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

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ABSTRACT

Increased availability of alcohol may 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 serving as commitment devices, expanded opening hours led to higher alcohol consumption and greater consumer credit demand, default, negative consequences in the labor market, and increase in crime on Saturdays. Our calculation shows that the effects of alcohol consumption on indebtedness could amount to 4.4 times the expenditure on alcohol.

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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., Thaler and Shefrin (1981), Laibson (1997), Hoch and Loewenstein (1991), Loewenstein and Prelec (1992), O'Donoghue and Rabin (1999b), O'Donoghue and Rabin (1999a), O'Donoghue and Rabin (2000), Gruber and Köszegi (2001)). For example, a person with present-biased time preferences might plan to skip dessert but change his mind once he sees the menu. At its core, present-biased time preferences means that people put excessive weight on the present experience. It thus could be a driving force behind widespread household borrowing (Campbell (2006)), as unplanned consumption today must come at the expense of consumption tomorrow. Furthermore, certain consumption items, alcohol included, have a spillover "multiplier effect," that is, their consumption may have further consequences down the road due to subsequent poor decision making. For example, in the case of alcohol, consumption could increase the likelihood of buying cigarettes (Burton and Tiffany (1997); Room (2004)), more drinking with one's peers (Fletcher (2012)), road accidents (Wagenaar et al. (2000); Levitt and Porter (2001)), and job losses (Mullahy and Sindelar (1996)). While the link between alcohol and decision making, especially among low-income populations, has been of interest to economists and regulators going back to at least Fisher (1927), there is little evidence about the causal effects of increased access to alcohol on financial wellbeing. One exception is Schilbach (2018), who conducts a field experiment with cab drivers in India and finds that incentivizing sobriety leads to an increase in savings by twice as the amount saved on alcohol expenditures. Still, the effect of impulsive consumption as well as the effects of alcohol consumption on financial decision-making and wellbeing among low-income population are not well understood.

To explore the effects of impulsive consumption of alcohol on individuals' financials, we analyze the results of a nationwide experiment in Sweden under which offsite liquor stores in some counties extended their operating hours into the weekend. The experimenters concluded that offsite alcohol sales increased in the treated counties following the experiment (Norström and Skog (2003)). Our study documents that the increase in alcohol consumption was harmful to individuals' financial wellbeing through higher indebtedness and a greater likelihood of default, as well as having negative consequences in the labor market. Our results imply 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 domains (e.g., labor) beyond the mechanical increase in expenditures. Given that access to alcohol is regulated in many countries around the world, our findings are important for the evaluation of the welfare consequences of such policies.

In Sweden, the sale of alcohol for offsite 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 in six of Sweden's 21 counties. The experiment was set up and evaluated by Swedish social scientists (Norström and Skog (2003)). They found that alcohol purchases in the treated counties increased by 4.0% on average (Norström and Skog (2005), Grönqvist and Niknami (2014)). This translates into an average annual increase in alcohol purchases of approximately 112 Swedish Kronor (SEK) (11 USD) per person.[1] Because early cross-section evaluations

[1]According to statistica.com, the total revenue from off-premise alcohol sales in Sweden in 2000 was 24.7 billion SEK. Norström and Skog (2005) and Grönqvist and Niknami (2014) reported an increase in alcohol sales of 3.7% and 4%, respectively this translates into about 900 million SEK in additional sales.

of the trial initially did not reveal material negative health or crime consequences, in July 2001 the extended-opening hours policy was implemented throughout Sweden.

The Swedish experiment allows us to test the effects of an increase in the supply of alcohol on the financial wellbeing of individuals with present bias. Our identifying assumption is that rational individuals can plan their consumption ahead of time and shop when the liquor stores are open. Because they can store alcohol for future use, the store's opening hours do not affect their consumption patterns. (A similar assumption is made in other studies that examine blue laws, e.g., Bernheim et al. (2016), Hinnosaar (2016)). In contrast, present-biased individuals underestimate their future (impulsive) demand for alcohol and therefore do not buy alcohol in advance. Thus, their consumption depends on the store's opening hours. If the store is open, they consume alcohol; if it is closed, they cannot purchase alcohol 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 alcohol consumption on the weekends.

Our empirical strategy is based on both double- and triple-difference specifications (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. In line with findings in the alcohol literature (Guttormsson and Grondahl (2017)) our results show that 20–25-year-olds, the youngest age group who could legally purchase alcohol, is the cohort most affected by increased access to alcohol. To verify that these results for the young are not confounded by unobserved differences between people who choose to live in the various counties or by county-level trends, we use a triple-difference strategy. Specifically, we exploit the fact that while people in the 18–19 age group are not allowed to buy alcohol anywhere in Sweden, they are still allowed to borrow. We use a triple-diff specification in which 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. We find that our double-diff results generally hold up in the triple-diff specifications.

Our main analysis focuses on the pawn and mainstream credit markets. We document that total credit balances (pawn and mainstream credit) increased by an average of 490 SEK (about 49 USD), or 10.5% relative to the pre-experiment mean.[2] We also test for loan performance and find some evidence of an increase in default. Looking at specific credit markets, we document that in the pawn market the expansion in opening hours led to an increase of 20% in the number of new pawn loans as well as a similar increase in loan sizes. We also test for loan performance and find deterioration in performance in some of the empirical specifications.

In the mainstream credit market, we explore the effects of increased liquor store hours on credit cards, installment loans (used to purchase large retail items), and credit lines. We document an 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 in balances. There is no increase in the number of credit lines issued, but we find that balances increased by 10%, on average. Furthermore, credit performance deteriorated, with the likelihood of default increasing by up to 8% (relative).

To evaluate the impact of alcohol consumption on the financials of consumers, we compare the increase in indebtedness to the expenditure on alcohol. While the increase in total credit balances was 490 SEK, the calculated increase in direct alcohol consumption was about 112 SEK. This results in a multiplier effect of up to 4.4

[2]The credit balance of the double difference is 43 SEK (pawn) + 118 SEK (credit cards) + 329 SEK (credit lines) = 490 SEK total increase in credit. The preperiod mean of the treated = 215 SEK (pawn) + 951 SEK (credit cards) + 3488 SEK (credit lines) = 4654 SEK total preperiod mean. So relative increase for the treated population 20–25 year olds = 490/4654 = 10.5%.

(490/112), meaning that every 1 SEK spent on alcohol translates into a 4.4 SEK increase in indebtedness. The fact that credit balances increased by more than the direct consumption cost indicates that the effects of alcohol consumption spilled over to other areas. This could occur for mundane reasons of complementary consumption to alcohol (e.g., greater appetite for Swedish meatballs) or could be the result of poor decision making on other dimensions. For example, as discussed above, prior research has found that alcohol consumption increases the likelihood of road accidents, injuries, and job loss. To the best of our knowledge, the only study that quantifies the multiplier effect in this literature is Schilbach (2018), who reports a multiplier of at least 2 in the context of Indian cab drivers.

To better understand the indirect effect of alcohol consumption on individuals' financial wellbeing, we decision-making-related consequences of drinking. First, we explore effects in the labor market. We match annual tax records to people in our main sample and test whether the greater availability of alcohol led individuals to lose their jobs or earn lower income. Indeed, young individuals in the treated counties were more likely to be unemployed and more likely to depend on welfare. Second, we show that alcohol-related crime, i.e., drunk driving and assaults, increased on Saturdays among the treated population in the post-period, especially among men. Beyond supporting the causal link, this finding demonstrates that the increase in alcohol consumption could result in further expenditure due to impaired judgement.

We further establish the causal link between the increased availability of alcohol and indebtedness. We examine the timing of pawn borrowing. Specifically, impromptu alcohol purchases on the weekend should result in frequent liquidity shortages on the weekend and thus higher rates of pawn borrowing on Mondays (pawn shops are closed on weekends). We test this conjecture by exploiting the daily frequency of our pawn borrowing data set and find that 24% to 27% of the increase in

borrowing takes place on Mondays. This result also supports the idea that alcohol purchases are impulsive rather than planned, as there otherwise would not have been a sharp increase in pawn borrowing following the weekend. Furthermore, we document an increase in alcohol-related mental and behavioral disorders in the treatment areas following the introduction of the extended opening hours.

We also explore some alternative explanations for our results. First, we investigate whether the results are driven by a few outliers (alcoholics) and whether the increase in the demand for credit is widespread 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 young individuals. Second, we examine the concern that the increase in alcohol consumption was driven by latent demand by busy people who could not shop during the week prior to the experiment. We test this possibility by comparing the indebtedness of people who have more time at hand the unemployed to employed and the nearly retired to the retirees in close age groups. Despite having sufficient statistical power, 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 greater indebtedness of relatively expensive consumer credit and higher default risk for the young. These outcomes are driven by both direct and indirect effects of alcohol consumption, exposing young individuals to spillover effects in the labor market. Our results suggest that the restriction of liquor store opening hours could serve as a commitment device for consumers with self-control problems.

We contribute to the literature on three fronts. First, we present evidence of a 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 by psychologists and economists. In psychology, researchers have 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; its availability triggers unplanned consumption and distorts consequent decision making. Banerjee and Mullainathan (2010) and Bernheim et al. (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 because they take up a large fraction of their disposable income. Our study presents supportive evidence that, indeed, an increase in the availability of alcohol triggers increased consumption. Additionally, we provide new evidence about the multiplier effect of alcohol consumption on spending and a spillover effect on the labor market from a nationwide experiment (as opposed to small-scale studies). The latter finding is in line with previous studies that find that alcohol impairs decision making and is correlated with reduced productivity (e.g., Blum et al. (1993), Jones et al. (1995), Fisher et al. (2000), McFarlin and Fals-Stewart (2002)).

Our study is complementary to Schilbach (2018). 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 at least two. Compared with Schilbach's carefully-controlled test, 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 vs. the Swedish low-income population). Both studies provide evidence for and quantify the indirect effect of alcohol consumption 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 has found mixed evidence about the nature of this correlation. Meier and Sprenger (2010) and Skiba and Tobacman (2008) 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 et al. (2013) and Carvalho et al. (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.[3] 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 an increase in defaults. We also provide evidence on the indirect consequences of alcohol consumption by young adults on the margins of mainstream labor and credit markets.

Third, our study adds to the debate in the literature about the effectiveness of commitment devices. Researchers have proposed that commitment mechanisms may help individuals stick to their planned consumption path (e.g., Laibson (1997), Thaler and Benartzi (2004)). Hinnosaar (2016) proposes that limited store opening hours can serve as such a commitment mechanism. Yet prior studies have found conflicting evidence about the effectiveness of restricting consumer access to temptation goods. Some studies find that store opening hours help reduce impulsive consumption (fast food: Currie et al. (2010); alcohol: Norström and Skog (2003), Norström and Skog (2005)). In contrast, Bernheim et al. (2016) find that changes in the opening hours of

[3]See Schilbach et al. (2016) for a review of the literature relating poverty and behavioral biases.

off-premise liquor stores in the United States on Sundays do not significantly affect alcohol consumption. Moreover, the effectiveness of self-enforced precommitment 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 time-inconsistent 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 discuss 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),[4]

$$U_0 = C_0 + \beta\delta C_1 + \beta\delta^2 C_2 + ...\beta\delta^T C_T.$$

This model encompasses two cases, depending on the value of β. When $\beta = 1$, consumers are rational and have exponential discounting. But when $\beta < 1$, their preferences are dynamically time-inconsistent (hereafter: 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 consumer 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 t_1, the consumer values C_1 versus C_2 in this way:

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

[4] Laibson (1997) is not the only theoretical framework to describe the decision making process which appear myopic. Gruber and Köszegi (2001) argue that addiction is resulting from present bias where individuals are heavily discounting the future consequences (e.g., difficulty of quitting). Gabaix and Laibson (2017) present an alternative framework in which individuals take into consideration uncertainty around future cash flows (or outcomes) when making decisions, and uncertain future events are discounted more heavily. Their model predicts that individuals who are unable to think carefully about an intertemporal tradeoff (i.e., view the future with greater uncertainty) will exhibit greater myopia in their decision making. Related to our work, this mechanism would apply to poorer population (cognitive load due to financial stress; see Mani et al. (2013) and Schilbach et al. (2016)) or people under the influence of alcohol (see Steele and Josephs (1990) and Giancola et al. (2010)).

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 to do so in the past, even though there is no new information.

2.2 Limited Opening Hours as a Commitment Device

Expanding opening hours of liquor stores into weekends should have differential effects on consumers depending on their ability to make plans and sustain them. Because alcohol can be stored at home at low cost and people generally buy alcohol frequently, unbiased consumers should be able to adjust their behavior relatively quickly to the opening 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 their level of consumption (Bernheim et al. (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, as 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 present-biased preferences, you will diverge from your plan and value drinking today again more than in the future. Thus, you will be tempted to buy alcohol. A closed store would thus 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 relaxed.

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 also be 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 a higher likelihood of financial distress or default in the future.

The indirect effects of greater alcohol consumption can be seen through other consumption and nonconsumption 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. In regard to nonconsumption decisions, alcohol can lead to lower net income due to poor decision making. For example, alcohol consumption may increase the likelihood of road accidents and injuries (Wagenaar et al. (2000), Levitt and Porter (2001)). Additionally, alcohol consumption may affect work performance (Frone (2006)), 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

We start by characterizing the Swedish alcohol market to give context to the experiment we analyze. In Figure 1, we present annual alcohol consumption trends over time for the Nordic countries (Sweden, Denmark, Norway, and Finland), as well as for the United Kingdom and the United States. The chart shows that, if anything, Swedish alcohol consumption levels are relatively low. In 1999, right before the experiment, Swedes consumed an average of 6 liters of pure alcohol per capita per year, compared to 8 in the United States. Among these six countries, Sweden ranks fifth, between the U.S. (fourth) and Norway (sixth). Hence, alcohol consumption rates in Sweden are not outside the norm for Western countries.

Next, we explore the cross-section of Swedish households. Because our analysis focuses on individuals with lower income levels, in Figure 2, Panel A, we explore how the share of alcohol expenditures changes over the income distribution. For this purpose, we obtained expenditure data from Statistics Sweden for the period of our experiment (1999–2001).[5] The results show that Swedish households spend, on average, 1.6% to 2.0% of their disposable income on alcohol. Households in the lower quintile of income spend about 2.0% of their disposable income on alcohol compared to 1.6%–1.7% among higher income households. To assess what fraction of people actually consume alcohol within each income decile, we plot the fraction of abstainers

[5]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 tracked their expenditures during a two-week period. Statistics Sweden also complemented the cash-journal data with a survey focused on larger expenditures covering longer time periods. Disposable income is computed using data from public registries and is used to balance the selection into the sample to achieve a representative sample of the total population. Expenditures are rescaled to annual level. We use data from 1999–2001 covering 4,688 households who responded out of 9,000 contacted (3,000 each year).

(measured as people who did not consume alcohol over a year) in Figure 2, Panel B. 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 attributed to an entire group of individuals is likely to be driven by the approximately 40% of individuals who actually consume alcohol. We also examine whether the composition of alcohol type varies across income deciles. Figure 2, Panel C, shows alcohol consumption by type (liquor, wine, and beer) and by income decile, using a later wave of expenditure data from 2003 to 2009.[6] The figure shows that the rate of expenditure on liquor is constant across income deciles. There is substitution from beer to wine as income deciles increase.

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 on-site (i.e., restaurants and bars are not included in the monopoly). In 2000, the state owned 420 stores named Systembolaget that 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 appear to be younger than 25 (Norström and Skog (2005), Grönqvist and Niknami (2014)).

[6]In 2003 the first wave of the survey Hushållens utgifter (HUT; Household Expenditures) 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. Pawnbrokers make fixed-term loan to consumers in exchange for collateral. The loan is provided solely based on the value of the collateral and does not depend on the borrower's credit quality.

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 credit depend on market interest rates as well factors like the borrower's creditworthiness, default risk, the bank's financing costs, and competition with other credit institutions. The banks' average deposit and lending rates have shown a clear downward trend since the early 1990s.[7]

3.4 Swedish Nationwide Experiment in Extending Store Hours

From 1981 to 2000, the state monopoly liquor stores were 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 to design and evaluate the experiment (Norström and Skog (2003)). 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. The sorting of counties into treatment and control was not random, as can be seen in Internet Appendix Table I. We devise a test for parallel pretrends in Section 5.2. As the experiment was launched by the Swedish government, the designers of the experiment made sure that there was no confounding legislation around that period that pertained to alcohol purchases (e.g., no change in the regulation or taxation of on-premises alcohol sales). Following the original researchers (Norström and Skog

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

(2003)), we also exclude the buffer counties from our analysis. The map in Figure 3 identifies the treatment, control, and buffer counties. At the time, nearly half of the total Swedish population lived in the treatment region.

Originally, 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 for 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 2001. 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 of 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. They documented 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 on Saturdays and 2pm on Sundays. A few years later, the results of the experiment were re-evaluated by Grönqvist and Niknami (2014), who used a richer data set 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 precommitment device, giving present-biased individuals access to alcohol that they would not have consumed had the liquor stores remained closed. Second, longer opening

hours could facilitate 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 opening hours. In our study, we address the different channels through which the relaxation of the opening 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.[8] An ideal experiment to identify this causal effect, therefore, would consider two identical groups of individuals, only one of which would have increased access to alcohol.

We use the variation in alcohol availability induced by the February 2000 Swedish experiment in two empirical approaches. The first empirical strategy is based on a difference-in-difference (double-diff) analysis that compares credit, default, and labor market behavior before and after the policy change and across treated and control counties. This analysis is performed for six different age cohorts, running from 18 to 65.

The second identification strategy exploits the age restriction on alcohol sales in Sweden. Specifically, individuals below the age of 20 are not allowed to purchase alcohol off-site in Sweden, but they are allowed to borrow and participate in the labor market. Hence, this group can serve as a control group in both treatment and control counties, allowing us to employ a within-county identification in a triple-difference strategy. This approach enables us to verify that the results found for the

[8]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).

young cohort in the double-diff approach are not driven by omitted variables and county-specific time trends.

Our specification is generally robust to cross-county shopping as we exclude the buffer counties from the sample (as did the original experiment designers). In the Internet Appendix we also verify that our results do not materially change when excluding the county bordering with Denmark (Skåne), which might allow easy cross-country shopping from abroad (Internet Appendix Table II to IV). We also exclude from the sample individuals who move across between counties (about 1.6% of the population) 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% of the total pawn-broking market, generously supplied this data set. This data set provides information on the transactions of 332,351 individuals who took out at least one pawn loan between 1999 and 2012. Around the years of the experiment, about 4% to 5% of the Swedish adult population every year borrowed at a pawnshop. This data set 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 data set with records from the mainstream credit data registry. This data set 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 from other government agencies, including the national enforcement agency (Kronofogden), which administers and executes private claims and all government claims. This data set contains bi-monthly snapshots of individual credit records from 1999 to 2001. Our first empirical strategy (double-diff) is to compare people in the treated counties to those in the control counties, before and after the experiment. We have bi-monthly data for 61,527 individuals in the control counties and for 102,855 individuals in the treatment counties. In the second empirical strategy (triple-diff), we add an age restriction: individuals enter the sample when they are 18, and leave the sample when they are 25. This sample contains 38,320 individuals.

Our data set has the advantage of greater detail than other data sets used in prior studies but also has a drawback. Because of data and regulatory constraints, our data construction is restricted to people who took out 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, particularly for the young population (18–25). Because borrowers must be at least 18 years old, we cannot avoid the look-ahead bias if we are interested in having a group of 18–19-year-old borrowers as part of our control sample. The look-ahead bias may affect 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 (our triple-diff 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 this look-ahead bias (see 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 annual frequency from 1998 to 2005 and include information on each individual's employment status. In our analysis, we explore the effects of the increased availability of alcohol on unemployment status, welfare dependence, pre-tax income, and the number of reported sick days.

Finally, we use additional datasets that contain additional information. Specifically, we merge our data with medical records registered by Swedish National Board of Health and Welfare (Social Styrelse), and with crime information from the Swedish conviction register administered by the National Council for Crime Prevention (BRÅ). These data are used to explore the effects of expanding the stores' operating hours on other aspects of individuals' lives.

4.2 Summary Statistics

We begin the empirical analysis by discussing select summary statistics for the treated population in the preperiod. Table 1 contains definitions of both the dependent and independent variables of interest. Table 2 provides the summary statistics for our outcome variables during the period before the experiment started (February 1999 to February 2000). Internet Appendix Table I presents comparative summary statistics for the sample used in our baseline analysis in the pre-treatment period.

Our sample is composed of people of relatively low socioeconomic status. Table 2, Panel A, presents summary statistics about pawn-borrowing. The average number of new pawn loans is 0.11 and the default rate is 0.7% per month in the preperiod. 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 4.1%. Furthermore, a large share of this population does not have a credit card; the mean number of credit cards is 0.147, with a mean revolving credit card balance of 680 SEK (68 USD), which constitutes 10% of their mean monthly total income, registered by the tax authorities at that time. 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 C.

5 Main Results

5.1 Empirical Implementation

We begin by exploring the effects of expanded liquor store opening hours on credit and labor markets. Specifically, we run the follow regression separately for six age cohorts: 18–19, 20–25, 26–35, 36–45, 46–55, and 56–65. We look at different cohorts separately in order to investigate the differential effects of the expansion in the supply of alcohol on different age groups, as there are different predictions for the effects on different cohorts. Young people (ages 18–19) are not allowed to buy alcohol off-site in Sweden; hence, we expect to see no effect for this cohort. In contrast, slightly older people (ages 20–25) are expected to be the most strongly affected by the increased access, as the alcohol literature shows that they are the most susceptible to the supply of alcohol (e.g., Guttormsson and Grondahl (2017)). Overall, there should be a discontinuity around 20 years of age.

We plot the coefficients β_1 and present standard errors for each cohort from our difference-in-difference OLS regressions:

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}, \tag{5.1}$$

where β_1, the coefficient of interest, measures the differential likelihood of the outcome variable $y_{i,t}$ between consumers living in the treated and control counties during both the pre- and post-periods. Standard errors are clustered at the individual level. We include individual (ω_i) and time (ω_t) fixed effects.[9] Because this double-diff specification does not allow within-county variation in the treatment, we cannot include county-time fixed effects. To mitigate this concern, we control for county-time-specific

[9] County fixed effects are omitted since they are subsumed by individual fixed effects.

GDP and employment growth.

As a preliminary assessment, we inspect the effect of the treatment on each age cohort. Figure 4 plots the distribution of β_1 from the regression estimating Equation (5.1). We examine the following credit market outcomes: number of pawn loans, pawn loan size, likelihood of default on a pawn loan within two months, number of credit cards, credit card balances, and likelihood of default on a credit card within two months. The figure shows that for all outcomes, there is no statistically significant effect for the 18–19-year-old treated group. Furthermore, there is a significant and positive increase in indebtedness among the 20–25-year-old treated group, and a typically smaller effect for older groups.

In a similar manner, Figure 5 plots the double-diff effects in the labor market for the various age cohorts. In this figure, we focus on the outcomes on both the extensive and intensive margins: the likelihood of being unemployed, welfare, pretax income, and the number of sick days. In a similar fashion to Figure 4, we observe no significant effect to the 18–19-year-old group, except for a decline in pretax income. Moreover, for most variables, we see a large effect among the 20–25-year-olds. The effect is generally weaker for older age groups.

Going forward, our tests focus on the two age groups for which the effects are predicted to be the sharpest: 18–19-year-olds (not eligible to buy alcohol) and 20–25-year-olds (most susceptible to alcohol consumption).

5.2 Pretrend Analysis

In our analysis, we treat the Swedish experiment of February 2000 as an exogenous event, orthogonal to the characteristics of the individuals in the treatment and control counties. To verify that the effects that we observe in the treatment counties relative

to the control counties are not driven by differences in the trajectories of the counties, we need to test whether the variables of interest evolved in a parallel manner in the period preceding the experiment. We provide graphical evidence in support of this identification assumption in Figures 7, 8, and 9. The top two panels of Figure 7 show the event-time evolution of β_τ and their 95% confidence intervals from the following model:

$$y_{i,t} = \Sigma_{t=-5}^{t=8} \beta_\tau Eligible_{i,t} * Treated_i * Period_t + \xi_1 Eligible_{i,t} * Treated_i + \xi_2 Eligible_{i,t} * Post_t + \xi_3 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t} \quad (5.2)$$

for the number of pawn loans the individual borrowed during the bi-monthly period, and for the number of credit cards individuals own. This specification also includes individual fixed effects (ω_i) and county-bi-monthly fixed effects (ω_{c*t}). Standard errors are clustered at the individual level. The x-axis shows event time bimonths, which are defined starting at zero in February 2000 when the Swedish government began opening liquor stores on Saturdays in some counties. For each bi-month τ, the figures show the coefficients β_τ, which reflect the difference between treatment and control and between eligible and non-eligible individuals in that particular bi-month. We follow Brown et al. (2016) and perform a Wald test to test the null hypothesis that the β_τ coefficients during the preperiod are jointly equal (i.e., $\beta_{(-5)} = \beta_{(-4)} = \cdots = \beta_{(-1)}$). Where β_τ captures the difference between consumers living in a treated versus control county, for each period (calendar-time bimonthly observations):[10]

$$y_{i,t} = \sum_{\tau=-5}^{8} \beta_\tau Period_t * Treated_i + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}. \quad (5.3)$$

[10] County fixed effects are omitted since they are subsumed by individual fixed effects.

Thus, the Wald test formally tests whether the parallel growth assumption holds. The p-values of this test are reported in the left corners of our pretrend figures. In support of our parallel growth assumption for the difference between the treatment and control counties, the estimates of the pre-treatment coefficients β_τ, for $\tau = -1...-5$, are jointly equal (i.e., we cannot reject the null with a p-value > 0.05).

Similar pretrend effects can be observed for pawn loan values, credit card balances, and the dummy variable equal to one when the consumer received a new arrear, 1(arrears >0) shown in the lower panels. Figure 8 presents the graphs for our labor market outcomes (annual frequency): the dummy variable equal to one if the consumer experienced at least one unemployment spell during the year, 1(unemployment >0), SEK received in welfare, pretax income in kronor and the number of sickdays. These graphs provide evidence consistent with the identification assumption of parallel trends. Finally, Figure 9 shows the graphs for drunk driving and assaults on Saturdays, where we plot the event-time (bi-monthly) evolution of β_τ and their 95% confidence intervals from the following model:

$$y_{i,t} = \Sigma_{t=-5}^{t=8} \beta_\tau Saturday_{i,t} * Treated_i * Period_t + \xi_1 Saturday_{i,t} * Treated_i + \xi_2 Saturday_{i,t} * Post_t + \xi_3 Saturday_{i,t} + \omega_i + \omega_{dayoftheweek} + \omega_{c*t} + \varepsilon_{i,t} \quad (5.4)$$

For both drunk driving and assaults on Saturdays the p-value of the Wald test confirms that we cannot reject the parallel trends assumption in the preperiod.

5.3 Credit Market

We use two approaches to measure the effects of the policy: double-diff and triple-diff specifications. The double-diff specification (see Equation (5.1)) measures the effect

on the 20–25 group in the treated counties in the post-period relative to the same group in the control counties and in the preperiod. The sample for this specification includes only the 20–25 group, and the identification of the effect comes from the credit behavior of the 20–25 group in the treated counties, where the control counties and the preperiod serve as the control group.

The triple-diff specification measures the effect on the 20–25 group in the treated counties in the post-period relative to the same group in the control counties and in the preperiod, as well as relative to the 18–19 group in both the treated and control counties and the pre- and post-periods. In this specification, the sample includes both the 18–19 and 20–25 populations. Relative to the double-diff specification, in the triple-diff specification, we augment the control group to also include the 18–19 group in the control observations so that the effect we report reflects the relative effect between the 18–19 group and the 20–25 group as well as between the pre- and post-periods and the treatment and control counties. We use the following specification:

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}, \quad (5.5)$$

where the variable of interest is the triple interaction. This specification also includes individual fixed effects (ω_i) and county-bi-monthly fixed effects (ω_{c*t}).

We begin by exploring the effects in the pawn and mainstream credit markets. Table 3 focuses on the pawn market. Odd-numbered columns present regression results from double-diff specifications, and the even-numbered columns report the corresponding results from the triple-diff specifications. Columns (1) and (2) show an increase in the extensive margin: 20–25-year-olds took out more pawn loans in the treated counties than their peers in the control countries (Column (1)) or 18–19-

year olds (who could not purchase alcohol) in the same counties (Column (2)). The effect is an economically significant increase of 19% relative to the preperiod mean. Columns (3) and (4) also show an increase in the intensive margin, that is, pawn loans became larger for the treated group. The magnitude is similar, about 19–20%.

As for the performance of pawn loans, the results are mixed. We find a significant and strong result in the double-diff specification (Column (5)), indicating a doubling in the number of defaulting loans. In contrast, there is no statistically significant result in the triple-diff specification (Column (6)). When considering pawn loan rollovers (Columns (7) and (8)), the point estimates are both positive, albeit statistically insignificant.

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, the odd-numbered columns present regression results from double-diff specifications, and the even-numbered columns report the corresponding results from triple-diff specifications. Columns (1)–(4) show an increase in the number of credit cards (10%–11% increase) and in the average credit card balance (12%–17% increase) among the treated group.

We detect little effect on installment loans. Installment loans are essentially credit provided when purchasing larger items, like the popular Billy bookcase and Dombås wardrobe sold at IKEA stores. This test can be viewed as a placebo because we do not expect the increase in alcohol availability to increase secured debt that is related to large purchases of durable goods. We observe no change in the number of installment loans (Columns (5) and (6)). Column (7) shows higher installment loan limits, but this result does not show up in the triple-diff specification (Column (8)).

The effects on credit lines are mixed. Table 5, Columns (1) and (2), show a decrease in the number of credit lines of 4%–8%. However, Columns (3) and (4)

show an increase in the average credit balance of credit lines of about 10%. As for credit performance, the treated group exhibits somewhat poorer performance. The likelihood of having any recorded arrear increases by 2% to 3% (Columns (5) and (6)). Overall, the analysis of the credit behavior of the treated group shows an increase in credit usage and deterioration in performance in both the pawn and mainstream credit markets.

5.4 Multiplier Effect of Alcohol Consumption

We next assess whether the increased availability of alcohol among the treated population impacted individuals beyond the higher spending on alcohol. In particular, we are interested in whether the increased alcohol consumption caused additional expenditure or financial consequences. For example, consumption of alcohol may be associated with additional purchases, road accidents, or loss of income. Schilbach (2018) documents a multiplier greater than 2. In his experimental setting, abstaining from alcohol consumption resulted in savings greater than twice the saving on mere alcohol expenditure.

To calculate the multiplier of alcohol consumption, we must first determine the increase in spending on alcohol that occurred when liquor store opening hours were expanded and then compare that figure to the observed increase in credit usage.

The increase in alcohol consumption can be estimated using past studies of alcohol consumption patterns in Sweden as well as the studies that analyzed the opening hours experiment. The Statistics Sweden (Statistiska centralbyrån) survey collects consumption information on different items. We use the survey covering the years 1999–2001, which collected expenditure data from about 4,800 people. The average annual spending on alcohol is provided in Internet Appendix Table V. The table

shows that the average spending for young people in the lowest income group (likely to be the population in our main sample) is about 2,800 SEK (about $280) a year.[11] Thus, an increase of 4% in their drinking translates into an increase of 112 SEK per capita per year for people in the 20–25 age cohort.[12] We obtain similar figures if we rely on aggregate data.[13]

Now, compare this estimation to our finding in Tables 3 to 5 that, on average, people ages 20–25 living in the treated counties increased their total debt balance by about 490 SEK (about $49). Comparing this amount to estimated amount spent on alcohol of 112 SEK suggests a multiplier effect of 4.4, which is consistent with the idea that increased alcohol consumption leads to poor decision making on other dimensions such as drinking and driving (Wagenaar et al. (2000), Levitt and Porter (2001)), lack of savings (Schilbach (2018)), or loss of income or jobs, as we report here. We also note that our estimate is larger in magnitude than that of Schilbach (2018), who reports a multiplier effect that is greater than 2 in a population of Indian cab drivers. One possibility is that the populations in the two studies are different (Indian

[11]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 an average of $349. Individuals in the 20–25 age range spend $368 per year.

[12]This calculation assumes that the increase in alcohol expenditure for the young is the equal to the average increase in alcohol sales, as found by Norström and Skog (2003) and Grönqvist and Niknami (2014). If, however, individuals at ages 20–25 increased their alcohol expenditure by more than the average then the multiplier that we calculate would be lower. It is unclear, however, whether the larger effect that we find for the young (e.g., on indebtedness) is generated by larger increase in spending on alcohol or by the fact that they are likely to be closer to their liquidity constraint than older cohorts.

[13]The total revenue from off-premise alcohol sales in Sweden in 2000 was 17.368 billion SEK (source: historical trends in Systembolaget's Responsibility Report for 2008). Norström and Skog (2005) and Grönqvist and Niknami (2014) report 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 2000, the Swedish population in the treatment counties was 3.822 million (Figure 3), approximately 75% of whom were between 20 and 80 years old, the population likely to drink (see https://www.cia.gov/library/publications/the-world-factbook/geos/sw.html) Hence, the average increase in alcohol consumption per capita in the treatment counties was 97 to 104 SEK per capita per year (330m SEK/(3.822m $\times$ 75%)).

cab drivers versus Swedish young people), e.g., Swedes are able borrow through pawn shops and credit cards and thus may be able to increase consumption more easily in response to greater availability of alcohol compared to Indian cab drivers.

5.5 Labor Market

Our results indicate that greater availability of alcohol leads to greater indebtedness by a magnitude greater than the pure increase in spending on alcohol. One channel through which alcohol could cause greater indebtedness is the labor market, where individuals' performance may deteriorate, causing them to lose income. We explore this channel by using annual employment and wage information from the Swedish tax authorities, filed by all Swedish residents beginning at age 18. As the tax filings have an annual frequency and the experiment started in the middle of 2000, we examine effects in both 2000 and 2001, and compare them to the preperiod of 1996 to 1999.[14]

Next, we conduct formal tests using both double-diff and triple-diff specifications for the 18–19 and 20–25 cohorts. In Table 6, we use the following variables: likelihood of being unemployed, welfare, pretax income, and the number of sick days. Except for the lower frequency of observations, the empirical specification is identical to that in previous tables. The odd and even columns, respectively, present results from double-diff and triple-diff specifications.

In general, the effects of the expanded opening hours on labor market outcomes are negative, albeit weak. All coefficient signs are in the expected direction, but only some of the results are statistically significant. Table 6 shows that the likelihood of

[14]The effects in the labor market themselves could be either direct or indirect. In particular, researchers have documented a direct channel of alcohol consumption resulting in reduced productivity at the workplace (e.g., Blum et al. (1993), Jones et al. (1995), Fisher et al. (2000), McFarlin and Fals-Stewart (2002)). There is also an indirect channel, where individuals have default flags on their credit records, which hurt the likelihood of hiring by potential employers who check their credit records (Bos et al. (2018); Cohen-Cole et al. (2016); Balance et al. (2016); Bartik and Nelson (2016)). In the current empirical setting, we cannot discriminate between the two channels.

unemployment increases for the treated group (Columns (1) and (2)). Furthermore, the average amount of welfare received increases (Columns (3) and (4)). On the intensive margin, the results are not statistically significant: Columns (5) and (6) show that income declined for the treated groups (t-statistic between 0.9 and 1.6), and Columns (7)–(8) show that sick days increased (t-statistic between 0.1 and 0.9).

Overall, we find modest, yet existent, effects in the labor market for the treated group. The effects might be small and estimated with noise because of the low (annual) frequency and the indirect channel.

5.6 Reckless Behavior: Evidence from Crime Records

The literature in behavioral sciences found a tight relation between alcohol availability, consumption, and the propensity to commit crime. The relation between the two may be due to the fact that alcohol consumption impair judgement and prompt violent actions (e.g., McClelland et al. (1972), Rush et al. (1986), Gliksman and Rush (1986)).

To explore if this relationship also holds for the individuals in our sample we analyze their incidence rate of crimes related to alcohol consumption. For this purpose we use the Swedish conviction register administered by the National Council for Crime Prevention (BRÅ), merged by Statistics Sweden to the individuals in our sample. The Swedish conviction register contains the complete records of all criminal convictions in Swedish district courts during the pre- and post-periods. The data includes information on type of crime as well as the sentence ruled by the court. While one conviction may involve several crimes (which we can observe), for ease of interpretation we focus on the primary crime. Based on statistics reported in Olseryd (2015) we focus our analysis primarily on crimes that have the highest incidence of being

commited while the perpetrator is intoxicated, i.e., drunk driving,[15] and assaults.[16] We also split the sample by gender as, it turns out, men are heavily overrepresented among criminal offenders in general and in particular under the influence of alcohol (see Olseryd (2015)).

An important advantage of the crime data for our purpose is the fact that the exact date of the crime is recorded.[17] This means that we can actually test whether crimes increased more on Saturday relative to the other days of the week in the treated counties and post-period. This detailed identification strategy is taken from Yörük and Lee (2018), who find an increase in crime rates in the U.S. on Sundays (but not on other days) following the legalization of Sunday alcohol sales. For this purpose we construct a daily panel for the individuals in our panel and use the specification:

$$Crime_{i,d,t} = \beta_1 Treated_i * Post_t * Saturday_{i,t} + \beta_2 Treated_i * Post_t + \beta_3 Saturday_{i,t} * Post_t + \beta_4 Saturday_{i,t} + \omega_i + \omega_{dow} + \omega_{c*t} + \varepsilon_{i,t}, \quad (5.6)$$

where the variable of interest is the triple interaction. This specification also includes individual fixed effects (ω_i), day-of-the-week fixed effects (ω_{dow}) and county-bi-monthly fixed effects (ω_{c*t}). The dependent variable is a dummy for whether an individual was convicted for a certain crime on a particular day.

We first look at the effects for different age cohorts. In Figure 6, we plot the

[15] Drunk driving is defined by Swedish Code of Statutes: Svensk författningssamling (SFS), law (1951:649) punishment for certain traffic violations, §4 drunk driving.

[16] Assaults are defined by the justice department's penal law: Brottsbalk (1962:700) t.o.m. SFS 2018:1745, Chapter 3; "Om brott mot liv och hälsa." we exclude crimes defined in §10 crime within a work environment (arbetsmiljöbrott)

[17] The exact day of the crime is in some cases unknown. For instance, it is not always clear what specific day a break-in occurred. In these cases, the court assigns a date based on an educated guess. However, since there exists no ambivalence about the exact date of an assault or drunk driving our results do not suffer from this error.

triple difference coefficients of Equation (5.6) above for the six age groups. While the charts show no significance increase in the alcohol-related crimes for the cohort of 18–19 year-olds, they do show an increase in assaults and drunk driving on Saturdays for 20–25 year-olds who are eligible to purchase alcohol. This result is in line with our previous result that the largest increase in indebtedness was for the 20–25 year-olds (Section 5.3). The results in Table 7 confirm the statistically-significant increase in assaults and drunk driving in the treated counties on Saturdays during the post-period in Columns (3) and (8), again strengthening the evidence for a causal link between the outcome (crime) and liquor stores' opening hours. For this population, the likelihood of assaults on Saturdays in the treatment counties increases by 0.051 percentage points (since coefficients in the Table 7 are multiplied by a 1000) in the post period, which reflects a relative increase of 90%, relative to the preperiod mean of 0.06 percent. Similarly, the likelihood of drunk driving on Saturdays increases by 0.029 percentage points, which is a relative increase of 88% relative to the pre period mean in the treatment counties of 0.03 percent. From Columns (4)-(5) and (9)-(10), we can see that these results are completely driven by young males, which is consistent with what is documented in the crime literature mentioned above.

Overall, we view this evidence as corroborative our previous results on two aspects. First, these findings are consistent with the idea that weekend-drinking increased due to the expansion in the operating hours of liquor stores. Second, these results suggest that individuals may have suffered greater unplanned expenditures because of the increase in alcohol consumption due to the increased access to alcohol on Saturday.

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. In this section, we provide additional evidence that support the mechanism that we propose: that extending the opening hours of liquor stores on weekends is equivalent to relaxing a commitment mechanism for some of the population. First, we examine the timing of the increase in borrowing. We expect that the demand for credit will peak right after the weekend (on Monday) among the treated group. Second, we provide new evidence about the increased likelihood of alcohol-related medical disorders in the treated counties. Third, we test whether the demand for credit was concentrated in a small part of the population (a few alcohol addicts) or whether it was spread across the treated population. Fourth, we explore the possibility of an alternative explanation for our results based on the idea of latent demand for alcohol, i.e., that weekend opening hours enabled busy (yet rational) people to visit liquor stores.

6.1 Monday Borrowing

To provide further corroborating evidence about the effect of Saturday opening hours on the financials of consumers in the treated areas, we 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 rather than at the end of the week. In other words, impulsive shoppers experience an "unexpected" negative cash

flow shock over the weekend and, thus, are more likely to borrow on Monday, when pawn shops open. 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 exact timing of pawn loans. We therefore construct a person-day data set (as opposed to the previously-used person-bi-monthly data set) in which we record the number of pawn loans (typically zero or one) that each person took out on a particular calendar day. We first verify our results from Table 3, this time on a daily frequency, in Table 8, Column (1) (double-diff) and Column (3) (triple-diff). The double-diff setting in Column (1) confirms that treated people borrow more (t-statistics is 4.5), while the triple-diff setting in Column (3) shows a positive, though statistically insignificant, coefficient (t-statistics is 1.0). We attribute the loss of significance in Column (3) to the granularity of the data on the daily frequency.

We next explore whether Monday borrowing activity is higher than during the rest of the week. In Columns (2) and (4), we interact the variable of interest with a Monday indicator and add day-of-the-week dummies to absorb the "normal" tendency to borrow on a certain day, where Monday is omitted and absorbed by our constant. The table shows that 24–27% of the increase in pawn borrowing due to the treatment takes place on Mondays.[18]

In summary, the results in Table 8 indicate a disproportionate increase in borrowing on Mondays in the treated group. This result is consistent with the idea that the extended opening hours on the weekend generate "unexpected" negative shocks

[18]We calculate 24% to 27% increase on Mondays in the following manner: (average daily effect + average effect on Monday)/((5 × average daily effect) + average effect on Monday)) = (0.00047 + 0.00013)/((5 ×0.00047) + 0.00013)) = 24%. Similar calculation using the coefficients on Column (4) yield 27%.

to present-biased consumers.

6.2 Increase in Alcohol-Related Health Disorders

To gain further comfort that the results that we report are indeed related to alcohol consumption, we explore the incidence rate of mental and behavioral disorders For the health outcomes we utilize the medical records registered by Swedish National Board of Health and Welfare (Social Styrelse) merged by Statistics Sweden to the individuals in our sample. We isolate cases classified by the International Classification of Diseases (ICD) version 10 as alcohol abuse (F10.1) in our data. These diagnoses include short-term incidences such as emergency room visits due to acute intoxication (F10.12); alcohol abuse, uncomplicated (F10.10); and alcohol abuse with induced mood disorder (F10.14).

Among the group of 20–25-year-olds in our sample, 0.026% have a diagnosis of alcohol abuse on their medical records during the preperiod see Table 2. Note that the incidence rate for the entire sample of people ages 20 and 65 in our sample is almost four times higher.

We start our analysis, as before, by looking at the effects for different age cohorts. In Figure 10, we plot the diff-in-diff coefficients for the six age groups. The chart shows no increase in the alcohol-related incidence rate for the cohort of 18–19-year-olds, which is comforting as this group should not be affected by our experiment. In contrast, we detect an increase in this incidence type for the population eligible to purchase alcohol. We find the largest effects among 46–55-year-olds.

To verify whether our earlier findings on credit default among the 20–25 cohort is driven by alcohol consumption, we run both double- and triple-diff specifications for this group. The dependent variable is a dummy for whether an individual was

diagnosed with an F10.1 disorder in a particular month. The results in Table 9 show that the likelihood of alcohol-related mental and behavioral disorders increases by 0.16 percentage points, which reflects a relative increase of 36%, relative to the preperiod mean of 0.44 percentage points.

6.3 A Few Alcoholics? The Distribution of Borrowers

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 in which we estimated the average effect, here our objective is to examine the distribution of the effect across individuals. We run the following regression:

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 Eligible_{i,t} * Post_t + \beta_3 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

Note that we purposely exclude the triple interaction, $Treated_i * Eligible_{i,t} * Post_t$, which has been the variable of interest in our previous analyses. We next 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 on the subset of individuals who actually borrowed. The question is whether the positive average in loan size among the treated group is driven by a small number of large loans or by across-the-board borrower demand.

The charts in Figure 11 show the distribution of the residuals of the loan sizes (conditional on being treated and on taking credit) for pawn and credit card bor-

rowing. In both figures, the distribution of the borrowing is concentrated in a single cluster, with no material outliers. We conclude that the increase in borrowing in the treated counties is not driven by a small part of the population, but rather is relatively wide-spread.

6.4 Present-Biased Preferences or Convenience Shopping?

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.

Another, non-mutually exclusive, 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 opening hours may simply allow people with busy schedule during the week to purchase alcohol. Thus, according to this narrative, the Saturday store opening hours represent a reduction in opportunity costs.

Our data allow us to test which effect—present-biased Preferences or convenience shopping—is likely to dominate. 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 opening 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 Internet Appendix Tables VI and VII. The tables contrast the financial effects for retirees (ages 65–75) versus older employees (ages 55–60), and unemployed individuals (ages 20–65) versus those who are employed within the same age group. Because the comparison with the 18 year olds is no longer appropriate, we run a triple difference specification in which the final difference, $\gamma_{i,t}$, is a dummy for being retired or unemployed. The tables show the coefficient β_1 from the following regression:

$$y_{i,t} = \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t + \beta_3 \gamma_{i,t} * Post_t + \beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \beta_5 GDP_{c,t} + \beta_6 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}. \quad (6.1)$$

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 (all samples have than 300,000 observations), but rather by coefficients that are close to zero with tight standard errors. For example, in Columns (1) and (2) we estimate the effect on the number of new pawn loans. The coefficients in Columns (1) and (2) of Internet Appendix Table VI, are –0.002 and –0.016, for retirees and unemployed, respectively, with standard errors of 0.008 and 0.014, respectively. In contrast, in Table 3, Column (1), the coefficient is 0.024.

These results suggest that less busier populations did not borrow more following

the extension of the opening hours on Saturdays. 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

We perform several robustness tests for our results 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 that is free from the bias. Second, we verify that there was no material spillage through the border counties that affected the results. Third, we demonstrate that the results are not particularly sensitive to the choice of error clustering. Fourth, we provide a series of permutation tests which randomly scramble the allocation of individual-months across 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.

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 a 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 contains indeed future information, it is not likely that this bias drives 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 allocation of counties to treatment and control groups. 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 Saturday opening hours experiment.

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 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 Internet Appendix Table VIII, we run double and triple-diff 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 calculated as "per quarter per 100,000 individuals". For example, one of the variables of interest is the number of defaults per quarter per 100,000 individuals living in a specific county and of a certain age (18, 19, 20, ..., 25). Importantly, in this calculation, the numerator (e.g., the number of defaults) is retrieved from our pawn credit registries and the denominator is the total number of people in each age group in each county, retrieved from Statistics Sweden.[19] Our

[19]This calculation overcomes the look-ahead bias since the denominator is based on the current

cross-sectional double-diff specification takes the following form:

$$y_{c,t} = \beta_1 Treated_c * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_c + \omega_t + \varepsilon_{c,t}, \tag{7.1}$$

and our cross-sectional triple-difference specification is:

$$y_{c,t,a} = \beta_1 Eligible_a * Treated_c * Post_t + \beta_2 Treated_c * Post_t + \beta_3 Eligible_a * Post_t + \beta_4 Eligible_a + \beta_5 GDP_{c,t} + \beta_6 Employ_{c,t} + \omega_c + \omega_t + \varepsilon_{c,t,a}. \tag{7.2}$$

Errors are clustered at the county level. Due to the small number of counties, we cluster the standard errors using robust Wild bootstrap with 1,000 replications. The double-diff specification omits the *Eligible* variable and its interactions and runs the regression on the 18–25 age group sample.

Internet Appendix Table VIII presents the regression results for our pawn credit outcome variables. Columns (1) and (2) show both the double- (Equation (7.1)) and the triple-diff (Equation (7.2)) specifications 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 are able to exploit within-county variation between consumers who can legally purchase alcohol and those who cannot. Looking at the triple diff results in Column (2), we find that Saturday opening hours increase the probability of taking out a pawn loan by an average of 90.1 per 100,000 residents. This effect is a 38% increase over the preperiod

population, and not population that will be included in the future due to future pawn borrowing.

average credit-borrowing rate among the treated counties. We also find significant increases in loan size (Columns (3) and (4)) at the county level of 19% and 35%, for the double- and triple-diff specifications, respectively. These results are similar, albeit 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 the small number of observations. Furthermore, the county-level specification gives similar weight to counties in the regression regardless of the number of residents.

Unfortunately, due to the quarterly frequency of our population statistics, we have insufficient observations in the preperiod to run county-level regressions for our mainstream credit or labor market outcomes.

7.2 Excluding Border Counties; Including Buffer Counties

We perform an additional test to ensure that our results are not affected by spillover to other countries. Specifically, the southern county of Skåne in Sweden borders Denmark, and 18–19-year-olds who cannot legally purchase alcohol in Sweden may cross the border to purchase alcohol, or Danish people may purchase alcohol in Swedish shops on Saturdays. In Internet Appendix Tables II to IV, we use a sample that excludes Skåne. The results are very similar to the ones presented in Table 3 to 5.

7.3 Sensitivity of Results to Clustering at Higher Levels

In the empirical analysis, we chose to cluster standard errors at the individual level. In Internet Appendix Tables IX to XI, 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.

7.4 Permutation Tests

As with many natural experiments, in our setting there is a concern that the effects we report are not related to the opening hours experiment but perhaps to an unobservable variation. We follow the procedure proposed by Chetty et al. (2009) to provide further comfort that this is not the case. Each individual-bimonth cell is randomly reassigned either to a treatment county or a control county. This is done by reshuffling the already-existing treatment variable such that the size of the treated group is constant. Then all interactions of the treatment are recomputed and the baseline regressions (both the double- and triple-diff specifications) are calculated for the constructed sample. The coefficient beta is stored, and the process starts over by again reshuffling the treatment. This procedure is repeated 2,000 times. All controls are as in the baseline regression, and individual fixed effects are included. We plot the distribution of the point estimates from the 2,000 regressions and also mark the original result.

The results of these placebo tests are presented in Internet Appendix Figures 1 to 3. Internet Appendix Figure 1 shows the cumulative distribution function (CDF) for the permutation analysis for two variables: the number of pawn loans and the balance of credit cards. Similar impressions arise from Internet Appendix Figures 2 to 3. The CDF charts show that the coefficient in the original regressions is above the 95th percentile in both distributions, suggesting that the effect is driven by 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.

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 opening hours into the weekend in some counties while remaining closed over the weekend in other counties. Our sample focuses on an arguably vulnerable population that borrows both from the mainstream and fringe credit markets. 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 also had spillover effects in the labor market. Specifically, treated populations were likely to experience higher rates of unemployment and greater reliance on welfare. Because alcohol consumption is partly triggered by present bias and its use imposes direct and indirect costs on consumers, policymakers can improve financial wellbeing of myopic consumers by limiting their access to alcohol. Policymakers who consider expanding opening hours have to weigh the cost for a vulnerable group in society with self-control problems and the externalities of their behavior on the rest of society against the benefit of more convenient opening hours for the group in society with no or low self-control.

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Figures

Figure 1: Average alcohol consumption over time: international comparison (1995–2015)

This figure shows the average number of liters of pure alcohol consumed per year per capita in Sweden, 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).

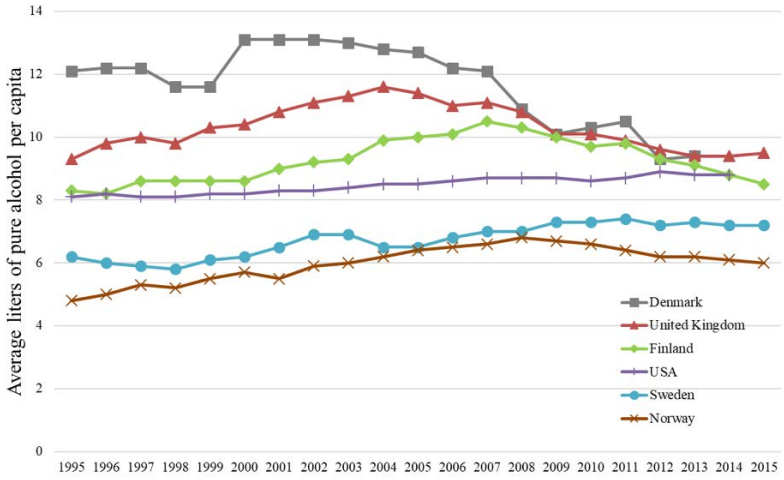
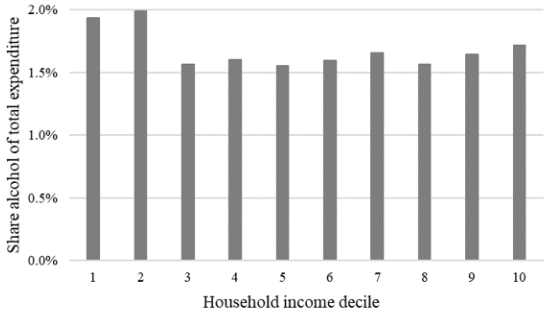
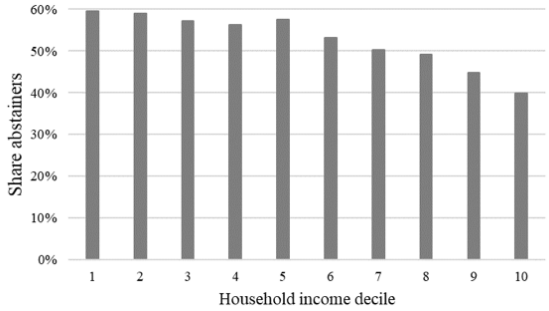


Figure 2: Alcohol consumption patterns in Sweden

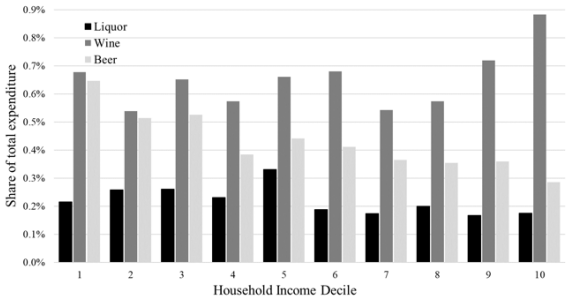
This figure depicts drinking patterns in Sweden. The bar charts in Panels A and B are based on a data set from Statistics Sweden called Household Expenditures (HUT). The data were gathered by administering cash journals to randomly selected households. The journals were complemented with information from Statistics Sweden's registries. Weights are used to achieve a representative sample of the total population. We study data for 2003–2007, during which time 10,895 persons participated out of 20,000 approached.



Panel A. Share Alcohol of Total Expenditure, by Household Income Decile (1999–2001).



Panel B. Share Abstainers, by Household Income Decile (1999–2001).



Panel C. Share of Total Expenditure, by Product Type and Income (2003–2009).

Figure 3: 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 (black) were Stockholm, Västernorrland, Jämtland, Västerbotten, Norrbotten, and Skåne. The control counties (grey) were Östergötland, Jönköping, Kalmar, Västra Götaland, Värmland, and Örebro. Gotland (white) 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 were excluded from our analysis to mitigate the concern that our findings are diluted by cross-county border shopping.

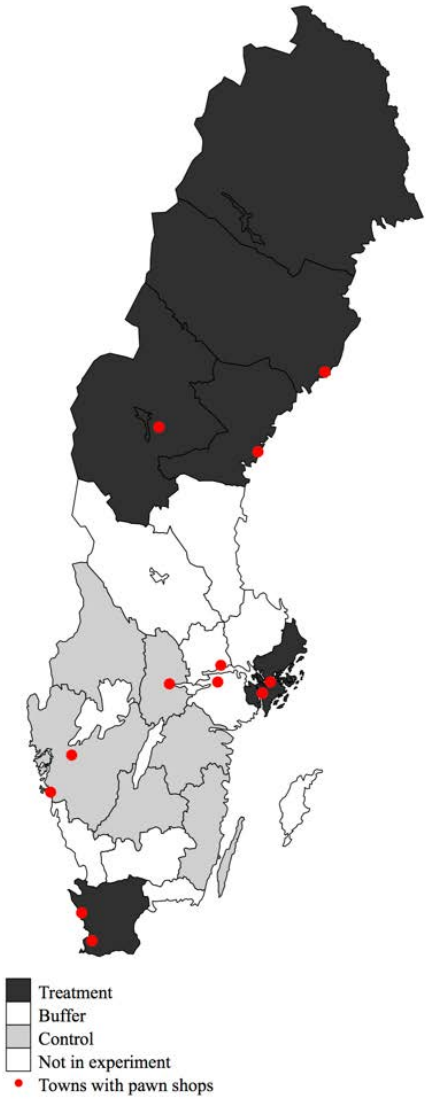


Figure 4: The effect of increased access to alcohol on credit and default

These figures show that increased access to alcohol causally increases credit borrowing and the default risk. For each cohort, we plot the coefficients β_1 and standard errors based on the following specification:

$$Credit_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$$

Standard errors are clustered at the individual level.

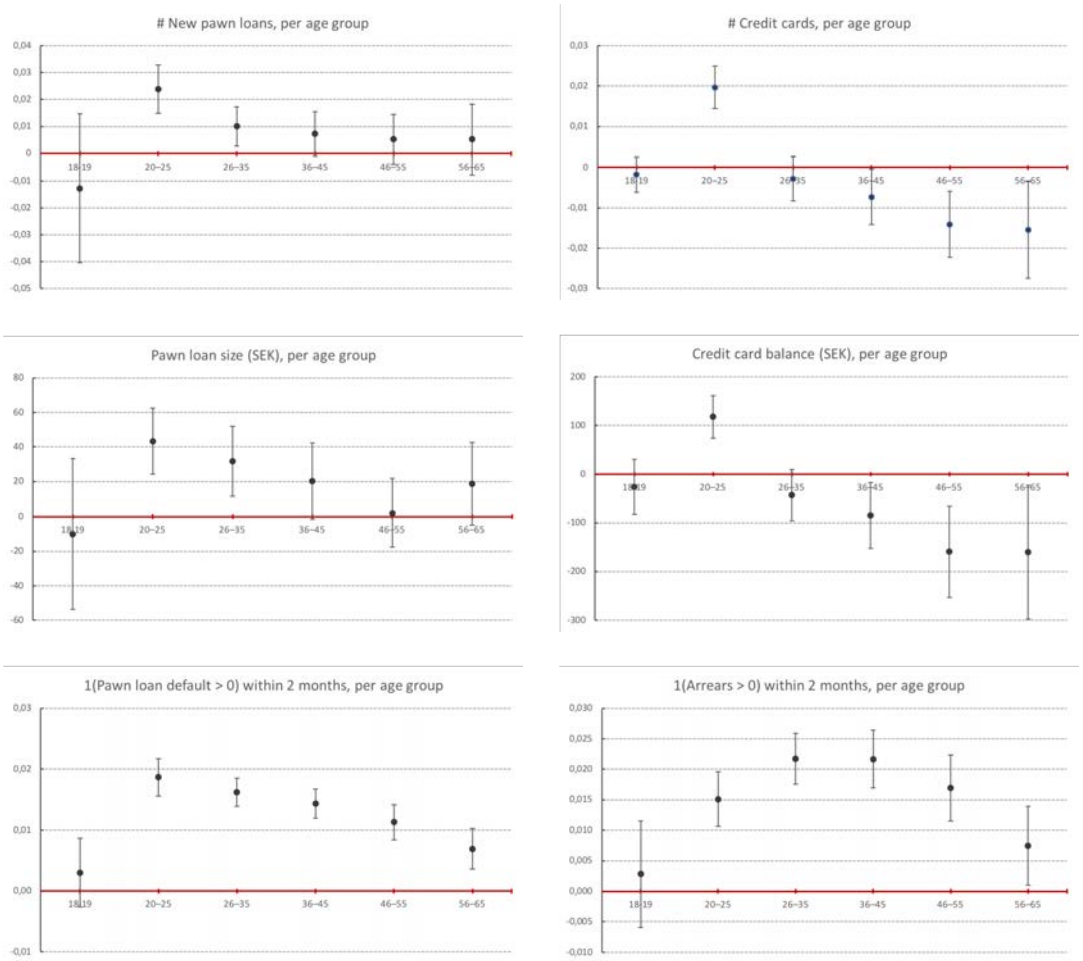


Figure 5: The effect of increased access to alcohol on unemployment and income

These figures show that increased access to alcohol increases unemployment, welfare dependence, and reported sick-days. Furthermore, we find a decrease in income. For each cohort, we plot the coefficients β_1 and standard errors based on the following specification:

$$Labormarket_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$$

Standard errors are clustered at the individual level.

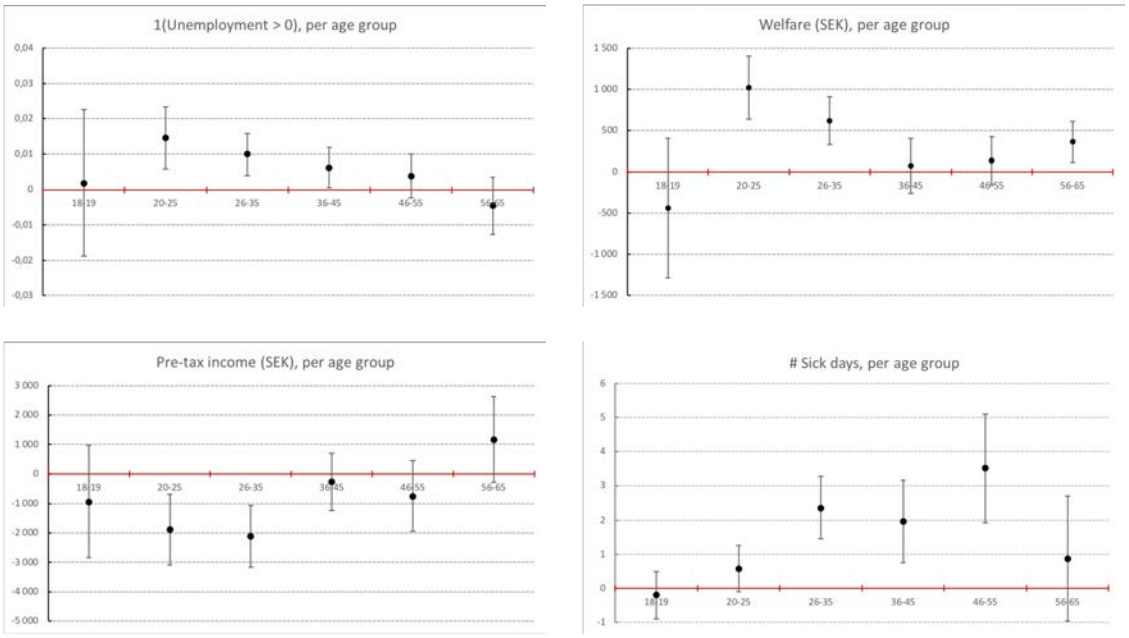


Figure 6: The effect of increased access to alcohol on Saturday crimes

The figures show that increased access to alcohol increases the probability that individuals commit alcohol related crimes on Saturday, i.e., drive while intoxicated and assault. For each cohort, we plot the coefficients β_1 and standard errors multiplied by a 1000, based on the following specification:

$$Crime_{i,d,t} = \beta_1 Treated_i * Post_t * Saturday_{i,t} + \beta_2 Treated_i * Post_t + \beta_3 Saturday_{i,t} * Post_t + \beta_4 Saturday_{i,t} + \omega_i + \omega_{dow} + \omega_{c*t} + \varepsilon_{i,t},$$

Standard errors are clustered at the individual level.

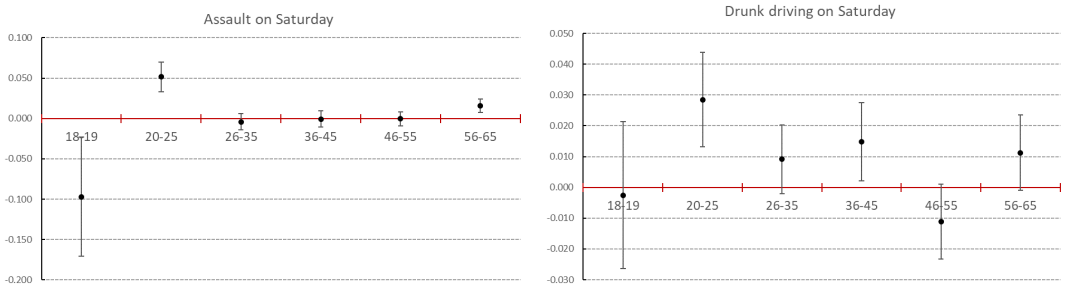


Figure 7: Pretrends in credit and default

This figure lends support to 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 depicts estimates of the β_τ coefficients and their 95% confidence intervals from the following model:

$$y_{i,t} = \Sigma_{t=-6}^{t=8} \beta_\tau Eligible_{i,t} * Treated_i * Period_t + \xi_1 Eligible_{i,t} * Treated_i + \xi_2 Eligible_{i,t} * Post_t + \xi_3 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t.}$$

The x-axis shows event time bimonths, which are defined as starting at zero in February 2000, when the Swedish government began opening liquor stores on Saturdays in some counties. The coefficients in the preperiod are normalized at $t = -1$ and the respective length of the preperiod is determined by data restrictions. In the boxes within the respective panels, we report (i) the p-value of a Wald test to check the null hypothesis that the β_τ coefficients in the preperiod are jointly equal, and (ii) the average difference $(\frac{1}{9}\Sigma_{t=0}^{t=8}\beta_\tau - \frac{1}{5}\Sigma_{t=-5}^{t=-1}\beta_\tau)$.

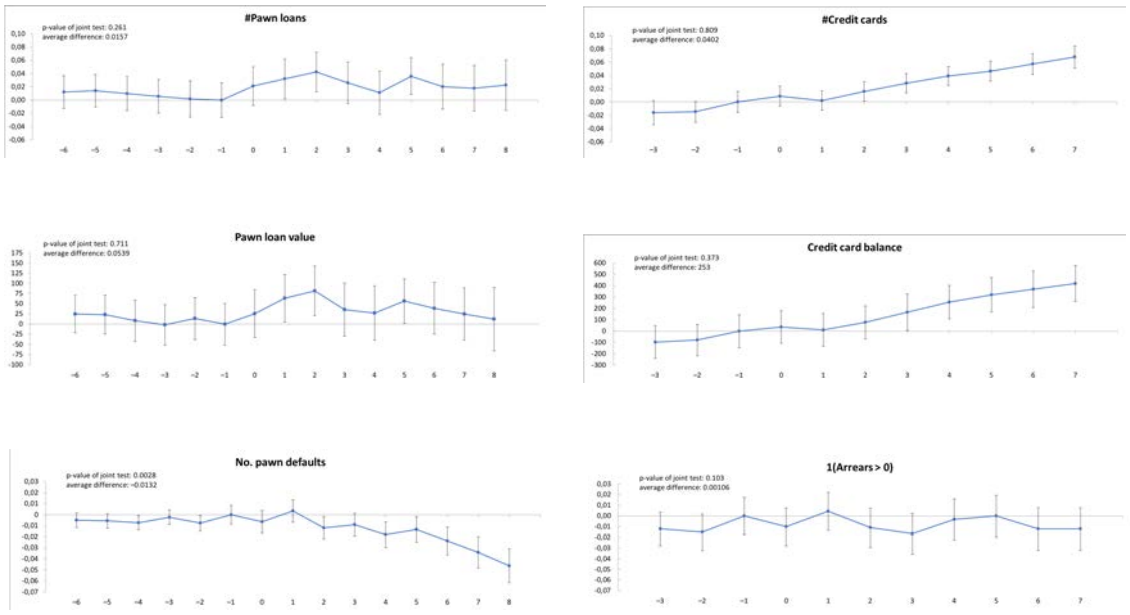


Figure 8: Pretrends in labor market outcomes

This figure lends support to 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 labor market outcomes. The panel depicts estimates of the β_τ coefficients and their 95% confidence intervals from the following model:

$$y_{i,t} = \Sigma_{t=-3}^{t=3} \beta_\tau Eligible_{i,t} * Treated_i * Period_t + \xi_1 Eligible_{i,t} * Treated_i + \xi_2 Eligible_{i,t} * Post_t + \xi_3 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t.}$$

Note that in the figure the preperiod is normalized at $t = -1$. Furthermore, the labor market outcomes are annual so the preperiod consists of three years. In the boxes within the respective panels, we report (i) the p-value of a Wald test to check the null hypothesis that the β_τ coefficients in the preperiod are jointly equal, and (ii) the average difference ($\frac{1}{3}\Sigma_{t=0}^{t=2}\beta_\tau - \frac{1}{3}\Sigma_{t=-3}^{t=-1}\beta_\tau$).

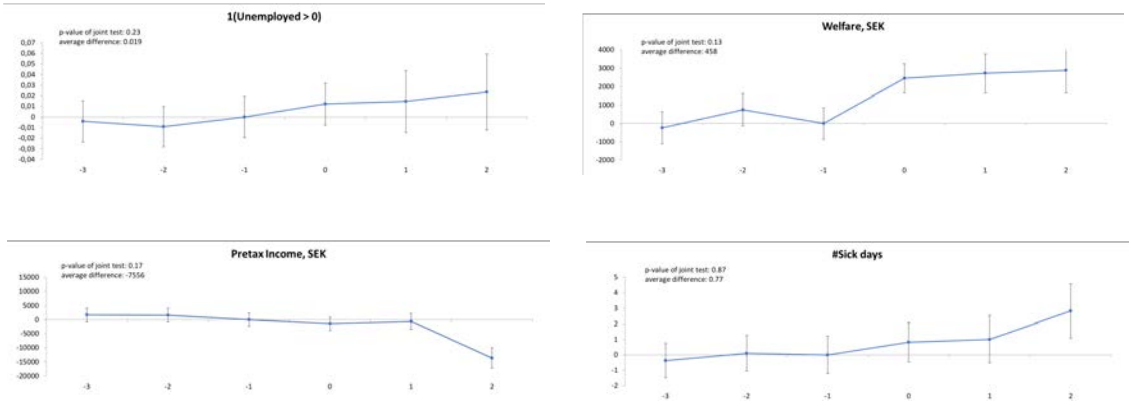


Figure 9: Pretrends in crime on Saturdays

This figure lends support to 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 depicts estimates of the β_τ coefficients and their 95% confidence intervals from the following model:

$$y_{i,t} = \Sigma_{t=-5}^{t=8} \beta_\tau Saturday_{i,t} * Treated_i * Period_t + \xi_1 Saturday_{i,t} * Treated_i + \xi_2 Saturday_{i,t} * Post_t + \xi_3 Saturday_{i,t} + \omega_i + \omega_{dow} + \omega_{c*t} + \varepsilon_{i,t.}$$

The x-axis shows event time bimonths, which are defined as starting at zero in February 2000, when the Swedish government began opening liquor stores on Saturdays in some counties. The coefficients in the preperiod are normalized at $t = -1$. In the boxes within the respective panels, we report (i) the p-value of a Wald test to check the null hypothesis that the β_τ coefficients in the preperiod are jointly equal, and (ii) the average difference $(\frac{1}{9}\Sigma_{t=0}^{t=8}\beta_\tau - \frac{1}{6}\Sigma_{t=-6}^{t=-1}\beta_\tau)$.

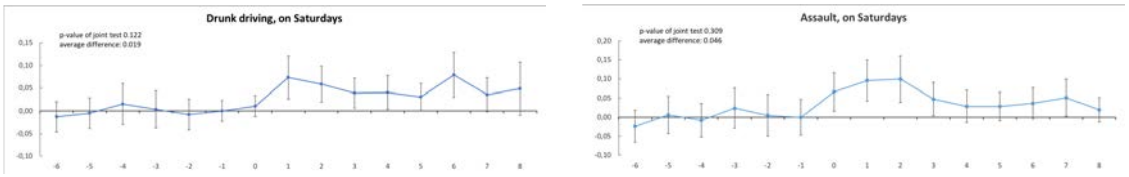


Figure 10: The effect of increased access to alcohol on alcohol-related health disorders

The figure in Panel A, shows that increased access to alcohol increases mental and behavioral disorders due to the use of alcohol (ICD code F10.0). For each cohort, we plot the coefficients β_1 and standard errors based on the following specification:

$$AlcoholRelatedDiagnosis_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$$

Standard errors are clustered at the individual level.

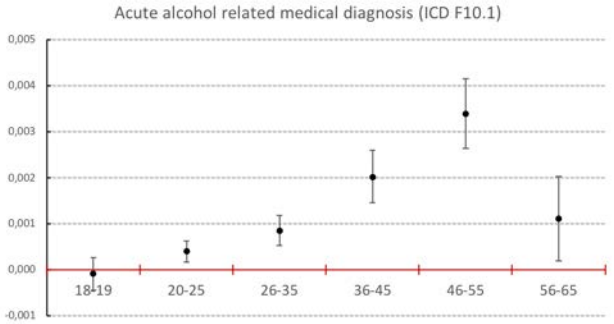
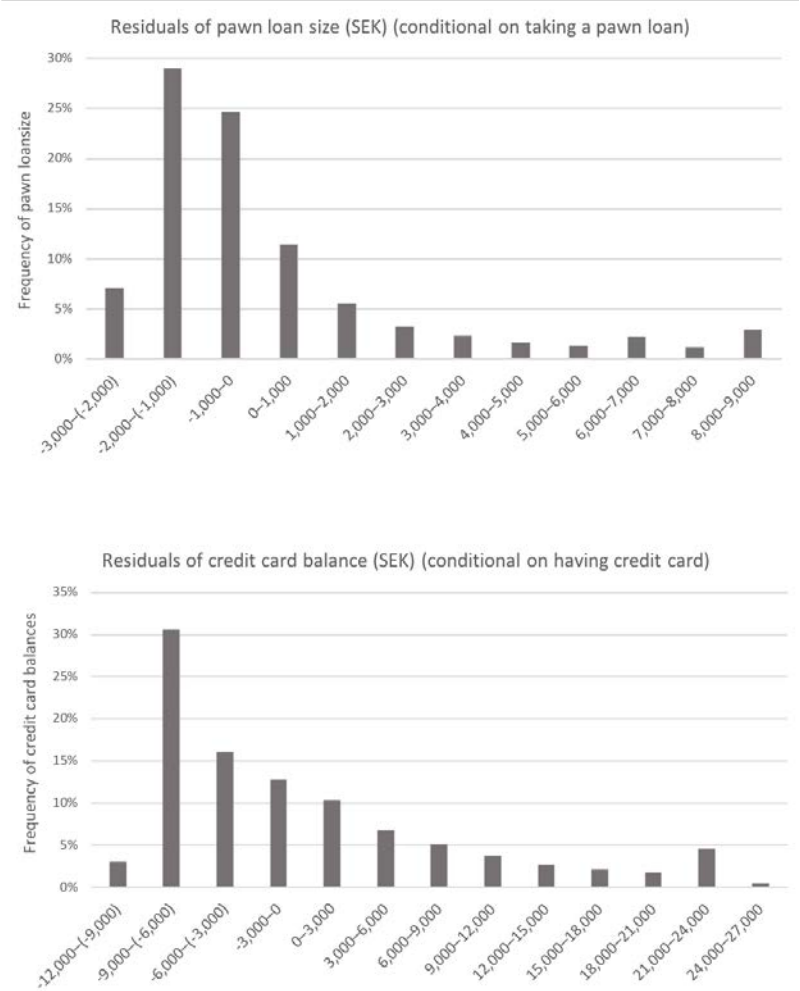


Figure 11: Distribution of residuals in the treatment counties of pawn and credit card loan sizes

The figures plot the distribution of the residuals for pawn loan size (top panel) and credit card balance (bottom panel) for the treatment cell, i.e., Post × Eligible × Treated county, from the baseline regression (Equation 5.1) without the triple-interaction;

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 Eligible_{i,t} * Post_t + \beta_3 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$



Tables

Table 1: Definitions of key outcome variables

Panel A: Definitions of pawn credit market dependent variables

# New pawn loans	Equal to the number of pawn loans borrowed.
Loan size (SEK)	Equal to the sum of balances of pawn credit the individual owns.
# Pawn defaults	Equal to the number of pawn loans held by the individual that went to auction.
# Pawn rollovers	Equal to the number of pawn loans held by the individual that were rolled over.

Panel B: Mainstream consumer credit market dependent variables

# Credit cards	Equal to the number of credit cards the individual owns.
Credit card balance (SEK)	Equal to the sum of balances of credit cards the individual owns.
# Installment loans	Equal to the number of installment loans the individual owns.
Installment loans limit (SEK)	Equal to the sum of limits of installment loans the individual owns.
# Credit lines	Equal to the number of credit lines the individual owns.
Credit lines balance (SEK)	Equal to the sum of balances of credit lines the individual owns.
1(Arrears $>$ 0)	Equal to one if the individual will receive at least one new credit arrear.

Panel C: Labor market, health and crime dependent variables

1(Unemployed $>$ 0)	Equal to one if the individual is full or part-time unemployed during the year.
Amount of welfare (SEK)	Equal to the sum of welfare the individual received during the year.
Pre-tax income (SEK)	Equal to the individuals total pre-tax yearly income.
# Sick days	Equal to the number of sick days registered during the year.
1(assault $>$ 0)	Equal to one if the individual is convicted for assault on Saturday
1(drunk driving $>$ 0)	Equal to one if the individual is convicted for drunk driving on Saturday
1(F10.1 $>$ 0)	Equal to one if the individual is diagnosed with an alcohol related disorder

Table 2: Summary statistics

This table presents sample statistics for the cohort of 20–25 year olds, during the period ***before*** the Swedish government expanded liquor store operating hours on Saturdays in some counties in February 2000. In the Appendix, we present the split treatment and control summary statistics for the same preperiod.

Panel A: Pawn credit market

Dependent variables	(1)	(2)	(3)
	mean	std dev	median
# New pawn loans	0.114	0.418	0
Loan size (SEK)	184.8	956.2	0
# Pawn defaults	0.008	0.097	0
# Pawn rollovers	0.036	0.232	0
# individuals		27,245	

Panel B: Mainstream consumer credit market dependent variables

Dependent variables	(1)	(2)	(3)
	mean	std dev	median
# Credit cards	0.147	0.575	0
Credit card balance (SEK)	679.7	3 442	0
# Installment loans	0.033	0.207	0
Installment loans limit (SEK)	876.5	8,253	0
# Credit lines	0.234	0.519	0
Credit lines balance (SEK)	2,593	11,501	0
1(Arrears > 0)	0.041	0.198	
# individuals		24,435	

Panel C: Labor market, crime and health dependent variables

Dependent variables	(1)	(2)	(3)
	mean	std dev	median
1(Unemployed > 0)	0.589	0.492	
Amount of welfare (SEK)	14,245	24,396	0
Pre-tax income (SEK)	61,466	71,380	30,127
# Sick days	5.393	31	0
# individuals		34,351	
1(assault > 0)	0.0000300	0.00548	0
1(drunk driving > 0)	0.0000140	0.00374	0
# individuals		27,111	
1(F10.1 > 0)	0.0026	0.051	0
# individuals		27,718	

Table 3: Pawn credit outcomes

This table shows that increased access to alcohol causally increases credit borrowing and the default risk. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours.

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}.$$

Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post.

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	# New pawn loans		Pawn loan size		# Pawn loan defaults		# Pawn rollovers	
Post × Treated	0.024***		43.2***		0.019***		0.003	
	(0.005)		(9.7)		(0.002)		(0.002)	
Post × Treated × Eligible		0.024**		40.4**		-0.003		0.004
		(0.011)		(20.1)		(0.004)		(0.004)
Post × Eligible		-0.020**		-24.4		0.004*		-0.007**
		(0.009)		(15.9)		(0.002)		(0.003)
Treated × Eligible		-0.016		-28.6		0.003		-0.007
		(0.011)		(19.9)		(0.003)		(0.004)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
Observations	353,264	399,178	353,264	399,178	353,264	399,178	353,264	399,178
R^2	0.006	0.007	0.002	0.003	0.026	0.025	0.001	0.001
# Individuals	32,826	38,320	32,826	38,320	32,826	38,320	32,826	38,320
preperiod mean	0.123	0.123	215.2	215.2	0.009	0.009	0.037	0.037
Relative effect	19%	19%	20%	19%	221%			

Table 4: Mainstream credit outcomes

This table shows that increased access to alcohol causally increases credit borrowing and the default risk. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours.

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}.$$

Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post.

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	#Credit cards		Credit card balance		#Installment loans		Installment limit	
Post × Treated	0.020***		117.8***		-0.001		139.2**	
	(0.003)		(22.4)		(0.001)		(63.2)	
Post × Treated × Eligible		0.023***		165.7***		-0.002		10.8
		(0.005)		(48.2)		(0.002)		(74.9)
Post × Eligible		-0.004		-72.9*		0.004***		130.6***
		(0.004)		(39.3)		(0.001)		(50.2)
Treated × Eligible		-0.019**		-87.5		0.009**		224.1
		(0.009)		(75.1)		(0.004)		(169.8)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
Observations	233,287	261,905	233,287	261,905	233,287	261,905	233,287	261,905
R^2	0.015	0.017	0.006	0.007	0.000	0.001	0.000	0.001
# Individuals	29,416	34,902	29,416	34,902	29,416	34,902	29,416	34,902
preperiod mean	0.208	0.208	951.2	951.2			1 209	
Relative effect	10%	11%	12%	17%			12%	

Table 5: Mainstream credit outcomes (continued)

This table shows that increased access to alcohol causally increases credit borrowing and the default risk. Columns (1), (3), and (5) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours.

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}.$$

Columns (2), (4), and (6) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post.

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

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

	(1)	(2)	(3)	(4)	(5)	(6)
	#Credit lines		Credit lines balance		1(Arrears > 0)	
Post × Treated	-0.012***		328.6***		0.015***	
	(0.003)		(74.01)		(0.002)	
Post × Treated × Eligible		-0.025***		336.3**		0.008
		(0.007)		(147.0)		(0.005)
Post × Eligible		0.003		50.2		0.009**
		(0.004)		(104.0)		(0.003)
Treated × Eligible		0.018		-404.3		-0.014*
		(0.013)		(286.3)		(0.008)
County FE	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25
Observations	233,287	261,905	233,287	261,905	233,139	261,748
R^2	0.014	0.017	0.006	0.007	0.007	0.009
# Individuals	29,416	34,902	29,416	34,902	29,372	34,852
preperiod mean	0.327	0.327	3 488	3 488	0.446	0.446
Relative effect	-4%	-8%	10%	10%	8%	2%

Table 6: Labor market outcomes

This table shows that increased access to alcohol causally increases credit borrowing and the default risk. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours.

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}.$$

Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post.

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1(Unemployed)		Welfare		Pre-tax income		#Sick days	
Post × Treated	0.015*		1,019***		-1,886		0.579	
	(0.009)		(385.5)		(1,207)		(0.676)	
Post × Treated × Eligible		0.011		1,917***		-1,530		0.071
		(0.017)		(629.4)		(1,656)		(0.800)
Post × Eligible		-0.003		-1,164**		698		0.078
		(0.014)		(537.8)		(1,307)		(0.636)
Treated × Eligible		-0.013		-1,717***		677		1.089**
		(0.008)		(389.5)		(1,075)		(0.527)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
Observations	135,480	175,924	136,382	177,012	136,382	177,012	135,480	175,924
R^2	0.043	0.065	0.021	0.019	0.159	0.222	0.020	0.024
# Individuals	42,087	47,986	42,204	48,076	42,204	48,076	42,087	47,986
preperiod mean	0.568		12,918	12,918				
Relative effect	2.6%		7.9%	15%				

Table 7: Effect of Saturday-open liquor stores on crime

This table tests whether increased access to alcohol increases the risk for crimes on Saturdays. The coefficients, standard errors have been multiplied by 1000. Columns (3, 4, 5) and (8, 9, 10) show triple difference regressions estimating equation 5.6.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	1(Assault > 0)					1(Drunk driving > 0)				
Post × Treated	0.0077					-0.0031				
	(0.0071)					-0.0046				
Post × Treated × Eligble		0.031**					-0.0070			
		(0.016)					(0.0091)			
Post × Treated × Saturday			0.051***	0.090***	0.0023			0.029*	0.053**	-0.0037
			(0.019)	(0.032)	(0.012)			(0.015)	(0.023)	(0.0054)
Post × Eligble		-0.028**					0.0072			
		(0.014)					(0.0070)			
Post × Saturday			-0.0439***	-0.073***	-0.0054			-0.028*	-0.043**	0.0038
			(0.0148)	(0.025)	(0.0094)			(0.012)	(0.022)	(0.0035)
Treated × Eligble		0.083					-0.023			
		(0.080)					(0.015)			
Treated × Saturday			-0.024	-0.039	-0.0011			-0.013	-0.023	0.0025
			(0.016)	(0.027)	(0.011)			(0.012)	(0.021)	(0.0030)
County FE	Yes	No	No	No	No	Yes	No	No	No	No
Calendar month FE	Yes	No	No	No	No	Yes	No	No	No	No
Individual and Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	No	No	No	Yes	No	No	No	No
Regional Employm.	Yes	No	No	No	No	Yes	No	No	No	No
Day of the week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Month FE	No	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Ages	20-25	18-25	20-25	20-25	20-25	20-25	18-25	20-25	20-25	20-25
Gender	all	all	all	male	female	all	all	all	male	female
Observations		20,702,518		11,346,586	9,355,932		20,702,518		11,346,586	9,355,932
# Individuals		32,667		17,874	14,793		32,667		17,874	14,793

Table 8: Weekly pattern of pawn credit borrowing

This table tests whether pawn borrowing in the treatment group was more likely to be take place on Mondays. The sample is at the person-day level and includes the years 1999 to 2001.Columns (1) and (2) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours:

$$y_{i,t} = \beta_1 Treated_i * Post_t * Monday + \beta_2 Treated_i * Post_t + \beta_3 GDP_{c,t} + \beta_4 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$$

Columns (3) and (4) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post:

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t * Monday + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \ldots Monday\ interactions \ldots + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

Monday is 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 *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)
	# New pawn loans			
Post × Treated	0.00050***	0.00047***		
	(0.00011)	(0.00011)		
Post × Treated × 1(Monday)		0.00013		
		(0.00009)		
Post × Treated × Eligible			0.00036	0.00033
			(0.00036)	(0.00036)
Post × Treated × Eligible × 1(Monday)				0.00015*
				(0.00009)
1(Monday)		0.00060***		0.00057***
		(0.00004)		(0.00004)
Post × Eligible			-0.00040	-0.00042*
			(0.00025)	(0.00025)
Treated × Eligible			-0.00043	-0.00043
			(0.00031)	(0.00031)
Specification	DD	DDD	DD	DDD
Ages	20–25	18-25	20–25	18-25
Observations	16,866,840	16,866,840	19,088,938	19,088,938
R^2	0.000	0.000	0.000	0.000
# Individuals	37,824	37,824	44,071	44,071

Table 9: Effect of Saturday-open liquor stores on disorders due to acute intoxication

This table tests whether increased access to alcohol increases mental and behavioral disorders due to use of alcohol, acute intoxication (ICD 10 code F10.1). Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours.

$$y_{i,t} = \beta_1 Treated_i * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}.$$

Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden (18 and 19 year olds). Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post.

$$y_{i,t} = \beta_1 Eligible_{i,t} * Treated_i * Post_t + \beta_2 Treated_i * Post_t + \beta_3 Eligible_{i,t} * Post_t + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

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

	(1)	(2)
	1(Diagnosed disorder due to use of alcohol > 0)	
Post × Treated	0.0016***	
	(0.00024)	
Post × Treated × Eligible		0.0019***
		(0.00062)
Post × Eligible		-0.0017***
		(0.00046)
Treated × Eligible		-0.0018***
		(0.00061)
County FE	Yes	No
Calendar month FE	Yes	No
Individual FE	Yes	Yes
Age FE	Yes	Yes
Regional GDP	Yes	No
Regional Employment	Yes	No
County × Calendar month FE	No	Yes
Ages	20–25	18-25
Observations	135,480	175,924
R2	0.000	0.000
# Individuals	42,087	47,986
preperiod mean	.00440	.00440
Relative effect	37%	42%

Supplemental Appendix: For Online Publication Only

This Internet Appendix contains the following Figures and Tables:

Table I: Summary statistics split by treatment and control counties

This table presents sample statistics for the 20–25 age cohort during the preperiod, i.e., ***before*** the Swedish government began opening liquor stores on Saturdays in some counties in February 2000.

Panel A: Pawn credit market

	(1)	(2)	(3)	(3)	(4)	(5)
Dependent variables	treated counties			control counties		
	mean	std dev	median	mean	std dev	median
# New pawn loans	0.123	0.436	0	0.100	0.387	0
Loan size (SEK)	215.2	1,050.5	0	134.4	772.0	0
# Pawn defaults	0.008	0.104	0	0.006	0.086	0
# Pawn rollovers	0.037	0.236	0	0.034	0.226	0
# individuals	17,027			10,255		

Panel B: Mainstream consumer credit market

	(1)	(2)	(3)	(3)	(4)	(5)
Dependent variables	treated counties			control counties		
	mean	std dev	median	mean	std dev	median
# Credit cards	0.208	0.677	0	0.047	0.317	0
Credit card balance (SEK)	951.2	4,032.4	0	229.1	2,047.7	0
# Installment loans	0.046	0.244	0	0.011	0.119	0
Installment loans limit (SEK)	1,209	9,692	0	324.1	4,976.9	0
# Credit lines	0.327	0.585	0	0.080	0.334	0
Credit lines balance (SEK)	3,488	13,176	0	1,107	7,757	0
1(Arrears > 0)	0.055	0.228	0	0.017	0.131	0
# individuals	15,252			9,206		

Panel C: Labor market, health and crime

	(1)	(2)	(3)	(3)	(4)	(5)
Dependent variables	treated counties			control counties		
	mean	std dev	median	mean	std dev	median
1(Unemployed > 0)	0.568	0.495		0.626	0.484	
Amount of welfare (SEK)	12,918	23,574	0	16,507	25,578	0
Pre-tax income (SEK)	64,258	71,893	34,939	56,707	70,242	23,027
# Sick days	5.523	30.753	0	5.171	30.793	0
# individuals	21,772			12,718		
1(Assault on Saturday > 0)	.0000302	.00549		.0000297	.00545	
1(Drunk driving on Saturday > 0)	.0000136	.00369		.0000148	.00383	
# individuals	16,950			10,198		
1(Diagnosis F10.0 > 0)	0.0044	0.011		0.0032	0.012	
# individuals	21,772			12,718		

Table II: Excluding border county: Pawn credit outcomes

This table displays the results of the same regressions as in Table 3, but now we exclude the county at the border with Denmark. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours. Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post. Standard errors are clustered at the individual level. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	# New pawn loans		Pawn loan size		# Pawn loan defaults		# Pawn rollovers	
Post × Treated	0.010**		15.6**		0.007***		0.003	
	(0.005)		(6.9)		(0.002)		(0.002)	
Post × Treated× Eligible		0.024**		42.4*		-0.005		0.008**
		(0.011)		(21.9)		(0.004)		(0.004)
Post × Eligible		-0.022**		-27.7*		0.005**		-0.006**
		(0.009)		(15.7)		(0.002)		(0.003)
Treated × Eligible		-0.023**		-40.3*		0.008**		-0.008*
		(0.011)		(20.9)		(0.004)		(0.005)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
Observations	287,787	324,195	287,787	324,195	287,787	324,195	287,787	324,195
R^2	0.006	0.007	0.002	0.003	0.033	0.031	0.001	0.001
# Individuals	26,787	31,145	26,787	31,145	26,787	31,145	26,787	31,145

Table III: Excluding border county: Mainstream credit outcomes

This table displays the results of the same regressions as in Table 4, but now we exclude the county at the border with Denmark. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours. Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post. Standard errors are clustered at the individual level. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	#Credit cards		Credit card balance		#Installment loans		Installment limit	
Post × Treated	0.023***		142.3***		0.000		112.8*	
	(0.003)		(27.1)		(0.001)		(58.4)	
Post × Treated× Eligible		0.026***		194.2***		-0.001		-10.9
		(0.005)		(52.6)		(0.001)		(82.4)
Post × Eligible		-0.003		-76.5*		0.003***		118.9**
		(0.004)		(39.4)		(0.001)		(48.4)
Treated × Eligible		-0.024***		-111.5		0.007		218.4
		(0.009)		(84.1)		(0.004)		(196.9)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
Observations	189,660	212,320	189,660	212,320	189,660	212,320	189,660	212,320
R^2	0.015	0.017	0.006	0.007	0.000	0.001	0.001	0.002
# Individuals	23,990	28,342	23,990	28,342	23,990	28,342	23,990	28,342

Table IV: Excluding border county: Mainstream credit outcomes (continued)

This table displays the results of the same regressions as in Table 5, but now we exclude the county at the border with Denmark. Columns (1), (3), and (5) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours. Columns (2), (4), and (6) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, who are ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post. Standard errors are clustered at the individual level. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	#Credit lines		Credit lines balance		1(Arrears > 0)	
Post × Treated	-0.014***		484.3***		0.033***	
	(0.003)		(83.3)		(0.003)	
Post × Treated× Eligible		-0.027***		405.7**		0.008
		(0.008)		(172.1)		(0.006)
Post × Eligible		0.001		39.0		-0.001
		(0.004)		(103.7)		(0.003)
Treated × Eligible		0.012		-534.0*		0.016
		(0.015)		(310.4)		(0.013)
County FE	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25
Observations	189,660	212,320	189,660	212,320	189,660	212,320
R^2	0.015	0.019	0.007	0.008	0.036	0.042
# Individuals	23,990	28,342	23,990	28,342	23,990	28,342

Table V: Alcohol spending, 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 quintile	1	2,786	3,341	2,749	4,935	2,072	1,721
	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

Table VI: Exploring groups that have time: Pawn credit outcomes

This table shows the financial effects for retirees (ages 65–75) versus older employees (ages 55–60) and for unemployed individuals (ages 20–65) versus those who are employed within the same age group. Because the comparison with the 18–19-year-olds is no longer appropriate, we run a triple-difference specification in which the final difference, $\gamma_{i,t}$, is a dummy for being retired (Columns (1), (3), (5) and (7)) or unemployed (Column (2), (4), (6), and (8)). The table shows the coefficient β_1 from the following regression:

$$y_{i,t} = \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t + \beta_3 \gamma_{i,t} * Post_t + \beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}.$$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	# New pawn loans		Pawn loan size		# Pawn loan defaults		# Pawn rollovers	
	(γ = retiree)	(γ = unemploy)	(γ = retiree)	(γ = unemploy)	(γ = retiree)	(γ = unemploy)	(γ = retiree)	(γ = unemploy)
Post × Treated × 1(γ)	-0.002	-0.016	-9.5	-23.0	-0.002	0.012***	-0.002	0.000
	(0.008)	(0.014)	(13.8)	(39.0)	(0.002)	(0.004)	(0.009)	(0.008)
Post × 1(γ)	-0.001	0.017	-3.6	41.8	0.004*	0.002	0.005	0.001
	(0.007)	(0.013)	(12.6)	(36.5)	(0.002)	(0.003)	(0.008)	(0.008)
Treated ×	-0.007	0.012	2.5	19.4	0.001	-0.005	-0.010	0.001
	(0.015)	(0.012)	(27.9)	(33.4)	(0.004)	(0.003)	(0.015)	(0.006)
1(γ)	-0.038	-0.031***	0.8	-55.4*	0.010	-0.004	-0.069	-0.007
	(0.049)	(0.012)	(94.6)	(31.4)	(0.014)	(0.003)	(0.053)	(0.006)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Month FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	388,540	1,296,765	388,540	1,296,765	388,540	1,296,765	388,540	1,296,765
R^2	0.003	0.005	0.002	0.002	0.018	0.016	0.003	0.002
# Individuals	29,841	92,700	29,841	92,700	29,841	92,700	29,841	92,700

Table VII: Exploring groups that have time: Mainstream credit outcomes

This table shows the financial effects for retirees (ages 65–75) versus older employees (ages 55–60) and for unemployed individuals (ages 20–65) versus those who are employed within the same age group. Because the comparison with the 18–19-year-olds is no longer appropriate, we run a triple-difference specification in which the final difference, $\gamma_{i,t}$, is a dummy for being retired (Columns (1), (3), (5), and (7)) or unemployed (Columns (2), (4), (6), and (8)). The table shows the coefficient β_1 from the following regression:

$$y_{i,t} = \beta_1 Treated_i * \gamma_{i,t} * Post_t + \beta_2 Treated_i * Post_t + \beta_3 \gamma_{i,t} * Post_t + \beta_3 \gamma_{i,t} * Post_t + \beta_4 \gamma_{i,t} + \omega_i + \omega_{c*t} + \varepsilon_{i,t}$$

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	# Credit cards		Credit card balance		#Installment loans		1(Arrear > 0)	
	(γ = retiree)	(γ = unemploy)	(γ = retiree)	(γ = unemploy)	(γ = retiree)	(γ = unemploy)	(γ = retiree)	(γ = unemploy)
Post × Treated × 1(γ)	0.005	0.011	23.42	96.74	0.001	0.002	-0.0000006	-0.011
	(0.011)	(0.009)	(111.1)	(85.64)	(0.005)	(0.004)	(0.004)	(0.009)
Post × 1(γ)	-0.005	0.013	67.67	-81.41	-0.002	0.010***	-0.009***	0.042***
	(0.008)	(0.008)	(78.28)	(77.56)	(0.003)	(0.004)	(0.003)	(0.008)
Treated ×	0.008	-0.007	-344.0	-113.2	0.001	0.002	0.005	0.0009
	(0.02)	(0.009)	(237.0)	(92.61)	(0.009)	(0.004)	(0.007)	(0.009)
1(γ)	0.07*	-0.017**	445.5	60.97	0.018	-0.012***	-0.012	-0.011
	(0.04)	(0.008)	(479.0)	(85.78)	(0.015)	(0.004)	(0.026)	(0.009)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	267,987	870,783	267,987	870,783	267,987	870,783	267,936	870,284
R^2	0.043	0.029	0.009	0.005	0.013	0.009	0.010	0.019
# Individuals	29,440	92,082	29,440	92,082	29,440	92,082	29,422	91,928

Table VIII: County-level regression: Pawn credit outcomes

This table shows double and triple-difference regressions for our pawn credit market outcomes, at the county level per 100,000 residents. The unit of observation is calculated as "per quarter per 100,000 individuals". For example, one of the variables of interest is the number of defaults per quarter per 100,000 individuals living in a specific county and of a certain age (18, 19, 20, ..., 25). Importantly, in this calculation, the numerator (e.g., the number of defaults) is retrieved from our pawn credit registries and the denominator is the total number of people in each age group in each county, retrieved from Statistics Sweden. Our cross-sectional double-difference specification is the following model:

$$y_{c,t} = \beta_1 Treated_c * Post_t + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_c + \omega_t + \varepsilon_{c,t},$$

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

$$y_{c,t,a} = \beta_1 Eligible_a * Treated_c * Post_t + \beta_2 Treated_c * Post_t + \beta_3 Eligible_a * Post_t + \beta_4 Eligible_a + \omega_{c*t} + \varepsilon_{c,t,a}$$

Errors are clustered at the county level. Due to the small number of counties, we cluster the standard errors using robust wild bootstrapping with 1,000 replications as well. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New pawn loans per 100,000		Avg pawn loan size		Pawn loan defaults per 100,000		#Pawn rollovers per 100,000	
Post × Treated	52.4***		69,175**		-3.2		6.5	
	(11.9)		(34,224)		(10.3)		(4.7)	
Boot strap std. error	(24.3)		(89,176)		(40.5)		(10.2)	
Boot strap *p*-value	0.114		0.652		0.176		0.278	
Post × Treated × Eligible		90.1***		150,031*		-3.6		6.2
		(28.0)		(78,296)		(23.0)		(10.9)
Boot strap std. error		(39.1)		(69 489)		(18.4)		(5.8)
Boot strap *p*-value		0.050**		0.065*		0.495		0.319
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
Observations	1,800	2,100	1,800	2,100	1,800	2,100	1,800	2,100
R^2	0.734	0.738	0.526	0.537	0.239	0.260	0.550	0.551
# Counties					10			
preperiod mean	234.1	234.1	4.336	424 937				
Relative effect	22%	38%	19%	35%				

Table IX: Robustness higher clusters: Pawn credit outcomes

This table displays the results of the same regressions as in Table 3, but now we present the standard errors when we cluster at the individual, parish, and municipality level. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion of liquor store opening hours. Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year olds, an age group that is ineligible to buy alcohol in Sweden). Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	# New pawn loans		Pawn loan size		# Pawn loan defaults		# Pawn rollovers	
Post × Treated	0.024***		43.2***		0.019***		0.003	
Individual-level	(0.005)		(9.7)		(0.002)		(0.002)	
Parish-level	(0.006)		(9.6)		(0.004)		(0.002)	
Municipality-level	(0.009)		(14.7)		(0.008)		(0.002)	
Post × Treated × Eligible		0.024**		40.4**		-0.003		0.004
Individual-level		(0.011)		(20.1)		(0.004)		(0.004)
Parish-level		(0.011)		(20.5)		(0.004)		(0.004)
Municipality-level		(0.012)		(29.6)		(0.004)		(0.004)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
#Individual clusters	32 826	38 320	32 826	38 320	32 826	38 320	32 826	38 320
#Parish clusters	1 312	1 403	1 312	1 403	1 312	1 403	1 312	1 403
#Municipality clusters	288	290	288	290	288	290	288	290

Table X: Robustness higher clusters: Mainstream credit outcomes

This table displays the results of the same regressions as in Table 4, but now we present the standard errors when we cluster at the individual, parish, and municipality level. Columns (1), (3), (5), and (7) show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion in liquor store opening hour. Columns (2), (4), (6), and (8) show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden. Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	#Credit cards		Credit card balance		#Installment loans		Installment limit	
Post × Treated	0.020***		117.8***		-0.001		139.2**	
Individual-level	(0.003)		(22.4)		(0.001)		(63.2)	
Parish-level	(0.003)		(22.8)		(0.001)		(60.2)	
Municipality-level	(0.003)		(24.4)		(0.001)		(48.2)	
Post × Treated × Eligible		0.023***		165.7***		-0.002		10.8
Individual-level		(0.005)		(48.2)		(0.002)		(74.9)
Parish-level		(0.005)		(51.3)		(0.002)		(75.4)
Municipality-level		(0.005)		(47.0)		(0.001)		(64.8)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25	20–25	18-25
#Individual clusters	29 416	34 902	29 416	34 902	29 416	34 902	29 416	34 902
#Parish clusters	1 278	1 358	1 278	1 358	1 278	1 358	1 278	1 358
#Municipality clusters	287	289	287	289	287	289	287	289

Table XI: Robustness higher clusters: Mainstream credit outcomes (continued)
This table displays the results of the same regressions as in Table 5, but now we present the standard errors when we cluster at the individual, parish, and municipality level. Columns (1), (3), and (5), show double-difference regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, before and after the expansion in liquor store opening hours. Columns (2), (4), and (6), show regressions from triple-difference regressions. The sample for this analysis also includes 18–19-year-olds, an age group that is ineligible to buy alcohol in Sweden (18 and 19 year olds). Thus, the regression is a triple-difference specification: eligible/ineligible, treatment/control, and pre/post. *, **, and *** indicate the 10%, 5%, and 1% significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	#Credit lines		Credit lines balance		1(Arrears > 0)	
Post × Treated	-0.012***		328.6***		0.015***	
Individual-level	(0.003)		(74.01)		(0.002)	
Parish-level	(0.003)		(72.8)		(0.003)	
Municipality-level	(0.003)		(84.8)		(0.006)	
Post × Treated × Eligible		-0.025***		336.3**		0.008
Individual-level		(0.007)		(147.0)		(0.005)
Parish-level		(0.007)		(148.7)		(0.005)
Municipality-level		(0.006)		(122.1)		(0.005)
County FE	Yes	No	Yes	No	Yes	No
Calendar month FE	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No
Regional Employm.	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes
Ages	20–25	18-25	20–25	18-25	20–25	18-25
#Individual clusters	29 416	34 902	29 416	34 902	29 416	34 902
#Parish clusters	1 278	1 358	1 278	1 358	1 278	1 358
#Municipality clusters	287	289	287	289	287	289

Figure 1: Distribution of placebo estimates: Pawn credit

This figure plots the cumulative distribution function (CDF) for the permutation analysis for all of our outcome variables. The CDF is constructed from 2,000 estimates of β_1, using our baseline triple difference specification for 18–25-year-olds. The CDF charts show that the coefficient in the original triple-difference regressions (the red vertical line) is above the 95th percentile in both distributions, suggesting that the effect is driven by 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.

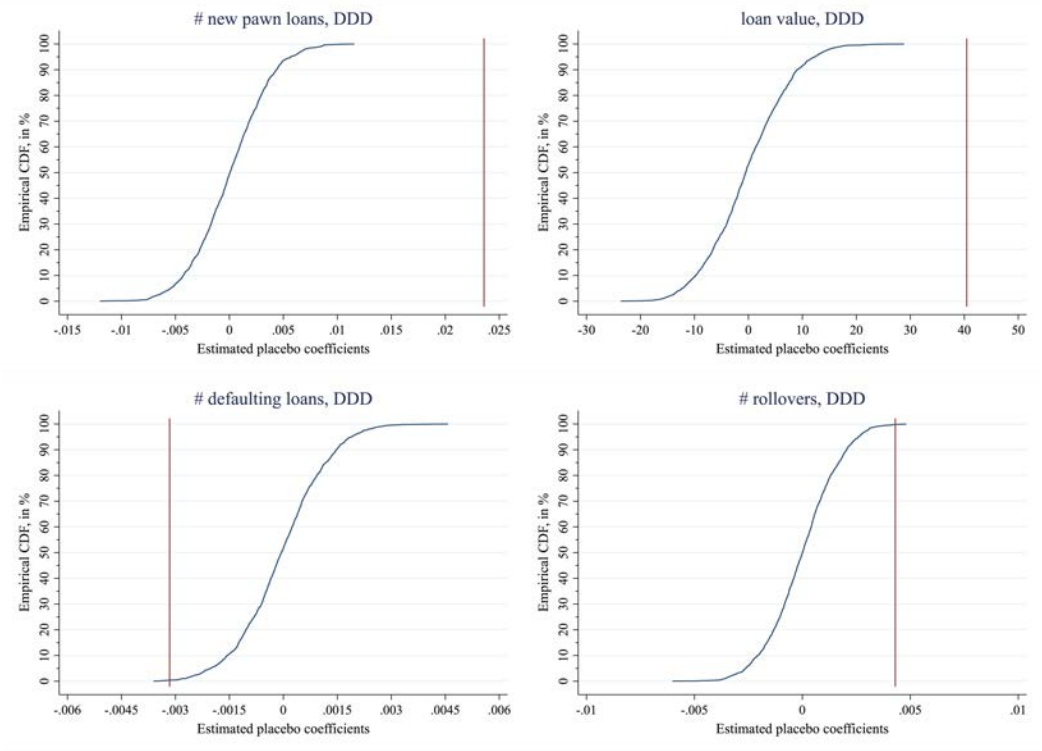


Figure 2: Distribution of placebo estimates: Mainstream credit

This figure plots the cumulative distribution function (CDF) for the permutation analysis for all of our outcome variables.The CDF is constructed from 2,000 estimates of β_1, using our baseline triple difference specification for 18–25-year-olds. The CDF charts show that the coefficient in the original triple-difference regressions (the red vertical line) is above the 95th percentile in both distributions, suggesting that the effect is driven by 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.

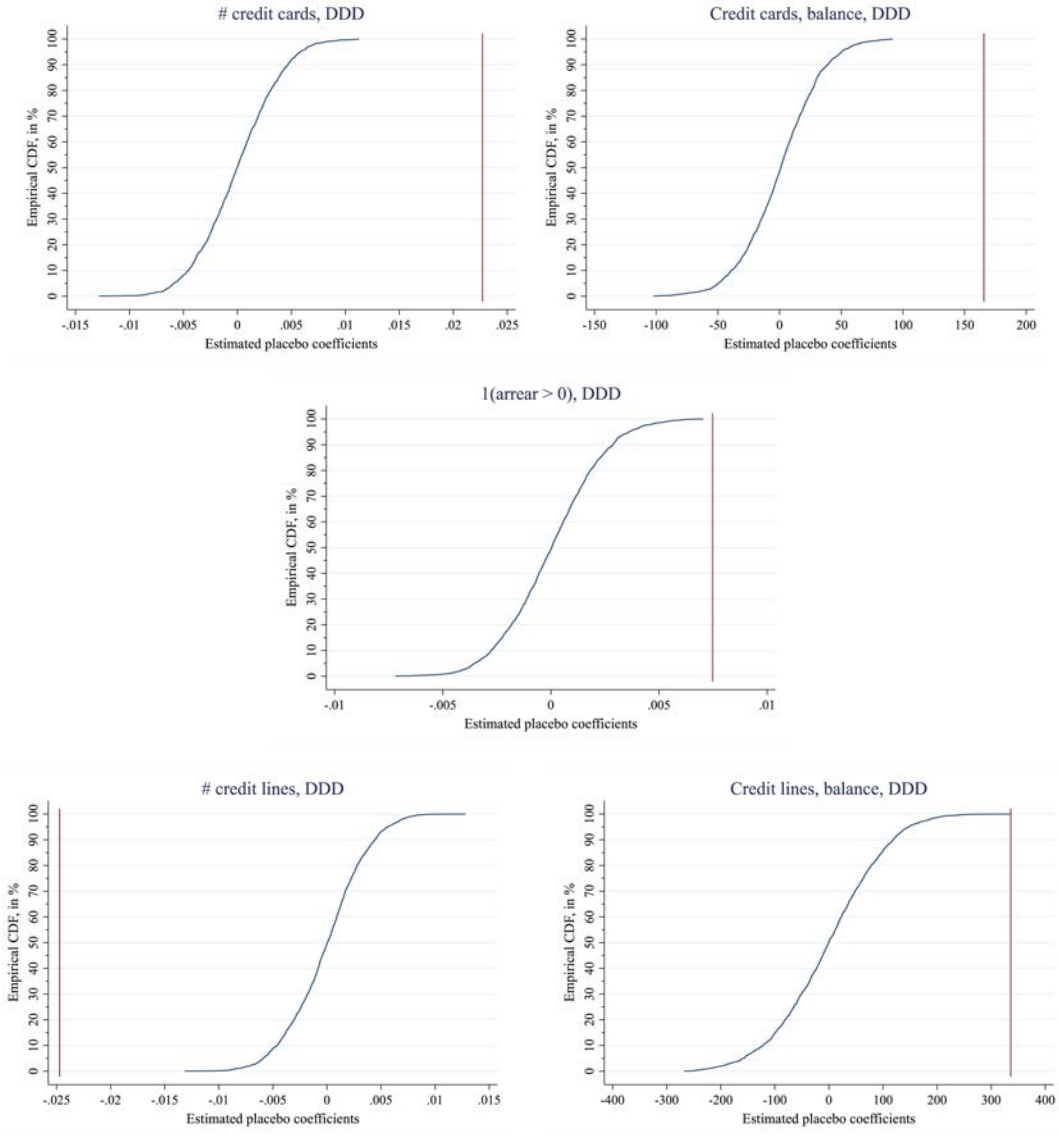


Figure 3: Distribution of placebo estimates: Labor market

This figure plots the cumulative distribution function (CDF) for the permutation analysis for all of our outcome variables. The CDF is constructed from 2,000 estimates of β_1, using our baseline triple-difference specification for 18–25-year-olds. The CDF charts show that the coefficient in the original triple difference regressions (the red vertical line) is above the 95th percentile in both distributions, suggesting that the effect is driven by 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.