_i * Eligible_{i,t} * Post_t + \beta_2 Treated_i * Post_t \\ &+ \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{time*county} + \varepsilon_{i,t}. \end{aligned} \quad (2)$$

The coefficient β_1 , which is our main outcome, measures the differential likelihood of taking out credit between the eligible and ineligible groups in the treated and control counties during the pre- and post-periods.

⁴ Sweden only has 21 counties, so the numbers of counties in the treatment and control groups (10) are too small to allow us to cluster at a county level.

⁵ Our mainstream credit data start in October 1999, which translates into one quarterly observation per county during the pre-period.

The exclusion restriction (identifying assumption) in this specification is that the treated and control populations would have followed parallel trajectories with respect to the outcome variables (e.g., credit take up, default) had the experiment not run. While this assumption is not testable, we find evidence suggesting that this assumption is likely to hold. First, we show below that the difference in behavior between those eligible and ineligible to purchase alcohol is similar in the control and treatment counties in the 18 months before the experiment began. Second, we show that the credit effects exhibit discontinuities in the outcome variables in the treatment group around the implementation date of the experiment.

Table 3 documents that an increase in the access to alcohol increased the demand for total credit and an increase in the likelihood of default. In Column (1) we regress the total credit balances (pawn and mainstream registries) on the interaction between eligible dummy, post-period, and treated dummy. The coefficient measures the average increase in credit balances of 344.2 SEK (38 USD) for individuals who are eligible to purchase liquor in the treated areas post-implementation. This is a non-negligible increase in balances given that the average balance in the pre-period is nearly 5,000 SEK.

In Column (2) we document an increase in the total number of defaults recorded in the pawn and mainstream credit registries. Individuals who are eligible to purchase liquor in the treated areas post-implementation exhibit higher number of defaults by about 27%, relative to the pre-period mean.

In the next sections we split our results into the demand for alternative credit (pawn) and mainstream credit (credit cards, credit lines and installment loans)

5.3 Demand for Pawn Credit: Individual-Level

Table 4 shows the demand for pawn credit when we control for individual fixed effects in our individual-level regression specification (see Equation 2). Columns (1) and (2) indicates that the treatment group, individuals older than 19 who live in counties with increased access to alcohol, are 2.9 percentage points more likely to take out a pawn loan (25% increase relative to the pre-period mean uptake rate). Furthermore, pawn loan size increased by nearly 12% for this group. Together this translates into an increase of 230 SEK (26 USD) per pawn loan. However, we find no significant effect on defaults and rollovers. Overall, these results are qualitatively similar to those in the aggregate data (Table 2).

The increase in the demand for pawn credit is also apparent in the charts provided in Figure 3. Figure 3a shows a sharp increase in the difference in the number of new loans taken by drinking-eligible and ineligible borrowers, relative to the control populations. The chart shows that the increase in the number of loans took place between March and May of 2000, right after liquor experiment began.

Similar effects, albeit somewhat less dramatic, are observable on the intensive margin in Figure 3b. The figure shows that the spread in loan sizes between drinking-eligible and ineligible borrowers increased in the treatment area but declined in the control area.

Next, we examine the default rate of pawn borrowers. Figure 3c shows a sharp, permanent increase in the default rate of treated borrowers (drinking-eligible borrowers who live in the treated counties). Prior to the experiment, the spread in the default rate between the drinking-eligible and ineligible was about 0.004 in the control counties and 0.001 in the treatment counties. Following the experiment, the spread in the default rate increased somewhat to 0.006 in the control counties and shot up to about 0.12 in the treatment counties.

The charts also help support the validity of the identification assumption. Specifically, the identification assumption for our regression (Equation 2) is that had the alcohol experiment not happened, the difference in the probability of credit uptake by drinking-eligible and ineligible individuals who live in the treatment and control counties would have followed parallel trajectories. Two important features in the Figure 3 charts support this assumption: First, the charts show parallel trends in the pre-period. Second, all three charts show a regime shift (a “jump”) right after the introduction of the experiment.

5.4 Demand for Mainstream Credit: Individual-Level

We next explore the effects of extending the opening hours of liquor stores on the mainstream credit market: credit cards, installment loans, and personal credit lines. Table 5 Panel A, Columns (1), (2), and (3) show patterns in the mainstream credit market that are similar to those in the pawn market. We find statistically and economically significant increases in credit card borrowing on both the extensive and intensive margins. In Column (1), the probability of taking an additional credit card increases by 1.9 percentage points (about 10% relative to the pre-period mean). Also, the average balance of the treated

population increased by 122 SEK (equivalent to 14 USD, Column (2)), and the average credit card limit increased by 234 SEK (equivalent to 26 USD, Column (3)).

Installment loans are credit provided when purchasing larger items, like a Billy bookcase. We find no increase in the number of or balance of installment loans. This result supports, to some extent, the idea that the effects that we document do not stem from an unobservable shock to the treated population, e.g., improvement in the credit conditions of this population.

In Columns (6) to (8), we explore the effects of Saturday liquor store opening hours on credit lines. The number of personal credit lines decreases by 1.6 percentage points (about 5% decrease relative to the pre-period mean), but the balance and credit limit of existing credit lines increases by 320 SEK and 336 SEK, respectively (about 10% increase relative to pre-period mean).

In Table 4, Panel B, we investigate how expanded access to alcohol affects the utilization of credit and the performance of credit card borrowers. The panel documents that credit card utilization increases by up to 15% (Columns (1) and (2)).

We also examine the performance of mainstream borrowers. In Sweden, defaults on any type of credit are recorded as an arrear flag on a person's credit file. In Panel B, Column (4), we regress an indicator variable as to whether there is an arrear flag on file (called an arrear receipt). The results show that following the Saturday hours experiment, the spread in the likelihood of having arrear receipts for the treated population increased by 1.3 percentage points, which is equivalent to about a 26% increase over the pre-period mean. The average number of arrear receipts per person increased by 0.051, or 4% relative to the pre-period mean (Column (5)). Overall, these results are in line with our findings for the pawn credit market.

An important sanity check is to verify that our results make economic sense—that the effects we report are plausible given the increase in alcohol consumption. According to statistica.com, the total revenue from off-premise alcohol sales in Sweden in 2000 was 24.7 billion SEK. Norström and Skog (2005) and Grönqvist and Niknami (2014) reported an increase in alcohol sales of 3.7% and 4%, respectively (about 900 million SEK in additional sales). We presume that the population in our sample consumed only part of this increase and that only part of it was financed by credit. Hence, the total increase in credit use that we document should be only a small fraction of the 900 million SEK.

Table 3 reports an increase in borrowing by the drinking-eligible population living in the treated counties of about 466 SEK per two months. This amounts for our population to roughly 11,7 million SEK, which is 1.3% of the 900 million⁶. Given that our sample is more likely to suffer from present-biased preferences than the general public (see e.g. Meier and Sprenger 2010, Skiba and Tobacman 2007) which makes them more vulnerable to impulsive consumption, we conclude that our estimates for individuals who live on the margins of the formal credit market are reasonable.

5.5 Convenience Shopping or Present-Biased Preferences?

So far we have documented an economically large cost of an increase in access to alcohol among individuals at the margins of the formal credit markets. We explained the effect as a response among consumers with impulsive consumption behavior to the wider availability of a temptation good.

An alternative explanation is possible, however. The extended opening hours could make purchasing alcohol more convenient. As a result, consumption of alcohol would increase as well as reliance on credit. If this were true, even with a fully rational population, we would observe an increase in alcohol purchases and higher use of credit in the counties where liquor stores are open on Saturdays. The Saturday hours might simply allow people who are busy during the week to purchase alcohol. Thus, according to this view, the Saturday store hours represent a reduction in opportunity costs.

Our data allow us to discriminate between the present-biased and rational consumer hypotheses. Specifically, we identify two subpopulations—retirees and the unemployed—for whom the inconvenience benefit from opening the stores on Saturdays is minimal. If retirees and the unemployed indeed do not have a present-bias, then they can execute their plan to purchase alcohol during the week with no inconvenience and consume the alcohol over the weekend, even if the stores are closed on Saturdays. In other words, opening the liquor store on Saturdays should not affect their behavior. Saturday hours should affect rational individuals who work during the week. Therefore, if the effects that we document are due to increased convenience, then we should find a large difference in the financial consequences

⁶ No of 20-25 year olds in the Swedish population in the year 2000 was 628,901 x 4% (pawn borrowers) = 25,156 individuals x 466 SEK = 70,3 million

for employed individuals relative to individuals who are not working (retirees and unemployed).

We test this hypothesis in Table 6 Panel A contrasts the financial effects for retirees (ages 65–75) versus older employees (ages 55–60). Panel B compares the financial outcomes of unemployed individuals (ages 20–65) to those who are employed within the same age group. Because the comparison with the 18 year olds is no longer appropriate, we run a triple difference in which the final difference, $\gamma_{i,t}$, is a dummy for being retired (Panel A) or unemployed (Panel B). The table shows the coefficient β_1 from the following regression:

$$\begin{aligned} CreditUpTake_{individual,time} = & \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t \\ & + \beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{individual,time} \end{aligned} \quad (3)$$

The results reveal little difference in the financial outcomes of the employed population and those with more flexible schedules. These non-results are not driven by low power (there are more than 300,000 and 1,000,000 observations in Panels A and B, respectively), but rather by coefficients that are close to zero with tight standard errors. For example, in Column (1) we estimate the effect on the number of new pawn loans. In Table 4 Column (1), the coefficient is 0.029. In contrast, the coefficients in Column (1) of Table 6 Panels A and B are -0.002 and -0.010, respectively, with standard errors of about 0.008.

We therefore conclude that the extended opening hours affected both populations similarly. This result is consistent with the idea that alcohol is a temptation good that triggers a present bias in people and leads to current consumption at the expense of future consumption.

6 Additional Results

The results presented in the previous sections show that individuals who were eligible to purchase off-premise alcohol in the treated counties during the post-period demonstrated greater demand for credit, greater utilization of credit, and a higher frequency of default. One caveat to our empirical setup and data is that although we have good credit data about the individuals studied, we cannot directly observe whether they actually purchased alcohol in the stores.

To mitigate concerns that the people who borrow more are also the people who drink more, we provide additional tests to tighten the identification. First, we split the sample by gender. Prior studies by the World Health Organization⁷ have shown that men consume greater quantities of alcohol than women; hence, we anticipate that the effects of greater access to alcohol on financial wellbeing are greater for them. Second, we examine the timing of the increase in borrowing. We expect that the treated population would demand credit following the weekend. Third, we test whether the demand for credit was concentrated in a small part of the population (a few alcohol addicts), or whether it was spread across the treated population.

6.1 Gender Split

Studies about alcohol drinking habits generally show that males consume larger quantities than females.⁸ We expect, therefore, that the financial effects will be amplified in the male treated population relative to the female treated population. To check whether this is the case, we repeat our main tests for males and females separately.

The results are presented in Table 7. They show that the effects are almost entirely driven by the male population. The effects on the number of new pawn loans and their sizes are statistically significantly different from zero for men, but are positive and statistically insignificant for women. We observe no material difference in the effects on pawn loan default (both are virtually zero) but find an unexpected effect for pawn loan rollovers: Women's rollovers are significantly greater than zero.

In the mainstream market, all results are stronger for the male population. The number of new credit cards and their balances as well as the balances of credit lines are higher for men. Furthermore, utilization of credit cards is higher for men. We observe no effects on installment loans (as in the main regressions) and no statistically significant effects on defaults, although the point estimate of the default probability is higher for males than for females.

⁷ See for example: http://www.who.int/substance_abuse/publications/global_status_report_2004_overview.pdf

⁸ The World Health Organization Global Status Report on Alcohol (2004) shows that Swedish males, on average, drink as much as two times more than females. Furthermore, males are about twice as likely to engage in heavy drinking (>30g pure alcohol per day for men and >20g pure alcohol per day for women) and binge drinking (>6 drinks per seating) than females.

Overall, these results indicate that most of the increase in the demand for credit is driven by the male population. This finding is consistent with prior evidence suggesting that men drink significantly more than women and that the effects of the treatment, therefore, should be stronger for men. This result also corroborates the implicit assumption in our study that the people who consume more alcohol in the treated areas are those who also demand more credit.

6.2 Monday Borrowing

Another way to provide further corroborating evidence about the effect of Saturday opening hours on the financials of consumers in the treated areas is to examine when the increased demand for credit occurs in the treatment group. During the time of the experiment, pawn shops in Sweden were open during weekdays and closed over the weekend. If a present-biased person engaged in an impulsive purchase of alcohol over the weekend, she would be more likely to borrow at the beginning than at the end of the week. In other words, impulsive shoppers experience a negative cash flow shock over the weekend, more likely to result in shortage of cash on Monday. A rational shopper who plans the purchase would borrow ahead of time. Thus, a rise in early-week borrowing would provide some evidence that the increase in alcohol consumption is driven by present bias.

Our pawn registry includes day-level transaction time stamps that allow us to examine the timing of pawn loans. We construct, therefore, a person-day dataset (as opposed to the previously-used person-bimonthly dataset) in which we record the number of loans (typically zero or one) that each person took a pawn loan on a particular calendar day. We first verify our results from Table 4, this time on a daily frequency. We regress the number of loans on the treatment indicator (triple interaction). The results are presented in Table 8, Columns (1) and (2) (different sets of fixed effects), and in Columns (5) and (6) (the dependent variable is logged). As in Table 4, we find that treated individuals are more likely to take a pawn loan. The results in Columns (1) and (5) are statistically significant, and those in Columns (2) and (6) are below significance level. We attribute the loss of significance in Columns (2) and (6) due to the granularity of data on the daily frequency.

Next, we break down the average daily effect into two: Monday and the rest of the week. In Columns (3)-(4) and (7)-(8) we interact the variable of interest with a Monday

interaction and add day of the week dummies to absorb the ‘regular’ tendency to take a loan on a certain day, were Monday is omitted and absorbed by our constant. The results show that 27-32% of the increase in pawn borrowing caused by additional access to alcohol on Saturday comes from borrowing on Monday.⁹

In summary, the results in Table 8 indeed indicate that the additional access to alcohol in the treated counties shifted the uptake of pawn loans significantly to Monday for the population that was eligible to buy alcohol.

6.3 A Few Alcoholics? The Distribution of the Additional Up-Take in Credit

The results so far have shown an increase in the average demand for credit. An important question is whether this increase is evenly spread across the population or is skewed. A skewed distribution would suggest that a small number of people (potentially alcoholics) are driving the results. Conversely, an even distribution would indicate that the effect is spread throughout the population.

In contrast to our previous analyses where we estimated the *average* effect, here our objective is to examine the distribution of the effect across individuals. We run the following regression:

$$\begin{aligned} CreditUpTake_{individual,time} \\ = \beta_1 treated_i * post_t + \beta_2 eligible_{i,t} \\ * post_t + \beta_3 eligible_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{individual,time} \end{aligned} \quad (4)$$

We exclude the triple interaction that was the variable of interest in Tables 3, 4 and 5: ***treated_i * eligible_{i,t} * post_t***. Next, we examine the distribution of the residuals *only* for the group that is subject to the treatment. In the regressions that originally showed an increase in credit demand, these residuals should have a positive average. We focus our attention on the subset of borrowers that took credit. The question is whether the positive average in loan size is driven by a small number of large loans or by demand of borrowers across the board.

⁹ We calculate 27% to 32% increase on Mondays in the following manner: (average daily effect + average effect on Monday)/((5*average daily effect) + average effect on Monday) = (0.0003596 + 0.0001799)/((5*0.0003596) + 0.0001799) = 27%. Similar calculation using the coefficients on Column (4) yield 32%.

Figures 5a and b show the distribution of the residuals of the loan sizes (conditional on being treated and on taking credit) for pawn and credit card borrowing, respectively. The figures show that the distribution of the borrowing is concentrated in a single cluster, with no material outliers.

7 Robustness Tests

In the Appendix, we present robustness tests to show that the empirical setup, population sample, and empirical choices do not drive the results. First, we provide a placebo test, rerunning the tests a year earlier (when no experiment took place). Second, we verify that the choices of buffer counties or border counties did not materially affect the results. Third, we show that the results are not particular to the specific choice of the age groups in the treatment group. Fourth, we demonstrate that the results are not particularly sensitive to the choice of error clustering.

7.1 Placebo Test

One concern is that we could be capturing some long-term trends in differences between populations. To explore whether this is the case, we perform a placebo test in which we repeat the test but move the timeline a year earlier. Appendix Table 1 presents the results of running our main regression test on a sample but shift the timing of the experiment exactly one year backward. That is, we define a Placebo New post-period from January 1999 to January 2000 and use pawn credit uptake, default, loan size, and rollovers as outcomes. In all cases, the estimated coefficient of interest is not significantly different from zero at conventional significance levels. These results support the assumption that our main results are not driven by differential secular trends of individuals. Unfortunately, our data on mainstream credit outcomes do not reach equally far back in time, preventing us from executing a similar exercise for these outcome variables.

7.2 Excluding Border Counties, Including Buffer Counties

We perform an additional test to ensure that our results are not affected by spillover to other countries. Specifically, the southern county of Skåne in Sweden borders Denmark, and

drinking-ineligible individuals may cross the border to purchase alcohol, or Danish people may purchase alcohol in Swedish shops on Saturdays. In Appendix Table 2, we use a sample that excludes Skåne. The results are very similar to the ones presented in Table 4 and 5.

In another test, we add the buffer counties to the control group and rerun the main tests. In the original experiment, the buffer counties were put in place to prevent spillover, i.e., to minimize the possibility that individuals in the control counties could travel to liquor shops in the treatment counties to purchase alcohol on Saturdays. In the main tests in this study, we excluded the buffer counties from the analysis. As a robustness check, we combine the populations in the buffer and control counties, and rerun the main tests. Appendix Table 3 shows that the main results are robust to the change in the definition of the control population.

7.3 Sensitivity of Results to Eligible Age Cut-Off

Another empirical choice that we made in our main analyses was to define the treated group as individuals ages 20 to 25. The motivation was that this group is closely related in characteristics of the control group: 18 to 19 years old who were below the legal age to purchase alcohol. Keeping the age range too tight (e.g., 20–21) could result in low statistical power, whereas widening the age range could increase the statistical power but reduce the comparability of the treatment and control groups.

To verify that the results are not unique to the specific choice made, in Appendix Table 4 we vary the age groups from 20–21 to 20–27 and show that the effects we document barely change with the choice of age bands. As expected, some of the results decline in magnitude and statistical significance (e.g., number of pawn loans, number of credit cards), while others increase in magnitude and statistical significance (e.g., default frequency, credit line balances).

Overall, it appears that our results do not change materially when varying the age band.

7.4 Sensitivity to Clustering at Higher Levels

In the empirical analysis, we also made a choice about the geographic level of error clustering (individual level). In Appendix Table 5, we compare the results when clustering at

the individual level, the parish level, and the municipality level. The significance of the results does not change much.

8 Conclusion

Whether present bias is responsible for the personal indebtedness of households is an important question for both academics and policymakers. Previous research has shown that present bias is responsible for impulsive consumption. In turn, higher consumption is thought to affect intertemporal substitution through the budget constraint. In particular, researchers have hypothesized that temptation goods, which one regrets consuming after the fact, may trigger myopic behavior by individuals and eventually affect their financial wellbeing (Banerjee and Mullainathan 2010). Until now, only a few empirical studies have been able to provide evidence that indeed the supply of such goods has a meaningful effect on household finances, particularly on households of low socioeconomic status.

Our study fills this gap in the literature and provides novel tests of the effects of changes in the supply of alcohol on borrower behavior. Our empirical analysis is based on an experiment conducted in Sweden in 2000 in which government-controlled liquor stores extended their operating hours into the weekend in some counties while remaining closed over the weekend in other counties. Our sample focuses on a population that borrows from the fringe credit market. Our findings show that greater access to alcohol led to higher demand for credit in both the pawn credit market and the mainstream credit market. In addition, we document that increased access to alcohol led to higher default rates.

Overall, our results provide empirical evidence that an increase in the supply of temptation goods causes individuals to consume more in the present at the expense of future consumption, resulting in higher borrowing rates and worsening financial wellbeing. Policymakers can improve financial wellbeing of myopic consumers by limiting their access to temptation goods.

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Figure 1. Map of Treated and Control Counties

In 2000, Sweden implemented a large experiment in which all alcohol retail stores in some counties were open on Saturdays. The researchers who designed the experiment selected the treatment counties (where the stores would be open on Saturday) based on size, geographic location, and degree of urbanization to increase the external validity of the experimental findings. The treatment counties (hashed pattern) were Stockholm, Skåne, Norrbotten, Västerbotten, Västernorrland, and Jämtland. The control counties (striped pattern) were Värmland, Örebro, Västra Götaland, Östergötland, Jönköping and Kalmar. Gotland (black) was not included in the experiment because of extreme seasonality in the alcohol consumption due to summer visitors on the island. The buffer counties (white) were also not treated, but excluded from our analysis to mitigate the concern that our findings are deluded by cross county border shopping.

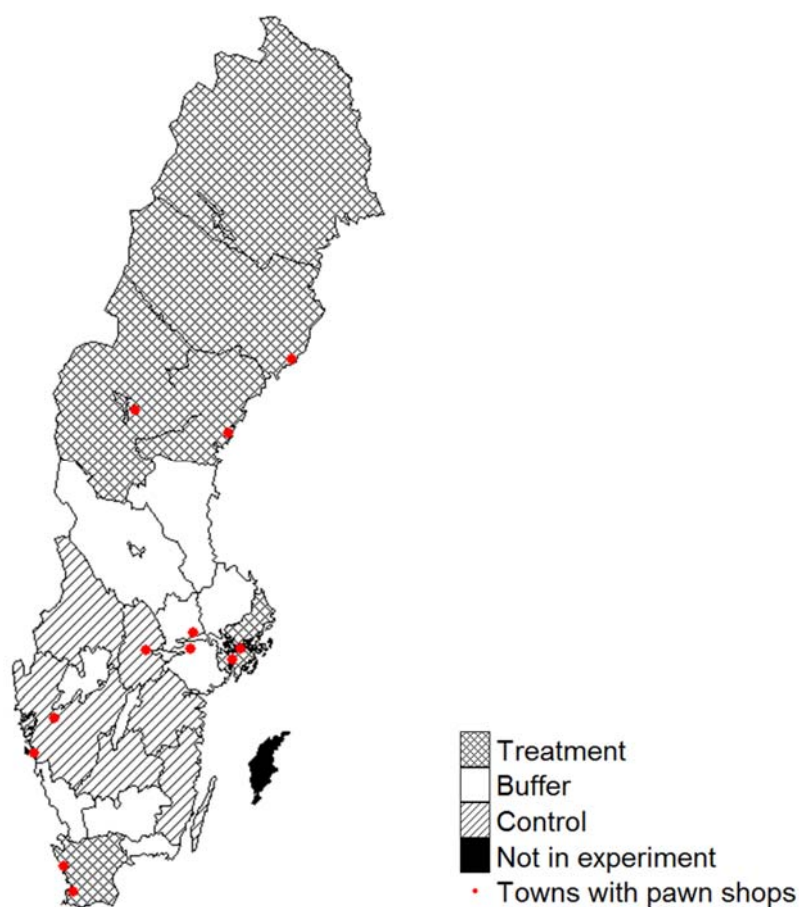


Figure 2. Alcohol Consumption in Sweden and the Select OECD Countries, 1994–2009

This figure shows the average number of liters of pure alcohol consumed per year per capita in Sweden (solid black line) and selected countries in the Organization for Economic Cooperation and Development (OECD) between 1994 and 2009.

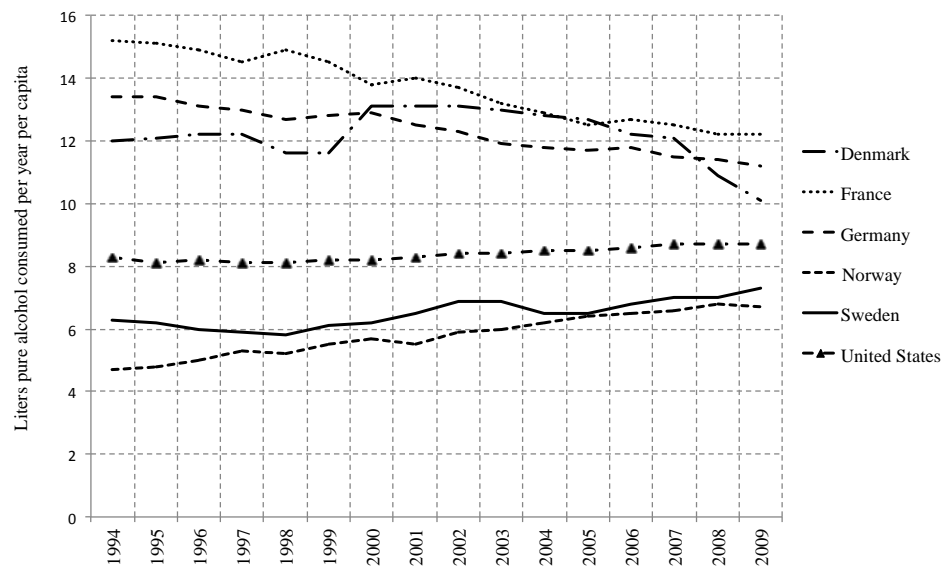


Figure 3. Pre-Trends in Pawn Credit Outcomes

This figure shows that there is no difference in the pre-period trends (before the policy change) of the difference between borrowers who could legally purchase alcohol and those who could not in the treatment and control counties for our main outcomes. The top panel shows pre-period trends for the probability of take out a pawn loan I(pawn loan>0). The lines represent the differences in averages of the respective outcome variables between individuals who were allowed to buy alcohol (eligible) and individuals who were not (ineligible) for individuals in the treatment counties. The dashed line represents the same difference for individuals in the control counties. The dashed line represents the same difference for individuals in the control counties.

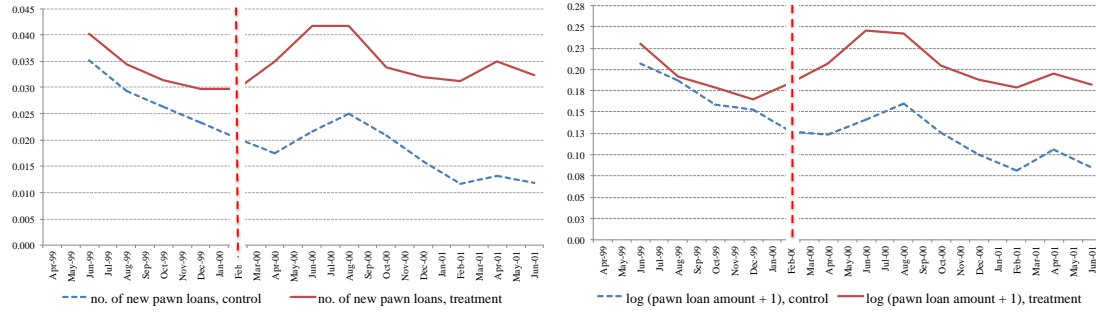


Figure 3a. Difference in the new pawn loans taken by drinking-eligible minus ineligible

Figure 3b. Difference in the logged pawn loan amount taken by drinking-eligible minus ineligible

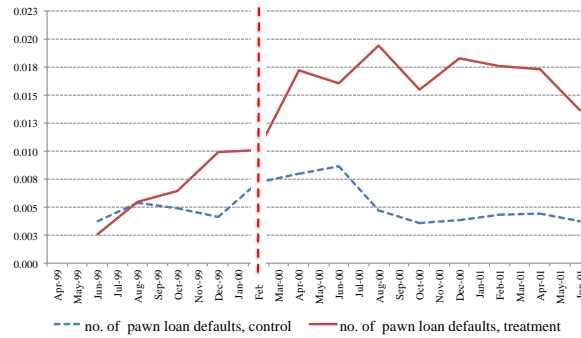


Figure 3c. Difference in the number of pawn loan defaults by drinking-eligible minus ineligible

Figure 4. Pre-Trends in Mainstream Credit Outcomes

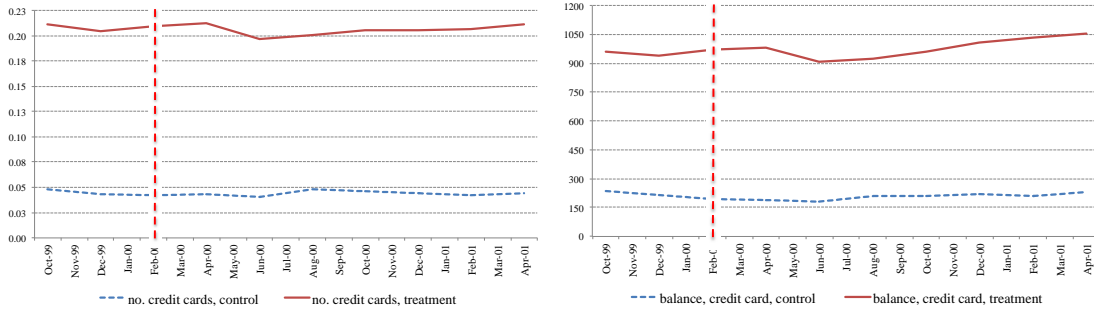


Figure 4a. Difference in the number of credit cards taken by drinking-eligible minus ineligible

Figure 4b. Difference in the balance of credit cards taken by drinking-eligible minus ineligible

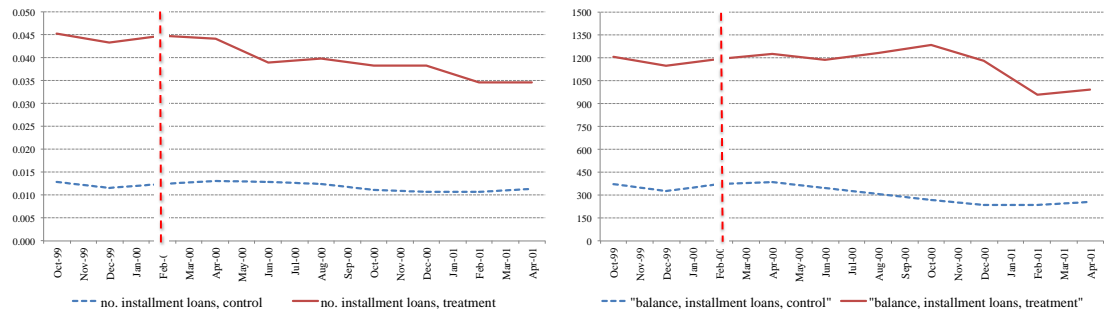


Figure 4c. Difference in the number of installment loans taken by drinking-eligible minus ineligible

Figure 4d. Difference in the balance of installment loans taken by drinking-eligible minus ineligible

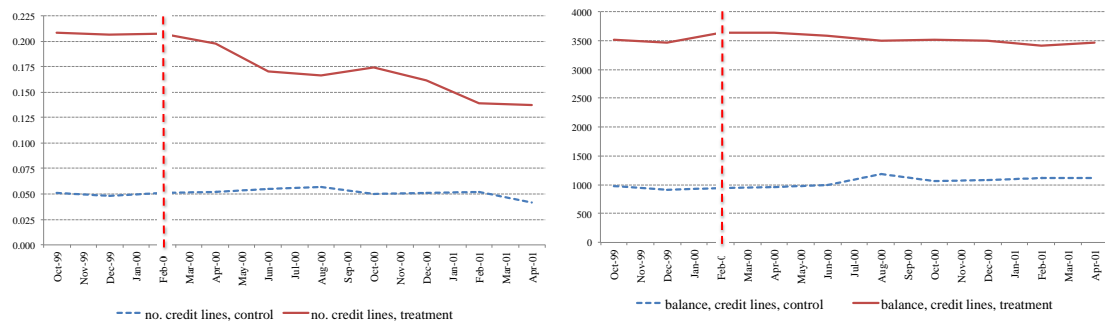


Figure 4e. Difference in the number of credit lines taken by drinking-eligible minus ineligible

Figure 4f. Difference in the balance of credit lines taken by drinking-eligible minus ineligible

Figure 4. Pre-Trends in Mainstream Credit Outcomes (Cont.)

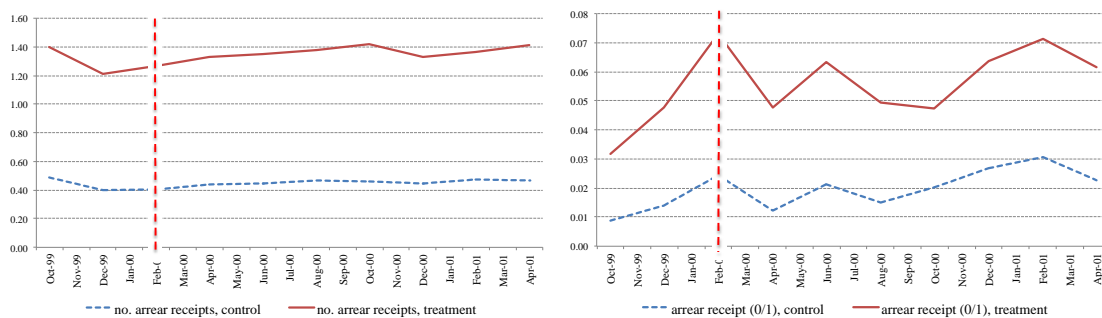


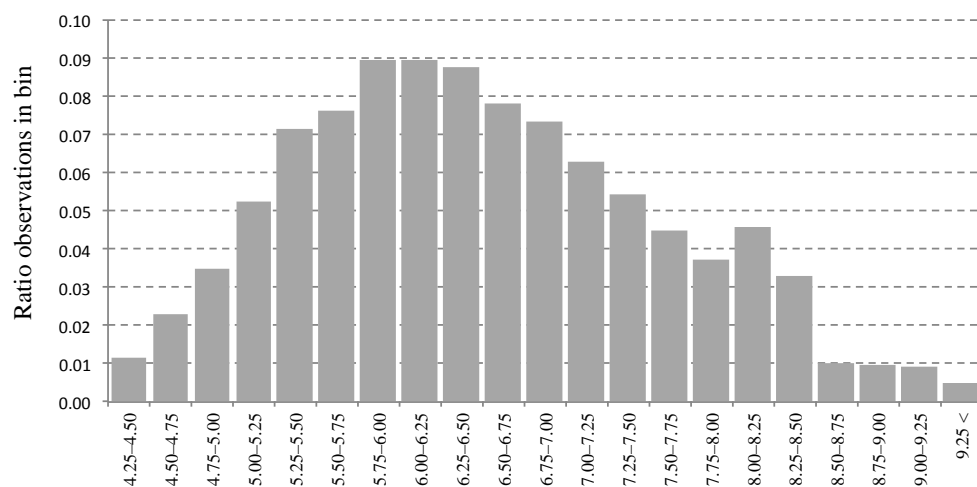
Figure 4g. Difference in arrears receipts by drinking-eligible minus ineligible

Figure 4i. Difference in the number of arrears receipts by drinking-eligible minus ineligible

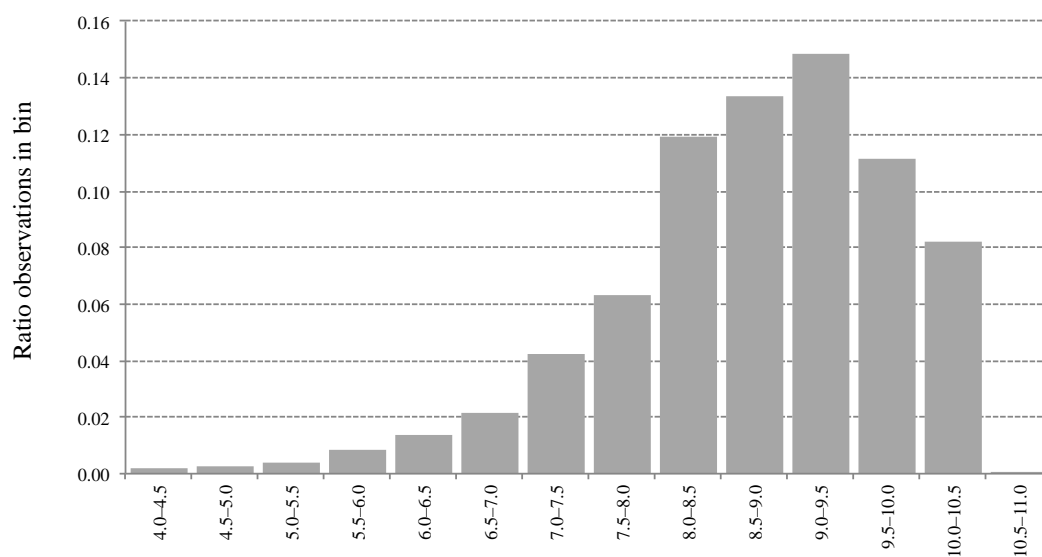
Figure 5. Distribution of Credit Card and Pawn Loan Size Uptake

The figures plot the distribution of the residuals for pawn loan size (Panel A) and credit card balance (Panel B) for the treatment cell, i.e., $\text{post} \times \text{eligible} \times \text{treated county}$, from the baseline regression (Equation 2) without the triple-interaction.

Panel A. Log (pawn loan size +1), Conditional on Taking a Pawn Loan



Panel B. Log (credit card balance +1), Conditional on Having a Credit Card



Appendix A. Variable Definitions

This table presents the definition of the independent and dependent variables of our regressions.

Panel A: Independent Variables

Variable	Definition
Treated	Equal to one if living in a county exposed for Saturday open shops and zero if living in a county used as a control county.
Post period	Equal to one if the date is post February 1st. and zero if the date is February 1st or earlier.
Eligible	Equal to one if age is 20 or older, and zero if age is 18.

Panel B: Dependent Variables

Variable	Definition
<u>Pawn Credit Market (bimonthly freq.)</u>	
No. new pawn loans	Equal to the number of pawn loans taken out in a pawn shop during that bimonth.
No. defaulting loans	Equal to the number of pawn loans held by that person that went to auction during that bimonth.
No. future defaulting loans	Equal to the number of pawn loans taken out in a pawn shop during that bimonth that in the future will default.
log(loan size)	Equal to the logarithm of the total loan principal taken out during that bimonth, plus 1.
No. rollovers	Equal to the number of pawn loans held by that person that rollover during that bimonth.
No. future rollovers	Equal to the number of pawn loans taken out in a pawn shop during that bimonth that in the future will rollover.
<u>Mainstream Credit market (bimonthly freq.)</u>	
No. credit arrears	Equal to the number of credit arrears the individual has on his her credit report.
Arrear receipts	Equal to one if the individual will receive at least one new credit arrear before the next observation, and equal to zero if the individual will receive none.
Credit card, number	Equal to the number of credit cards the individual owns.
Credit card, balance (SEK)	Equal to the sum of balances of credit cards the individual owns.
Credit card, limit (SEK)	Equal to the sum of limits of credit cards the individual owns.
Installment, number	Same as credit card, but for installment loans.
Installment, balance (SEK)	Same as credit card, but for installment loans.
Installment, limit (SEK)	Same as credit card, but for installment loans.
Credit line, number	Same as credit card, but for credit lines.
Credit line, balance (SEK)	Same as credit card, but for credit lines.
Credit line, limit (SEK)	Same as credit card, but for credit lines.

Panel C: County-Level Dependent Variables

<u>Pawn Credit Market on County Level (monthly freq.)</u>	
	Variables are defined as above, then summed per age of the borrower, per county divided by 100,000 inhabitants, per age, per county. So that we end up with a fraction of individuals that borrow out of all individuals in that age living in that county for each period.

Table 1. Summary Statistics

This table presents the summary statistics for our dependent variables in the pre-period, which corresponds to January 1999 to January 2000 for the credit outcomes. The sample is 18–25 year olds in both the treated and control counties.

Panel A: Pawn Credit Market

Bimonthly frequency	Pre-period									no. obs.	no. individ.
	Mean	Std Dev	Min	10pctl	25pctl	50pctl	75pctl	90pctl	Max		
No. new loans (p. month)	0.110	0.411	0.000	0.000	0.000	0.000	0.000	0.000	3.000	164,382	38,320
No. defaulting loans (p. month)	0.007	0.094	0.000	0.000	0.000	0.000	0.000	0.000	2.000	164,382	38,320
Loan size unconditional	174.4	925.2	0.000	0.000	0.000	0.000	0.000	0.000	10,050	164,382	38,320
Loan size on participation	2,120	2,506	90.00	350.0	600.0	1,100	2,500	5,600	10,050	13,380	38,320
log (loan size + 1)	0.581	1.970	0.000	0.000	0.000	0.000	0.000	0.000	9.220	164,382	38,320
No. rollovers	0.032	0.221	0.000	0.000	0.000	0.000	0.000	0.000	3.000	164,382	38,320

Panel B: Mainstream Consumer Credit Market

Bimonthly frequency	Pre-period									no. obs.	no. individ.
	Mean	Std Dev	Min	10pctl	25pctl	50pctl	75pctl	90pctl	Max		
No. credit arrears	0.998	2.960	0.000	0.000	0.000	0.000	0.000	3.000	19.00	53,921	34,902
log(no. credit arrears)	0.300	0.688	0.000	0.000	0.000	0.000	0.000	1.390	3.000	53,921	34,902
Arrear receipt	0.037	0.190	0.000	0.000	0.000	0.000	0.000	0.000	1.000	53,841	34,852
Utilization credit lines (%)	0.035	0.161	0.000	0.000	0.000	0.000	0.000	0.000	1.020	53,921	34,902
log(utilization credit lines)	0.026	0.117	0.000	0.000	0.000	0.000	0.000	0.000	0.701	53,921	34,902
Incr. in credit card utilization	0.023	0.151	0.000	0.000	0.000	0.000	0.000	0.000	1.000	26,812	34,125
Credit card, number	0.130	0.541	0.000	0.000	0.000	0.000	0.000	0.000	4.000	53,921	34,902
Credit card, balance (SEK)	599.0	3,238	0.000	0.000	0.000	0.000	0.000	0.000	31,534	53,921	34,902
Credit card, limit (SEK)	1,160	5,276	0.000	0.000	0.000	0.000	0.000	0.000	45,000	53,921	34,902
Credit card, limit per card											
Having a credit card (SEK)	9,038	5,511	1.000	3,667	5,000	8,000	11,250	15,000	45,000	3,872	2,070
Installment, number	0.029	0.195	0.000	0.000	0.000	0.000	0.000	0.000	2.000	53,921	34,902
Installment, limit (SEK)	774.0	7,754	0.000	0.000	0.000	0.000	0.000	0.000	133,396	53,921	34,902
Credit line, number	0.228	0.545	0.000	0.000	0.000	0.000	0.000	1.000	3.000	53,921	34,902
Credit line, limit (SEK)	2,329	10,872	0.000	0.000	0.000	0.000	0.000	117.0	86,466	53,921	34,902

Panel C: Pawn Credit Market, per County

Monthly frequency, county level, per 100,000 individuals	Pre-period									no. obs.	no. Counties
	Mean	Std Dev	Min	10pctl	25pctl	50pctl	75pctl	90pctl	Max		
No. new loans	315.0	230.0	0.000	52.00	129.0	285.0	469.0	604.0	1,042	560	10
log(no. new loans + 1)	5.250	1.440	0.000	3.970	4.870	5.650	6.150	6.410	6.950	560	10
No. defaults	41.80	95.20	0.000	0.000	0.000	0.000	31.30	123.0	740.0	560	10
No. roll overs	66.70	73.30	0.000	0.000	0.000	44.40	116.0	157.0	460.0	560	10
log(loan size + 1)	11.10	4.46	0.000	0.000	11.40	12.90	13.50	14.00	14.60	560	10

Table 2. County-Level Regressions: Pawn Credit Outcomes

This table shows that increased access to alcohol causally increases pawn credit uptake and the default risk. The table shows the coefficient β_1 from Equation 1:

$$\begin{aligned} CreditUpTake_{county,age,time} = & \beta_1 Treated_c * Eligible_{agegroup} * Post_t + \beta_2 Treated_{county} * Post_{time} \\ & + \beta_3 Eligible_{age} * Post_{time} + \beta_4 Eligible_{age} + \omega_{county} + \omega_{time} + \varepsilon_{county,age,time} \end{aligned}$$

Standard errors are shown in parentheses and additional p-values are computed by using wild bootstrap standard errors clustered at the county level (1,000 replicates). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. new loans	log(no. new loans + 1)	log(loan size + 1)	No. defaults	No. rollover
	(1)	(2)	(3)	(4)	(5)
Eligible \times Post \times Treated	73.54** (32.21)	0.443* (0.237)	0.784* (0.448)	16.93 (27.20)	2.52 (12.14)
p-value (wild bootstrap)	0.034	0.216	0.238	0.400	0.718
Pre-period mean	259.9	259.9	473.7		
Effect	28%	44%	78%		
County FE	Yes	Yes	Yes	Yes	Yes
County x Calendar Quarter F	Yes	Yes	Yes	Yes	Yes
Observations	1,750	1,750	1,750	1,750	1,750
R ²	0.722	0.545	0.842	0.252	0.549
# Counties	10	10	10	10	10

Table 3. Individual Level Regressions: Total Credit Outcomes

This table shows that increased access to alcohol causally increases credit uptake and the default risk. The table shows the coefficient β_1 from Equation 2:

$$\begin{aligned} CreditUpTake_{individual,time} &= \beta_1 treated_i * eligible_{i,t} * post_t + \beta_2 treated_i * post_t + \beta_3 eligible_{i,t} \\ &\quad * post_t + \beta_4 eligible_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{individual,time} \end{aligned}$$

Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	Total Credit (pawn + mainstream)	Total no. of defaults (pawn + arrears)
	(1)	(2)
Eligible \times Post \times Treated	344.2*** (120.7)	0.023*** (0.007)
Pre-period mean	4,971	0.090
Effect	7%	27%
County \times Calendar Month FE	Yes	Yes
Individual FE	Yes	Yes
Observations	261,905	234,719
R ²	0.068	0.031
# Individuals	34,902	34,123

Table 4. Individual Level Regressions: Pawn Credit Outcomes

This table shows that increased access to alcohol causally increases pawn credit uptake and the default risk. The table shows the coefficient β_1 from Equation 2:

$$Credit\ Takeup_{i,t} = \beta_1 treated_i * eligible_{i,t} * post_t + \beta_2 treated_i * post_t + \beta_3 eligible_{i,t} * post_t + \beta_4 eligible_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{i,t}$$

Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. new loans	log(no. new loans + 1)	log(loan size + 1)	No. defaults	No. rollovers
	(1)	(2)	(3)	(4)	(5)
Eligible \times Post \times Treated	0.029** (0.012)	0.016** (0.007)	0.117** (0.059)	-0.005 (0.006)	0.006 (0.004)
Pre-period mean (not logged)	0.119	0.119	203		
Effect	25%	15%	12%		
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	399,178	399,178	399,178	399,178	399,178
R ²	0.007	0.008	0.007	0.025	0.001
# Individuals	38,320	38,320	38,320	38,320	38,320

Table 5. Individual Level Regressions: Mainstream Credit Outcomes

This table shows that increased access to alcohol causally increases consumer credit uptake and risk of delinquency within the mainstream consumer credit market. The table shows the coefficient β_1 from Equation 2:

$$\text{Credit Uptake}_{i,t} = \beta_1 \text{Treated}_i * \text{Eligible}_{i,t} * \text{Post}_t + \beta_2 \text{Treated}_i * \text{Post}_t + \beta_3 \text{Eligible}_{i,t} * \text{Post}_t + \beta_4 \text{Eligible}_{i,t} + \omega_{\text{individual}} + \omega_{\text{time*county}} + \varepsilon_{i,t}$$

Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Mainstream Credit Uptake

Dependent variable:	Credit cards			Installment loans		Credit lines		
	Number	Balance	Limit	Number	Limit	Number	Balance	Limit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible \times Post \times Treated	0.019*** (0.004)	122.0*** (40.6)	233.7*** (58.9)	-0.001 (0.001)	47.3 (61.1)	-0.016** (0.006)	320.0*** (120.2)	336.2*** (119.9)
Preperiod mean	0.183	835.7	1,629			0.317	3,071	3,123
Effect	10%	15%	14%			-5%	10%	11%
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	261,905	261,905	261,905	261,905	261,905	261,905	261,905	261,905
R ²	0.016	0.007	0.018	0.001	0.001	0.017	0.006	0.007
# Individuals	34,902	34,902	34,902	34,902	34,902	34,902	34,902	34,902

Panel B: Credit Card Utilization and Delinquencies

Dependent variable:	Credit card utilization		Receipts (0/1)	Arrears	
	Utilization (%)	log(utilization + 1)		No. receipts	log(no. receipts + 1)
	(1)	(2)		(4)	(5)
Eligible \times Post \times Treated	0.007*** (0.002)	0.006*** (0.001)	0.013*** (0.005)	0.051*** (0.016)	0.003 (0.005)
Preperiod mean (not logged)	0.049	0.049	0.050	1.340	
Effect	15%	13%	26%	3.8%	
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	261,905	261,905	261,748	261,905	261,905
R ²	0.01	0.01	0.01	0.05	0.05
# Individuals	34,902	34,852	34,852	34,902	34,902

Table 6. Inconvenience or Present-Bias?

To investigate whether there is a significant difference between individuals that have more time to buy alcohol during the week, we run our baseline regression (Equation 2) but substitute the eligible dummy with a dummy $\gamma_{i,t}$ for whether the individual is retired (Panel A) or unemployed (Panel B). Because the comparison with the 18 year olds is no longer appropriate, we use an older sample. In Panel A, we use 55–65 year olds to represent non-retirees, and 65–75 year olds to represent retirees. For Panel B, we create our unemployed subsample by taking our sample of 20–65 year olds and determining from our data whether they receive income from work. The table shows the coefficient β_1 from Regression (Equation 3):

$$\begin{aligned} CreditUpTake_{individual,time} = & \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t \\ & + \beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{i,individual,time} \end{aligned}$$

Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Retirees

Dependent variable:	No. new loans (1)	log(no. new loans + 1) (2)	log(loan size + 1) (3)	No. defaults (4)	No. rollovers (5)
Retired \times Post \times Treated	-0.002 (0.008)	-0.001 (0.005)	0.002 (0.039)	-0.003 (0.002)	-0.003 (0.008)
Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	329,310	329,310	329,310	329,310	329,310
R ²	0.003	0.003	0.003	0.009	0.003
# Individuals	21,954	21,954	21,954	21,954	21,954

Panel B: Unemployed Individuals

Dependent variable:	No. new loans (1)	log(no. new loans + 1) (2)	log(loan size + 1) (3)	No. defaults (4)	No. rollovers (5)
Unemployed \times Post \times Treated	-0.010 (0.007)	-0.005 (0.004)	-0.035 (0.033)	0.008*** (0.002)	0.000 (0.005)
Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	1,112,265	1,112,265	1,112,265	1,112,265	1,112,265
R ²	0.004	0.005	0.004	0.014	0.002
# Individuals	85,856	85,856	85,856	85,856	85,856

Table 7. Gender Split

This table shows the results of running our main regression split by gender. Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Pawn Credit Outcomes

Dependent variable:	No. new loans		log(no. new loans + 1)		log(loan size + 1)		No. defaults		No. rollovers	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Eligible \times Post \times Treated	0.034** (0.016)	0.018 (0.016)	0.021** (0.009)	0.007 (0.010)	0.199*** (0.076)	0.016 (0.079)	-0.005 (0.005)	-0.001 (0.005)	-0.004 (0.005)	0.012** (0.005)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	250,291	206,352	250,291	206,352	250,291	206,352	250,291	206,352	250,291	206,352
R ²	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002	0.002
# Individuals	23,829	19,649	23,829	19,649	23,829	19,649	23,829	19,649	23,829	19,649

Panel B: Mainstream Credit Outcomes

Dependent variable:	Credit cards						Installment loans			
	Number		Balance		Limit		Number		Limit	
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Eligible \times Post \times Treated	0.018*** (0.005)	0.011* (0.006)	144.1** (56.32)	61.05 (38.09)	252.6*** (75.29)	137.3*** (47.43)	-0.000 (0.002)	-0.001 (0.002)	112.1 (84.85)	-39.95 (82.35)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	164,287	135,727	164,287	135,727	164,287	135,727	164,287	135,727	164,287	135,727
R ²	0.021	0.015	0.009	0.007	0.021	0.016	0.002	0.002	0.003	0.006
# Individuals	21,714	17,946	21,714	17,946	21,714	17,946	21,714	17,946	21,714	17,946

Panel B: Continued

Dependent variable:	Credit lines					
	Number		Balance		Limit	
	Male	Female	Male	Female	Male	Female
	(11)	(12)	(13)	(14)	(15)	(16)
Eligible \times Post \times Treated	-0.010 (0.009)	-0.012 (0.009)	312.1** (158.9)	61.41 (167.2)	331.6** (159.6)	71.27 (166.5)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	164,287	135,727	164,287	135,727	164,287	135,727
R ²	0.016	0.018	0.008	0.009	0.009	0.009
# Individuals	21,714	17,946	21,714	17,946	21,714	17,946

Table 7. Gender Split (Continued)**Panel C: Mainstream Credit Outcomes**

Dependent variable:	Credit card utilization			
	Utilization (%)		log(utilization + 1)	
	Male	Female	Male	Female
	(1)	(2)	(3)	(4)
Eligible \times Post \times Treated	0.007*** (0.003)	0.005* (0.003)	0.006*** (0.002)	0.004* (0.002)
County \times Calendar Month FE	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes
Observations	164,287	135,727	164,287	135,727
R ²	0.007	0.009	0.008	0.009
# Individuals	21,714	17,946	21,714	17,946

Panel C: Continued

Dependent variable:	Arrears					
	Receipts (0/1)		No. receipts		log(no. receipts + 1)	
	Male	Female	Male	Female	Male	Female
	(5)	(6)	(7)	(8)	(9)	(10)
Eligible \times Post \times Treated	0.006 (0.007)	-0.001 (0.007)	0.037 (0.025)	0.012 (0.019)	0.003 (0.007)	0.002 (0.007)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	164,159	135,653	164,287	135,727	164,287	135,727
R ²	0.010	0.013	0.059	0.054	0.056	0.048
# Individuals	21,684	17,923	21,714	17,946	21,714	17,946

Table 8. Weekly Pattern of Pawn Credit Uptake

This table shows the results of our main regression (Equation 2) where we added a quadruple interaction with a dummy variable that is equal to one if the pawn loan was taken on a Monday and zero otherwise. For this exercise we use our panel on a daily frequency. The data includes borrower-calendar day observations in which we count the number of pawn loans were taken in every calendar day of the week (typically zero or one). Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. new loans				log(no. new loans + 1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible \times Post \times Treated	0.00039*** (0.00010)	0.00024 (0.00020)	0.00036*** (0.00010)	0.00021 (0.00020)	0.00027*** (0.00006)	0.00016 (0.00013)	0.00025*** (0.00007)	0.00013 (0.00020)
Eligible \times Post \times Treated \times Monday			0.00018** (0.00009)	0.00018** (0.00009)			0.00012* (0.00006)	0.00012* (0.00006)
County \times Calendar Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Weekday FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,205,659	16,205,659	16,205,659	16,205,659	16,205,659	16,205,659	16,205,659	16,205,659
R ²	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002	0.0002
# Individuals	38,320	38,320	38,320	38,320	38,320	38,320	38,320	38,320

Appendix Table 1. Placebo Test

This table shows the results of running our main regression test (Equation 1) on a placebo sample in which we shifted the whole experiment one year back in time. We define the pre-period as January 1998 to January 1999 and the post-period as January 1999 to January 2000. Note that the whole experiment falls in a time period when there was no difference in opening hours. Our panel for the other outcome variables does not include earlier years. Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Dependent variable:	No. new loans	log(no. new loans + 1)	log(loan size + 1)	No. defaults	No. rollovers
	(1)	(2)	(3)	(4)	(5)
Eligible \times Post \times Treated	-0.007 (0.011)	-0.003 (0.007)	-0.020 (0.054)	-0.003 (0.002)	0.004 (0.003)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	331,938	331,938	331,938	331,938	331,938
R ²	0.004	0.004	0.004	0.024	0.001
# Individuals	38,344	38,344	38,344	38,344	38,344

Appendix Table 2. Border County Exclusion

This table shows the results of running our main test (Equation 2) but excluding the county that borders Denmark (Skåne), which has a more liberal alcohol sales environment (Norway has an equally strict environment). Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Pawn Credit Outcomes

Dependent variable:	No. new loans	log(no. new loans + 1)	log(loan size + 1)	No. defaults	No. rollovers
	(1)	(2)	(3)	(4)	(5)
Eligible \times Post \times Treated	0.030** (0.013)	0.017** (0.008)	0.122* (0.064)	-0.007 (0.006)	0.010*** (0.004)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	324,195	324,195	324,195	324,195	324,195
R ²	0.007	0.008	0.007	0.031	0.001
# Individuals	31,145	31,145	31,145	31,145	31,145

Panel B: Mainstream Credit Outcomes

Dependent variable:	Credit cards			Installment loans		Credit lines		
	Number	Balance	Limit	Number	Limit	Number	Balance	Limit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Eligible \times Post \times Treated	0.022*** (0.004)	143.1*** (43.9)	279.9*** (62.3)	-0.000 (0.001)	22.1 (61.6)	-0.020** (0.008)	371.1*** (143.3)	392.4*** (143.8)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	212,320	212,320	212,320	212,320	212,320	212,320	212,320	212,320
R ²	0.017	0.007	0.018	0.039	0.002	0.019	0.008	0.008
# Individuals	28,342	28,342	28,342	28,342	28,342	28,342	28,342	28,342

Panel C: Mainstream Credit Outcomes

Dependent variable:	Credit card utilization		Arrears		
	(%)	1)	(0/1)	receipts	1)
	(1)	(2)	(3)	(4)	(5)
Eligible \times Post \times Treated	0.008*** (0.002)	0.006*** (0.001)	0.015*** (0.005)	0.035** (0.018)	0.002 (0.005)
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	212,320	212,320	212,183	212,320	212,320
R ²	0.007	0.008	0.009	0.041	0.042
# Individuals	38,342	38,342	28,295	28,342	28,336

Appendix Table 3. Buffer Counties Included in the Control Group

This table shows the results of running our main regression test (Equation 2) and including the buffer counties in the control group. Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Pawn Credit Outcomes

Dependent variable:	No. new loans (1)	log(no. new loans + 1) (2)	log(loan size + 1) (3)	No. defaults (4)	No. rollovers (5)
Eligible \times Post \times Treated	0.027** (0.011)	0.015** (0.007)	0.116** (0.055)	-0.003 (0.003)	0.003 (0.003)
Preperiod mean (not logged)	0.119	0.119	203.0		
Effect	23%	20%	12%		
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	456,643	456,643	456,643	456,643	456,643
R ²	0.002	0.002	0.002	0.009	0.001
# Individuals	43,478	43,478	43,478	43,478	43,478

Panel B: Mainstream Credit Outcomes

Dependent variable:	Credit cards			Installment loans		Credit lines		
	Number (1)	Balance (2)	Limit (3)	Number (4)	Limit (5)	Number (6)	Balance (7)	Limit (8)
Eligible \times Post \times Treated	0.014*** (0.004)	105.3*** (35.4)	198.0*** (46.7)	-0.001 (0.001)	31.2 (60.5)	-0.011* (0.007)	193.7* (115.7)	206.5* (116.2)
Preperiod mean	0.183	832.9	1,622			0.317	3,080	3,135
Effect	8%	13%	12%			-4%	6%	7%
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	300,014	300,014	300,014	300,014	300,014	300,014	300,014	300,014
R ²	0.017	0.007	0.018	0.001	0.002	0.016	0.007	0.008
# Individuals	39,660	39,660	39,660	39,660	39,660	39,660	39,660	39,660

Panel C: Mainstream Credit Outcomes (Continued)

Dependent variable:	Credit card utilization		Arrears		
	Utilization (%) (1)	log(utilization + 1) (2)	(0/1) (3)	receipts (4)	1) (5)
Eligible \times Post \times Treated	0.006*** (0.002)	0.005*** (0.001)	0.003 (0.005)	0.024 (0.016)	0.003 (0.005)
Preperiod mean (not logged)	0.05	0.05			
Effect	13%	13%			
County \times Calendar Month FE	Yes	Yes	Yes	Yes	Yes
Person FE	Yes	Yes	Yes	Yes	Yes
Observations	300,014	300,014	299,812	300,014	300,014
R ²	0.007	0.008	0.010	0.054	0.050
# Individuals	39,660	39,660	39,607	39,660	39,660

Appendix Table 4. Sensitivity to Eligible Age Cut-Off

This table shows the results of running our main regression (Equation 2) and gradually increasing the age cut off of our treatment group (i.e., those eligible to buy alcohol who live in the treated and control counties). Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Pawn Credit Outcomes

Age Eligible	No. new loans (1)	log(no. new loans + 1) (2)	No. defaults (3)	log(loan value + 1) (4)	No. rollovers (5)
20–21	0.033**	0.018**	–0.005	0.143**	0.006
20–22	0.031**	0.017**	–0.005	0.133**	0.005
20–23	0.029**	0.016**	–0.006	0.121**	0.005
20–24	0.032***	0.017**	–0.005	0.128**	0.004
20–25	0.029**	0.016**	–0.005	0.117**	0.006
20–26	0.028**	0.015**	–0.005	0.109*	0.005
20–27	0.027**	0.015**	–0.005	0.101*	0.006

Panel B: Mainstream Credit Outcomes

Age Eligible	Credit cards			Installment loans		Credit lines		
	Number (1)	Balance (2)	Limit (3)	Number (4)	Limit (5)	Number (6)	Balance (7)	Limit (8)
20–21	0.022***	150.8***	226.0***	–0.002	19.1	–0.0069	143.5**	149.8***
20–22	0.025***	158.5***	246.9***	–0.001	20.3	–0.0107	242.5***	250.2***
20–23	0.023***	155.3***	255.6***	0.000	82.9	–0.013*	257.8***	269.0***
20–24	0.021***	133.8***	245.5***	–0.000	91.4	–0.012*	314.2***	329.4***
20–25	0.019***	124.9***	235.0***	–0.001	50.0	–0.015**	316.2***	331.7***
20–26	0.014***	103.0**	193.9***	–0.001	1.9	–0.017***	288.3**	305.4***
20–27	0.013***	105.4***	191.7***	–0.002	–20.9	–0.016***	281.3**	297.8**

Panel C: Mainstream Credit Outcomes (Continued)

Age Eligible	Credit card utilization		Arrears		
	Utilization	log(utilization + 1) (2)	Receipts (0/1)	No. receipts	log(no. receipts + 1)
	(%) (1)		(3)	(4)	(5)
20–21	0.010***	0.007***	0.003	0.047***	0.012**
20–22	0.010***	0.007***	0.007	0.050***	0.009*
20–23	0.009***	0.006***	0.009*	0.045***	0.005
20–24	0.008***	0.006***	0.012**	0.042***	0.004
20–25	0.008***	0.006***	0.013***	0.044***	0.003
20–26	0.007***	0.005***	0.013***	0.045***	0.001
20–27	0.007***	0.005***	0.012***	0.048***	0.001

Appendix Table 5. Sensitivity to Clustering at Higher Levels

This table shows the results of running our main regression (Equation 2) and clustering the error terms first at the individual level, as we do in our baseline regressions (row one); second, at the parish level (row two); and lastly, at the municipality level (row 3). *, **, and *** represent the 10%, 5%, and 1% significance level, respectively.

Panel A: Pawn Credit Outcomes

Dependent variable:	No. new loans	log(no. new loans + 1)	log(loan size + 1)	No. defaults	No. rollovers
	(1)	(2)	(3)	(4)	(5)
Clustered err. individual level	0.029** (0.012)	0.016** (0.007)	0.117** (0.059)	-0.005 (0.006)	0.006 (0.004)
# cluster	38,320	38,320	38,320	38,320	38,320
Clustered err. parish level	0.029*** (0.001)	0.016*** (0.005)	0.117*** (0.040)	-0.005 (0.005)	0.006 (0.005)
# cluster	1,069	1,069	1,069	1,069	1,069
Clustered err. municipality level	0.029*** (0.009)	0.016*** (0.006)	0.117** (0.052)	-0.005 (0.005)	0.006 (0.005)
# cluster	293	293	293	293	293

Panel B: Mainstream Credit Outcomes

Dependent variable:	Credit cards			Installment loans		Credit lines		
	Number	Balance	Limit	Number	Limit	Number	Balance	Limit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Clustered err. individual level	0.020*** (0.000)	122.0*** (40.6)	233.7*** (58.9)	-0.000 (0.000)	47.3 (61.1)	-0.020** (0.010)	320.0*** (120.2)	336.3*** (119.9)
# cluster	34,902	34,852	34,125	34,852	35,191	34,902	34,854	34,902
Clustered err. parish level	0.020*** (0.010)	122.0** (51.2)	233.7*** (75.1)	-0.000 (0.000)	47.3 (88.1)	-0.020* (0.010)	320.0** (147.5)	336.3** (150.4)
# cluster	1,068	1,068	1,068	1,068	1,068	1,068	1,068	1,068
Clustered err. municipality level	0.020*** (0.010)	122.0*** (49.1)	233.7*** (67.6)	-0.000 (0.000)	47.3 (87.8)	-0.020** (0.010)	320.0** (122.7)	336.3*** (127.6)
# cluster	292	292	292	292	292	292	292	292

Panel C: Mainstream Credit Outcomes (Continued)

Dependent variable:	Credit card utilization		Arrears		
	Utilization (%)	log(utilization + 1)	Receipts (0/1)	No. receipts	log(no. receipts + 1)
	(1)	(2)	(3)	(4)	(5)
Clustered err. individual level	0.007*** (0.002)	0.006*** (0.001)	0.013*** (0.005)	0.051*** (0.016)	0.003 (0.005)
# cluster	34,902	34,852	34,852	34,902	34,902
Clustered err. parish level	0.007*** (0.002)	0.006*** (0.002)	0.013** (0.006)	0.047 (0.038)	0.003 (0.006)
# cluster	1,068	1,068	1,068	1,068	1,068
Clustered err. municipality level	0.007*** (0.003)	0.006*** (0.002)	0.013** (0.006)	0.047 (0.040)	0.003 (0.005)
# cluster	292	292	292	292	292