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

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**Abstract**

Increased availability of alcohol might harm individuals if they have time-inconsistent preferences and consume more 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 serve as commitment devices, expanded operating hours led to higher alcohol consumption and greater consumer credit demand, default, and negative consequences in the labor market. Our calculation shows that the effects of alcohol consumption on indebtedness could amount to 3.2 times the expenditure on alcohol.

*Keywords*: household finance, behavioral finance, time-inconsistent preferences, commitment mechanisms, alcohol, consumer credit

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# 1 Introduction

Individuals have time-inconsistent preferences ("present bias") if they change their prior consumption plan as time passes in a systematic manner (e.g., Shefrin and Thaler 1981, Laibson 1997, Hoch and Loewenstein 1991, Loewenstein and Prelec 1992, O'Donogheu and Rabin 1999a, 1999b, 2000). For example, a person with present bias might plan to skip eating dessert, but changes his mind once he sees the menu. In its core, present bias means that people overvalue the present experience, and therefore could help understanding he widespread phenomenon of excessive borrowing, since unplanned consumption today must come at the expense of consumption tomorrow. Furthermore, certain consumption items, alcohol included, have a spillover "multiplier effect" in which their consumption might cause further consequences down the road due to poor decision making. In the case of alcohol, consumption could lead to higher likelihood of road accidents (Wagenaar, Murray, and Toomey 2000; Levitt and Porter 2001) or job loss (Mullahy and 1996). While the relation between unplanned consumption and myopia are central to understanding the key financial behaviors by households, there is little evidence from the field demonstrating the effects of impulsive consumption on financial wellbeing.

In this study we focus on the effects of impulsive consumption of alcohol. We analyze the results of a national experiment in Sweden, where offsite liquor stores in some counties extended their operation hours into the weekend. Previous research have documented that in the treated counties offsite alcohol sales increased (Nordström and Skog 2003), possibly showing that some households are myopic and engage in unplanned consumption of alcohol. Our study documents that the increase in alcohol consumption was harmful to individuals' financial wellbeing through higher indebtedness, greater likelihood of default, as well as negative consequences in the labor market. The implication of our results is that households did not substitute current offsite alcohol consumption with other current consumption, but rather borrowed from future consumption through increased indebtedness. Furthermore, alcohol consumption has a spillover effect to other areas (e.g., labor) beyond the mechanical increase in expenditure.

In the experiment we analyze regulated retail liquor stores in only some counties in Sweden started operating on Saturdays in February 2000. Our identifying assumption is that rational individuals can plan ahead their consumption and shop when the store is open; for these customers, operating hours do not affect consumption patterns (similar assumption is made in other studies studying blue laws, e.g., Bernheim, Meer, and Novarro 2016, Hinnosaar 2016). In contrast, present-biased individuals underestimate their future demand for alcohol and thus consumption depends on opening hours. If the store is open, they consume alcohol; if it is closed, they cannot and therefore do not consume. For these individuals, the exogenous increase in the availability of alcohol is a relaxation of a commitment device that previously prevented them from impulsive consumption of alcohol on the weekends.

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. In February 2000, the government initiated an experiment 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 3.7–4.0% on average (Nordström and Skog 2005, Grönqvist and Niknami 2014). This translates into an average annual increase of alcohol consumption of approximately 100 SEK (11 USD). Because early evaluations of the trial initially did not reveal negative health or crime consequences, in July 2001, the Saturday extended opening hours were implemented throughout Sweden.

Our empirical strategy is based on both double and triple differences (DD and DDD, respectively). In a double-difference setting, we compare consumers in the counties with increased access to alcohol to consumers in another set of counties without increased access. One concern with such analysis is that our results could be confounded by unobserved differences between people who choose to live in the various counties, or by county-level trends. Therefore, we employ also a second strategy which focuses on a triple-difference specification. In this strategy, we compare two groups of young people (18-19 year olds and 20-25 year olds) within the treatment counties, across counties, and across time. The idea is to exploit the fact that while people in the 18-19 group are not allowed to buy alcohol anywhere in Sweden, they are still allowed to borrow.

We begin the analysis with examining the change in total credit balances (pawn + credit cards). We document that these balances increased on average by 345 SEK (about 41 USD), which reflects an increase of 6.9% relative to pre-experiment mean. Default on all credit instrument increase by 2.3 percentage points, which reflects a relative increase of 27%.

Next, we turn to exploring the specific credit markets: pawn credit market and mainstream credit market. In the pawn market we observe that expanding the operating hours increased the number of new pawn loans by about 20%, and increased loan sizes by 16%. We test also for loan performance, but find only mixed results, whereas the double-diff results indicate poorer performance following the experiment, but the triple-diff approach does not detect a statistically significant effect.

In the mainstream credit market, we explore the effects on credit cards, installment loans (used to purchase large retail items), and credit lines. We document a statistically and economically increase of 11% in the number of credit cards issued as well as an increase in credit card balances of 15%. We find no effect on installment loans, either in the number of loans or balances. There is no increase in the number of credit lines issued, however, we find that balances increased by 11% on average. Importantly, we document a significant deterioration in credit performance, where default increased by 28%.

A key result is that the increase in individuals' indebtedness is not only a result of the mechanical increase in alcohol spending, but is rather reflects a further net expenditure. To understand better the indirect effect of alcohol consumption on the financial wellbeing of individuals, we explore effects in the labor market. We obtain annual tax records, and match them to people in our main sample. We test whether the greater availability of alcohol caused individuals to lose their jobs, or earn lower income. Indeed, individuals in the treated areas were less likely to be employed in the year following the experiment period, and their wages and income were lower. Importantly, we find a correlation between these negative employment effects and indebtedness. It is plausible that this correlation reflects the indirect effects of alcohol consumption, i.e., greater alcohol consumption leads to job loss, which leads to higher demand for credit.

To understand the impact of alcohol consumption on the financials of consumers we make a rough estimation comparing the increase in indebtedness and the increase in alcohol expenditure. Specifically, we compare the increase in total balances (345 SEK) to the calculated increase in direct alcohol consumption (112 SEK), and recover a multiplier effect of 3.2 (345/112), meaning that every Swedish Kronor spent on alcohol translates to higher indebtedness of 3.2 Kronor. The fact that credit balances increased by more than the direct consumption indicates that consumers experienced spillover effects of alcohol consumption on other areas. This could be caused by mundane reasons of complementary consumption to alcohol (e.g., Swedish meatballs), or a result of poor decision making on other dimensions. For example, as discussed above, researchers found that alcohol consumption increases the likelihood of road accidents, injuries, or job loss. To our best knowledge, the only study that quantifies the multiplier effect in this literature is Schilbach (2014) who reports a multiplier of about 2 in the context of Indian cab drivers.

To further establish the causal link between the increase availability of alcohol over weekends and indebtedness, we examine the timing of pawn borrowing at high resolution. Specifically, impromptu alcohol purchases on the weekend should result in frequent liquidity shortage on the weekend and thus higher rate of pawn borrowing on Monday (pawn shops are closed on weekends). 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 on Mondays. This result also supports the idea that alcohol purchases are impulsive rather than planned, otherwise, there would not have been a sharp increase in pawn borrowing following the weekend.

We also explore some alternative explanations for our results. First, we investigate whether the results driven by a few outliers (alcoholics), or whether the increase in the demand for credit is wide spread across consumers. Our analysis of the distribution of the credit increase amounts indicates that the demand for credit following the experiment was widespread and relatively smooth across individuals. Second, we examine the concern that the increase in alcohol coinsumption was driven by latent demand by busy people who could not shop during the week prior to the experiment. We test whether this is the case by comparing the indebtedness of people who have more time at hand (retirees, unemployed) to more employed people in close age groups. We find no differential effect between the groups, supporting the idea that the effects are not driven by time constraints, i.e., convenience shopping.

Overall, our findings indicate that greater availability of alcohol led to a deterioration in consumers' financial wellbeing. The effect is driven by both direct and indirect effects of alcohol consumption, exposing individuals to spillover effects such as greater default probability and lower wage income. Our results indicate that the restriction of alcohol stores' operating hours serves a commitment device for present-biased consumers.

We contribute to the literature on three fronts. First, we present evidence for the causal link between the increase in unplanned consumption, specifically alcohol, and the effects on financial wellbeing. The causal link between alcohol and savings has been previously studied in psychology and economics. In psychology, researchers explored the psychological constructs that allow alcohol to generate myopic behavior (e.g., Steele and Josephs 1990). In economics, alcohol is considered a temptation good, where its availability triggers unplanned consumption and distorts consequent decision making. Banerjee and Mullainathan (2010) and Bernheim, Ray, and Yeltekin (2015) differentiate between normal goods and temptation goods (e.g., alcohol, sugary and fatty foods). They argue that temptation goods are especially detrimental for the poor as it takes up a large fraction of their disposable income. Our study presents supportive evidence that indeed the increase in the availability of alcohol triggers consumption, however, we cannot compare the effects of alcohol to those of other goods. Importantly, our study provides new evidence from the field about the multiplier effect of alcohol consumption, specifically on the labor market. While many previous studies have shown that alcohol impair decision making, and in particular reduce productivity (e.g., Blum, Roman, and Martin 1993, Jones, Casswell, and Zhang 1995, Fisher, Hoffman, Austin-Lane, and Kao 2000, McFarlin and Fals-Stewart 2002), identifying a causal relationship is always a challenge.

Our study is complementary to Schilbach (2014). In his study, Schilbach performs a field experiment in India, in which he varies the propensity of Indian cab drivers to consume alcohol through incentives. Schilbach documents that those who drink alcohol save less, to a degree that is twice as large as the mere costs of alcohol, suggesting that consuming alcohol has a multiplier effect of two. Compared with Schilbach's carefully-controlled, our experiment provides large-scale causal evidence from the real world at the cost of looser experimental control. The two studies are also complementary because they explore similar effects in different environments (Indian cab drivers versus Swedish low-income population). Both studies provide evidence for and quantify the indirect effect of consumption of alcohol on financial behaviors, although in completely different empirical settings.

Second, our results also shed light on the causal relation between present bias and financial behavior, where previous literature found mixed evidence about the nature of this correlation. 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. See Schilbach, Schofield, and Mullainathan (2016) for a review of the literature relating poverty and behavioral biases. We contribute to this literature by providing empirical real-world evidence that greater access to alcohol leads to an increase in the demand for alternative and mainstream credit and defaults, as well as to the indirect consequences of alcohol.

Third, our study adds to the debate in the literature about the effectiveness of commitment devices. Researchers have proposed that commitment mechanisms might help individuals stick to their planned consumption path (e.g., Laibson 1997, Thaler and Benartzi 2004), and Hinnosaar (2015) proposes that limited store operating hours can serve as such commitment mechanism. However, prior studies found conflicting evidence about the effectiveness of restricting consumer access to temptation goods. Some studies find that store opening hours help reducing impulsive consumption (fast food: Currie, DellaVigna, Moretti, and Pathania 2010, alcohol: Norström and Skog 2003, 2005). 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. Moreover, the effectiveness of self-enforced pre-commitment devices is questionable. DellaVigna and Malmendier (2006) find that gym memberships, which serve as precommitment devices for exercising, are not effective as such.

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 Theoretical 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),

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

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:

$$\frac{\partial U_0}{\partial C_1} = \beta\delta \; and \; \frac{\partial U_0}{\partial C_2} = \beta\delta^2 \;\Rightarrow\; MRS_{c_1,c_2} = {}^{\beta\delta^2}\!/_{\beta\delta} = \delta,$$

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

$$\frac{\partial U_1}{\partial C_1} = 1 \; and \; \frac{\partial U_1}{\partial C_2} = \beta\delta \;\Rightarrow\; MRS_{c_1,c_2} = {}^{\beta\delta}\!/_{1} = \beta\delta.$$

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 Limited Opening Hours and the Effect on Financial Wellbeing

The effects of expanding operating hours of liquor stores into weekends should have differential effects on consumers according to their ability to make plans and sustain them. Since alcohol can be stored at home at low cost and people generally buy alcohol frequently, then 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).

In contrast, when consumers have present-biased preferences, limited opening hours can function as a commitment device that helps consumers stick to their planned consumption path. Imagine that you plan *not to drink* tomorrow. Whether you are unbiased or have present-biased time preferences, you will not buy additional alcohol today, since you are not planning to drink tomorrow. But when tomorrow comes, the behaviors of the two types bifurcate. If you have unbiased preferences (consistent over time), you will not change your mind and thus will follow your plan not to drink, independent of whether stores are open or closed. If, however, 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. Thus, a closed store would then function as a commitment device that helps you stick to your plan not to drink. In other words, if we observe an increase in alcohol consumption (not due to substitution of onsite drinking), then it might be indicative of present-biased consumers shopping and a commitment device being released.

Impromptu consumption of alcohol could have both direct and indirect effects on consumers. The direct channel is through the budget constraint: spending money today that otherwise would have been used in the future. For liquidity constrained consumers, there could be also an effect on borrowing, as they might need to borrow to finance everyday expenses, such as their grocery shopping or electricity bill later in the week. Furthermore, greater borrowing may lead to higher likelihood of financial distress or default in the future.

The indirect effects of greater alcohol consumption can come through effects on other consumption and non-consumption decisions that people make. On the consumption side, 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. On non-consumption decisions, alcohol could lead to lower net income due to bad decisions. For example, alcohol consumption may increase the likelihood of road accident and injury (Wagenaar, Murray, and Toomey 2000, Levitt and Poter 2001). Additionally, alcohol consumption may lead to poor performance at the workplace, which may lead to firing or lower career prospects, feeding back to financial wellbeing.

## 3 Background: A Swedish Nationwide Experiment

### 3.1 Swedish Alcohol Market

To understand the economic context of the experiment that we analyze and evaluate whether the results are externally valid outside Sweden, we first characterize the Swedish alcohol market. In Figure 1a, we present alcohol consumption trends over time for the Nordic countries (Sweden, Denmark, Norway, and Finland), as well as the United Kingdom and the United States. The chart shows that, if anything, alcohol consumption levels by Swedish are relatively low. In 1999, right before the experiment, Swedes consumed an average of 6 liters per capita, compared to 8 in the United States Among these six countries, Sweden is ranked fifth, between the U.S. (fourth) and Norway (sixth). Hence, alcohol consumption rates are not unusual to western countries. Next, we explore the cross-section of Swedish households.

Since our analysis focuses individuals who come from lower income levels we explore how the share of alcohol expenditures changes over the income distribution in Figure 1b. For this purpose, we obtained expenditure data from Statistics Sweden for the period of our experiment (1999-2001).[1] The results show that Swedish households spend, on average, 1.6% to 2.0% of their

[1] Statistics Sweden conducted the survey Utgiftsbarometern during 1995–2001 and 2002-2009. The data was gathered by administering cash journals to randomly selected households that after an over-the-phone introduction bookkept their expenditure during a two-week period. Statistics Sweden also complemented the cash-journal data with a survey for bigger expenditures covering longer time periods. Disposable income is computed by data from public registries and is used to balance the selection into the sample to achieve a representative sample of the total population. They contacted 3,000 individuals of age 0–74 per year (thus 9,000 in total). 4,688 households participated. Expenditures are rescaled to annual level and we use data from 1999–2001 covering 4,688 households.

disposable income on alcohol. Within this range, lower households at the lower quintile of income spend about 2.0% of their disposable income, while higher-income households spend 1.6%-1.7% of their disposable income on alcohol. To assess what fraction of people actually consume alcohol within each income decile, we plot the fraction of abstainers (measured as people who did not consume alcohol over a year) in Figure 1c. The figure shows that the fraction of abstainers is roughly 60% for the lowest income decile, and declines steadily to 40% for the top income decile. This means that the average effect that we measure in our estimates attribute to an entire group of individuals, is likely to be driven by about 60% of individuals who actually consume alcohol. Also, we examine whether the composition of alcohol types vary across income deciles. Figure 1d presents a breakdown of the consumption by alcohol type (liquor, wine, and beer) by income decile, from a later wave of the expenditure data conducted between the years 2003-2009.[2] The figure shows that the rate expenditure on liquor is constant across income deciles. There is substitution from beer to wine as income decile increases.

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).

[2] In 2003 the first wave of the survey Hushållens utgifter (HUT) was initiated. The target group was individuals in ages 0–79 and selected as in Utgiftsbarometern. Statistics Sweden contacted 4,000 people. During two weeks participants bookkept expenditures in a cash journal, and then filled out a survey and answered questions in a phone interview. HUT coded expenditure categories differently than Utgiftsbarometern. We study data for 2003–2007 where 10,895 persons participated out of 20,000 approached.

### 3.2 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.

### 3.3 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 credits depend on market interest rates as well factors like the borrower's creditworthiness, default risk, the bank's financing costs, and the competition with other credit institutions. The banks' average deposit and lending rates have shown a clear downward trend since the early 1990s.[3]

[3]Source: http://www.swedishbankers.se, Banks in Sweden.

## 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 2 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 3.7–4.0%. 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 alcohol 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

Our goal is to identify the causal effects of impulsive consumption on financial wellbeing. A simple correlation between alcohol consumption and financial wellbeing would likely suffer from both reverse causality and omitted variable bias.[4] An ideal experiment to identify this causal effect, therefore, would consider two identical groups of individuals where the access to alcohol would increase for one group more than the other.

We use the variation in alcohol availability induced by the February 2000 policy change in Sweden in two empirical approaches. The first empirical strategy is based on a diff-in-diff analysis which compares credit behavior *before and after* the policy change and *across* treated and control counties.

The second approach also compares pre- and post-experiment behavior, as well as cross-county variation, but it exploits also differences in response to the treatment *within* the treated counties. This approach addresses the concern that individuals who live in different counties are likely to differ in ways that may be correlated with credit market outcomes, thus a comparison of

[4] 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).

the credit behavior of individuals who live in treated and control counties could potentially be biased. For this reason, our second 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.

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 evidence pre-trend analysis showing that indeed, in the pre-period there is little difference in the behavior across treatment and control counties.

Throughout the study we exclude the buffer counties to mitigate cross-county shopping. In the Appendix we verify that our results do not materially change by including the buffer counties to the control group (Appendix Table A3), or by excluding the county bordering with Denmark (Skåne), which allows easy cross-country shopping (Appendix Table A4). We also exclude people who move (about 1.6% of individuals) to avoid capturing strategic behavior focused on having greater access to alcohol.

# 4 Data and Summary Statistics

## 4.1 Data

The population that we use in the study includes nearly all individuals who borrowed from pawn shops in the 14 years between 1999 and 2012. 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 bi-monthly 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.

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 bi-monthly snapshots of individual credit records from 1999 to 2001.The first empirical strategy (double-diff) is comparing people in treated counties to those in control counties, pre- and post the experiment. We have bi-monthly data for 61,527 individuals in the control counties and 102,855 individuals in the treatment counties. For the second empirical strategy (triple-diff), we add an age restriction where individuals enter the sample when they are 18, and leave the sample when they are 25. This sample results with a total of 38,320 individuals.

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 especially for the young population (18–25). 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. To remove concerns, we devise a test that is free of such a look-ahead bias (discusses in Section 7.1).

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.

We also measure labor market outcomes; we match the credit registry data with information obtained from the tax authorities through Statistics Sweden (SCB). These data are at the yearly level from 1998 to 2005 and include information on each individual's employment status. We start with a dummy that equals one if the individual received any income from work; I(Wages>0). Furthe more, we use a log transformation of our income measures, total after tax income and income from work, which are in units of hundreds of Swedish Kronor (SEK), as the outcome variable in our regression tests. Total after tax income includes, besides income from work, income from capital and social transfers.

### 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. We also examine consolidated credit outcomes, the summary statistics of which are presented in Panel C. For the analysis examining labor market outcomes, we focus on the availability of wages, the amount of wage income, and total income. The summary statistics for these variables are presented in Panel D. Panel E shows pawn credit market outcomes, at the county level per 100,000 residents. Appendix Table A1 presents comparative summary statistics for the sample employed in our baseline analysis in the pre-treatment period. In Section 5.1 we provide formal pre-trend tests.

# 5 Main Results

## 5.1 Graphical Evidence and Pre-Trends Analysis

We begin by showing graphically the event-time evolution of the average outcomes, which provides evidence in support of our identification assumption. The identification assumption is that, in the absence of a policy change, the difference in the probability to participate in the credit market between consumers living in the treatment and control counties would have evolved in parallel between eligible and ineligible consumers. In Figures 3, 4, and 5 we follow (Brown, Grigsby, van der Klaauw, Wen, and Zafar 2016) and depict the estimates of the $\beta_\tau$ coefficients and their 95-percent confidence intervals for debt-related outcomes. For example, Figure 3 uses the following model:

$$y_i = \sum_{\tau=-5}^{8} \left(\beta_\tau \, Eligible_{i,t} * Period_t * Treated_i\right) + \xi_1 Treated_i * Eligible_{i,t} \qquad (1)$$
$$+ \xi_2 Eligible_{i,t} * Post_t + \xi_3 Eligible_{i,t} + \omega_i + \omega_{time*county} + \varepsilon_{i,t},$$

where $\beta_\tau$ captures the difference between consumers that are eligible to buy alcohol living in a treated county and everyone else, for each period (calendar time bi-monthly observations). The regression equations for other figures are adjusted to the number of periods available. In addition we perform a Wald test to check the null-hypothesis that the $\beta_\tau$ coefficients in the pre-period are jointly equal (i.e., $\beta_{-5} = \beta_{-4} = \cdots = \beta_{-1}$). Thus, the Wald-test formally tests if the parallel growth assumption holds. The *p*-values of this test are reported in the boxes of the respective panels of Figures 3, 4 and 5. In these boxes we report the average difference as well as the baseline difference in difference between consumers that are eligible and ineligible to buy alcohol living in a treated versus control counties, between the pre and post period. Mapping into our triple-difference baseline findings reported in Tables 4, 5, and 6, respectively.

In support of our parallel growth assumption for the difference between eligible and ineligible in the treatment and control counties we note that the estimates of the pretreatment coefficients ($\sum_{\tau=-5}^{-1} \beta_\tau$) are basically jointly equal in most cases (i.e., with $p$-value > 0.05 we cannot reject the null). Furthermore, the figures show that the post-treatment estimates are different from the pre-treatment estimates for most outcomes. In the respective boxes within the panels we report the average differences ($\frac{1}{9}\sum_{\tau=0}^{8} \beta_\tau - \frac{1}{5}\sum_{\tau=-5}^{-1} \beta_\tau$) and the corresponding baseline differences which refers to the coefficient $\beta_{-1}$ in Figures 3 and 4, and to the coefficient $\beta_0$ in Figure 5 (where the frequency is annual), from Equation (1) above.

### 5.3 Increases in Total Credit and Default Probability

We next turn to our main panel regression specification so that we can take into account individual fixed effects and exploit additional mainstream credit outcomes:

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

And again the double difference specification omits Eligible and runs the regression on the sample aged eighteen to twenty-five. The coefficient $\beta_1$, which is our main outcome, measures the differential likelihood of taking out credit between the eligible and ineligible age groups in the treated and control counties during the pre- and post-periods.

Table 2 documents that an increase in the access to alcohol increased the demand for total credit and an increase in the likelihood of default. Columns (1)-(2) present results from double-diff regressions and Columns (3)-(4) show results from the triple-diff specification. In Column (1) we first regress the total credit balances (pawn and mainstream registries) on the interaction between post-period, and treated dummy and in Column (3) we add our third interaction our eligible dummy. In Column (3) the coefficient measures the average increase in credit balances of 345 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. The coefficient in Column (1) is larger, but potentially influenced by county trends. In Columns (2) and (4) we document an increase in the total number of defaults recorded in the pawn and mainstream credit registries. Column (4) shows that 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, installment loans, credit lines).

### 5.4 Demand for Pawn Credit

Next, we analyze the demand for pawn credit. Table 3, Columns (1) to (5) present regression results from double-diff specification, whereas Columns (6) to (10) report the corresponding results from triple-diff specification. Columns (1), (2), (6), and (7) show that more pawn loans were issued in treated counties following the experiment, and in particular when comparing to non-eligible borrowers within the treated counties. Columns (6) and (7) indicate that in the treatment group, individuals older than 19 are 2.6 percentage points more likely to take out a pawn loan (22% increase relative to the pre-period mean take-up rate). Furthermore, pawn loan size increased by nearly 16%[5] for this group (Column (8)). Given that the average size of a pawn loan is 174 SEK, the nominal increase in pawn loan size is of 28 SEK.

When examining default and rollover rates, we find mixed results across specifications. While the triple-difference regressions show no significant effect on defaults and rollovers, there are significant increases for both default and rollover rates. The difference in the results suggests that the effects in the double-diff regressions may be driven by county effects potentially unrelated to the alcohol experiment.

[5] We calculate the relative effect for log +1 outcome variables in the following way: beta*(pre-mean treated x + 1)/(pre-mean x)

### 5.5 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. In Table 4, Columns (1) to (8) present regression results from double-diff specification, and Columns (9) to (16) report the corresponding results from triple-diff specification. Columns (1), (2), (9), and (10) 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 (7), based on the triple interaction, for example, the probability of taking an additional credit card increases by 1.9 percentage points (about 11% relative to the pre-period mean). Also, the average balance of the treated population increased by 127 SEK (equivalent to 15 USD, Column (10)).

In Columns (3), (4), (11), and (12) we examine the effects on installment loans. Installment loans are essentially credit provided when purchasing larger items, like the famous Billy bookcase and Dombås wardrobe, sold at IKEA stores. 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. There is a small effect to the limit of installment loans in the double-diff specification, however, again, it might be driven by unrelated county trends.

In Columns (5), (6), (13), and (14), we explore the effects of Saturday liquor store opening hours on credit lines. The triple-diff regressions show that the number of personal credit cards decreases by 1.4 percentage points (about 5% decrease relative to the pre-period mean), but the balance of existing credit lines increases by 326 SEK (about 11% increase relative to pre-period mean).

In the remaining columns of Table 4 (Columns (7), (8), (15), and (16)), we investigate how expanded access to alcohol affects the performance of borrowers in the mainstream credit market. In Sweden, defaults on any type of mainstream credit are recorded as an arrear flag on a person's credit file. Column (7) (double-diff) shows that in there is an increase in the number of defaults (called arrear receipts in Sweden) opened, however, Column (15) (triple-diff) shows no significant result suggesting that the effect might be driven by county trends. In Columns (8) and (16), we regress an indicator variable as to whether there is an arrear receipt flag on file. Both specifications show an increase in the likelihood. In Column (16), triple-diff specification, the results show that following the Saturday hours experiment, the spread in the likelihood of having arrear receipts for the treated population increased by 1.4 percentage points, which is equivalent to about a 28% increase over the pre-period mean. Overall, these results are in line with our findings for the pawn credit market.

### 5.6 Labor Market Outcomes

Greater availability of alcohol can cause also spillover effects which go beyond the direct effects of the increase in consumption. In particular, researchers have documented a correlation between alcohol consumption and reduced productivity at the workplace (e.g., Blum, Roman, and Martin 1993, Jones, Casswell, and Zhang 1995, Fisher, Hoffman, Austin-Lane, and Kao 2000, McFarlin and Fals-Stewart 2002). In the current empirical setting, we can test the causal link between alcohol consumption and labor market outcomes.

We rely on annual tax data, filed by all Swedish residents above the age of 18. The data includes income from work (wage) and total after tax income which includes besides income from work, income from capital and social transfers. As the tax filings have an annual frequency and the experiment started in middle of the year 2000, we examine effects both in 2000 and 2001, and compare them to the pre-period of 1998-1999.

In Table 5, we use three main dependent variables: an indicator to whether the person reports a wage (i.e., is employed in the year) (Panel A), the logged value of the wage (measured in ’00 SEK, plus one) (Panel B), and the logged value of the income (measured in ’00 SEK, plus one) (Panel C). We use variables defined in the current year as well as in the following year. Except for the different frequency, the empirical specification is identical to that in previous tables.

In general, the effects of the expanded operating hours on labor market outcomes are negative, although not always statistically significant. Table 5, Panel A, shows that individuals exposed to treatment have lower likelihood of earning income from work (I(Wage>0)) in the following year by about 1.7% (Column (2)) (double-diff) ($t$ = 1.7). The estimated decrease in likelihood is greater, 3.0%, in the triple-diff specification (Column (6)), however, it is statistically insignificant ($t$ = 1.03). The effects on wages and income are also negative (Panels B and C). In Panel B, the effects in the current and the following year are between -1.9% to -12.0% (Columns (1), (2), (5), and (6)), although only mildly statistically significant. In Panel C, income declines at -0.4% to -11.9%, again with mild statistical significance.

Importantly, we observe that there is a relation between credit worthiness, credit balances, and the effects on the labor market. In particular, the negative effect of the expanded operating hours on labor market outcomes is stronger for individuals with more outstanding credit (Column (3)) and with lower (better) credit score i.e. default probability (Column (4)). These interactions reinforce the notion that the increase in indebtedness is not only due to greater expenditure, but also because wages and income are decreased due to the exposure to greater availability of alcohol.

### 5.7 Assessing the Magnitude

We also verify that our results make economic sense, i.e., that the effects we report are plausible given the reported increase in alcohol sales. The total revenue from off-premise alcohol sales in Sweden in 2000 was 17.368 billion SEK.[6] Norström and Skog (2005) and Grönqvist and Niknami (2014) reported an increase in alcohol sales of 3.7% and 4.0%, respectively. The increase in sales translates to about 277 to 299 million SEK in additional sales, assuming that the increase in sales is spread over 43% of the transactions, corresponding to the fraction of the population in the treatment areas). In the year 2000, the Swedish population in the treatment counties was 3.822 million (Figure 2), and of these approximately 75% were in the likely drinking age bracket of ages of 20 to 80 years old.[7] Hence, the average increase in alcohol consumption per capita in the treatment counties was 97 to 104 SEK.[8]

From examining other sources of data, it appears that young people spend on alcohol more than average. The Statistics Sweden (Statistiska centralbyrån) survey covering the years 1999–

[6] See historical sales figures in Systembolaget's Responsibility Report for 2008, available on http://web.wvb.com/corporations/update/download/src/$mnt$pdf_mount$reports$2008$SWE$000$091$SWE000091432.2008.G.00.E.12.31.PDF
[7] https://www.cia.gov/library/publications/the-world-factbook/geos/sw.html
[8] 330m SEK/(3.822m × 75%).

2001 collected expenditure data from about 4,800 people. The average annual spending on alcohol is provided in Table 7. The table shows that the average spending for young people in lowest income group (likely to be the population in our main sample) is about 2,800 SEK (about $310) a year.[9] Thus, an increase of 3.7% to 4% in their drinking translates to 104 to 112 SEK (we will use an average of 108 SEK).

Now, compare this estimation to our finding in Table 2 that drinking-eligible population ages 20-25 living in the treated counties increased their total debt balance by about 345 SEK, on average (with a 95% confidence interval of 108-521 SEK). Comparing this figure to spending estimation of alcohol of 108 SEK suggests a multiplier effect of 3.2, which is consistent the idea that increased alcohol consumption leads to poor decision making on other dimensions (e.g., drinking and driving, Wagenaar, Murray, and Toomey 2000, Levitt and Porter 2001), lack of savings (e.g., Schilbach 2014), or loss of income or jobs as we report here. We also note that our estimate is larger in magnitude than that of Schilbach (2014) who reports a multiplier effect that is greater than 2 in a population of Indian cab drivers. It is important, however, to remember that our estimation of the multiplier is based on several cumulative assumptions, (e.g., the increase in alcohol consumption due to experiment is uniform across populations), which increase the uncertainty around our calculation.

## 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, a higher frequency of default, and negative consequences in the labor market. 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 examine the timing of the increase in borrowing. We expect that the treated population would demand credit following the weekend. Second, we test whether the demand for credit was

[9] These figures could be compared to a similar study done in the U.S. in 2001 (https://www.bls.gov/cex/csxann01.pdf). Individuals in lowest income quintile spend $220 per year on alcohol, relative to average of $349. Individuals at 20-25 age range spend $368 per year.

concentrated in a small part of the population (a few alcohol addicts), or whether it was spread across the treated population. Third, we explore the possibility to an alternative explanation for our results, based on the idea of latent demand, i.e., that weekend opening hours enabled busy (yet rational) people to visit liquor stores.

### 6.1 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-bi-monthly 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 3, this time on a daily frequency. We regress the number of loans on the treatment indicator (triple interaction). The results are presented in Table 7, Columns (1) and (2) (different sets of fixed effects), and in Columns (5) and (6) (the dependent variable is logged). As in Table 3, 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.[10]

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

### 6.2 A Few Alcoholics? The Distribution of the Additional Take Up 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:

$$\begin{aligned} Credit\ TakeUp_{individual,time} &\\ &= \beta_1 Treated_i * Post_t + \beta_2 Eligible_{i,t} \qquad (4)\\ &* Post_t + \beta_3 Eligible_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{individual,time}. \end{aligned}$$

Note that we exclude on purpose the triple interaction that was the variable of interest in Tables 3, 4 and 5: $\boldsymbol{Treated_i * Eligible_{i,t} * Post_t}$. 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.

[10] 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 6a and 6b 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.

### 6.3 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 alcohol.

An alternative explanation is possible, however. The extended opening hours could make purchasing alcohol more convenient and thus may expose latent demand of busy consumers. 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 for employed individuals relative to individuals who are not working (retirees and unemployed).

We test this hypothesis in Appendix Table A3. 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 th 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 $\beta_1$ from the following regression:

$$Credit\ TakeUp_{individual,time} = \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t \qquad (3)$$
$$+\beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{individual,time}$$

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 3 Column (1), the coefficient is 0.029. In contrast, the coefficients in Column (1) of Appendix Table A3, 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.

## 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 address the concern that our sample might have an implicit look ahead bias due to the way it was constructed, by rerunning the main test with county-level data. Second, we provide a series of permutation tests which randomly allocates individuals to the treatment and control samples. By rerunning the previous statistical tests on these samples, we can assess whether the previous results were generated by unusual correlations in the data or whether they were driven by the opening hours experiment. Third, we verify that the choices of buffer counties or border counties did not materially affect the results. Fourth, we show that the results are not particular to the specific choice of the age groups in the treatment group. Fifth, we demonstrate that the results are not particularly sensitive to the choice of error clustering.

### 7.1 Demand for Pawn Credit: County-Level

Our first robustness test addresses a concern regarding a look ahead bias embedded in the construction of our sample. Specifically, our main analysis in Section 5 is based on the universe of pawn borrowers in the years 1992 to 2012 (see detailed description in Section 4). We use this sample to ask which borrowers took a new pawn loan in the period of 1999 to 2001. Most people who are included in this sample actually did not take a pawn loan in this period but rather took a pawn loan either in an earlier or later period. It is important to mention that for our empirical design we have to look into the future, otherwise we would not have information of those who turned 18 during the 1999-2001 period.

While our sample contain indeed future information, it is not likely that this bias drive our results. People who did not take pawn loans during the period of 1999-2001 appear as those who did not respond to the experiment (they did not borrow). To drive the results, the behavior of the non-borrowers should be correlated with the experiment. This could happen only if the experiment had long term effects that increased the likelihood of borrowing in the future. But this means that would have more non-borrowers in the treated counties, actually working against finding an effect of the legislation.

To provide additional comfort that the look ahead bias does not materially affect our results, we propose a method that allows to avoid the look ahead bias. In particular, instead of using past and future borrowers as the non-borrowers, we simply measure pawn borrowing per 100,000 residents in the county. Essentially we are measuring borrowing rate per capita. This way, there is no future borrowing information entering the sample design. The downside of this approach is that our observation unit is no longer a person-bi-month level, but rather county-bi-month.

In Appendix Table A2 we run double and triple-difference regressions at the county level so that we can control for potential variation over time in the number of residents in each age group. The unit of observation is the number of credits taken/defaults/etc. per quarter per 100,000 individuals living in a specific county and of a certain age (18, 19, 20, …, 25). 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-sectional double-difference specification is the following model:

$$Credit\ TakeUp_{county,time} = \beta_1 Treated_c * Post_t + \omega_{county} + \omega_{time} + \varepsilon_{county,time}, \tag{2}$$

and our cross-sectional triple-difference specification is the following model:

$$Credit\ TakeUp_{county,age,time} = \beta_1 Eligible_{age} * Treated_c * Post_t + \beta_2 Treated_c * Post_t + \beta_3 Eligible_{age} * Post_t + \beta_4 Eligible_{age} + \omega_{county} + \omega_{time} + \varepsilon_{county,age,time}. \tag{3}$$

Errors are clustered at the county level. Due to the small number of counties, we cluster the standard errors robust Wild bootstrap with 1,000 replications.[11] The double-difference specification omits the Eligible variable and interactions and runs the regression on the sample aged 18 to 25.

Appendix Table A2 presents the regression results for our pawn credit outcome variables. Column (1) shows both the double difference (Equation (2)) and the triple difference (Equation

[11] Sweden only has 21 counties, so the numbers of counties in the treatment and control groups (10) are too small to use robust clustering on the county level.

(3)) for the probability to take out a pawn loan. In both specifications, we find a significant increase in 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. The triple difference however, allows us to control for county specific time trends, because we have variation within the county in the treatment between eligible and ineligible consumers. Looking at our triple difference in Column (6) we find that Saturday opening hours increased the probability to take out a pawn loan by an average of 90.1 per 100,000 residents. This effect is a 38% increase over the pre-period average credit take-up rate by the treated. Column (7) shows that the effect is instead more moderate 15% when the estimation uses the log specification. Column (8) shows a significant effect (although only at a 5-10 percent level) when we combine the extensive (taking a pawn loan) and intensive margins (size of the loan) of 35%. These results are similar, albeit the point estimates not identical, to those in Table 3 (individual-level analysis). One potential reason is simply that the county-level regressions are noisier because of the aggregation (loss of personal information), and small number of observations.

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 or labor market outcomes.[12]

### 7.2 Permutation Test

As in many natural experiments, also in our setting there is a concern that the effects that we report are not related to the opening hours experiment but perhaps to an unobservable variation. We follow the procedure proposed by Chetty, Looney, and Kroft (2009). Each individual in the data is randomly reassigned to live in either a treatment county or not. This is done by reshuffling the already existing treatment variable, so the size of the treated group is constant. Then all interactions of treatment is recomputed, and the baseline regressions (both DD and DDD) are calculated for the constructed sample. The coefficient beta is stored, and the process starts over by again reshuffling the treatment. This is iterated 2,000 times. All controls are as in the baseline

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

regression and individual fixed effects are included. We plot the distribution of the point estimates from the 2,000 regressions, and in addition, we mark the original result.

The results of these placebo tests are presented in Figure 7 and Appendix Figures 1 to 3. Figure 7 shows the cumulative distribution function (CDF) for the permutation analysis for two variables: the number of pawn loans and the balance of credit cards. The CDF charts show that the coefficient in the original regressions is above the 95$^{th}$ percentile in both distributions, suggesting that the effect is driven the experiment. In other words, once we remove the effects of the experiment by reshuffling observations, across treatment and control counties, the effect essentially disappears.

We provide charts corresponding to additional regressions in Appendix Figures 1 to 3, where each chart corresponds with a regression. It presents the distribution of coefficients on the interaction of interest (Eligible × Post × Treated). Overwhelmingly, in all charts the results of the experiment fall to the sides of the distribution of the randomized allocations. This suggests that the results that we observe in our empirical analysis are driven by the specific allocation of individuals to the treatment and control and not likely to be due to chance.

### 7.3 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 A4, we use a sample that excludes Skåne. The results are very similar to the ones presented in Table 3 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 A5 shows that the main results are robust to the change in the definition of the control population.

### 7.4 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 is 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 A6 we vary the age groups from 20–21 to 20–30, in addition to 20–45 and 20–65 and show that the effects we document change only modestly 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 in a systematic manner when varying the age band.

### 7.5 Sensitivity of Results 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 A7, 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 alcohol may trigger myopic behavior by individuals and eventually affect their financial wellbeing. Until now, only a few empirical studies have been able to provide evidence from the field 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. Finally, consistent with the idea that alcohol may lead to poor decision making in other dimensions, and therefore has indirect costs, we document that the increase in alcohol consumption had also spillovers to the labor market. Specifically, treated populations were likely to experience higher rates of unemployment and lower wages.

Since alcohol consumption is triggered by present bias, and its consumption imposes direct and indirect costs on consumers, policymakers can improve financial wellbeing of myopic consumers by limiting their access to alcohol.

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## Appendix A. Variable Definitions

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

| Variable | Definition |
|---|---|
| Treated | Equals 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 | Equals to one if the date is post February 1st. and zero if the date is February 1st or earlier. |
| Eligible | Equals to one if age is 20 or older, and zero if age is 18. |

| Variable | Definition |
|---|---|
| Pawn credit market (bimonthly freq.) | |
| # New loans | Equals to the number of new pawn loans taken out in a pawn shop during that bimonth. |
| log(loan size+1) | Equals to the log of the total loan principal taken out during that bimonth, plus one. |
| # Defaults | Equals to the number of pawn loans held by that person that went to auction during that |
| # Rollovers | Equals to the number of pawn loans held by that person that rollover during that bimonth. |
| | |
| Mainstream credit market (bimonthly freq.) | |
| Credit card, number | Equals to the number of credit cards the individual owns. |
| Credit card, balance (SEK) | Equals to the sum of balances of credit cards the individual owns. |
| Installment, number | Equals to the number of installment loans the individual owns. |
| Installment, limit (SEK) | Equals to the sum of balances of installment loans the individual owns. |
| Credit line, number | Equals to the number of credit lines the individual owns. |
| Credit line, balance (SEK) | Equals to the sum of balances of credit lines the individual owns. |
| # Arrears | Equals to the number of credit arrears the individual has on his her credit report. |
| I(Arrears>0) | Indicator variable to whetehr the individual receives at least one new credit arrear before the next observation. |
| | |
| Both credit market (bimonthly freq.) | |
| Total credit (pawn + mainstream) | Equals to total credit in the pawn and mainstream credit markets. |
| Total # of defaults (pawn + arrears) | Equals to total number of defaults in the pawn and mainstream credit markets. |
| | |
| Labor market outcomes (annual freq.) | |
| I(Wage>0) (t) | Indicator variable to whether a wage is recorded in the current year. |
| I(Wage>0) (t+1) | Indicator variable to whether a wage is recorded in the following year. |
| log(Wage+1) (t) ('00 SEK) | Equals to the log of current wage (measured in '00 SEK) plus one; zero otherwise. |
| log(Wage+1) (t+1) ('00 SEK) | Equals to the log of wage in the following year (measured in '00 SEK) plus one; zero otherwise. |
| log(Income+1) (t) ('00 SEK) | Equals to the log of registered total income after taxes (measured in '00 SEK) plus one. |
| log(Income+1) (t+1) ('00 SEK) | Equals to the log of registered total income after taxes in the following year (measured in '00 SEK) plus one. |

| Pawn Credit Market on County Level (bi-monthly freq.) | |
|---|---|
| | 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 and 1998, 1999 for the annual labor market outcomes. The sample is 18–25 year olds in both the treated and control counties. See Appendix Table 1 for the statistics split by treatment and control county.

### Panel A: Pawn Credit Market Outcomes

| Variable | # Obs | # Indviduals | Mean | Std Dev | Min | 10pctl | 25pctl | 50pctl | 75pctl | 90pctl | Max |
|---|---|---|---|---|---|---|---|---|---|---|---|
| # New loans | 164,382 | 38,320 | 0.110 | 0.411 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |
| log(# New loans+1) | 164,382 | 38,320 | 0.068 | 0.235 | 0 | 0 | 0 | 0 | 0 | 0 | 1.39 |
| Loan size | 164,382 | 38,320 | 174 | 925 | 0 | 0 | 0 | 0 | 0 | 0 | 10,050 |
| # Defaults | 164,382 | 38,320 | 0.007 | 0.094 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| # Rollovers | 164,382 | 38,320 | 0.032 | 0.221 | 0 | 0 | 0 | 0 | 0 | 0 | 3 |

### Panel B: Mainstream Consumer Credit Market Outcomes

| Variable | # Obs | # Indviduals | Mean | Std Dev | Min | 10pctl | 25pctl | 50pctl | 75pctl | 90pctl | Max |
|---|---|---|---|---|---|---|---|---|---|---|---|
| # Credit cards | 53,921 | 34,902 | 0.13 | 0.541 | 0 | 0 | 0 | 0 | 0 | 0 | 4 |
| Credit card balance (SEK) | 53,921 | 34,902 | 598 | 3,237 | 0 | 0 | 0 | 0 | 0 | 0 | 31,483 |
| # Installment loans | 53,921 | 34,902 | 0.029 | 0.195 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| Installment loan limit (SEK) | 53,921 | 34,902 | 774 | 7,754 | 0 | 0 | 0 | 0 | 0 | 0 | 133,357 |
| # Credit lines | 53,921 | 34,902 | 0.218 | 0.501 | 0 | 0 | 0 | 0 | 0 | 1 | 2 |
| Credit line balance (SEK) | 53,921 | 34,902 | 2,294 | 10,848 | 0 | 0 | 0 | 0 | 0 | 107 | 87,165 |
| log(# Arrears+1) | 53,921 | 34,902 | 0.300 | 0.690 | 0 | 0 | 0 | 0 | 0 | 1.39 | 3.04 |
| I(Arrears>0) | 53,841 | 34,852 | 0.037 | 0.190 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |

### Panel C: Consolidated Outcomes

| Variable | # Obs | # Indviduals | Mean | Std Dev | Min | 10pctl | 25pctl | 50pctl | 75pctl | 90pctl | Max |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Total credit (pawn + mainstream) | 28,204 | 53,921 | 3,672 | 13,633 | 0 | 0 | 0 | 0 | 32 | 6,209 | 88,957 |
| Total # of defaults (pawn + arrears) | 26,812 | 26,812 | 0.067 | 0.278 | 0 | 0 | 0 | 0 | 0 | 0 | 9 |

### Panel D: Labor Market Outcomes

| Variable | # Obs | # Indviduals | Mean | Std Dev | Min | 10pctl | 25pctl | 50pctl | 75pctl | 90pctl | Max |
|---|---|---|---|---|---|---|---|---|---|---|---|
| I(Wage>0) (t) | 62,595 | 22,504 | 0.792 | 0.406 | 0 | 0 | 1 | 1 | 1 | 1 | 1 |
| I(Wage>0) (t+1) | 63,114 | 22,517 | 0.837 | 0.369 | 0 | 0 | 1 | 1 | 1 | 1 | 1 |
| log(Wage+1) (t) ('00 SEK) | 62,595 | 22,504 | 5.047 | 2.773 | 0.00 | 0.00 | 4.49 | 6.21 | 7.11 | 7.47 | 9.59 |
| log(Wage+1) (t+1) ('00 SEK) | 63,114 | 22,517 | 5.493 | 2.623 | 0.00 | 0.00 | 5.06 | 6.61 | 7.30 | 7.58 | 9.59 |
| log(Income+1) (t) ('00 SEK) | 70,179 | 24,202 | 4.197 | 2.939 | 0.00 | 0.00 | 0.00 | 5.58 | 6.67 | 7.04 | 10.77 |
| log(Income+1) (t+1) ('00 SEK) | 69,480 | 24,075 | 4.682 | 2.869 | 0.00 | 0.00 | 0.00 | 6.06 | 6.88 | 7.16 | 10.76 |
| Credit score | 17,448 | 17,448 | 29.864 | 36.379 | 0.34 | 2.40 | 3.83 | 7.46 | 62.29 | 95.20 | 99.89 |
| log(Credit score) | 17,448 | 17,448 | 2.639 | 1.298 | 0.29 | 1.22 | 1.57 | 2.14 | 4.20 | 4.57 | 4.61 |
| Credit limit | 17,448 | 17,448 | 9,976 | 36,140 | 0 | 0 | 0 | 0 | 1,322 | 27,380 | 2,031,285 |
| log(Credit limit+1) | 17,448 | 17,448 | 3.113 | 4.251 | 0.00 | 0.00 | 0.00 | 0.00 | 7.19 | 10.22 | 14.52 |

### Panel E: Pawn Credit Market Outcomes, County-Level

| Monthly frequency, county level, per 100,000 individuals | Pre-period | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | # Obs | # Counties | Mean | Std Dev | Min | 10pctl | 25pctl | 50pctl | 75pctl | 90pctl | Max |
| # New loans | 560 | 10 | 315 | 230 | 0 | 52 | 129 | 285 | 469 | 604 | 1,042 |
| log(# New loans+1) | 560 | 10 | 5.25 | 1.44 | 0.0 | 4.0 | 4.9 | 5.7 | 6.2 | 6.4 | 7.0 |
| # Defaults | 560 | 10 | 41.8 | 95.2 | 0 | 0 | 0 | 0 | 31 | 123 | 740 |
| # Rollovers | 560 | 10 | 66.7 | 73.3 | 0 | 0 | 0 | 44 | 116 | 157 | 460 |
| log(Aggregate loan size+1) | 560 | 10 | 11.1 | 4.5 | 0.0 | 0.0 | 11.4 | 12.9 | 13.5 | 14.0 | 14.6 |

## Table 2. Individual Level Regressions: Total Credit Outcomes

This table shows that increased access to alcohol causally increases credit take-up and the default risk. The table shows the coefficient $\beta_1$ from Equation 2:

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

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 # of defaults (pawn + arrears) |
|---|---|---|
| (i) DD estimate | (1) | (2) |
| Post × Treated | 635*** | 0.041*** |
| | (87) | (0.003) |
| | | |
| County FE | Yes | Yes |
| Calendar Month FE | Yes | Yes |
| Individual FE | Yes | Yes |
| | | |
| (ii) DDD estimate | (3) | (4) |
| Eligible × Post × Treated | 345*** | 0.023*** |
| | (121) | (0.007) |
| | | |
| Pre-period mean | 5,000 | 0.085 |
| Effect | 6.9% | 27% |
| | | |
| County × Calendar Month FE | Yes | Yes |
| Individual FE | Yes | Yes |
| | | |
| Observations | 261,905 | 234,719 |
| $R^2$ | 0.084 | 0.031 |
| # Individuals | 34,902 | 34,123 |

## Table 3. Individual Level Regressions: Pawn Credit Outcomes

This table shows that increased access to alcohol causally increases pawn credit take-up and the default risk. The table shows the coefficient $\beta_1$ from Equation 2:

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

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

| Dependent variable: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| (i) DD estimate | (1) | (2) | (3) | (4) | (5) |
| Treated × Post | 0.014*** | 0.009*** | 0.081*** | 0.024*** | 0.003* |
| | (0.003) | (0.002) | (0.015) | (0.001) | (0.002) |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes |
| (ii) DDD estimate | (6) | (7) | (8) | (9) | (10) |
| Eligible × Treated × Post | 0.026** | 0.014** | 0.101* | -0.005 | 0.005 |
| | (0.012) | (0.007) | (0.058) | (0.004) | (0.004) |
| Pre-mean: | 0.119 | 0.119 | 0.627 | | |
| Relative effect: | 22% | 19% | 16% | | |
| County × Calendar Month FE | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.007 | 0.007 | 0.007 | 0.025 | 0.001 |
| Observations | 399,178 | 399,178 | 399,178 | 399,178 | 399,178 |
| # Individuals | 38,320 | 38,320 | 38,320 | 38,320 | 38,320 |

## Table 4. Individual Level Regressions: Mainstream Credit Outcomes

This table shows that increased access to alcohol causally increases consumer credit take-up and risk of delinquency within the mainstream consumer credit market. The table shows the coefficient $\beta_1$ from Equation 2:

$$Credit\ TakeUp_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t$$
$$+\beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{i,t}$$

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

| | Credit cards | | Installment loans | | Credit lines | | Performance | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Number | Balance | Number | Limit | Number | Balance | log(# Arrears+1) | I(Arrear>0) |
| (i) DD estimate | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Treated × Post | 0.031*** | 189*** | -0.001 | 126** | -0.025*** | 488*** | 0.041*** | 0.020*** |
| | (0.003) | (24) | (0.001) | (57) | (0.003) | (74) | (0.003) | (0.002) |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| (ii) DDD estimate | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
| Eligible × Treated × Post | 0.020*** | 127*** | -0.001 | 45 | -0.014** | 326*** | 0.004 | 0.014*** |
| | (0.004) | (41) | (0.001) | (61) | (0.006) | (120) | (0.005) | (0.005) |
| Pre-mean: | 0.183 | 836 | | | 0.304 | 3,070 | | 0.0500 |
| Relative effect: | 11% | 15% | | | -4.6% | 11% | | 28% |
| County × Calendar Month FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.016 | 0.007 | 0.001 | 0.001 | 0.017 | 0.006 | 0.046 | 0.009 |
| Observations | 261,905 | 261,905 | 261,905 | 261,905 | 261,905 | 261,905 | 261,905 | 261,905 |
| # Individuals | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 |

## Table 5. Labor Market Outcomes

This table explores the effects of extended opening hours on labor market outcomes. Standard errors are clustered at the individual level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

**Panel A: Likelihood of Having Income from Work (wage)**

| Dependent variable: | I(Wage>0) (t) | I(Wage>0) (t+1) | | |
|---|---|---|---|---|
| (i) DD estimate (ages 20-25) | (1) | (2) | (3) | (4) |
| Treated × Post | -0.003 | -0.017* | -0.010 | -0.034*** |
| | (0.011) | (0.010) | (0.012) | (0.014) |
| Treated × Post × log(Credit limit+1) | | | -0.002*** | |
| | | | (0.001) | |
| Treated × Post × log(Credit score+1) | | | | 0.006* |
| | | | | (0.003) |
| County FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Observations | 49,160 | 49,192 | 49,192 | 49,192 |
| # Individuals | 19,266 | 19,198 | 19,198 | 19,198 |

| (ii) DDD estimate (ages 18-25) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|
| Eligible × Treated × Post | -0.008 | -0.030 | -0.022 | 0.113 |
| | (0.036) | (0.031) | (0.045) | (0.090) |
| Eligible × Treated × Post × log(Credit limit+1) | | | -0.009 | |
| | | | (0.045) | |
| Eligible × Treated × Post × log(Credit score+1) | | | | -0.063* |
| | | | | (0.038) |
| County FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes |
| County × Calendar Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Observations | 62,595 | 63,114 | 63,114 | 63,114 |
| # Individuals | 22,504 | 22,517 | 22,517 | 22,517 |

**Table 5. Labor Market Outcomes**

**Panel B: Effect on Wages**

| Dependent variable: | log(Wage+1) (t) ('00 SEK) | log(Wage+1) (t+1) ('00 SEK) | | |
|---|---|---|---|---|
| (i) DD estimate (ages 20-25) | (1) | (2) | (3) | (4) |
| Treated × Post | -0.019 | -0.120* | 0.009 | -0.224*** |
| | (0.067) | (0.062) | (0.071) | (0.082) |
| Treated × Post × log(Credit limit+1) | | | -0.026*** | |
| | | | (0.005) | |
| Treated × Post × log(Credit score+1) | | | | 0.046** |
| | | | | (0.020) |
| County FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Observations | 49,160 | 49,192 | 49,192 | 49,192 |
| # Individuals | 19,266 | 19,198 | 19,198 | 19,198 |
| (ii) DDD estimate (ages 18-25) | (5) | (6) | (7) | (8) |
| Eligible × Treated × Post | -0.095 | -0.096 | 0.003 | 0.511 |
| | (0.198) | (0.173) | (0.254) | (0.516) |
| Eligible × Treated × Post × log(Credit limit+1) | | | -0.084 | |
| | | | (0.056) | |
| Eligible × Treated × Post × log(Credit score+1) | | | | -0.278 |
| | | | | (0.208) |
| County FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes |
| County × Calendar Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Observations | 62,595 | 63,114 | 63,114 | 63,114 |
| # Individuals | 22,504 | 22,517 | 22,517 | 22,517 |

## Table 5. Labor Market Outcomes

### Panel C: Effect on Income

| Dependent variable: | log(Income+1) (t) ('00 SEK) | log(Income+1) (t+1) ('00 SEK) | | |
|---|---|---|---|---|
| (i) DD estimate (ages 20-25) | (1) | (2) | (3) | (4) |
| Treated × Post | -0.004 | -0.118** | 0.039 | -0.165** |
| | (0.057) | (0.055) | (0.062) | (0.072) |
| Treated × Post × log(Credit limit+1) | | | -0.021*** | |
| | | | (0.004) | |
| Treated × Post × log(Credit score+1) | | | | 0.016 |
| | | | | (0.017) |
| County FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Observations | 54,560 | 53,998 | 53,998 | 53,998 |
| # Individuals | 20,717 | 20,589 | 20,589 | 20,589 |
| (ii) DDD estimate (ages 18-25) | (5) | (6) | (7) | (8) |
| Eligible × Treated × Post | -0.129 | -0.034 | 0.156 | 0.729* |
| | (0.147) | (0.139) | (0.205) | (0.403) |
| Eligible × Treated × Post × log(Credit limit+1) | | | -0.117** | |
| | | | (0.0494) | |
| Eligible × Treated × Post × log(Credit score+1) | | | | -0.304** |
| | | | | (0.138) |
| County FE | Yes | Yes | Yes | Yes |
| Calendar Month FE | Yes | Yes | Yes | Yes |
| County × Calendar Month FE | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes |
| Observations | 70,179 | 69,480 | 69,480 | 69,480 |
| # Individuals | 24,202 | 24,075 | 24,075 | 24,075 |

## Table 6. Weekly Pattern of Pawn Credit Take-Up

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: | # New loans | | log(# New loans+1) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Eligible × Post × Treated | 0.00033 | 0.00019 | 0.00021 | 0.00012 |
| | (0.00022) | (0.00022) | (0.00014) | (0.00014) |
| Eligible × Post × Treated × Monday | | 0.00073*** | | 0.00049*** |
| | | (0.00008) | | (0.00005) |
| County × Calendar Month FE | Yes | Yes | Yes | Yes |
| Person FE | Yes | Yes | Yes | Yes |
| Observations | 15,681,601 | 15,681,601 | 15,681,601 | 15,681,601 |
| $R^2$ | 0.0002 | 0.0002 | 0.0002 | 0.0002 |
| # Individuals | 38,468 | 38,468 | 38,468 | 38,468 |

## Table 7. Alcohol Spending of Swedish Individuals, by Age Group and Income Quintile

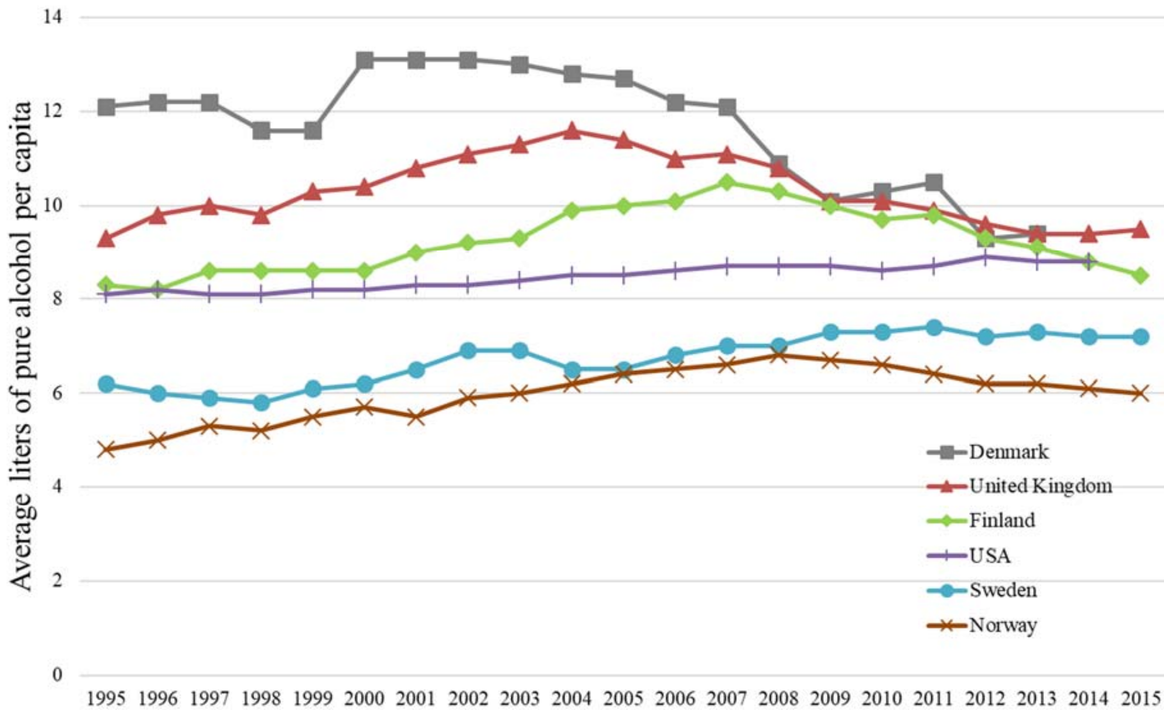
This table shows a breakdown of average annual alcohol expenditure of about 4,800 Swedish individuals in the years 1999-2001. The sample is broken into age groups and income quintiles. The survey was conducted by Statistics Sweden (Statistiska centralbyrån).

| | | Age | | | | | |
|---|---|---|---|---|---|---|---|
| | | <25 | 25-34 | 35-44 | 45-54 | 55-64 | >65 |
| Income | 1 | 2,786 | 3,341 | 2,749 | 4,935 | 2,072 | 1,721 |
| quintile | 2 | 2,822 | 3,527 | 2,957 | 3,741 | 2,728 | 2,884 |
| | 3 | 4,316 | 2,942 | 3,073 | 3,945 | 4,145 | 4,397 |
| | 4 | 9,295 | 4,179 | 3,910 | 5,796 | 4,976 | 4,780 |
| | 5 | 8,831 | 3,757 | 5,502 | 6,600 | 7,493 | 9,630 |

## Figure 1. Alcohol Consumption Statistics

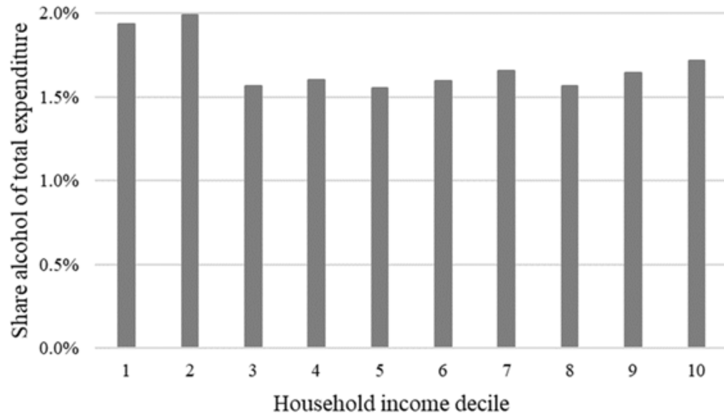
This figure shows the average number of liters of pure alcohol consumed per year per capita in Sweden (solid line), the other Nordic countries (Denmark, Finland and Norway) the United Kingdom and the United States; between 1995 and 2015. The statistics are taken from OECD (2017).

### Figure 1a. Average Alcohol Consumption over Time International Comparison (1995-2015)

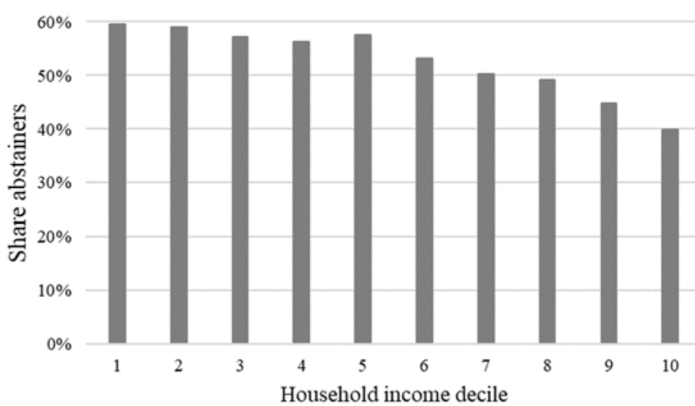


## Figure 1. Drinking patterns in Sweden (Cont.)

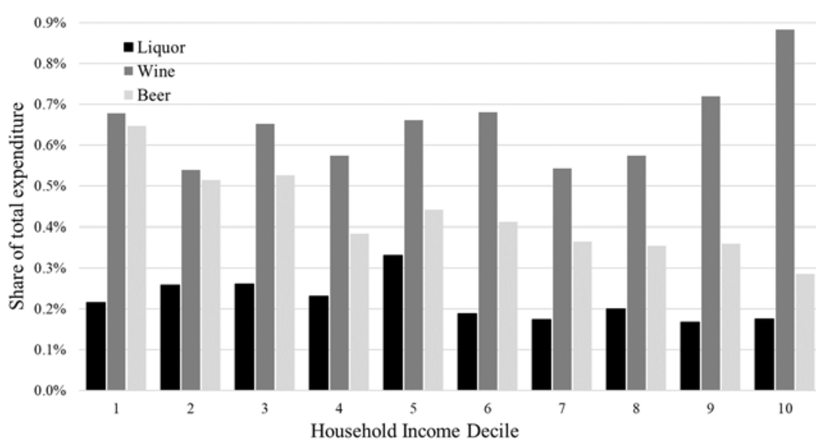
### Figure 1b. Share Alcohol of Total Expenditure, by Household Income Decile (1999-2001)




### Figure 1c. Share Abstainers, by Household Income Decile (1999-2001)

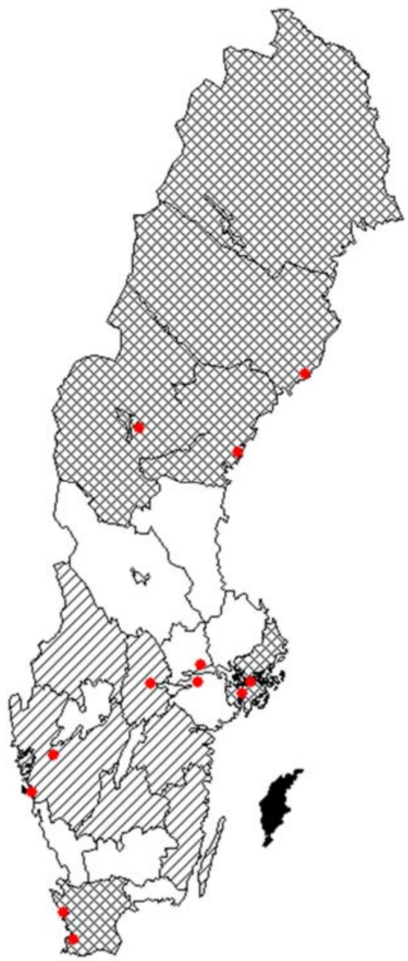



### Figure 1d. Share of Total Expenditure, by Product Type and Income (2003-2009)

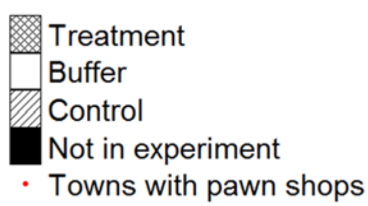


# Figure 2. Map of Treated and Control Counties

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, Västernorrland, Jämtland, Västerbotten, Norrbotten, and Skåne. The control counties (striped pattern) were Östergötland, Jönköping, Kalmar, Västra Götaland, Värmland, and Örebro. 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.

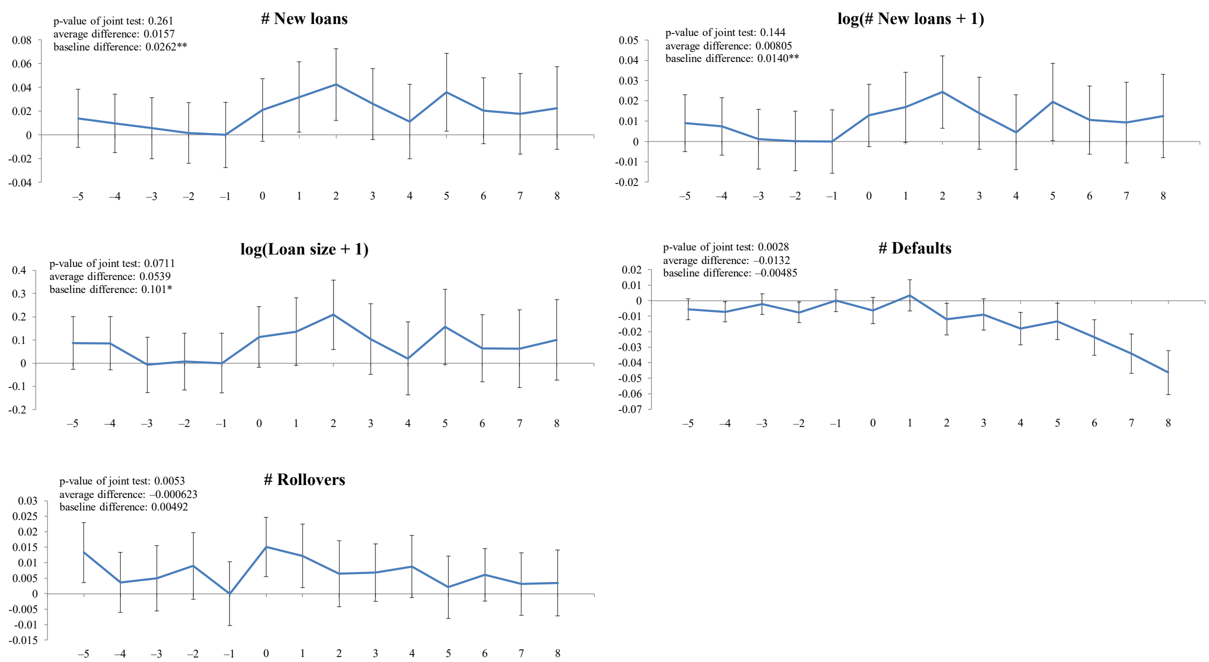


| | Total population | Share of Sweden |
|---|---|---|
| Treatment counties | | |
| Stockholm | 1,803,377 | 20% |
| Västernorrland | 249,299 | 3% |
| Jämtland | 130,705 | 1% |
| Västerbotten | 256,710 | 3% |
| Norrbotten | 258,094 | 3% |
| Skåne | 1,123,786 | 13% |
| Total | 3,821,971 | 43% |
| | | |
| Control counties | | |
| Östergötland | 411,320 | 5% |
| Jönköping | 327,266 | 4% |
| Kalmar | 236,501 | 3% |
| Västra Götaland | 1,488,709 | 17% |
| Värmland | 276,600 | 3% |
| Örebro | 273,822 | 3% |
| Total | 3,014,218 | 34% |
| | | |
| Buffer counties | | |
| Uppsala | 292,415 | 3% |
| Södermanland | 255,890 | 3% |
| Kronoberg | 177,149 | 2% |
| Blekinge | 150,625 | 2% |
| Halland | 273,537 | 3% |
| Dalarna | 280,575 | 3% |
| Gävleborg | 280,717 | 3% |
| Västmanland | 256,901 | 3% |
| Total | 1,967,809 | 22% |



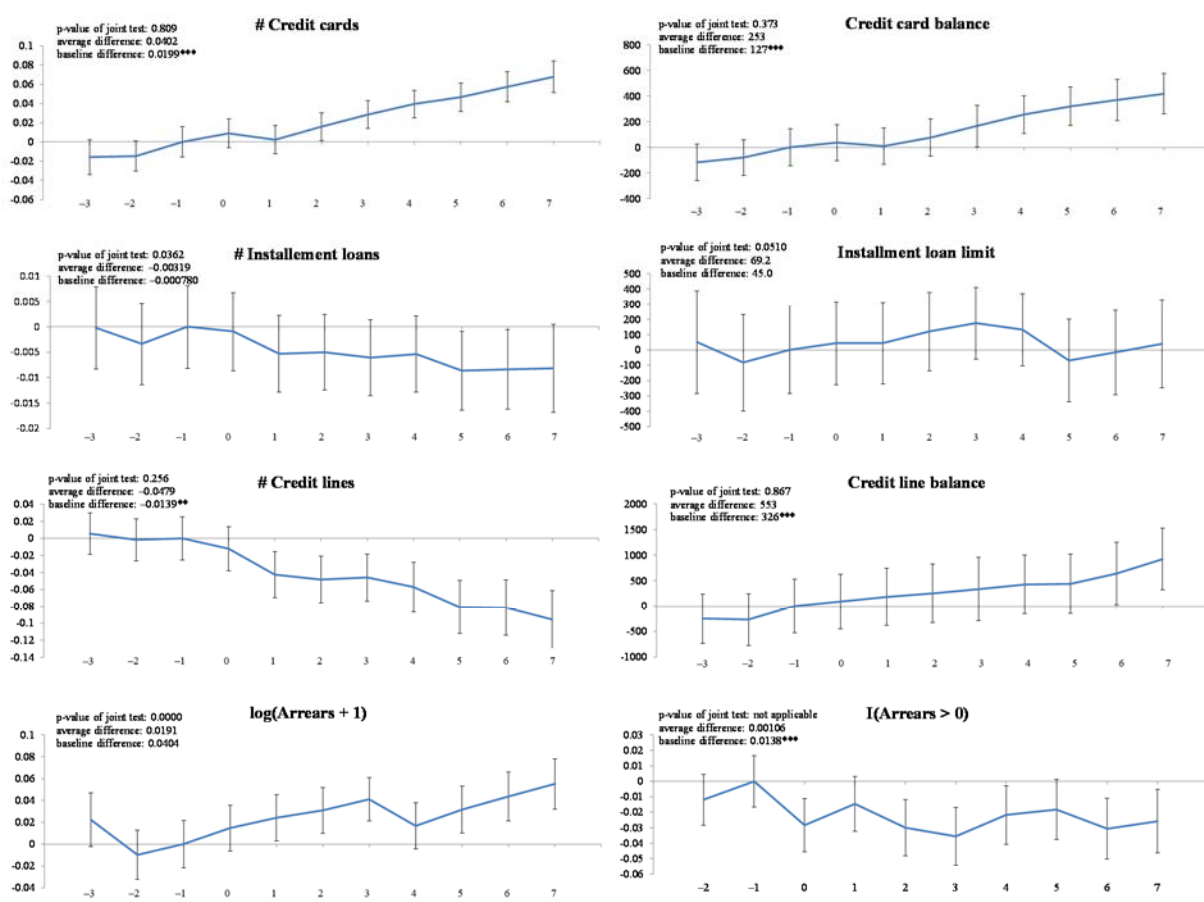
## Figure 3. Saturday Opening Hour reform Estimates: Pawn Credit Outcomes

This figure lends support of our parallel growth assumption for the difference between borrowers who could legally purchase alcohol and those who could not in the treatment and control counties for our pawn credit outcomes. The panel depict estimates of the $\boldsymbol{\beta_\tau}$ coefficients and their 95 percent confidence intervals from the model: $\boldsymbol{y_i} = \sum_{\tau=-5}^{8}\left(\boldsymbol{\beta_\tau}\ \boldsymbol{Eligible_{i,t}} * \boldsymbol{Treated_i} * \boldsymbol{Period_t}\right) + \boldsymbol{\xi_1 Eligible_{i,t}} * \boldsymbol{Treated_i} + \boldsymbol{\xi_2 Eligible_{i,t}} * \boldsymbol{Post_t} + \boldsymbol{\xi_3 Eligible_{i,t}} + \boldsymbol{\omega_i} + \boldsymbol{\omega_{time*county}} + \boldsymbol{\varepsilon_{i,t}}$. In the boxes within the respective panels we report (i) the *p*-value of a Wald test to check the null-hypothesis that the $\boldsymbol{\beta_\tau}$ coefficients in the pre-period are jointly equal, (ii) the average difference ($\frac{1}{9}\sum_{\tau=0}^{8}\boldsymbol{\beta_\tau} - \frac{1}{5}\sum_{\tau=-5}^{-1}\boldsymbol{\beta_\tau}$) and (iii) the baseline difference obtained from regression (Equation (1)).



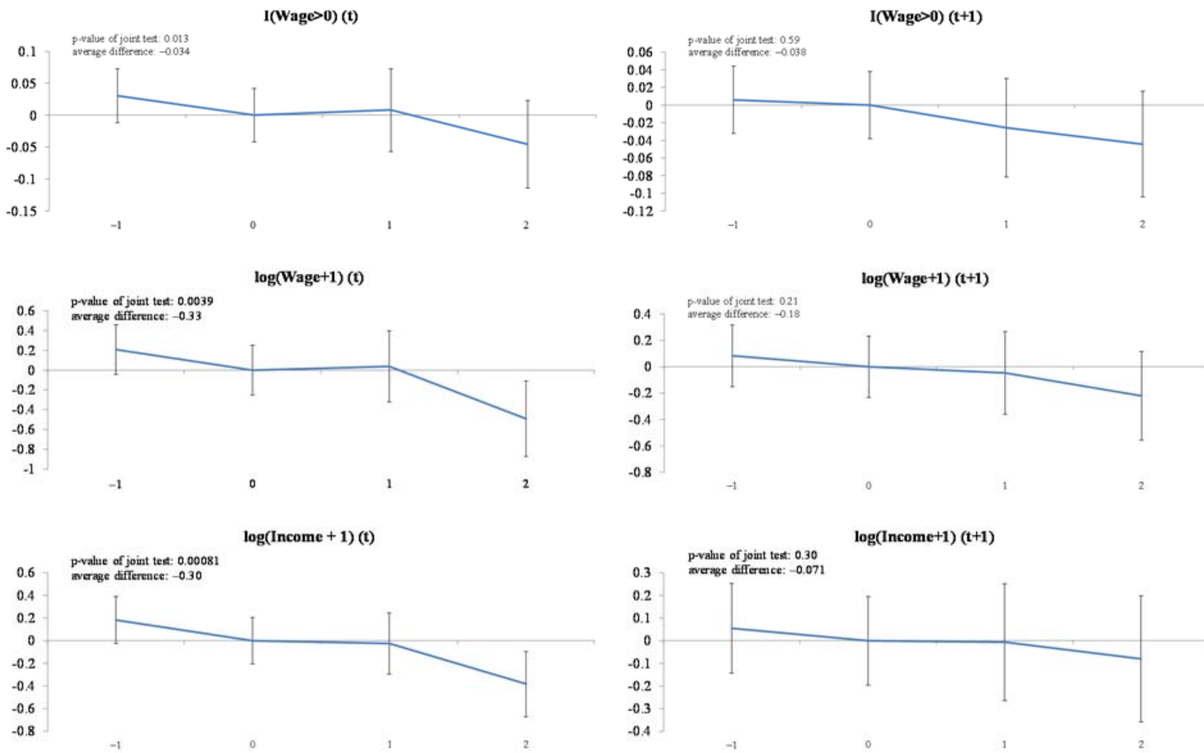
## Figure 4. Saturday Opening Hour Reform Estimates: Mainstream Credit Outcomes

This figure lends support of our parallel growth assumption for the difference between borrowers who could legally purchase alcohol and those who could not in the treatment and control counties for our mainstream credit outcomes. The panel depict estimates of the $\beta_\tau$ coefficients and their 95 percent confidence intervals from the model: $\boldsymbol{y_i} = \sum_{\tau=-3}^{7}\left(\boldsymbol{\beta_\tau}\, Eligible_{i,t} * \boldsymbol{Treated_i} * \boldsymbol{Period_t}\right) + \boldsymbol{\xi_1 Eligible_{i,t}} * \boldsymbol{Treated_i} + \boldsymbol{\xi_2 Eligible_{i,t}} * \boldsymbol{Post_t} + \boldsymbol{\xi_3 Eligible_{i,t}} + \boldsymbol{\omega_i} + \boldsymbol{\omega_{time*county}} + \boldsymbol{\varepsilon_{i,t}}$. In the boxes within the respective panels we report (i) the *p*-value of a Wald test to check the null-hypothesis that the $\beta_\tau$ coefficients in the pre-period are jointly equal, (ii) the average difference ($\frac{1}{8}\sum_{\tau=0}^{7}\beta_\tau - \frac{1}{3}\sum_{\tau=-3}^{-1}\beta_\tau$) and (iii) the baseline difference obtained from regression (Equation (1)).



## Figure 5. Saturday Opening Hour Reform Estimates: Labor Market Outcomes

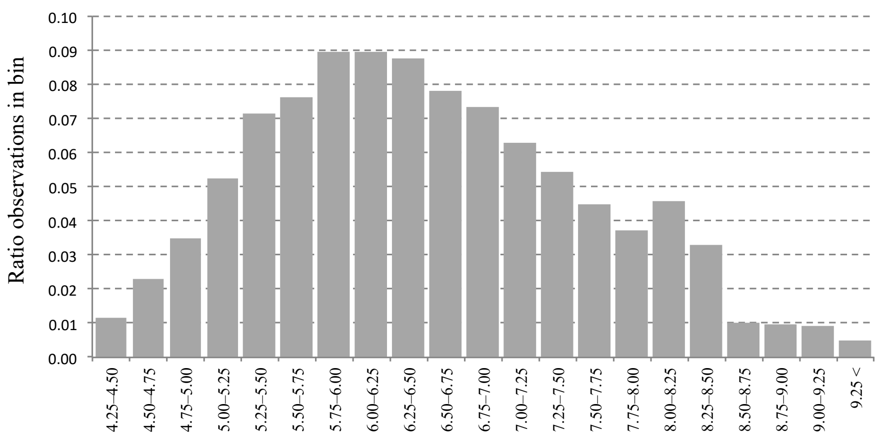
This figure lends support of our parallel growth assumption for the difference between borrowers who could legally purchase alcohol and those who could not in the treatment and control counties for our mainstream credit outcomes. The panel depict estimates of the $\beta_\tau$ coefficients and their 95 percent confidence intervals from the model: $\boldsymbol{y_i} = \sum_{\tau=-1}^{2}\left(\boldsymbol{\beta_\tau}\, Eligible_{i,t} * \boldsymbol{Treated_i} * \boldsymbol{Period_t}\right) + \boldsymbol{\xi_1 Eligible_{i,t}} * \boldsymbol{Treated_i} + \boldsymbol{\xi_2 Eligible_{i,t}} * \boldsymbol{Post_t} + \boldsymbol{\xi_3 Eligible_{i,t}} + \boldsymbol{\omega_i} + \boldsymbol{\omega_{time*county}} + \boldsymbol{\varepsilon_{i,t}}$. In the boxes within the respective panels we report (i) the *p*-value of a Wald test to check the null-hypothesis that the $\beta_\tau$ coefficients in the pre-period are jointly equal, (ii) the average difference ($\frac{1}{2}\sum_{\tau=1}^{2}\beta_\tau - \frac{1}{2}\sum_{\tau=-1}^{0}\beta_\tau$).



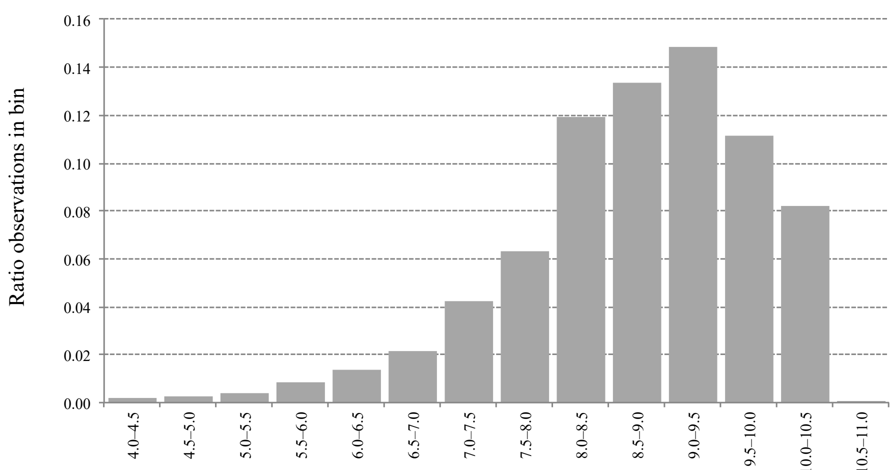
## Figure 6. Distribution of Residuals in the Treatment Counties of Pawn and Credit Card Loan Sizes

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., post × eligible × treated county, from the baseline regression (Equation 2) without the triple-interaction.

### Panel A. Residuals of log(Pawn loan size+1), Conditional on Taking a Pawn Loan

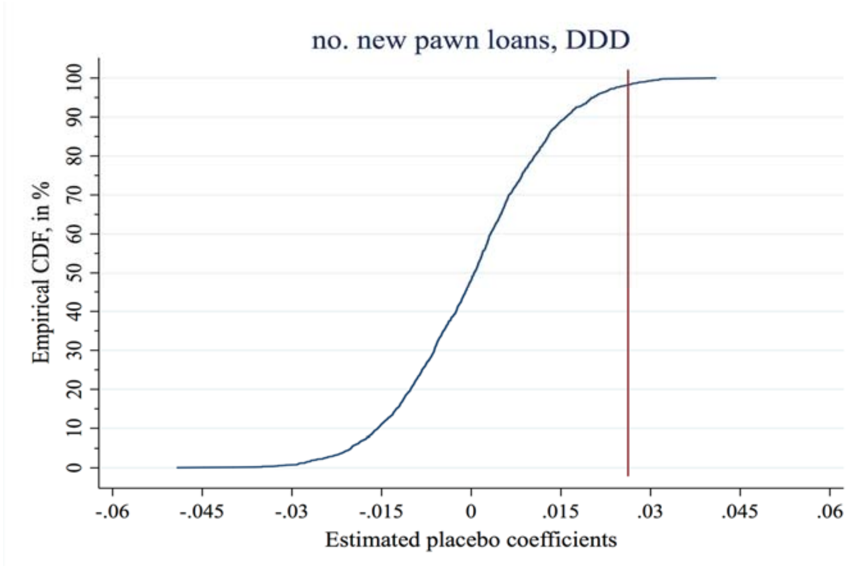


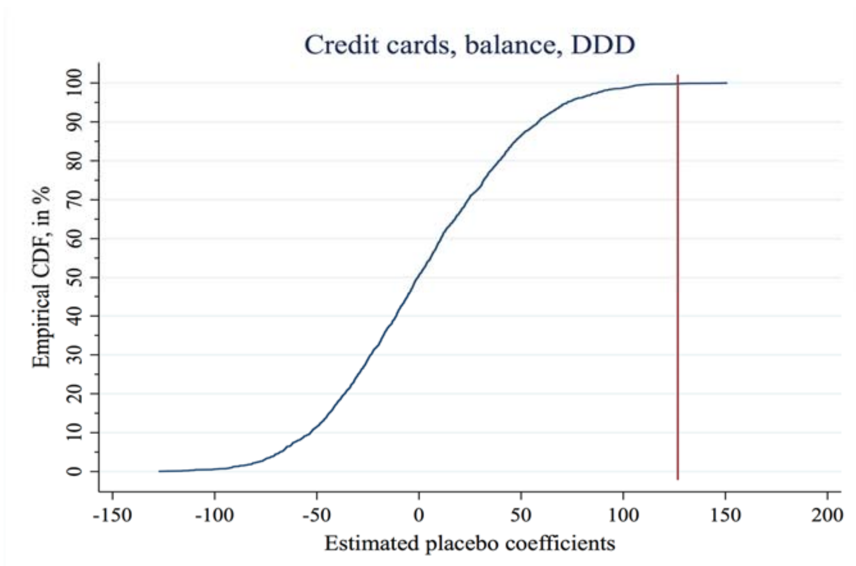

### Panel B. Residuals of log(Credit card balance+1), Conditional on Having a Credit Card



## Figure 7. Distribution of Placebo Estimates

This figure plots the empirical distribution of placebo effects (G) for our respective outcome variables. The PDF is constructed from 2,000 estimates of $\beta_1$, using our baseline specification of Equation 3. The vertical line shows the treatment effect estimate reported in Table 2 and 4.



## Appendix Table A1. Summary Statistics Split by Treatment and Control Counties

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.

| | Control Counties | | | | | | Treatment Counties | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | # Obs | Mean | Std Dev | Min | Median | Max | # Obs | Mean | Std Dev | Min | Median | Max |
| # New loans | 61,527 | 0.096 | 0.381 | 0.00 | 0.00 | 3.00 | 102,855 | 0.119 | 0.428 | 0.00 | 0.00 | 3.00 |
| log(# New loans+1) | 61,527 | 0.059 | 0.220 | 0.00 | 0.00 | 1.39 | 102,855 | 0.072 | 0.244 | 0.00 | 0.00 | 1.39 |
| log(Loan size+1) | 61,527 | 0.504 | 1.820 | 0.00 | 0.00 | 9.22 | 102,855 | 0.627 | 2.060 | 0.00 | 0.00 | 9.22 |
| # Defaults | 61,527 | 0.006 | 0.082 | 0.00 | 0.00 | 2.00 | 102,855 | 0.008 | 0.100 | 0.00 | 0.00 | 2.00 |
| # Rollovers | 61,527 | 0.031 | 0.215 | 0.00 | 0.00 | 3.00 | 102,855 | 0.034 | 0.224 | 0.00 | 0.00 | 3.00 |
| | | | | | | | | | | | | |
| # Credit cards | 20,200 | 0.041 | 0.299 | 0.00 | 0.00 | 4.00 | 33,721 | 0.183 | 0.638 | 0.00 | 0.00 | 4.00 |
| Credit card balance (SEK) | 20,200 | 203 | 1,927 | 0.00 | 0.00 | 31,483 | 33,721 | 836 | 3,792 | 0.00 | 0.00 | 31,483 |
| # Installment loans | 20,200 | 0.010 | 0.112 | 0.00 | 0.00 | 2.00 | 33,721 | 0.041 | 0.230 | 0.00 | 0.00 | 2.00 |
| Installment loan limit (SEK) | 20,200 | 286 | 4,678 | 0.00 | 0.00 | 133,357 | 33,721 | 1,066 | 9,099 | 0.00 | 0.00 | 133,357 |
| # Credit lines | 20,200 | 0.075 | 0.322 | 0.00 | 0.00 | 2.00 | 33,721 | 0.304 | 0.565 | 0.00 | 0.00 | 2.00 |
| Credit line balance (SEK) | 20,200 | 997 | 7,378 | 0.00 | 0.00 | 87,165 | 33,721 | 3,071 | 12,408 | 0.00 | 0.00 | 87,165 |
| log(# Arrear receipts+1) | 20,200 | 0.129 | 0.480 | 0.00 | 0.00 | 3.04 | 33,721 | 0.402 | 0.771 | 0.00 | 0.00 | 3.04 |
| I(Arrears>0) | 20,167 | 0.016 | 0.126 | 0.00 | 0.00 | 1.00 | 33,674 | 0.050 | 0.218 | 0.00 | 0.00 | 1.00 |
| | | | | | | | | | | | | |
| I(Wage>0) | 4,590 | 0.776 | 0.417 | 0.00 | 1.00 | 1.00 | 27,306 | 0.795 | 0.404 | 0.00 | 1.00 | 1.00 |
| I(Wage>0) (t+1) | 4,659 | 0.823 | 0.382 | 0.00 | 1.00 | 1.00 | 27,901 | 0.840 | 0.367 | 0.00 | 1.00 | 1.00 |
| log(Wage+1) (t) ('00 SEK) | 4,590 | 4.880 | 2.816 | 0.00 | 6.02 | 8.43 | 27,306 | 5.075 | 2.765 | 0.00 | 6.23 | 9.59 |
| log(Wage+1) (t+1) ('00 SEK) | 4,659 | 5.339 | 2.680 | 0.00 | 6.46 | 9.10 | 27,901 | 5.518 | 2.613 | 0.00 | 6.62 | 9.59 |
| log(Income+1) (t) ('00 SEK) | 5,292 | 4.003 | 2.953 | 0.00 | 5.34 | 8.93 | 31,033 | 4.230 | 2.935 | 0.00 | 5.61 | 10.77 |
| log(Income+1) (t+1) ('00 SEK) | 5,243 | 4.470 | 2.913 | 0.00 | 5.87 | 9.16 | 30,949 | 4.718 | 2.860 | 0.00 | 6.10 | 10.76 |
| Credit score | 2,527 | 33.37 | 38.071 | 0.71 | 8.06 | 100 | 14,921 | 29.86 | 36.38 | 0.34 | 7.46 | 99.89 |
| log(Credit score) | 2,527 | 2.732 | 1.333 | 0.54 | 2.20 | 5 | 14,921 | 2.624 | 1.292 | 0.29 | 2.14 | 4.61 |
| Credit limit | 2,527 | 10,243 | 32,807 | 0.00 | 0.00 | 426,300 | 14,921 | 9,931 | 36,676 | 0.00 | 0.00 | 2,031,285 |
| log(Credit limit+1) | 2,527 | 2.920 | 4.293 | 0.00 | 0.00 | 13 | 14,921 | 3.146 | 4.243 | 0.00 | 0.00 | 14.52 |
| | | | | | | | | | | | | |
| Total credit (pawn + mainstream) | 10,153 | 1,545 | 9,076 | 0 | 0 | 88,957 | 16,956 | 4,981 | 15,698 | 0 | 0 | 88,957 |
| Total # of defaults (pawn + arrears) | 10,047 | 0.038 | 0.239 | 0 | 0 | 9 | 16,765 | 0.085 | 0.298 | 0 | 0 | 4 |

## Appendix Table A2. County-Level Regressions: Pawn Credit Outcomes

This table shows that increased access to alcohol causally increases pawn credit take-up and the default risk. The table shows the coefficient $\beta_1$ from:

$$Credit\ TakeUp_{county,age,time} = \beta_1 Treated_c * Eligible_{agegroup} * Post_t + \beta_2 Treated_{county} * Post_{time}$$
$$+\beta_3 Eligible_{age} * Post_{time} + \beta_4 Eligible_{age} + \omega_{county} + \omega_{time} + \varepsilon_{county,age,time}$$

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: | # New loans | log(# New loans + 1) | Loan size | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| (i) DD estimate | (1) | (2) | (3) | (4) | (5) |
| Post × Treated | 52.4*** | 0.812*** | 69,200** | –3.2 | 6.5 |
| | (11.9) | (0.100) | (34,200) | (10.3) | (4.6) |
| Bootstrapped std. errors | (24.3) | (0.452) | (53,400) | (20.6) | (6.3) |
| | | | | | |
| County FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes |
| | | | | | |
| (ii) DDD estimate | (6) | (7) | (8) | (9) | (10) |
| Eligible × Post × Treated | 90.1*** | 0.658*** | 150,000* | –3.6 | 6.2 |
| | (28.0) | (0.232) | (78,300) | (23.0) | (10.8) |
| Bootstrapped std. errors | (27.6)** | (0.372) | (61,300)* | (18.8) | (5.9) |
| | | | | | |
| Pre-period mean | 234 | 4.34 | 425,000 | | |
| Effect | 38% | 15% | 35% | | |
| | | | | | |
| County × Year FE | Yes | Yes | Yes | Yes | Yes |
| Month FE | Yes | Yes | Yes | Yes | Yes |
| | | | | | |
| Observations | 2,100 | 2,100 | 2,100 | 2,100 | 2,100 |
| $R^2$ | 0.738 | 0.590 | 0.537 | 0.260 | 0.551 |
| # Counties | 10 | 10 | 10 | 10 | 10 |

## Appendix Table A3. Convenience Shopping 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 old individuals to represent non-retirees, and 65–75 year old individuals to represent retirees. For Panel B, we create our unemployed subsample by taking our sample of 20–65 year old individuals and determining from our data whether they receive income from work. The table shows the coefficient $\beta_1$:

$$Credit\ TakeUp_{individual,time} = \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t$$
$$+\beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \omega_{individual} + \omega_{time*county} + \varepsilon_{i,ndividual,time}$$

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

### Panel A: Individuals on Retirement

| Dependent variable: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Retired × Post × Treated | -0.002 | -0.001 | 0.002 | -0.003 | -0.003 |
| | (0.008) | (0.005) | (0.039) | (0.002) | (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: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Unmployed × Post × Treated | -0.010 | -0.005 | -0.035 | 0.008*** | 0.000 |
| | (0.007) | (0.004) | (0.033) | (0.002) | (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 |

## Appendix Table A4. 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: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Eligible × Post × Treated | 0.025* | 0.014* | 0.102 | -0.007 | 0.009** |
| | (0.013) | (0.008) | (0.064) | (0.004) | (0.004) |
| County × 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

| | Credit cards | | Installment loans | | Credit lines | | Performance | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Number | Balance | Number | Limit | Number | Balance | log(# Arrears+1) | I(Arrear>0) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Eligible × Post × Treated | 0.023*** | 147*** | -0.001 | 20 | -0.0179** | 381*** | 0.0026 | 0.0158*** |
| | (0.004) | (44) | (0.001) | (61) | (0.0077) | (144) | (0.0052) | (0.0050) |
| County × 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 | 0.001 | 0.002 | 0.019 | 0.008 | 0.041 | 0.008 |
| # Individuals | 28,342 | 28,342 | 28,342 | 28,342 | 28,342 | 28,342 | 28,342 | 28,295 |

## Appendix Table A4. Border County Exclusion (Cont.)

### Panel C: Labor Market Outcomes

| | I(Wage>0) (t) | I(Wage>0) (t+1) | log(Wage+1) (t) | log(Wage+1) (t+1) | log(Wage+1) (t) | log(Wage+1) (t+1) |
|---|---|---|---|---|---|---|
| (i) DD estimate (20-25) | (1) | (2) | (3) | (4) | (5) | (6) |
| postXtreat | -0.005 | -0.020** | -0.013 | -0.144** | 0.004 | -0.122** |
| | (0.011) | (0.010) | (0.069) | (0.063) | (0.059) | (0.057) |
| | | | | | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| | | | | | | |
| Observations | 37,463 | 37,413 | 37,463 | 37,413 | 41,176 | 40,676 |
| $R^2$ | .0207 | .00869 | .0592 | .0295 | .0632 | .0301 |
| # Individuals | 14,733 | 14,669 | 14,733 | 14,669 | 15,746 | 15,630 |
| | | | | | | |
| (ii) DDD estimate (18-25) | (7) | (8) | (9) | (10) | (11) | (12) |
| Eligible × Post × Treated | 0.010 | -0.040 | -0.039 | -0.145 | -0.108 | -0.033 |
| | (0.036) | (0.032) | (0.204) | (0.180) | (0.152) | (0.145) |
| | | | | | | |
| County FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Calendar month FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Individual FE | Yes | Yes | Yes | Yes | Yes | Yes |
| County x Calendar month FI | Yes | Yes | Yes | Yes | Yes | Yes |
| | | | | | | |
| Observations | 47,587 | 47,813 | 47,587 | 47,813 | 52,707 | 52,083 |
| $R^2$ | .0358 | .012 | .0915 | .039 | .0942 | .0385 |
| # Individuals | 17,195 | 17,175 | 17,195 | 17,175 | 18,356 | 18,241 |

## Appendix Table A5. 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: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Eligible × Post × Treated | 0.0287** | 0.0158** | 0.1250** | -0.0015 | 0.0036 |
| | (0.0111) | (0.0066) | (0.0545) | (0.0035) | (0.0035) |
| County × 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 |
| $R^2$ | 0.007 | 0.008 | 0.008 | 0.029 | 0.001 |
| # Individuals | 43,478 | 43,478 | 43,478 | 43,478 | 43,478 |

### Panel B: Mainstream Credit Outcomes

| | Credit cards | | Installment loans | | Credit lines | | Performance | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Number | Balance | Number | Limit | Number | Balance | log(# Arrears+1) | I(Arrear>0) |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Eligible × Post × Treated | 0.014*** | 105*** | -0.001 | 31 | -0.011* | 194* | 0.003 | 0.003 |
| | (0.004) | (35) | (0.001) | (60) | (0.007) | (116) | (0.005) | (0.005) |
| County × 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 | 299,812 |
| $R^2$ | 0.017 | 0.007 | 0.001 | 0.002 | 0.016 | 0.007 | 0.051 | 0.010 |
| # Individuals | 39,660 | 39,660 | 39,660 | 39,660 | 39,660 | 39,660 | 39,660 | 39,607 |

## Appendix Table A6. Varying Clustering Levels

This table shows the results of running our main test where standard error clustering is done at different geographical levels. We present results with clustering at the individual level, parish level, and at the municipality level. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

### Panel A: Pawn Credit Market

| Dependent variable: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Eligible × Post × Treated | 0.026** | 0.014** | 0.101* | -0.005 | 0.005 |
| Individual clustering level | (0.012) | (0.007) | (0.058) | (0.004) | (0.004) |
| # Clusters | 38,320 | 38,320 | 38,320 | 38,320 | 38,320 |
| | | | | | |
| Eligible × Post × Treated | 0.026** | 0.014** | 0.101* | -0.005 | 0.005 |
| Parish clustering level | (0.008) | (0.005) | (0.039) | (0.005) | (0.005) |
| # Clusters | 1,069 | 1,069 | 1,069 | 1,069 | 1,069 |
| | | | | | |
| Eligible × Post × Treated | 0.026** | 0.014** | 0.101* | -0.005 | 0.005 |
| Municipality clustering level | (0.009) | (0.005) | (0.051) | (0.005) | (0.004) |
| # Clusters | 293 | 293 | 293 | 293 | 293 |

### Panel B: Mainstream Credit Market

| | Credit cards | | Installment loans | | Credit lines | | Performance | |
|---|---|---|---|---|---|---|---|---|
| Dependent variable: | Number | Balance | Number | Limit | Number | Balance | log(# Arrears+1) | I(Arrear>0) |
| Clustering level | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Eligible × Post × Treated | 0.020*** | 127*** | -0.001 | 45 | -0.014** | 326*** | 0.004 | 0.014*** |
| Individual clustering level | (0.004) | (41) | (0.001) | (61) | (0.006) | (120) | (0.005) | (0.005) |
| # Clusters | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 | 34,902 | 34,852 |
| | | | | | | | | |
| Eligible × Post × Treated | 0.020*** | 127*** | -0.001 | 45 | -0.014** | 326*** | 0.004 | 0.014*** |
| Parish clustering level | (0.006) | (52) | (0.002) | (88) | (0.008) | (151) | (0.006) | (0.007) |
| # Clusters | 1,068 | 1,068 | 1,068 | 1,068 | 1,068 | 1,068 | 1,068 | 1,066 |
| | | | | | | | | |
| Eligible × Post × Treated | 0.020*** | 127*** | -0.001 | 45 | -0.014** | 326*** | 0.004 | 0.014*** |
| Municipality clustering level | (0.006) | (50) | (0.002) | (88) | (0.008) | (127) | (0.005) | (0.006) |
| # Clusters | 292 | 292 | 292 | 292 | 292 | 292 | 292 | 292 |

## Appendix Table A7. 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

| Dependent variable: | # New loans | log(# New loans+1) | log(Loan size+1) | # Defaults | # Rollovers |
|---|---|---|---|---|---|
| Age | (1) | (2) | (3) | (4) | (5) |
| (i) DD estimate: Post × Treated | | | | | |
| 20-21 | 0.010** | 0.0064** | 0.061** | 0.024*** | -0.0002 |
| 20-22 | 0.012*** | 0.0080*** | 0.078*** | 0.025*** | -0.0000 |
| 20-23 | 0.013*** | 0.0083*** | 0.080*** | 0.024*** | 0.0018 |
| 20-24 | 0.016*** | 0.0099*** | 0.092*** | 0.024*** | 0.0013 |
| 20-25 | 0.014*** | 0.0086*** | 0.081*** | 0.024*** | 0.0026* |
| 20-26 | 0.013*** | 0.0080*** | 0.024*** | 0.074*** | 0.0024* |
| 20-27 | 0.013*** | 0.0081*** | 0.023*** | 0.074*** | 0.0022 |
| | (6) | (7) | (8) | (9) | (10) |
| (ii) DDD estimate: Eligible × Treated × Post | | | | | |
| 20-21 | 0.030** | 0.017** | -0.0047 | 0.129** | 0.0048 |
| 20-22 | 0.028** | 0.015** | -0.0049 | 0.119* | 0.0043 |
| 20-23 | 0.026** | 0.014** | -0.0054 | 0.106* | 0.0039 |
| 20-24 | 0.029** | 0.015** | -0.0047 | 0.114* | 0.0031 |
| 20-25 | 0.026** | 0.014** | -0.0048 | 0.101* | 0.0049 |
| 20-26 | 0.026** | 0.014** | -0.0043 | 0.096* | 0.0041 |
| 20-27 | 0.025** | 0.013** | -0.0048 | 0.087 | 0.0048 |

## Appendix Table A7. Sensitivity to Eligible Age Cut-Off (Cont.)

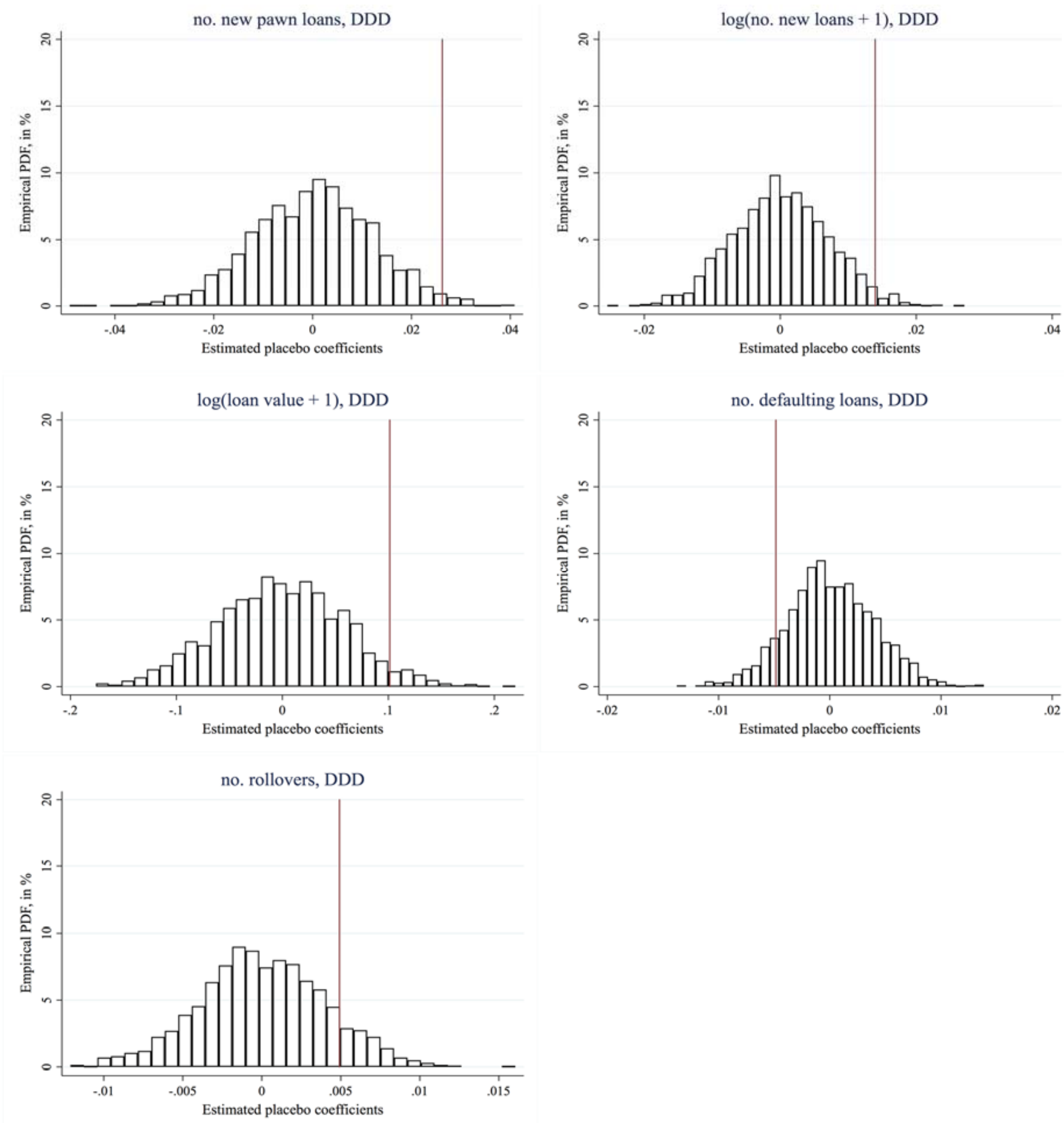
### Panel B: Mainstream Credit Outcomes

| Dependent variable: | Credit cards | | Installment loans | | Credit lines | | Performance | |
|---|---|---|---|---|---|---|---|---|
| | Number | Balance | Number | Limit | Number | Balance | log(# Arrears+1) | I(Arrear>0) |
| Age | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| (i) DD estimate: Treated × Post | | | | | | | | |
| 20-21 | 0.024*** | 150*** | -0.0005 | 67 | -0.007* | 200*** | 0.011*** | 0.13*** |
| 20-22 | 0.032*** | 180*** | -0.0013 | 78 | -0.015*** | 340*** | 0.014*** | 0.16*** |
| 20-23 | 0.033*** | 190*** | -0.0004 | 150** | -0.020*** | 380*** | 0.016*** | 0.18*** |
| 20-24 | 0.033*** | 200*** | -0.0012 | 130** | -0.020*** | 460*** | 0.018*** | 0.18*** |
| 20-25 | 0.031*** | 190*** | -0.0013 | 120** | -0.025*** | 470*** | 0.020*** | 0.19*** |
| 20-26 | 0.024*** | 150*** | -0.0024* | 81 | -0.031*** | 440*** | 0.021*** | 0.20*** |
| 20-27 | 0.022*** | 150*** | -0.0033** | 40 | -0.032*** | 450*** | 0.021*** | 0.21*** |
| | (9) | (10) | (11) | (12) | (13) | (14) | (15) | (16) |
| (ii) DDD estimate: Eligible × Treated × Post | | | | | | | | |
| 20-21 | 0.023*** | 150*** | -0.0018 | 19 | -0.0054 | 150** | 0.004 | 0.048*** |
| 20-22 | 0.026*** | 160*** | -0.0014 | 18 | -0.0096 | 250*** | 0.008 | 0.053*** |
| 20-23 | 0.024*** | 160*** | 0.0002 | 83 | -0.012* | 270*** | 0.010** | 0.047*** |
| 20-24 | 0.023*** | 140*** | -0.0003 | 90 | -0.011* | 320*** | 0.012*** | 0.044*** |
| 20-25 | 0.020*** | 130*** | -0.0008 | 48 | -0.013** | 320*** | 0.013*** | 0.047*** |
| 20-26 | 0.015*** | 100*** | -0.0016 | -0.2 | -0.016** | 300** | 0.014*** | 0.048*** |
| 20-27 | 0.014*** | 100*** | -0.0018 | -24 | -0.015** | 300** | 0.013*** | 0.052*** |

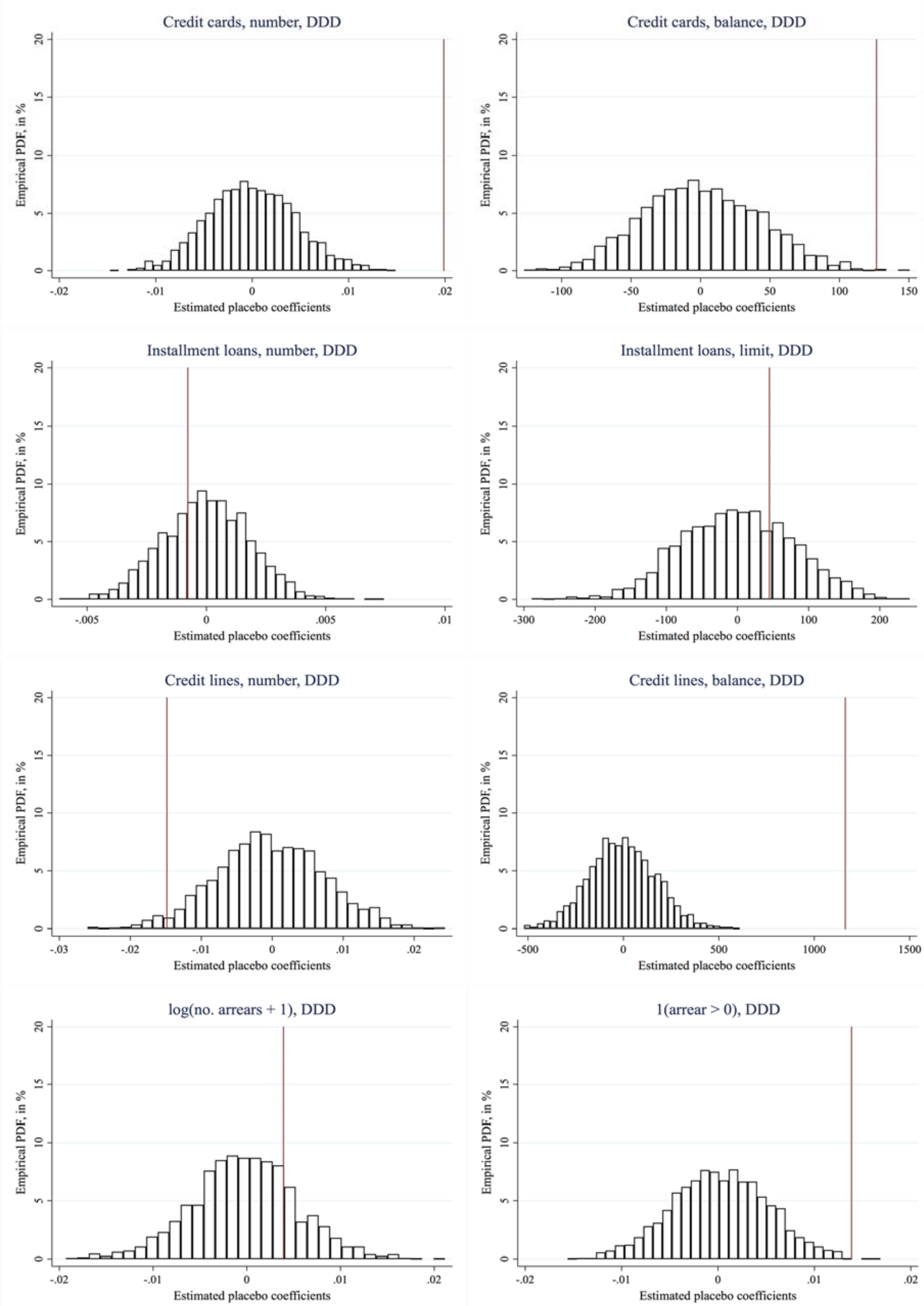
### Panel C: Labor Market Outcomes

| Dependent variable: | I(Wage>0) | | log(Wage+1) | | log(Income+1) | |
|---|---|---|---|---|---|---|
| | (t) | (t+1) | (t) | (t+1) | (t) | (t+1) |
| Age | (1) | (2) | (3) | (4) | (5) | (6) |
| (i) DD estimate: Treated × Post | | | | | | |
| 20–21 | –0.031 | –0.023 | –0.21 | –0.15 | –0.18 | –0.01 |
| 20–22 | –0.000 | –0.048*** | –0.05 | –0.28*** | –0.02 | –0.22** |
| 20–23 | –0.016 | –0.035** | –0.11 | –0.21** | –0.06 | –0.20*** |
| 20–24 | –0.012 | –0.019* | –0.06 | –0.11 | –0.02 | –0.13** |
| 20–25 | –0.003 | –0.016* | –0.02 | –0.12* | –0.00 | –0.12** |
| 20–26 | 0.001 | –0.011 | –0.01 | –0.09 | 0.01 | –0.09* |
| 20–27 | 0.002 | –0.006 | 0.01 | –0.06 | 0.03 | –0.08* |
| | (7) | (8) | (9) | (10) | (11) | (12) |
| (ii) DDD estimate: Eligible × Treated × Post | | | | | | |
| 20–21 | –0.040 | –0.019 | –0.31 | –0.09 | –0.28 | 0.12 |
| 20–22 | –0.018 | –0.054 | –0.18 | –0.23 | –0.19 | –0.07 |
| 20–23 | –0.022 | –0.044 | –0.18 | –0.15 | –0.18 | –0.07 |
| 20–24 | –0.015 | –0.032 | –0.12 | –0.08 | –0.14 | –0.03 |
| 20–25 | –0.008 | –0.030 | –0.10 | –0.10 | –0.13 | –0.03 |
| 20–26 | –0.006 | –0.027 | –0.09 | –0.08 | –0.12 | –0.03 |
| 20–27 | –0.007 | –0.025 | –0.08 | –0.08 | –0.11 | –0.03 |

## Appendix Figure 1. Distribution of Placebo Estimates: Pawn Credit Market



**Appendix Figure 2. Distribution of Placebo Estimates: Mainstream Credit Market**



**Appendix Figure 3. Distribution of Placebo Estimates: Labor Market**