NBER WORKING PAPER SERIES

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

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Working Paper 23211
http://www.nber.org/papers/w23211

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
March 2017

We thank Anna Dreber-Almenberg, Roc Armenter, Laurent Bach, Bruce Carlin, Hans Grönqvist, David Laibson, Christine Laudenbach, Susan Niknami, Leonard Nakamura, Martin Schmaltz, and numerous seminar and conference participants for helpful comments. Jesper Böjeryd and Johan Orrenius provided excellent research assistance. Funding from VINNOVA is gratefully acknowledged. All errors are our own. The views expressed here are those of the authors and do not necessarily represent those of the Federal Reserve Bank of Philadelphia, the Federal Reserve System, or the National Bureau of Economic Research.

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

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NBER Working Paper No. 23211
March 2017, Revised April 2020
JEL No. D03,D12,I18,L51,L66

ABSTRACT

Increased availability of alcohol may harm individuals if they have present-focused preferences and consume more than initially planned. Using a nationwide experiment in Sweden, we study the credit behavior of low-income households around the expansion of liquor stores' operating hours on Saturdays. Consistent with store closures serving as commitment devices, the policy led to higher credit demand, more default, increased dependence on welfare, and higher crime on Saturdays. The effects are concentrated among the young population due to higher alcohol consumption combined with tight liquidity constraints. The policy's impact on indebtedness is estimated at 4.5 times the expenditure on alcohol.

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1 Introduction

Individuals have present-focused preferences if they systematically change their prior consumption plan as time passes (e.g., Thaler and Shefrin, 1981; Laibson, 1997; Bernheim and Rangel, 2004).[1] For example, a person with present-focused preferences might plan to skip dessert but change his mind once he sees the menu. At their core, present-focused preferences mean that people put much weight on the present experience.[2] They 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," meaning 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), road accidents (Wagenaar, Murray, and Toomey, 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 only limited evidence about the causal effects of increased access to alcohol—and thus greater opportunity for impulsive consumption—on financial wellbeing.

To explore the effects of impulsive consumption of alcohol on individuals' financial wellbeing, we analyze the results of a nationwide experiment in Sweden under which off-site liquor stores in some counties extended their operating hours into the weekend. The experimenters concluded that off-site alcohol sales increased in the treated counties following the experiment (Norström and Skog, 2003). Our study documents that individuals exposed to the longer operating hours increased their indebtedness and had a greater likelihood of default. Furthermore, alcohol consumption has a spillover effect to other domains (e.g., labor,

[1] Following Ericson and Laibson (2019), who categorize models about intertemporal choice, we refer to agents who value commitment about future actions as having "present-focused preferences."

[2] Such behavior can arise from a variety of psychological mechanisms. See the review by Ericson and Laibson (2019) and a further discussion in Online Appendix A.

crime) beyond the mechanical increase in consumption.

In Sweden, the sale of alcohol for off-site consumption is permitted only in government-owned stores. Prior to the experiment, liquor stores were open only on weekdays and were closed on weekends. In February 2000, the government initiated an experiment to evaluate the impact of opening the stores on Saturdays in six of Sweden's 21 counties (Norström and Skog, 2003). The experiment resulted in an increase in alcohol purchases in the treated counties of 4% on average (Norström and Skog, 2005; Grönqvist and Niknami, 2014). The rise in consumption following the increase in opening hours indicates that alcohol consumers potentially have present-focused preferences, leading them to engage in impulsive consumption. We can infer this because alcohol can be easily stored; hence, opening hours should not distort the consumption of forward-looking consumers.[3]

Our empirical strategy is based on both double- and triple-difference specifications (DD and DDD, respectively). In a DD setting, we compare consumers in the counties with increased access to alcohol to those in counties without increased access, pre- and post-experiment. Because we do not observe alcohol consumption directly, we use reduced-form estimations in an intent-to-treat framework (Angrist, Imbens, and Rubin, 1996). To ensure that these results are not confounded by unobserved differences between people who chose to live in the various counties or by county-level trends, we also pursue a DDD strategy. Specifically, we exploit the fact that while 18–19-year-olds are not allowed to buy alcohol anywhere in Sweden, they are still allowed to borrow. Thus, the DDD specification compares the two groups: 18–19- and 20–25-year-olds within the treatment counties, across counties, and across time. The DD results for the 20–25-year-old cohort generally hold up in the DDD specifications.

We perform a preliminary analysis in which we measure the change in alcohol expenditure for the different cohorts around the policy change. Using cash journal data, we document that the young cohort, 20–25-year-olds, increased their expenditure on alcohol the most in

[3]See Online Appendix A for further details about the relation between present-focused preferences and store opening hours.

the treated counties. This evidence is consistent with the findings in the alcohol literature that young people are most affected by access to alcohol (e.g., Guttormsson and Grondahl, 2017). In further analysis, we find that for all groups exposed to the policy change, the young cohort increased their credit utilization, and suffered adverse consequences in the labor market and alcohol-related crime the most.

Our main analysis focuses on the pawn and mainstream credit markets. We document that total credit balances for the 20–25 cohort (pawn and mainstream credit) increased by an average of 502 SEK (about 50 USD), or 11.3% relative to the pre-experiment mean. We also test for loan performance and find some evidence of an increase in delinquencies. The effect comes from both the pawn and mainstream credit markets. We further establish a causal link between the increased availability of alcohol and indebtedness by examining the timing of pawn borrowing. We document that in the treated group, pawn borrowing significantly increased on Mondays, consistent with a liquidity shortage over the weekend. This result also supports the idea that alcohol purchases are impulsive rather than planned.

We calculate that the increase in consumption is likely to explain only a fraction of the increase in indebtedness. We estimate that the increase in direct alcohol consumption was only about 112 SEK, whereas the increase in total credit balances was 502 SEK. The multiplier of up to 4.5 (502/112) 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 (road accidents, job loss, etc.). To the best of our knowledge, the only study to date that quantifies the multiplier effect is Schilbach (2019), who reports a multiplier of at least 2 in the context of Indian cab drivers.

To better understand the indirect effects of alcohol consumption on individuals' financial wellbeing, we study potential spillover consequences of drinking. First, we explore effects in the labor market. We match annual tax records to people in our main sample and find that young people exposed to the longer operating hours were more likely to be unemployed

and more likely to depend on welfare. Second, we show that two types of alcohol-related crime, 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 expenditures due to impaired judgment.

We also further investigate the mechanism behind the negative consequences of increased access to alcohol that we document and provide additional robustness tests. First, we explore the role of liquidity constraints in generating the results. We test this by splitting the sample by a proxy for liquidity constraints: bank account balance. We find that the impact of the opening hours experiment is concentrated in the below-median-liquidity group. Second, we investigate whether the results are driven by a few outliers (alcoholics). We find that the demand for credit following the experiment was widespread and relatively smooth across young individuals. Third, we examine whether 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 individuals as well as the nearly retired to 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. In addition, we provide robustness analyses by excluding border counties, randomly assigning treatment and control groups in a permutation test, and running our analysis on an aggregated county level.

Overall, our findings indicate that greater availability of alcohol led to 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 people to spillover effects in the labor market. Our results suggest that restricted liquor store opening hours could serve as a commitment device for consumers with self-control problems.

Our study is closest to Schilbach (2019), who performs a field experiment in India in which he provides incentives to rickshaw drivers to remain sober. 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. Both papers study the effect of commitment devices on drinking and another patience-related activity: savings in Schilbach case, credit utilization and default in ours. The two studies have several important differences, which make them complementary. First, Schilbach's test is a positive reinforcement for a savings activity, whereas our study tests a negative reinforcement (temptation) to increase consumption. This means that the commitment device is *imposed and/or offered* in Schilbach's test, but it is *lifted* in our study (which is equivalent to the period after the end of Schilbach's experiment). Second, Schilbach's experiment is designed to provide a *temporary* and *experimental* set of incentives. In contrast, our study examines a policy that has been used by governments around the globe for decades. The change in policy was designed to be permanent. Third, there is a difference in the social settings between the studies (Indian cab drivers vs. the Swedish low-income population). Despite the difference in the social context, the two studies find qualitatively similar effects (with opposite signs), large multipliers associated with the availability of alcohol. Finally, our study complements Schilbach's study in that it examines the self-reported conjecture documented in his Online Appendix Table A.3, in which 80% of surveyed participants report that "life would be better if liquor stores closed."

We contribute to the literature on three fronts. First, we present evidence of a causal link between an increase in access to alcohol and financial wellbeing. The link between alcohol and savings has been previously studied by psychologists and economists. Prior research has 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, Ray, and Yeltekin (2015) differentiate

between normal goods and temptation goods like alcohol or 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, Roman, and Martin, 1993; Jones, Casswell, and Zhang, 1995; Fisher, Hoffman, Austin-Lane, and Kao, 2000; McFarlin and Fals-Stewart, 2002).

Second, our results also shed light on the relation between present-focused preferences and financial behavior, where previous studies have found evidence about the nature of this correlation in normal and distressed circumstances. Meier and Sprenger (2010) and Skiba and Tobacman (2008) document a positive correlation between present-focused preferences (elicited or estimated) and high-interest-rate borrowing. Carvalho, Meier, and Wang (2016) observe that individuals under financial stress exhibit behavior consistent with present-focused preferences with respect to monetary-related decisions.[4]

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). Yet prior studies have found conflicting evidence about the effectiveness of restricting consumer access to temptation goods as a commitment mechanism. Hinnosaar (2016) proposes a model that ties present-focused preferences and alcohol consumption. Then, using longitudinal survey data of 500 families, she finds evidence that store opening hours matter for off-site consumption for about a third of the families. She also suggests that the best way to test whether store opening hours serve as a commitment device is to use the Swedish exper-

[4]See Schilbach, Schofield, and Mullainathan (2016) and Dean, Schilbach, and Schofield (2017) for reviews of the literature connecting poverty and behavioral biases.

iment (p. 110) that we study in this paper. Using the same Swedish experiment, Norström and Skog (2005) and Grönqvist and Niknami (2014) document an increase in off-site alcohol consumption of about 4% following the expansion of operating hours on Saturdays. Bernheim, Meer, and Novarro (2016) study the effects of Sunday operating hours for liquor stores in the United States on aggregate state-level on-site and off-site consumption. They find that while on-site alcohol consumption increased following the relaxation in store opening hours, the increase in off-site consumption was statistically insignificant (t-statistic around 1). Currie, DellaVigna, Moretti, and Pathania (2010) find a strong association between the proximity of fast food restaurants to schools and students' weight and obesity, evidence consistent with the idea that controlled access to temptation goods can serve as a commitment device.

2 Background and Identification

2.1 The Swedish Alcohol Experiment

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, Systembolaget had about 520 retail agents in rural areas, through which consumers could pre-order alcohol. The minimum legal age to buy alcohol at Systembolaget

is 20, a rule that is strictly enforced.[5] Cashiers are instructed to ask for identification from customers who appear younger than 25 (Norström and Skog, 2005; Grönqvist and Niknami, 2014).[6] For further details about the Swedish alcohol market, see Online Appendix B.

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 in 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 the social researchers Thor Norström and Ole-Jörgen 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.[7] In addition, they selected control counties and designated buffer counties that were excluded from the experiment to prevent spillage across county lines.[8] The sorting of counties into treatment and control was not random; however, 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). The map in Figure 1 identifies the treatment, control, and buffer counties.

The initial assessment of the experiment was conducted a few months after its introduc-

[5]Restricting access to alcohol and drugs is one of the top long-term goals of Swedish authorities. For example, see Government Bill 2010/11:47, which is summarized in the report titled "A cohesive strategy for alcohol, narcotic drugs, doping and tobacco (ANDT) policy" by the Ministry of Health and Social Affairs Sweden (see `https://www.government.se/information-material/2011/05/a-cohesive-strategy-for-alcohol-narcotic-drugs-doping-and-tobacco-andt-policy/`). According to this document, the Swedish strategic policy has seven long-term objectives. Two of these seven objectives focus on restricting the under-age consumption of alcohol (along with other substances). Long-term objective #2 states "Protecting children against the harmful effects of alcohol, narcotic drugs, doping and tobacco." Long-term objective #3 states "Gradually reducing the number of children and young people who initiate the use of tobacco, narcotic drugs or doping substances or begin drinking alcohol early."

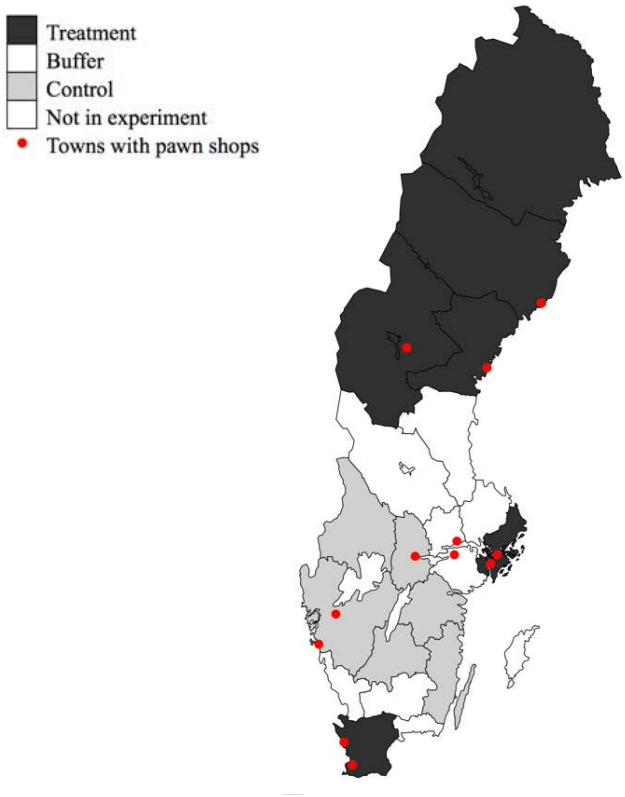
[6]Systembolaget also conducts mystery customer audits using a third-party contractor and has reported consistently that in 95–96% of the cases, cashiers ID customers who appear young.

[7]The treatment counties were Stockholm, Skåne, Norrbotten, Västerbotten, Västernorrland, and Jämtland. At the time, nearly half of the total Swedish population lived in the treatment region.

[8]Following the original researchers, Norström and Skog (2003), we also exclude the buffer counties from our analysis.

Figure 1. Map of Treated and Control Counties

In 2000, Sweden implemented a large experiment in which all alcohol retail stores in some counties were open on Saturdays. The researchers who designed the experiment selected the treatment counties (where the stores would be open on Saturdays) 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 (gray) were Östergötland, Jönköping, Kalmar, Västra Gätaland, Värmland, and Örbro. Gotland (white) was not included in the experiment because of extreme seasonality in 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 could be diluted by cross-county border shopping.



tion 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 4% 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 in July 2001. In a follow-up study, Norström and Skog (2005) updated their estimates to a 3.7% increase in consumption and found that the purchased alcohol was consumed immediately. 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) using a richer data set. Their findings confirmed the overall increase in alcohol sales of 4%, but they also found that overall crime increased by about 20%.

The extended opening hours of the liquor stores could have affected people's motivation to purchase alcohol in two ways. First, Saturday sales could relax a pre-commitment device, giving individuals with present-focused preferences 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 examine the various channels through which the relaxation of the opening hours might have affected consumption patterns.

2.2 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.[9] An ideal experiment to identify this causal effect, therefore, would consider two identical groups of individuals, only one of which is exposed to increased access to alcohol.

We use the variation in alcohol availability induced by the February 2000 Swedish experiment in two empirical approaches. The first is based on a difference-in-difference (double-diff; DD) analysis that compares credit, default, and labor market behavior before and after the policy change and across treated and control counties. Importantly, as we do not observe

[9]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 trouble (omitted variables).

actual alcohol consumption, our specification is an intent-to-treat framework (Angrist et al., 1996). This framework avoids the selection bias into the treatment. The DD specification is

$$y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}, \quad (1)$$

where β_1, the coefficient of interest, measures the differential likelihood of the outcome variable $y_{i,t}$ between consumers living in the treated versus the control counties during both the pre- and post-periods. We include individual (ω_i) and time (ω_t) fixed effects.[10] Because this DD specification does not allow within-county variation in the treatment, we cannot include county-time fixed effects. To mitigate this concern, we control for the county-time-specific gross domestic product (GDP) and employment rate.

The second identification strategy exploits the age restriction on alcohol sales in Sweden. Specifically, individuals below age 20 are not allowed to purchase alcohol off-site in Sweden, but they are allowed to borrow and participate in the labor market.[11] 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-diff (DDD) strategy. This approach enables us to verify that the results found for the young cohort in the DD approach are not driven by omitted variables and county-specific time trends. We use the following specification:

$$y_{i,t} = \beta_1 Post_t \times Treated_c \times Eligible_{i,t} + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times Eligible_{i,t} + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}, \quad (2)$$

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

[11] A critical assumption of using the DDD setting is that age-based alcohol policies are strictly enforced in Sweden. Systembolaget, the government monopoly stores, requires that buyers who look younger than 25 present identification (ID). In addition, Systembolaget uses third-party young auditors to conduct random checks of age verification. Systembolaget's annual reports show that around the time of the experiment (between 1998 and 2002), approximately 5,000 checks were conducted each year by external auditors. In each year, sellers in the Systembolaget stores verified the ages of 79% to 81% of the young auditors, on average. These facts provide reassurance that there is no material leakage between the 18–19 group and the 20–25 group with respect to access to alcohol. For further information, see `https://www.omSystembolaget.se/om-Systembolaget/foretagsfakta/ekonomisk-information/`.

where the variable of interest is the triple interaction. This specification also includes individual fixed effects (ω_i) and county-bimonthly (every other month) fixed effects ($\omega_{c\times t}$).

In several settings we can implement even sharper identification. Specifically, when examining pawn borrowing and crime, our data have the specific date of the event. Thus, we can assess whether the experiment led to increased borrowing following the weekend and whether crime increased on Saturday. For this purpose, we construct a daily panel for the individuals in our sample and use the following specification:

$$y_{i,t} = \beta_1 Post_t \times Treated_i \times 1(Day_t) + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times 1(Day_t) + \omega_i + \omega_d + \omega_{c\times t} + \varepsilon_{i,t}, \quad (3)$$

where the dependent variable is a dummy for whether an event took place on a particular date (e.g., individual was convicted of a certain crime on a particular date). The variable of interest is the triple interaction, and Day_t is an indicator variable for the weekday of interest (e.g., Saturday). This specification also includes individual fixed effects (ω_i), day-of-the-week fixed effects (ω_d), and county-bimonthly (every other month) fixed effects ($\omega_{c\times t}$).

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 Online Appendix C.2, we also verify that our results do not materially change when excluding the county that borders Denmark (Skåne), which might allow easy cross-country shopping from abroad. We also made sure that our results are not driven by strategic behavior focused on having greater access to alcohol by excluding the individuals who move between counties (about 1.6% of the population).

In general, our samples for the main regressions are at the individual-bimonthly (every other month) level, and standard errors are clustered at the municipal level. Due to the small number of counties (12), simply clustering errors at the county level may result in overstated statistical significance (Bertrand, Duflo, and Mullainathan, 2004). To find the preferred

cluster level, we run a test based on Roodman, Nielsen, MacKinnon, and Webb (2019). We implement a wild-cluster bootstrap (Cameron, Gelbach, and Miller, 2008; Cameron and Miller, 2015) by using the Stata command –boottest– (Roodman et al., 2019) for different levels of clustering (county, municipal, and individual level).[12]

3 Data and Summary Statistics

3.1 Data

Our main data set was provided by the Swedish pawnbrokers' association, which covers 99% of the total pawn-brokering market. This data set contains information on pawn transactions of 332,351 individuals between 1999 and 2012 on a daily frequency, including loan size, value and type of pledge, and subsequent repayment behavior.

The pawn credit industry and its customer base in Sweden are similar to that of the United States. In 2000, Sweden had 25 pawnbroker chains with 56 pawnshops, 14 of which were based in Stockholm. Pawnbrokers make fixed-term loans 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. The loan term in a standard contract is three to four months; borrowers can roll over their debt for a fee. In our data, pawnbrokers charge approximately 3.5% per month. Once the loan is repaid, the customer receives her collateral back. In the event of a default, the collateral is auctioned by the pawnbroker. Around the years of the experiment, 4% to 5% of the Swedish adult population engaged in pawn borrowing every year.

In the next stage, we convert the pawn data to a bimonthly (every other month) frequency and match the population from the pawn data set with records from the mainstream credit

[12]Then, we compare the wild-cluster bootstrap p-values when the coefficient of interest is fixed at zero ("restricted") or when we impose no restrictions ("unrestricted") in the boottest. Based on this test, we conclude that the coarsest cluster level that minimizes inconsistencies between the *restricted* and the *unrestricted* estimations is the municipal level.

data registry, provided 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. This data set contains snapshots every other month of individual credit records from 1999 to 2001. Overall, we have bimonthly (every other month) data for 61,527 individuals in the control counties and for 102,855 individuals in the treatment counties. Once we restrict the population to those between ages 18 and 25, the sample reduces to 38,320 individuals.

We also collect labor market information from the tax authorities through Statistics Sweden (SCB). These data are at an annual frequency from 1998 to 2005 and include information on each individual's employment status. We use information about employment status, welfare dependence, pre-tax income, and the number of reported sick days.

Finally, we use crime incident data from the Swedish conviction register administered by the National Council for Crime Prevention (BRÅ). The register contains the complete records of all criminal convictions in Swedish district courts during the pre- and post-periods, including information on the type of crime as well as the sentence given by the court. Although 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 on crimes that have the highest incidence of being committed while the perpetrator is intoxicated, i.e., drunk driving[13] and assaults.[14] We also split the sample by gender as we find that men are heavily overrepresented among criminal offenders in general and in particular those that involve a perpetrator who is under the influence of alcohol (see Olseryd, 2015).

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

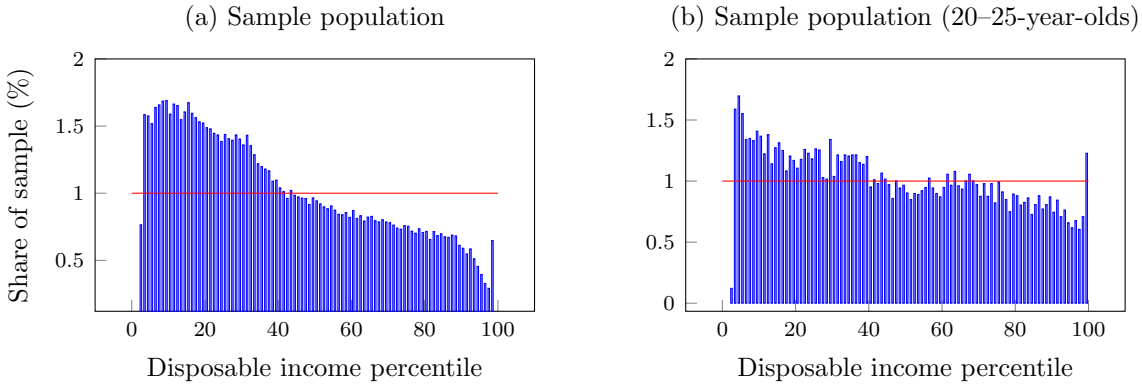
[14]Assaults are defined by the justice department's penal law: Brottsbalk (1962:700) t.o.m. SFS 2018:1745, Chapter 3: "Om brott mot liv och hälsa." We exclude crimes defined in §10, crime within a work environment (arbetsmiljöbrott).

3.2 Summary Statistics

We begin the empirical analysis by discussing select summary statistics for the treated population in the pre-period. In general, our sample is composed of people of relatively low socioeconomic status. Illustrated in Figure 2, Panel (a), by their relative over (under) representation in the lower (higher) percentiles of the disposable income distribution. Since wage disparities become larger as careers evolve, there are smaller differences for the younger cohort, see Figure 2, Panel (b).

Figure 2. Disposable Income of Sample vs. the Swedish General Population

This figure displays the share of individuals in our sample in each disposable income percentile (bars in blue) relative to the share of individuals in the Swedish general population (horizontal line in red). Panel (a) displays the income distribution for all ages. Panel (b) displays all individuals in the age range of 20–25 years old.



In addition, Table 1 provides the summary statistics for our outcome variables during the period before the experiment started (February 1999 to February 2000). Panel A presents summary statistics about pawn borrowing. The average number of new pawn loans is 0.11, and the default rate is 0.8% per month in the pre-period. Panel B presents the mainstream credit outcome variables for the pawn-borrowing population. As we focus on the Swedish population that lives on the margins of formal credit markets, it is no surprise that the percentage of individuals with an arrear is 4.1%. 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

Table 1. Summary Statistics

This table presents sample statistics for the cohort of 20–25-year-olds for the period *prior* to the experiment: the Swedish government's expansion of liquor store operating hours on Saturdays in some counties (i.e., pre-February 2000). Note that the crime mean and standard deviations are multiplied by 1,000. In Table 2, we present the split treatment and control summary statistics for the same pre-period.

	Mean	Std dev	Median
Panel A: Pawn credit market			
# 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			
# 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 within 2 months)	0.041	0.198	
# Individuals		24,435	
Panel C: Labor market and crime			
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 on Saturday > 0) $\times 1,000$	0.0302	5.48	0
1(Drunk driving on Saturday > 0) $\times 1,000$	0.0140	3.74	0
# Individuals		27,111	

total income, as registered by the tax authorities at that time. For the analysis examining labor market outcomes, we focus on unemployment, the amount of welfare received in the year, pretax income, and the number of sick days taken. For crime, we analyze both assaults and drunk driving on the day that the alcohol stores' opening hours were expanded, i.e., on Saturdays. The summary statistics for these variables are presented in Panel C.

Table 2 presents comparative summary statistics for the sample used in our baseline

Table 2. Summary Statistics, Split by Treatment and Control Counties

This table presents sample statistics for the 20–25 age cohort during the pre-period, i.e., *prior* to the Swedish government began opening liquor stores on Saturdays in some counties in February 2000. About 40 people move between treatment and control counties in the preperiod and are double counted. We test to ensure that our results are not driven by strategic movers (see Section Online Appendix C)

	Treated counties			Control counties		
	Mean	Std dev	Median	Mean	Std dev	Median
Panel A: Pawn credit market						
# New pawn loans	0.123	0.436	0	0.100	0.387	0
Loan size (SEK)	215.2	1,051	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						
# Credit cards	0.208	0.677	0	0.047	0.317	0
Credit card balance (SEK)	951.2	4,032	0	229.1	2,048	0
# Installment loans	0.046	0.244	0	0.011	0.119	0
Installment loans limit (SEK)	1,209	9,692	0	324.1	4,977	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 within 2 months)	0.055	0.228		0.017	0.131	
# Individuals		15,252			9,206	
Panel C: Labor market and crime						
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)	0.0000302	0.00549		0.0000297	0.00545	
1(Drunk driving on Saturday > 0)	0.0000136	0.00369		0.0000148	0.00383	
# Individuals		16,950			10,198	

analysis in the pre-treatment period for the treatment and the control counties. Online Appendix Table A1 contains definitions of both the dependent and independent variables of interest. Online Appendix Table A2 presents summary statistics for the cash journal data used in the study (see Section 4.1.1).

4 Main Results

4.1 Empirical Implementation

4.1.1 Cohort Effects of Increased Access on Alcohol Consumption

As the focus of our study is the effect of increased access to alcohol on individuals' financial wellbeing, we begin by exploring the effects on consumption, which is the channel between the increased access and the effects in the credit market, labor market, and crime outcomes. Earlier literature has documented that the increase in opening hours in Sweden resulted in an increase in the sales of alcohol of approximately 4% (Norström and Skog, 2003, 2005; Grönqvist and Niknami, 2014).

We estimate the effects of increased access to alcohol on alcohol consumption using cash journals that document individuals' expenditure on various consumption items. The cash journals were administered by Statistics Sweden for randomly selected households of the Swedish population during 1999–2001.[15] The data consist of annual repeated cross-sections. We use the year 1999 as the pre-period and 2000–2001 as the post-period. We split the data into age cohorts (20–25, 26–35, 36–45, 46–55, and 56–65) and run the following regression for each cohort of respondents:

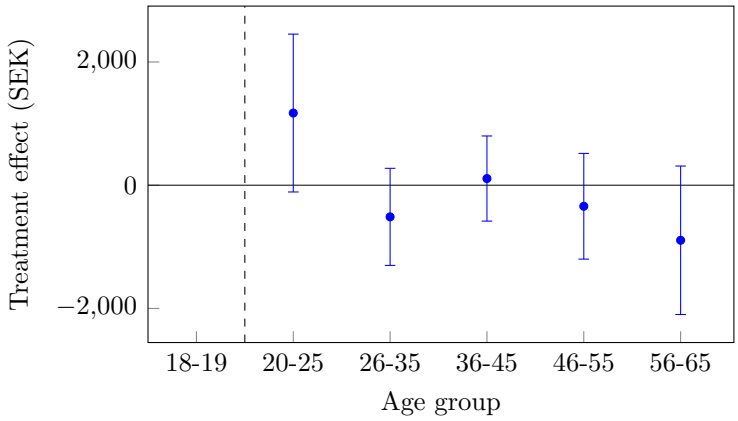
$$Alcohol\ Expenditure_i = \beta_1 Post_t \times Treat_c + \beta_2 Disposable\ Income_i + \omega_t + \omega_c + \epsilon_i, \quad (4)$$

where $Post_t \times Treat_c$ is an indicator whether the individual reports in the post period (2000 or 2001) and whether she lives in one of the treated counties. $Alcohol\ Expenditure_i$ measures the annual expenditure on alcohol in SEK. $Disposable\ Income_i$ is annual disposable income,

[15]The data are called HUT "Utgiftsbarometern." The journal data were 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 the annual level. We use data from 1999–2001 covering the 4,688 households that responded out of 9,000 contacted (3,000 each year).

Figure 3. The Effect of Increased Access to Alcohol on Alcohol Consumption

The figure shows that increased access to alcohol causally increased alcohol consumption. For each cohort, we plot the coefficient β_1 and standard errors based on the following specification: $Alcohol\ Expenditure_i = \alpha + \beta_1 Post_t \times Treated_c + \beta_2 Disposable\ Income_i + \omega_c + \omega_t + \varepsilon_i$. Standard errors are clustered at the municipality level. The whiskers represent 1.96 standard errors on each side. The figure is 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.



and ω_t and ω_c represent year and county fixed effects, respectively.

The results are presented in Figure 3. The analysis shows that individuals ages 20–25 increased their alcohol consumption the most following the expansion in opening hours. The increase in consumption is on the order of 1,100 SEK per year. This result is based on a small number of individuals,[16] as reflected in the large standard errors, hence should be taken with caution.

4.1.2 Cohort Effects of Increased Access on Credit, Labor, and Crime

Next, we examine the effects of expanded liquor store opening hours on credit and labor market outcomes using detailed administrative credit and labor data. Specifically, we run the following DD regression:

$$y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}. \quad (5)$$

[16]The sample includes 97 individuals who are 20–25-year olds and lived in the control counties, and 77 individuals who are 20–25-year olds and lived in the treatment counties.

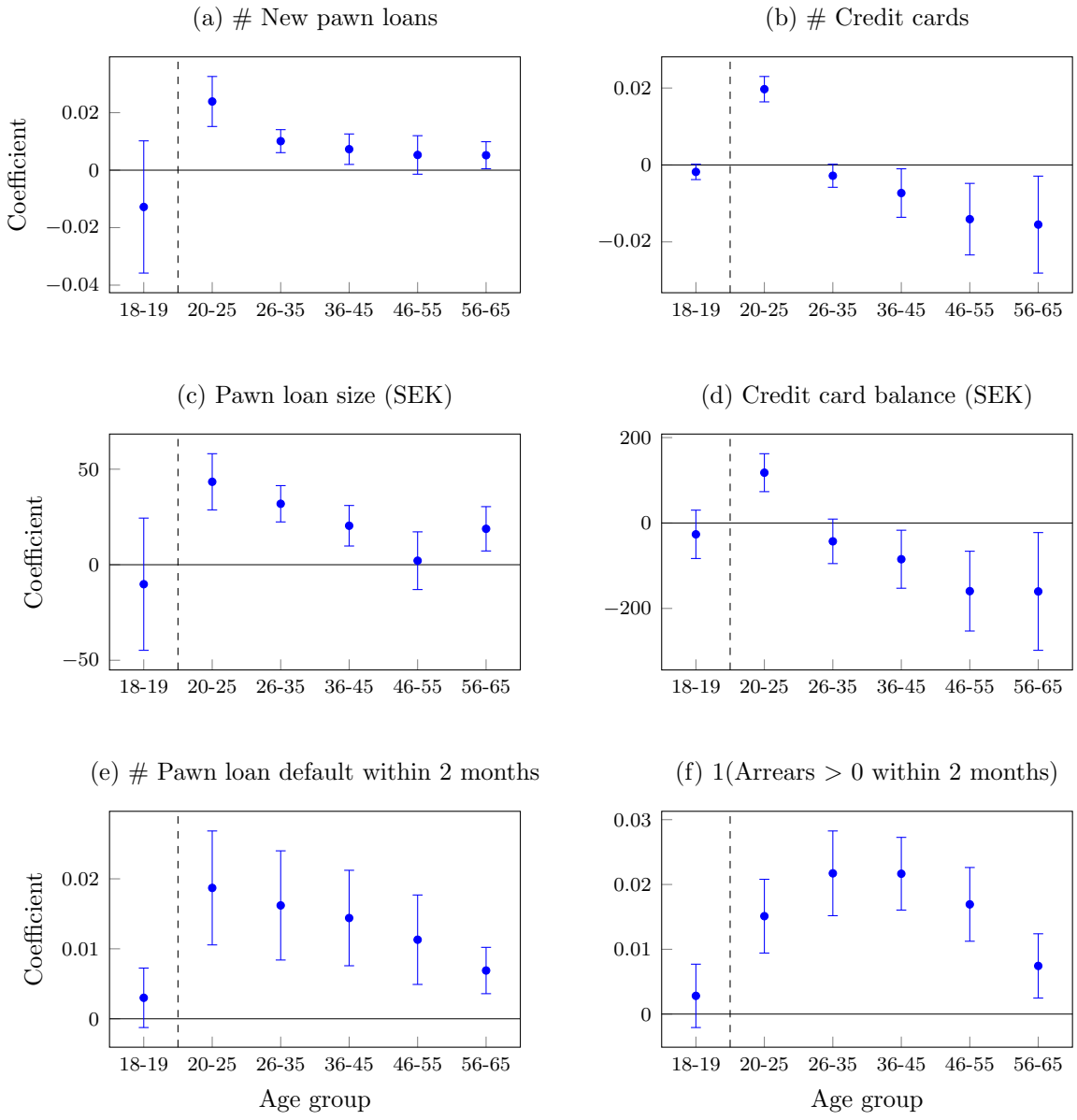
We run the analysis separately for six age cohorts: 18–19, 20–25, 26–35, 36–45, 46–55, and 56–65. We look at these cohorts separately in order to investigate the differential effects of the expansion in the supply of alcohol on different age groups. Young people (ages 18–19) are not allowed to buy alcohol 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.

Figure 4 plots the distribution of the coefficients β_1 from estimates produced using Regression (1). The figure show outcomes for pawn loans (Panels (a), (c), and (e)): number of pawn loans, pawn loan size, likelihood of default on a pawn loan within two months. In addition, it shows outcomes for the mainstream credit market (Panels (b), (d), and (f)): 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. There is, however, a large increase in the credit utilization, indebtedness, and default for the 20–25-year-old treated group, and a typically smaller effect for older groups.

In a similar fashion, Figure 5, Panels (a) to (d), plot the DD 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, amount of welfare received, pretax income, and the number of sick days taken. Again, we observe no significant effect among the 18–19-year-old group. Moreover, for all variables except sick days, we observe a large effect for the 20–25-year-olds and a weaker effect for older age groups. For the number of sick days, the effect is statistically insignificant for the 20–25 group, and it is strongest for the 46–55-year-olds, perhaps due to differences in alcohol tolerance by age. Likewise, Figure 5, Panels (e) and (f), show that the effects of increased crime on Saturdays (drunk driving and assaults) are strongest for the 20–25 cohort. Crime on Saturdays is statistically insignificant

Figure 4. The Effect of Increased Access to Alcohol on Credit and Default

These figures show that increased access to alcohol causally increased credit borrowing and default risk. For each cohort, we plot the coefficients β_1 and standard errors based on the following specification: $y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$. Standard errors are clustered at the municipality level. The whiskers represent 1.96 standard errors on each side.

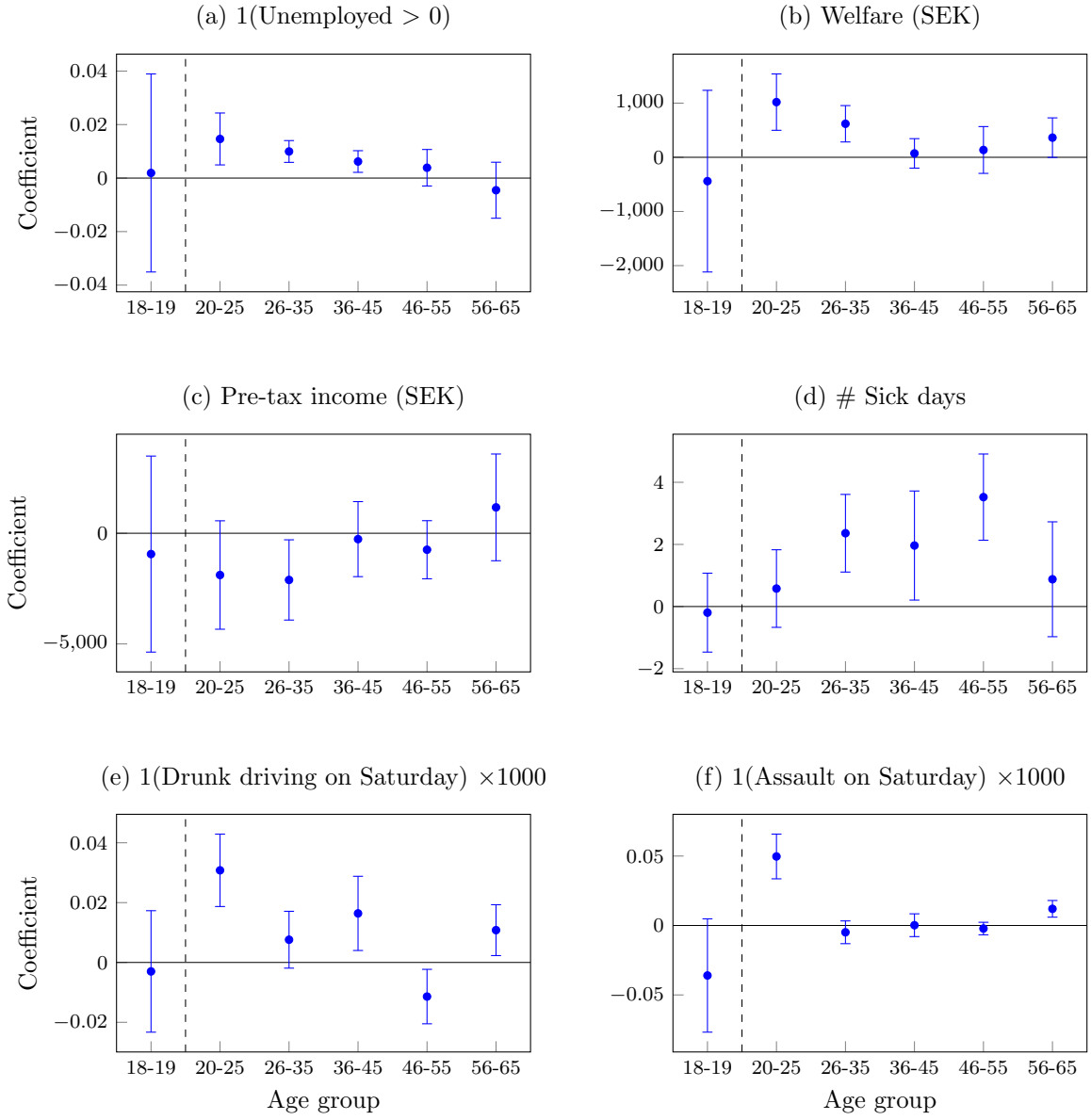


for the 18–19 group.

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).

Figure 5. The Effects of Increased Access to Alcohol on Labor and Crime Outcomes

These figures show that increased access to alcohol increased unemployment, welfare dependence, and reported sick days, as well as drunk driving on Saturdays and assaults on Saturdays. Furthermore, we find a decrease in income. For each cohort, we plot the coefficients β_1 and standard errors based on the following specification: $y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$. Standard errors are clustered at the municipality level. The whiskers represent 1.96 standard errors on each side.



4.2 Pre-trend 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.

As such, 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 6 and 7. The coefficients presented in these figures were obtained from the DDD specification in Regression (2), but we replace the *Post* variable in the main interaction with a series of indicators for every other month, centered around February 2000 (the beginning of the experiment). The coefficients reflect the difference between treatment and control and between individuals eligible and not eligible to buy alcohol in that particular period. We follow Brown, Grigsby, van der Klaauw, Wen, and Zafar (2016) and perform a Wald test of the null hypothesis, that the coefficients during the pre-period are jointly equal. Our results generally show that we cannot reject the null with a p-value > 0.05. The Wald test results are presented in the top left corner of each chart.

Figure 6 shows the event-time evolution of the period-by-period effects of the treatment for the number of pawn loans the individual borrowed during the 60-day period, pawn loan size, and pawn default. The figure also shows the number of credit cards individuals own, credit card balances, and new arrears. The Wald tests indicates that no pre-trends exist.

Figure 7 repeats this exercise for the labor market and crime results. We examine the following labor market outcomes (annual frequency instead of every other month): an unemployment dummy, welfare received, pretax income, and the number of sick days taken. Again, the graphs and the associated Wald tests provide evidence consistent with the identification assumption of parallel trends. Also, Figure 7 shows the graphs for convictions for drunk driving and assault that took place on Saturdays (Regression (3)). 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 pre-period.

4.3 Credit Market

Our analysis of the effects of the increased availability of alcohol on indebtedness explores the pawn and mainstream credit markets. Table 3 focuses on the pawn market. Odd-

Figure 6. Pre-Trends for Credit 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 in regard to mainstream credit outcomes. The panel depicts estimates of the β_τ coefficients and their 95% confidence intervals from the following model: $y_{i,t} = \Sigma_{t=-3}^{t=7}\beta_\tau Period_t \times Treated_c \times Eligible_{i,t} + \xi_1 Treated_c \times Eligible_{i,t} + \xi_2 Post_t \times Eligible_{i,t} + \xi_3 Eligible_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. The x-axis shows event time, which is 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 pre-period are normalized at $t = -2$, and the respective length of the pre-period is determined by data restrictions. Standard errors are clustered at the municipality level. The whiskers represent 1.96 standard errors on each side.

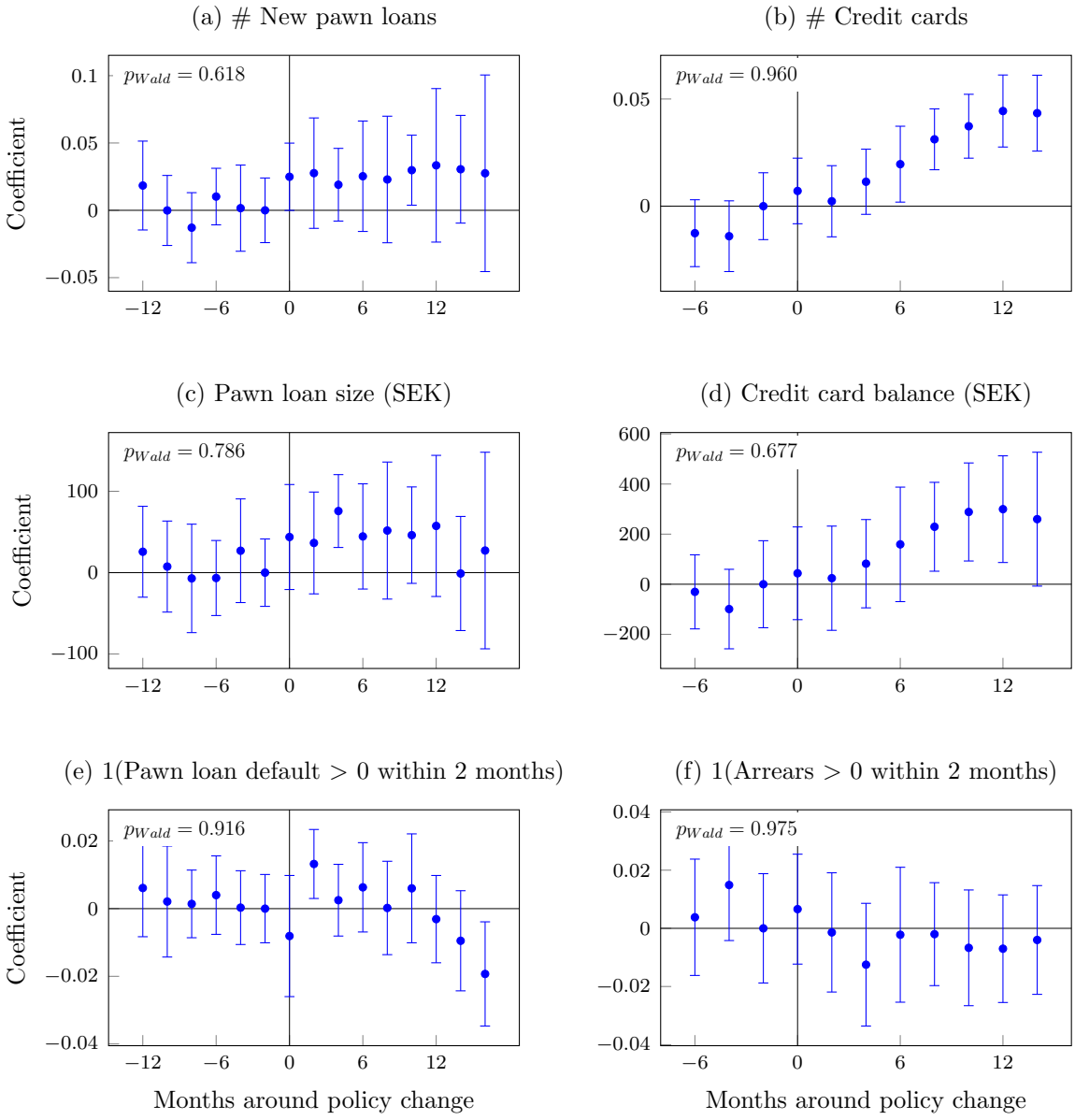
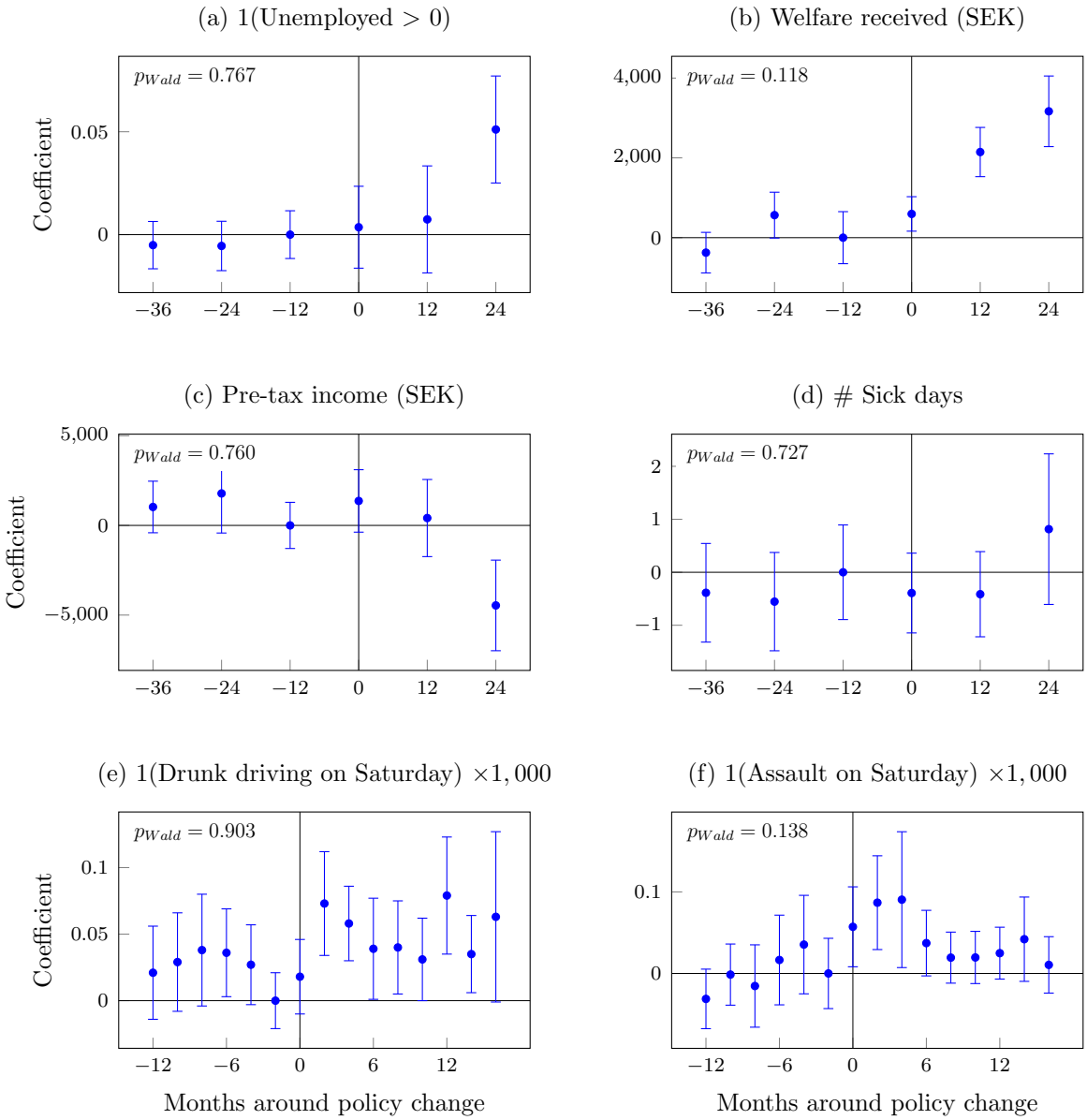


Figure 7. Pre-Trends for Labor Market and Crime Outcomes

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 in regard to labor market and crime outcomes. Panels (a) to (d) depict estimates of the β_τ coefficients and their 95% confidence intervals from the following model: $y_{i,t} = \Sigma_{t=-3}^{t=2}\beta_\tau Period_t \times Treated_c \times Eligible_{i,t} + \xi_1 Treated_c \times Eligible_{i,t} + \xi_2 Post_t \times Eligible_{i,t} + \xi_3 Eligible_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. Panels (e) and (f) depict estimates of the β_τ coefficients and their 95% confidence intervals from the following model: $y_{i,t} = \Sigma_{t=-6}^{t=8}\beta_\tau Period_t \times Treated_c \times Saturday_t + \xi_1 Treated_c \times Saturday_t + \xi_2 Post_t \times Saturday_t + \xi_3 Saturday_t + \omega_i + \omega_d + \omega_{c\times t} + \varepsilon_{i,t}$. Pre-period estimates are normalized at $t = -12$ for Panels (a) to (d) and at $t = -2$ for Panels (e) and (f). Standard errors are clustered at the municipality level. The whiskers represent 1.96 standard errors on each side.



numbered columns present regression results from DD specifications, and the even-numbered columns report the corresponding results from the DDD specifications. Columns (1) and (2) show an increase in the extensive margin. Specifically, 20–25-year-olds took out more pawn loans in the treated counties relative to peers in control countries (Column (1)) and relative to 18–19-year-olds within the same county (Column (2)). The size of the effect is the same in both specifications and reflects an increase of 19% relative to the pre-period mean. Columns (3) and (4) show an increase in the intensive margin, i.e., 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 DD specification (Column (5)), indicating a doubling in the number of defaulting loans. In contrast, there is no statistically significant result in the DDD specification (Column (6)). When considering pawn loan rollovers (Columns (7) and (8)), the point estimates are both positive, albeit statistically significant only in Column (7).

We next explore the effects of extending the opening hours of liquor stores on the mainstream credit market: credit cards, installment loans, and personal credit lines. In Table 4, Columns (1)–(4) report 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 no meaningful effect on the number of installment loans and credit lines. Installment loans are essentially credit provided when purchasing larger items, like the popular Billy bookcase and Dombäs 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 used to the finance 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 DDD specification (Column (8)). The effects on credit lines are mixed: Columns (9) and (10) show a decrease in the number of credit lines of 4% to 8%. However, Columns (11) and (12) show an increase in the average credit balance of credit lines of about 10%.

As for credit performance, the treated group exhibits poorer performance. The likelihood

Table 3. Pawn Credit Outcomes

This table shows that increased access to alcohol causally increases credit borrowing and 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. Columns (2), (4), (6), and (8) show regressions from DDD regressions. The sample for this analysis also includes 18–19-year-olds, an age group ineligible to buy alcohol in Sweden. The regression is a triple-difference specification (eligible/ineligible, treatment/control, and pre/post). Standard errors are clustered at the municipal level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# New pawn loans		Pawn loan size (SEK)		# Pawn loan defaults		# Pawn rollovers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	0.0239***		43.23***		0.0187**		0.00313*	
	(0.0087)		(14.67)		(0.0081)		(0.00159)	
Post × Treated × Eligible		0.0236**		40.44		-0.00316		0.00430
		(0.0117)		(29.63)		(0.00377)		(0.00435)
Post × Eligible		-0.0199*		-24.36		0.00433**		-0.00688**
		(0.0105)		(24.12)		(0.00217)		(0.00325)
Treated × Eligible		-0.0155		-28.59		0.00323		-0.00668
		(0.0109)		(21.88)		(0.00292)		(0.00553)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
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 employment	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	0.320	0.315	0.310	0.308	0.165	0.157	0.292	0.286
# Individuals	32,826	38,320	32,826	38,320	32,826	38,320	32,826	38,320
Pre-period mean	0.1233	0.1233	215.2	215.2	0.00845	0.00845	0.03708	0.03708
Relative effect	19%	19%	20%	19%	221%	-37%	8.4%	12%

of having any recorded arrears increases by 28% to 14% (Columns (13) and (14)).

Overall, our analysis of credit behavior shows that the treated group increased its credit usage and experienced a deterioration in performance in both pawn and credit card instruments.

Table 4. Mainstream Credit Outcomes

This table shows that increased access to alcohol causally increases credit borrowing and default risk. The odd-numbered columns show double-difference (DD) regressions comparing individuals ages 20–25 in the treatment counties to those in the control counties, pre and post the expansion in opening hours: $y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$. The even-numbered columns show results from triple-difference (DDD) regressions: $y_{i,t} = \beta_1 Post_t \times Treated_c \times Eligible_{i,t} + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times Eligible_{i,t} + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. The sample for this analysis also includes 18–19-year-olds, an age group ineligible to buy alcohol in Sweden. Standard errors are clustered at the municipal level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# Credit cards		Credit cards balance (SEK)		# Installment loans		Installment limit (SEK)		# Credit lines		Credit lines balance (SEK)		1(Arrears> 0)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Post × Treated	0.0197***		117.8***		-0.0011		139.2***		-0.0118***		328.6***		0.0151***	
	(0.0033)		(24.4)		(0.0014)		(48.2)		(0.0030)		(84.8)		(0.0057)	
Post × Treated × Eligible		0.0227***		165.7***		-0.0019		10.8		-0.0247***		336.3***		0.0075*
		(0.0050)		(47.0)		(0.0013)		(64.8)		(0.0064)		(121.9)		(0.0045)
Post × Eligible		-0.0035		-72.9***		0.0035***		130.6**		0.0027		50.2		0.0086***
		(0.0032)		(26.5)		(0.0012)		(54.2)		(0.0056)		(81.9)		(0.0032)
Treated × Eligible		-0.0191***		-87.5		0.0093**		224.1*		0.0180		-404.3*		-0.0140*
		(0.0073)		(74.8)		(0.0047)		(122.0)		(0.0113)		(239.7)		(0.0074)
County FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Month FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Regional employment	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	233,287	261,905	233,287	261,905	233,287	261,905	233,287	261,905	233,287	261,905	233,287	261,905	233,139	261,748
R^2	0.897	0.897	0.761	0.760	0.783	0.782	0.766	0.765	0.828	0.826	0.830	0.830	0.435	0.435
# Individuals	29,416	34,902	29,416	34,902	29,416	34,902	29,416	34,902	29,416	34,902	29,416	34,902	29,372	34,852
Pre-period mean	0.208	0.208	951.2	951.2	0.046	0.046	1,209	1,209	0.328	0.328	3,488	3,488	0.055	0.055
Relative effect	9.5%	11%	12%	17%	-2.4%	-4.1%	12%	0.9%	-3.6%	-7.6%	9.4%	9.6%	28%	14%

4.4 Monday Borrowing

To provide further corroborating evidence about the causal relation between the increased availability of alcohol and indebtedness, we take advantage of the high-frequency nature of the pawn registry. The impromptu consumption of alcohol on Saturdays causes individuals in the treated counties to be more likely to hit an unexpected liquidity shortage over the weekend. However, since pawn shops were closed over the weekends in the early 2000s, the liquidity shortage would translate into borrowing once pawn shops opened, on Monday.

We construct a person-date data set in which we record the number of pawn loans that each person took on a particular date (typically zero or one). Table 5, Columns (1) and (3) show the baseline regression results (DD and DDD, respectively). In Columns (2) and (4), we explore whether there was an uptick in pawn borrowing activity on Mondays in counties exposed to greater availability of alcohol. We interact the variable of interest with a Monday indicator and add day-of-the-week dummies to absorb the average tendency to borrow on a certain day. The table shows that 24–27% of the increase in pawn borrowing due to the treatment takes place on Mondays.[17]

In summary, the results in Table 5 indicate a disproportionate increase in borrowing on Mondays among the treated group, consistent with the idea that the extended opening hours on the weekend generated "unexpected" negative shocks to consumers with present-focused preferences. A rational consumer would be able to avoid the liquidity shortage on the weekend as she would plan the purchase and would borrow ahead of time, if needed.

4.5 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, i.e., caused additional expenditure or financial consequences. For example, consumption of alcohol may be associated

[17]We calculate the 24–27% 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%. A similar calculation using the coefficients in Column (4) yields 27%.

Table 5. Weekly Pattern of Pawn Credit Borrowing

This table tests whether pawn borrowing in the treatment group was more likely to take place on Mondays. The sample is at the person-day level and covers the years 1999 to 2001. Columns (1) and (2) show results from the double-difference (DD) 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: $y_{i,t} = \beta_1 Post_t \times Treated_c \times 1(Monday_t) + \beta_2 Post_t \times Treated_c + \beta_3 GDP_{c,t} + \beta_4 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$. Columns (3) and (4) show results from triple-difference (DDD) regressions. The sample for this analysis also includes 18–19-year-olds, an age group ineligible to buy alcohol in Sweden. The regression is a DDD specification (eligible/ineligible, treatment/control, and pre/post): $y_{i,t} = \beta_1 Post_t \times Treated_c \times Eligible_{i,t} \times 1(Monday_t) + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times Eligible_{i,t} + \beta_4 Eligible_{i,t} + \ldots 1(Monday_t)\ interactions \ldots + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. $1(Monday)$ 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 at a daily frequency. The data include borrower-calendar day observations in which we count the number of pawn loans that were taken in every calendar day of the week (typically zero or one). Standard errors are clustered at the individual level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# New pawn loans			
	(1)	(2)	(3)	(4)
Post × Treated	0.00050**	0.00047**		
	(0.00021)	(0.00018)		
Post × Treated × 1(Monday)		0.00013		
		(0.00016)		
Post × Treated × Eligible			0.00036	0.00033
			(0.00040)	(0.00039)
Post × Treated × Eligible × 1(Monday)				0.00015*
				(0.00016)
1(Monday)		0.00060***		0.00057***
		(0.00007)		(0.00006)
Post × Eligible			-0.00040	-0.00042*
			(0.00025)	(0.00025)
Treated × Eligible			-0.00043	-0.00043
			(0.00034)	(0.00034)
Weekday FE	No	Yes	No	Yes
County FE	Yes	Yes	No	No
Calendar month FE	Yes	Yes	No	No
Individual FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Regional GDP	Yes	Yes	No	No
Regional employment	Yes	Yes	No	No
County × Calendar month FE	No	No	Yes	Yes
Observations	16,866,840	16,866,840	19,088,938	19,088,938
R^2	0.015	0.015	0.015	0.015
# Individuals	37,824	37,824	44,071	44,071
Sample period	1999–2001	1999–2001	1999–2001	1999–2001
Ages	20–25	20–25	18–25	18–25

with additional purchases, road accidents, or loss of income.

To calculate the multiplier effect 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 expenditure can be estimated using past studies of alcohol consumption patterns in Sweden as well as the studies that analyzed the opening hours experiment. Statistics Sweden (Statistiska centralbyrån) collects expenditure information for various items using cash journals distributed to a sample of individuals. We use the data covering expenditure information by 4,688 people for the years 1999–2001. The average annual spending on alcohol is provided in Online Appendix Table A5. The table shows that the average spending on alcohol among young people in the lowest income group (likely to be the population in our main sample) was about 2,800 SEK (about $280) a year.[18] 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.[19] We obtain similar figures if we rely on aggregate data.[20]

Now, compare this estimation to our finding in Tables 3 and 4 that, on average, people ages 20–25 living in the treated counties increased their total debt balance by about 502 SEK

[18]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 the lowest income quintile spend $220 per year on alcohol, relative to the average consumption of $349. Individuals in the 20–25 age range spent $368 per year, on average.

[19]We do not observe individuals' change in alcohol consumption directly and therefore make the assumption that the increase in alcohol expenditure for the young is equal to the average increase in alcohol sales for the population (see estimations by Norström and Skog, 2003; Grönqvist and Niknami, 2014). If, however, individuals ages 20–25 increased their alcohol expenditure by more than the average person in the population did, then the multiplier that we calculate would be lower (and would be closer to the estimation by Schilbach, 2019). It is unclear whether the larger effect that we find for the young relative to older cohorts (e.g., on indebtedness) is generated by a larger increase in spending on alcohol or by the fact that they are likely to be closer to their liquidity constraint than older cohorts. If indeed young people have a stronger consumption response to the availability of alcohol, then the 4.5 multiplier estimate is likely to be an upper bound of the multiplier.

[20]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 4%. 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 1), 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%)).

(about $50).[21] Comparing this amount to the estimated amount spent on alcohol of 112 SEK suggests a multiplier effect of 4.5, which is consistent with the idea that increased alcohol consumption leads to poor decision making on other dimensions such as driving under the influence (Wagenaar et al., 2000; Levitt and Porter, 2001), lack of savings (Schilbach, 2019), or loss of income or jobs, as we report here. This figure is twice as large as the magnitude of the multiplier that Schilbach (2019) reports in the population of Indian rickshaw drivers, though, of course, the populations in the two studies are different (Indian cab drivers versus Swedish young people) with differential access to credit. For example, Swedes are able to borrow through credit cards and thus may be able to increase consumption more easily in response to greater availability of alcohol compared to Indian cab drivers.

This estimate reflects a lower bound for both the average effect across the population and for those who suffer from present-focused preferences. The increase in debt balances is estimated using the accounts of all 20–25-year-olds. Our measurement of the effects are based on *debt* balances. However, some individuals may finance their consumption through other sources, e.g., available cash. When we split the sample by financial constraints, the effect increases by 50% to about 750 SEK, on average, for the group that we identify as more financially constrained (see Table 8 discussed in more detail in Section 5.1).

It is useful to quantify the impact of the increase in debt balance by examining it relative to their income. An increase of 502 SEK in their revolving credit balance is equivalent to a 10.5% increase relative to their pre-experiment mean outstanding debt balance and 7.1% of their pre-mean monthly disposable income.[22]

The increase in debt balances also reinforces the explanatory mechanism—present-focused preferences. Specifically, since the increase in access to alcohol resulted in an additional high-interest debt, the interest rate on this debt could be a lower-bound estimate for the discount

[21]The credit balance of the DDD is 165.7 SEK (credit cards) + 336.3 SEK (credit lines) = 502 SEK total increase in credit. The pre-period mean of the treated counties = 951 SEK (credit cards) + 3,488 SEK (credit lines) = 4,439 SEK total pre-period mean. Thus, the relative increase for the treated population 20–25-year-olds = 502/4,439 = 11.3%.

[22]The mean annual disposable income is 85,355 SEK for the 20–25-year-olds living in the treatment counties, measured in the pre-period. Then, 85,355/12 = 7,113 SEK/month; 502/7,113 = 7.1%.

rate of individuals. Our data do not allow us to directly observe the annualized percentage rate (APR) paid on the different credit instruments; however, it is well known that the APR for unsecured consumer debt was, and still is, over 20%. Hence, the implicit discount rate that individuals use when trading off current and future consumption must be higher than this figure.

In addition to the direct trade-off, there are less salient indirect costs that are especially deleterious for the individuals we study who are situated at the margins of the formal credit and labor market. These costs are generated by the increase in default risk. We find that the probability of receiving a new arrear within the next 12 months increases by 0.07 percentage points, which is an increase of 14% relative to their pre-experiment mean. These arrears will remain on the individuals' credit register for three years after the debt has been repaid. From the literature, we know that negative information on an individual's' credit register not only worsens his or her credit access,[23] but also can lead to costly labor market exclusion. These effects on employment and earnings are four times larger than the effects on credit (Bos, Breza, and Liberman, 2018).

4.6 Labor Market

Our results thus far indicate that increased 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 authority, filed by all Swedish residents beginning at age 18. As tax filings are annual in frequency and the experiment started in the middle of 2000, we examine effects in both 2000 and 2001, and

[23]See for example, Musto (2004), Brown and Zehnder (2007); Djankov, McLiesh, and Shleifer (2007), Bos and Nakamura (2014), Liberman (2016), and Dobbie, Goldsmith-Pinkham, Mahoney, and Song (2016).

compare them to the pre-period of 1996 to 1999.[24]

Table 6. Labor Market Outcomes

This table shows that increased access to alcohol causally increases credit borrowing and default risk. Columns (1), (3), (5), and (7) show results from the following double-difference (DD) regression 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: $y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_i + \omega_t + \varepsilon_{i,t}$. Columns (2), (4), (6), and (8) show results from triple-difference (DDD) regressions. The sample for this analysis also includes 18–19-year-olds, an age group ineligible to buy alcohol in Sweden. The regression is a DDD specification (eligible/ineligible, treatment/control, and pre/post): $y_{i,t} = \beta_1 Post_t \times Treated_c \times Eligible_{i,t} + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times Eligible_{i,t} + \beta_4 Eligible_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. Standard errors are clustered at the municipal level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	1(Unemployed)		Welfare		Pre-tax income		#Sick days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	0.0146		1,019*		-1,886		0.579	
	(0.0097)		(522.0)		(2,451)		(1.249)	
Post × Treated× Eligible		0.0110		1,917***		-1,530		0.0711
		(0.0229)		(659.2)		(1,617)		(0.732)
Treated× Eligible		-0.0126		-1,717***		676.5		1.089
		(0.0089)		(356.8)		(1,321)		(0.668)
Post × Eligible		-0.0029		-1,164**		697.7		0.0775
		(0.0104)		(481.7)		(1,218)		(0.574)
County FE	Yes	No	Yes	No	Yes	No	Yes	No
Calendar Year 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 employment	Yes	No	Yes	No	Yes	No	Yes	No
County × Year 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	0.622	0.584	0.701	0.659	0.738	0.701	0.569	0.508
# Individuals	42,087	47,986	42,204	48,076	42,204	48,076	42,087	47,986
Pre-period mean	0.5679	0.5679	12,918	12,918	64,258	64,258	5.523	5.523
Relative effect	2.6%	1.9%	7.9%	15%	-2.9%	-2.4%	10%	1.3%

Next, we conduct formal tests using both DD and DDD specifications for the 18–19 and

[24]The effects in the labor market could be either direct or indirect. Prior research has documented a direct channel of alcohol consumption resulting in reduced workplace productivity (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: A default flag on one's credit record could hurt the likelihood of being hired by potential employers who check credit records (e.g., Balance, Clifford, and Shoag, 2016; Bartik and Nelson, 2019; Cohen-Cole, Herkenhoff, and Phillips, 2016; Bos et al., 2018). In the current empirical setting, we cannot discriminate between the two channels.

20–25 cohorts. In Table 6, we use the following dependent variables: likelihood of being unemployed, welfare receipts, pretax income, and the number of sick days taken. Aside from the lower frequency of observations, the empirical specification is identical to that used in previous tables. The odd and even columns, respectively, present results from DD and DDD 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 the welfare results (Columns (3) and (4)) are statistically significant.

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.

4.7 Reckless Behavior: Evidence from Crime Records

The behavioral sciences literature has found a tight relation between alcohol availability, consumption, and the propensity to commit crime, perhaps because alcohol consumption impairs judgment and can prompt violent actions (McClelland, Davis, Kalin, and Wanner, 1972; Gliksman and Rush, 1986; Rush, Gliksman, and Brook, 1986).

To explore whether this relation holds for the individuals in our sample, we analyze their incidence rate of crimes related to alcohol consumption. We first look at the effects for different age cohorts. In Figure 5, we plot the DDD coefficients measuring upticks in criminal activity on Saturdays among the treated group from Regression (3) for the six age groups. As expected, the charts show no significant increase in alcohol-related crimes on Saturdays for the cohort of 18–19-year-olds. Assaults and drunk driving on Saturdays do, however, increase among 20–25-year-olds, who are eligible to purchase alcohol. This finding is in line with our previous result that the largest increase in indebtedness was among 20–25-year-olds (Section 4.3).

Table 7. Effect of Liquor Stores Being Open on 1(Saturday) Crime

This table tests whether increased access to alcohol increases the risk for crimes on Saturdays. Columns (1) and (6) show the regression: $y_{i,t} = \beta_1 Post_t \times Treated_i + \beta_2 Post_t \times Treated_c + \omega_i + \omega_d + \omega_{c\times t} + \varepsilon_{i,t}$. Columns (2)–(4) and (7)–(9) show the regression: $y_{i,t} = \beta_1 Post_t \times Treated_i \times 1(Saturday_t) + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times 1(Saturday_t) + \omega_i + \omega_d + \omega_{c\times t} + \varepsilon_{i,t}$. Columns (5) and (10) show triple-difference (DDD) regressions estimating Regression (3).

Dependent variable:	1(Assault>0) × 1000					1(Drunk driving>0) × 1000				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Post × Treated	0.0092					-0.0028				
	(0.0067)					(0.0029)				
Post × Treated × 1(Saturday)		0.0497***	0.0867***	0.0023			0.0308**	0.0577***	-0.0037	
		(0.0161)	(0.0313)	(0.0089)			(0.0121)	(0.0221)	(0.0033)	
Post × 1(Saturday)		-0.0437***	-0.0728***	-0.0054			-0.0232***	-0.0436***	0.0038*	
		(0.0131)	(0.0255)	(0.0075)			(0.0089)	(0.0159)	(0.0023)	
Treated× 1(Saturday)		-0.0225	-0.0368	-0.0011			-0.0131	-0.0235	0.0025	
		(0.0148)	(0.0275)	(0.0083)			(0.0101)	(0.0184)	(0.0018)	
Post × Treated × Eligible					0.0273**					-0.0053
					(0.0115)					(0.0078)
Post × Eligible					-0.0226***					0.0080
					(0.0080)					(0.0057)
Treated × Eligible					0.0231					-0.0062
					(0.0165)					(0.0098)
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE and Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Regional employment	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Day of Week FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Year FE	No	No	No	No	Yes	No	No	No	No	Yes
Ages	20–25	20–25	20–25	20–25	20–25	18–25	20–25	20–25	20–25	18–25
Gender	All	All	Male	Female	All	All	All	Male	Female	All
Observations	20,702,518	20,702,518	11,346,586	9,355,932	23,409,472	20,702,518	20,702,518	11,346,586	9,355,932	23,409,472
# Individuals	32,667	32,667	17,874	14,793	38,143	32,667	32,667	17,874	14,793	38,143
Pre-mean	0.0302	0.058	0.0976	0.0112	0.0302	0.0139	0.0322	0.0572	0.00281	0.0139
Relative effect	31%	86%	89%	20%	90%	-20%	96%	101%	-130%	-38%

Our data set contains the exact date of the crime;[25] therefore, we can test whether crimes increased more on Saturdays relative to the other days of the week in the treated counties and in the 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 data set at the daily frequency and run Regression (3).

The results in Columns (2) and (7) of Table 7 confirm that the treated counties saw a statistically significant increase in assaults and drunk driving on Saturdays during the post-period. For this population, the likelihood of assaults on Saturdays in the treatment counties increased by 0.0050 percentage points (relative to the pre-period mean of 0.0058 percentage points). Similarly, the likelihood of drunk driving on Saturdays increased by 0.0031 percentage points, an increase of 95% relative to the pre-period mean in the treatment counties of 0.0032 percentage points. Columns (3)–(4) and (8)–(9) show that these results are completely driven by young males, which is consistent with the crime literature mentioned above.

Overall, this evidence corroborates our previous results on two fronts. First, these findings are consistent with the idea that weekend drinking increased when liquor store operating hours expanded. Second, these results suggest that individuals may have had more unplanned expenditures because of increased alcohol consumption stemming from greater access to alcohol on Saturdays.

5 Supplementary Results

In this section, we provide supplementary results to our main analysis. First, we explore the role of liquidity constraints in the decision making of the young cohort. Second, we test whether the demand for credit was concentrated in a small part of the population (a few

[25] In some cases, such as break-ins, the exact day of the crime may be unknown. In these cases, the court assigns a date based on an educated guess. However, because no ambivalence exists about the exact date of an assault or drunk driving, our results do not suffer from this error.

alcohol addicts) or whether it was spread across the treated population. Third, 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. Finally, we conduct several robustness tests of our main results in Online Appendix C. These include excluding border counties, a permutation (placebo) test, and running our analysis at the aggregated county level.

5.1 The Role of Liquidity Constraints

As we saw in the earlier analysis, the young cohorts increase consumption more than older cohorts due to drinking patterns (Figure 3). Still, liquidity constraints—which are likely to be more acute for young people (e.g., Hayashi, 1985)—could intensify the response of the young, especially because young people had little opportunity to accumulate liquidity.

We test this hypothesis by focusing on the 20–25 cohort, and splitting them by a proxy for liquidity constraints, i.e., having a below-the-median bank account balance. We follow Calvet, Campbell, and Sodini (2007) and Calvet and Sodini (2014) and impute the bank account balances based on the subsample of individuals (about 250,000) in the wealth data for which bank account balances are observed even though the earned interest is less than 100 SEK.[26] This way, we hold age constant and vary liquidity.

The results of this test, in Table 8, show that the impact of the opening hours experiment is concentrated in the below-median liquidity group. It is important to note that while we can directly measure the impact on the alcohol expenditure per age cohort, we cannot directly pinpoint the causal link between lack of liquidity and the negative impacts of alcohol expenditure. We can say, however, that there is an effect that is correlated with liquidity *beyond* simply the age effect.

[26]The observed balance is regressed onto the following observable characteristics: age and squared age of the household head, household size, real estate wealth, level and squared level of household disposable income, and financial wealth other than bank accounts. The coefficients from this regression are then used to impute the account balances of individuals who report no bank account. Note that the fraction of Swedes ages 15 years and above who have a bank account has been reported to be 99% (Honohan, 2008).

Table 8. DDD Regressions: Sample Splits Based on Imputed Bank Liquidity

The table presents the DDD estimations for the main variable of interest in the original sample as well as in two subsamples based on their imputed bank balance: *Illiquid* (below-the-median) and *Liquid* (above-the-median). Standard errors are clustered at the municipality level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Sample:	Original	Illiquid	Liquid	Original	Illiquid	Liquid
	(1)	(2)	(3)	(4)	(5)	(6)
	# New pawn loans			Pawn loan value		
Post × Treated × Eligible	0.024**	0.057***	-0.005	40	71*	17
	(0.012)	(0.018)	(0.014)	(30)	(38)	(30)
# Individuals	38,320	18,134	18,183	38,320	18,134	18,183
	Pawn default			# Credit cards		
Post × Treated × Eligible	-0.003	-0.004	-0.004	0.023***	0.031***	0.016**
	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
# Individuals	38,320	18,134	18,183	34,902	16,738	16,305
	Credit cards balance			Credit lines balance		
Post × Treated × Eligible	166***	266***	87*	336***	481***	241
	(47)	(73)	(48)	(122)	(171)	(152)
# Individuals	34,902	16,738	16,305	34,902	16,738	16,305
	Welfare (SEK)			Pre-tax income (SEK)		
Post × Treated × Eligible	1,917***	2,279***	1,393	-1,530	-2,265	-224
	(659)	(650)	(905)	(1,617)	(2,367)	(2,964)
# Individuals	48,076	23,301	23,610	48,076	23,301	23,610
Individual FE and Age FE	Yes	Yes	Yes	Yes	Yes	Yes
County × Time FE	Yes	Yes	Yes	Yes	Yes	Yes
	Drunk driving			Assault		
Post × Treated × Saturday	0.031***	0.029*	0.034	0.050***	0.119***	-0.013
	(0.012)	(0.015)	(0.021)	(0.016)	(0.022)	(0.024)
# Individuals	32,667	15,545	15,545	32,667	15,545	15,545
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Calendar month FE	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE and Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	Yes	Yes	Yes	Yes	Yes
Regional employment	Yes	Yes	Yes	Yes	Yes	Yes
Day of the week FE	Yes	Yes	Yes	Yes	Yes	Yes

5.2 A Few Alcoholics? The Distribution of Borrowers

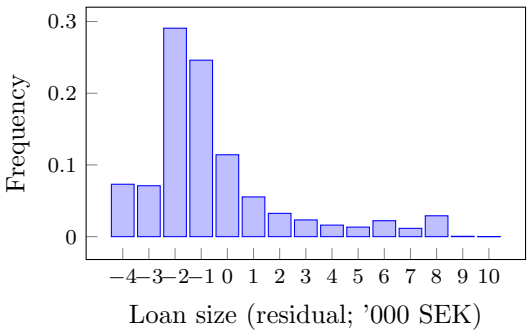
The results so far have shown an increase in the average demand for credit. An important question is whether this increase is evenly spread across the population or is skewed. A skewed distribution would suggest that a small number of people (potentially alcoholics) are driving the results. Conversely, 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 rerun Regression (2) but exclude the triple interaction. This exclusion adds this demeaned variable to the residual, allowing us to examine the distribution of the effects for individuals in the treatment group. We focus on the subset of individuals who actually borrowed and ask whether the positive increase in loan size among the treated group is driven by a small number of large loans or by across-the-board borrower demand.

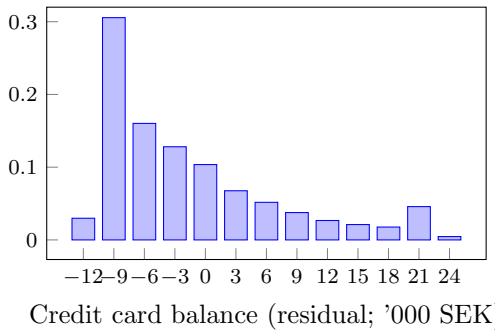
Figure 8. Distribution of Residuals of Loan Sizes and Balances in the Treatment Counties

The charts plot the distribution of the residuals for pawn loan size and credit card balance for the treatment cell, i.e., $Post \times Treated \times Eligible$, from the baseline specification (Regression (1)) without the triple-interaction: $y_{i,t} = \beta_1 Post_t \times Treated_c + \beta_2 Post_t \times Eligible_{i,t} + \beta_3 Eligible_{i,t} + \omega_i + \omega_{c \times t} + \varepsilon_{i,t}$. The sample used in Panel (a) includes all individuals who took a pawn loan. The sample used in Panel (b) includes all individuals who have a credit card.

(a) Residuals of pawn loan size



(b) Residuals of credit card balance



The charts in Figure 8 show the distribution of the residuals of the loan sizes of pawn and credit card borrowers. In both charts, 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 dispersed.

5.3 Present-Focused Preferences or Convenience Shopping?

So far, we have documented an economically large cost to the 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. The extended opening hours could make purchasing alcohol more convenient and thus may reveal latent demand among busy consumers. As a result, both consumption of alcohol and reliance on credit would increase. 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 weekday schedules to purchase alcohol. Thus, according to this narrative, the Saturday store opening hours represent a reduction in opportunity costs.

The ideal test—randomly assigned leisure—does not exist in our data. However, we identify two subgroups—retirees and the unemployed—for whom the convenience benefit from opening the stores on Saturdays is endogenously minimal. If retirees and the unemployed indeed do not have present-focused preferences, 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 those who are not working.

We test this hypothesis in Online Appendix Tables A6 and A7. The tables contrast the financial effects for retirees (ages 65–75) versus older employees (ages 55–60), and unemployed versus employed individuals (both in the 20–65 age range). Because the comparison with 18–19-year-olds is no longer appropriate, we run a DDD specification in which the final difference is a dummy for being retired or unemployed.[27]

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 (these samples are large: 267,000 to 1,296,000 observations), but rather by coefficients that are close to zero with tight standard errors. For example, in Columns (1) and (2) of Online Appendix Table A6, we estimate the effect on the number of new pawn loans. The coefficients are -0.002 and -0.016 for retirees and the unemployed, respectively, with standard errors of 0.008 and 0.014. In comparison, in Table 3, Columns (1) and (2), the coefficients are 0.024. Similarly, in Online Appendix Table A7, we estimate the effect on the number of credit cards. The coefficients are 0.005 and 0.011 for retirees and the unemployed, respectively, with standard errors of 0.011 and 0.009. In comparison, in Table 4, Columns (1) and (2), the coefficients are 0.020 and 0.023, respectively.

These results suggest that busier populations did not borrow more following the extension of opening hours on Saturdays. This result is consistent with the idea that alcohol is a temptation good that triggers present-biased behavior in people and leads to current consumption at the expense of future consumption.

6 Discussion

Our results have important implications for the lives of poor individuals living in wealthy countries. Our study measures the effects of extending the opening hours of liquor stores, which is effectively a relaxation of a commitment device that prevented impulsive consump-

[27] One caveat is that the power of the retiree test might not be high, given that this age group might not be very susceptible to the increase in alcohol availability (see Figures 4 and 5).

tion prior to the extension of the operating hours.

Two main theories link present-focused preferences to poverty, and both conclude that the value of (and therefore the demand for) commitment devices is highest among the poor. In Banerjee and Mullainathan (2010), while all people are equally sensitive to impulsive consumption, poor people have lower ability to absorb the adverse costs of impulsive consumption due to lack of a financial buffer. In Bernheim et al. (2015), individuals are endowed with different levels of present-focused preferences. People with severe present-focused preferences deplete their assets quickly and become poor. Because they are poor, they have lower ability to punish themselves for deviating from their planned consumption path, and therefore are more likely to cave in to impulsive consumption.

Through its policies, the government can institute commitment devices that can help poor individuals improve their financial wellbeing. One particular way that the government can intervene is by limiting the opportunities for impulsive consumption. Restricting the opening hours of alcohol stores over the weekend is one such policy.

In practice, however, governments may choose to *avoid* instituting commitment devices—restrictive access to temptation goods, in our case—for two reasons. First, while enabling free access to temptation goods induces costs due to impulsive consumption, it might also create value by offering more flexible shopping options and hence convenience. Alluding to this tradeoff, Systembolaget's CEO Anitra Steen says in an interview (February 5, 2000, right before the initiation of the nationwide experiment): "The Parliament believes that our legitimacy with the Swedish people must be strengthened in order to guarantee the long-term survival of Systembolaget. One way to do that is to open on Saturdays and thus improve the service to the customers."[28] Second, the costs of impulsive consumption are disproportionately borne by the poor (as discussed in Banerjee and Mullainathan, 2010; Bernheim et al., 2015), whereas the added convenience benefits a larger group in the population. Given that the latter group is larger in wealthy countries and also possess greater political power, the

[28]See `http://www.systembolagethistoria.se/teman/butikerna/de-lordagsstangda-aren/`.

government would prefer catering to them rather than serving a relatively small group of poor individuals.

The government's decision to relax access to alcohol does not necessarily benefit the wealthy population, as the negative effects of impulsive consumption borne by the poor are actually paid for by the wealthy majority, albeit indirectly. In the short run, the wealthy may suffer from the externalities to greater alcohol consumption, e.g., higher crime rates, violence, and drunk driving. This was shown, for example, in Grönqvist and Niknami (2014), as well as in our analysis. In the long run, the wealthy majority pays for greater impulsive consumption through taxation that funds social transfers, e.g., welfare programs.

7 Conclusion

Whether present-focused preferences are responsible for the personal indebtedness of households is an important question for both academics and policymakers. Previous research has shown that present-focused preferences are to blame for impulsive consumption. In turn, higher consumption is thought to affect intertemporal substitution through the budget constraint. 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 others. 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 and mainstream credit markets. 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 availability resulted in adverse consequences in labor market (greater unemployment and reliance on welfare) as well as increased crime activity.

Because alcohol consumption is partly triggered by present-focused preferences and its use imposes direct and indirect costs on consumers, policymakers can improve the 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 benefits of more convenient opening hours for the group in society with no or few self-control issues.

To close, our study focuses on one temptation good among many that individuals are exposed to. The effects that we document should be considered as marginal: increases in high-interest and high-fee debt, increases in default risk, and negative employment and crime outcomes. Consumers are exposed to temptations left and right: lotteries and gambling, sugary drinks, enticing advertising campaigns, the ease of impulsive online shopping. Among these, the supply of alcohol and lottery/gambling opportunities are generally regulated by the government, but many other opportunities are not. Our study indicates that impulsive consumption could have detrimental consequences for a population at risk.

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Supplemental Appendix: For Online Publication Only

This Online Appendix contains the following figures and tables:

Online Appendix A Simple Theoretical Framework

This section provides a simple framework to demonstrate how limited opening hours may affect consumption and consumers' financial wellbeing. The purpose of the framework is to illustrate why a change in liquor store opening hours may affect consumption among individuals with present-focused preferences.

Many models have been offered to explain present-focused preferences. Here, we are interested in models of present-focused preferences in which agents value commitment mechanisms, as they help them achieve their goals. Ericson and Laibson (2019) classify these models into several categories: (a) present-biased (quasi-hyperbolic discounting) preferences with partial sophistication (Laibson, 1997; O'Donoghue and Rabin, 1999) and hyperbolic discounting (Loewenstein and Prelec, 1992), (b) unitary-self models with temptation (Dekel, Lipman, and Rustichini, 2001; Gul and Pesendorfer, 2001; Laibson, 2001; Bernheim and Rangel, 2004; Gul and Pesendorfer, 2004; Noor, 2007; Dekel, Lipman, and Rustichini, 2009; Noor, 2011; Lipman and Pesendorfer, 2013), and (c) multiple-self models with simultaneous selves (Thaler and Shefrin, 1981; Shefrin and Thaler, 1988; Hoch and Loewenstein, 1991; Loewenstein, 1996; Bénabou and Tirole, 2004; Bernheim and Rangel, 2004; Loewenstein and O'Donoghue, 2004; McClure, Laibson, Loewenstein, and Cohen, 2004; McClure, Ericson, Laibson, Loewenstein, and Cohen, 2007; Fudenberg and Levine, 2006, 2011, 2012; Brocas and Carrillo, 2008a,b; Jackson and Yariv, 2014, 2015).

Several works have linked present-focused preferences and consumption of temptation goods in contexts akin to ours (see a review by Bryan, Karlan, and Nelson, 2010). Gruber and Köszegi (2001) argue that addiction results from time-inconsistent preferences wherein individuals are heavily discounting future consequences (e.g., the difficulty of quitting). Gabaix and Laibson (2017) present a model 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 trade-off (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 lower income populations (higher cognitive load due to financial stress; see Mani, Mullainathan, Shafir, and Zhao, 2013; Schilbach et al., 2016) or people under the influence of alcohol (see Steele and Josephs, 1990; Giancola, Josephs, Parrott, and Duke, 2010).

A.1 Setup

To create a simple model that captures present-focused preferences and generates a demand for a commitment mechanism, we follow Laibson (1997) and describe the utility function of individuals as:

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

This model encompasses two cases, depending on the value of β. When $\beta = 1$, consumers are rational and have exponential discounting. However, when $\beta < 1$, their preferences are dynamically time-inconsistent. A consumer with present-focused 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:

$$\frac{\partial U_0}{\partial C_1} = \beta\delta, \; and \; \frac{\partial U_0}{\partial C_2} = \beta\delta^2 \Rightarrow MRS_{C_1,C_2} = \frac{\beta\delta^2}{\beta\delta} = \delta,$$

whereas at t_1, the consumer values C_1 versus C_2 in this way:

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

Thus, over time the MRS_{C_1,C_2} changes. In other words, when $\beta < 1$, the individual consumes more in the present despite not having planned to do so in the past, even though there is no new information. This behavior is consistent with a category of models that

incorporate present-focused preferences in which an agent views commitment as something valuable (e.g., Barro, 1999; Krusell, Kuruşçu, and Smith Jr., 2010; Gustman and Steinmeier, 2012; Ericson and Laibson, 2019; Gomes, Haliassos, and Ramadorai, 2020).

A.2 Limited Opening Hours as a Commitment Device

Expanding the opening hours of liquor stores into weekends should have differential effects on consumers depending on their ability to make plans and hold to 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-focused preferences, limited opening hours can function as a commitment device that helps them stick to their planned consumption path. Imagine that you plan *not to drink* tomorrow. Whether you have present-focused preferences or not, 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 do not have present-focused preferences, 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-focused 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 on-site drinking), then it might be indicative of consumers with present-focused preferences 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 cafe, 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.

Online Appendix B The Swedish Alcohol Market

We next provide some descriptive statistics about the Swedish alcohol market. In Figure A1, 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 A2, Panel A, we explore how the share of alcohol expenditures out of total expenditures changes over the income distribution. For this purpose, we obtained expenditure data from Statistics Sweden for the period of our experiment (1999–2001).[29] 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. We find that individuals in the lowest two income deciles spent a slightly higher share of their total expenditures on alcohol relative to the other income deciles.

Panel B presents the share of alcohol expenditure out of the disposable income. The figure show that the share of alcohol consumption out of the disposable income is especially high for households in the bottom two deciles: 4.6% and 3.0%, for the first and second income deciles, respectively.

Online Appendix C Robustness Tests

C.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 4 is based on the universe of pawn borrowers in the years 1992 to 2012 (see Section 3). We use this sample to examine which borrowers took a new pawn loan during the 1999–2001 period.

To provide comfort that the look-ahead bias does not materially affect our results, we propose a method that allows us to avoid the bias. Instead of using past and future bor-

[29]Statistics Sweden collected cash journal data (Utgiftsbarometern). The data were 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 the annual level. We use data from 1999–2001 covering the 4,688 households that responded out of 9,000 contacted (3,000 each year).

rowers as non-borrowers, we simply measure pawn borrowing per 100,000 residents in the county. Essentially, we are measuring the borrowing rate per capita. This way, no future borrowing information is entering the sample design. The downside of this approach is that our observation unit is no longer at the person-bimonth (every other month) level, but rather at the county-bimonth (every other month) level. In Online Appendix Table A8, we run DD and DDD 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 per quarter per 100,000 individuals. For example, one variable 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) comes 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.[30]

The results are qualitatively similar to those seen in Table 3. Columns (1) and (2) show the DD and the DDD specification results for the probability of taking out a pawn loan.[31] 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 DDD, 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. The DDD results in Column (2) show 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 pre-period average credit-borrowing rate among the treated counties. We also find significant increases in loan size (measured as total pawn loans taken by 100,000 residents; Columns (3) and (4)) at the county level of 16% and 35% for the DD and DDD specifications, respectively. These results are similar, albeit not identical, to those in Table 3 (individual-level analysis).

[30]The reason that this calculation overcomes the look-ahead bias is that the denominator is based on the *current* population and not the population that will be included in the future due to future pawn borrowing.

[31]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 repetitions.

One potential reason for these differences 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 all counties in the regression regardless of the number of residents.

Unfortunately, due to the quarterly frequency of our population statistics, we do not have sufficient observations in the pre-period to run county-level regressions for our mainstream credit or labor market outcomes.

C.2 Excluding Border 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 Online Appendix Tables A3 and A4, we use a sample that excludes Skåne. The results are very similar to the ones presented in Tables 3 and 4.

C.3 Permutation (Placebo) Tests

As with many natural experiments, in our setting there is a concern that the effects we report are not related to the treatment (opening hours experiment) but perhaps to some unobservable variation. We follow the procedure proposed by Chetty, Looney, and Kroft (2009) to provide further comfort that this is not the case. Each individual-bimonth (every other month) 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 DD and DDD 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 Online Appendix Figures A3 and A4. The figures show the cumulative distribution function (CDF) for the permutation analysis for the outcome variables used in the study. The CDF charts show that the coefficient in the original regressions is either below the 10^{th} percentile or above the 90^{th} percentile of the 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 effects essentially disappear.

Table A1. Definitions of Key Outcome Variables

Variable	Definition
Panel A: 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 has.
# Pawn defaults within two months	Equal to the number of pawn loans held by the individual that went to auction within two months.
# Pawn rollovers	Equal to the number of pawn loans held by the individual that were rolled over within two months.
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 has borrowed.
Installment loans limit (SEK)	Equal to the sum of limits of installment loans the individual has taken out.
# Credit lines	Equal to the number of credit lines the individual has open.
Credit lines balance (SEK)	Equal to the sum of balances of credit lines the individual has open.
1(Arrears $>$ 0 within two months)	Equal to one if the individual receives at least one new credit arrear within two months.
Panel C: Labor market and crime dependent variables	
1(Unemployed $>$ 0)	Equal to one if the individual is unemployed at least part of 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 individual's 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 of assault.
1(Assault on Saturday $>$ 0)	Equal to one if the individual is convicted of assault on Saturday.
1(Drunk driving $>$ 0)	Equal to one if the individual is convicted of drunk driving.
1(Drunk driving on Saturday $>$ 0)	Equal to one if the individual is convicted of drunk driving on Saturday.

Table A2. Summary Statistics, Cash Journals (HUT Sample)

This table presents sample statistics for the individuals in the HUT sample that administrated cash journals to randomly assigned households. This sample is utilized for Figure 3 and Table A5. Panel A presents statistics for individuals ages 20–65. Panel B presents statistics for individuals ages 20–25. Both panels show the statistics for the period *prior* to the Swedish government's expansion of liquor store operating hours on Saturdays in some counties (i.e., pre-February 2000).

	Panel A: HUT ages 20–65, 1999			**Panel B: HUT ages 20–25, 1999**		
	Mean	Std dev	Median	Mean	Std dev	Median
Total expenditure	283,352	263,266	131,300	175,810	159,948	85,487
Alcohol expenditure	4,789	1,294	8,246	2,975	0	4,158
Disposable income	288,249	281,867	130,170	16,4174	131,728	110,021
# Individuals	1,464			90		

Table A3. 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 that borders Denmark. The odd-numbered columns 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. The even-numbered columns show regressions from triple-difference (DDD) regressions. The sample for this analysis also includes 18–19-year-olds, an age group ineligible to buy alcohol in Sweden. The regression is a DDD specification (eligible/ineligible, treatment/control, and pre/post). Standard errors are clustered at the municipal level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# New pawn loans		Pawn loan size (SEK)		# Pawn loan defaults		# Pawn rollovers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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
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 employment	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes
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
Pre-period mean	0.112	0.112	190.2	190.2	0.010	0.010	0.034	0.034
Relative effect	9%	21%	8%	22%	71%	-52%	7%	23%

Table A4. Excluding Border County: Mainstream Credit Outcomes

This table shows the results of the same regressions as in Table 4, but now we exclude the county that borders Denmark. the odd-numbered columns 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. The even-numbered columns show triple-difference (DDD) regressions. The sample for this analysis also includes 18–19-year-olds, an age group ineligible to buy alcohol in Sweden. The regression is a DDD specification (eligible/ineligible, treatment/control, and pre/post). Standard errors are clustered at the municipal level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# Credit cards		Credit card balance (SEK)		# Installment loans		Installment loans limit (SEK)		# Credit lines		Credit lines balance (SEK)		1(Arrears > 0)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Post × Treated	0.023***		142.3***		0.000		112.8*		-0.014***		484.3***		0.009***	
	(0.003)		(27.1)		(0.001)		(58.4)		(0.003)		(83.3)		(0.003)	
Post × Treated × Eligible		0.026***		194.2***		-0.001		-10.9		-0.027***		405.7**		0.009
		(0.005)		(52.6)		(0.001)		(82.4)		(0.008)		(172.1)		(0.006)
Post × Eligible		-0.003		-76.5*		0.003***		118.9**		0.001		39.0		0.009***
		(0.004)		(39.4)		(0.001)		(48.4)		(0.004)		(103.7)		(0.003)
Treated × Eligible		-0.024***		-111.5		0.007		218.4		0.012		-534.0*		-0.018**
		(0.009)		(84.1)		(0.004)		(196.9)		(0.015)		(310.4)		(0.009)
County FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Month FE	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Regional employment	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
County × Month FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	189,660	212,320	189,660	212,320	189,660	212,320	189,660	212,320	189,660	212,320	189,660	212,320	189,532	212,183
R^2	0.015	0.017	0.006	0.007	0.000	0.001	0.001	0.002	0.015	0.019	0.007	0.008	0.007	0.008
# Individuals	23,990	28,342	23,990	28,342	23,990	28,342	23,990	28,342	23,990	28,342	23,990	28,342	23,949	28,295
Pre-period mean	0.232	0.232	1,085	1,085	0.043	0.043	1,122	1,122	0.364	0.364	3,582	3,582	0.051	0.051
Relative effect	10%	11%	13%	18%	0%	-3%	10%	-1%	-4%	-8%	14%	11%	17%	17%

Table A5. Alcohol Spending, by Age Group and Income Quintile

This table shows a breakdown of the 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 cash journal data were collected by Statistics Sweden (Statistiska centralbyrån).

		Age					
		20–25	26–34	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 A6. Groups That Have Free 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 coefficients from the following regression: $y_{i,t} = \beta_1 Post_t \times Treated_c \times \gamma_{i,t} + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times \gamma_{i,t} + \beta_4 Treated_c \times \gamma_{i,t} + \beta_5 \gamma_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. Standard errors are clustered at the municipal level. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# New pawn loans		Loan size (SEK)		# Defaults		# Rollovers	
$\gamma =$	Retiree	Unemployed	Retiree	Unemployed	Retiree	Unemployed	Retiree	Unemployed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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 × 1(γ)	-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
Regional GDP	No	No	No	No	No	No	No	No
Regional employment	No	No	No	No	No	No	No	No
County × Month FE	Yes	Yes	Yes	Yes	Yes	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
Pre-mean	0.170	0.198	219.1	482.7	0.004	0.009	0.277	0.102
Relative effect	-1%	-8%	-4%	-5%	-52%	126%	-1%	0%

Table A7. Groups That Have Free 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 from the following regression: $y_{i,t} = \beta_1 Post_t \times Treated_c \times \gamma_{i,t} + \beta_2 Post_t \times Treated_c + \beta_3 Post_t \times \gamma_{i,t} + \beta_4 Treated_c \times \gamma_{i,t} + \beta_5 \gamma_{i,t} + \omega_i + \omega_{c\times t} + \varepsilon_{i,t}$. Standard errors are clustered at the municipal level.*, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	# Credit cards		Credit card balance (SEK)		# Installment loans		1(#Arrears > 0)	
$\gamma =$	Retiree	Unemployed	Retiree	Unemployed	Retiree	Unemployed	Retiree	Unemployed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated × 1(γ)	0.005	0.011	23.4	96.7	0.001	0.002	0.000	-0.011
	(0.011)	(0.009)	(111.1)	(85.6)	-0.005	(0.004)	(0.004)	(0.009)
Post × 1(γ)	-0.005	0.013	67.7	-81.4	-0.001	0.010***	-0.009***	0.042***
	(0.008)	(0.008)	(78.3)	(77.5)	(0.003)	(0.004)	(0.003)	(0.008)
Treated × 1(γ)	0.008	-0.007	-344.0	-113.2	0.001	0.001	0.005	0.001
	(0.025)	(0.009)	(237.0)	(92.6)	(0.009)	(0.004)	(0.007)	(0.009)
1(γ)	0.071*	-0.017**	445.5	61.0	0.018	-0.012***	-0.012	-0.011
	(0.042)	(0.008)	(479.0)	(85.8)	(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
Regional GDP	No	No	No	No	No	No	No	No
Regional employment	No	No	No	No	No	No	No	No
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
Pre-mean	0.973	0.254	5,847	1,276	0.062	0.040	0.030	0.132
Relative effect	0%	4%	0%	8%	2%	5%	0%	-8%

Table A8. County-Level Regressions: Pawn Credit Outcomes

This table shows double- (DD) and triple-difference (DDD) 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 DD specification is the following model: $y_{c,t} = \beta_1 Post_t \times Treated_c + \beta_2 GDP_{c,t} + \beta_3 Employ_{c,t} + \omega_c + \omega_t + \varepsilon_{c,t}$, and our DDD specification is the following model: $y_{c,t,a} = \beta_1 Post_t \times Treated_c \times Eligible_a + \beta_2 \times + \beta_3 Post_t \times Eligible_a + \beta_4 Eligible_a + \omega_{c \times t} + \varepsilon_{c,t,a}$. 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. *, **, and *** indicate significance at the 10%, 5%, and 1% level, respectively.

Dependent variable:	New pawn loans per 100,000		Total pawn loans (SEK), per 100,000		Pawn loan defaults per 100,000		# Pawn rollovers per 100,000	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	52.38***		69,175**		-3.2		6.52	
	(11.90)		(34,224)		(10.33)		(4.652)	
Wild bootstrap p-value (implied significance)	0.114		0.270		0.895		0.435	
Post × Treated × Eligible		90.11***		150,031*		-3.61		6.19
		(27.97)		(78,297)		(22.96)		(10.85)
Wild bootstrap p-value (implied significance)		0.024 (**)		0.056 (*)		0.860		0.389
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual FE and Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional GDP	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional employment	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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	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	12	12	12	12	12	12	12	12
Pre-period mean	234.1	234.1	424,937	424,937	14.64	14.64	50.22	50.22
Relative effect	22%	38%	16%	35%	-22%	-25%	13%	12%

Figure A1. Average Alcohol Consumption: International Comparison

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).

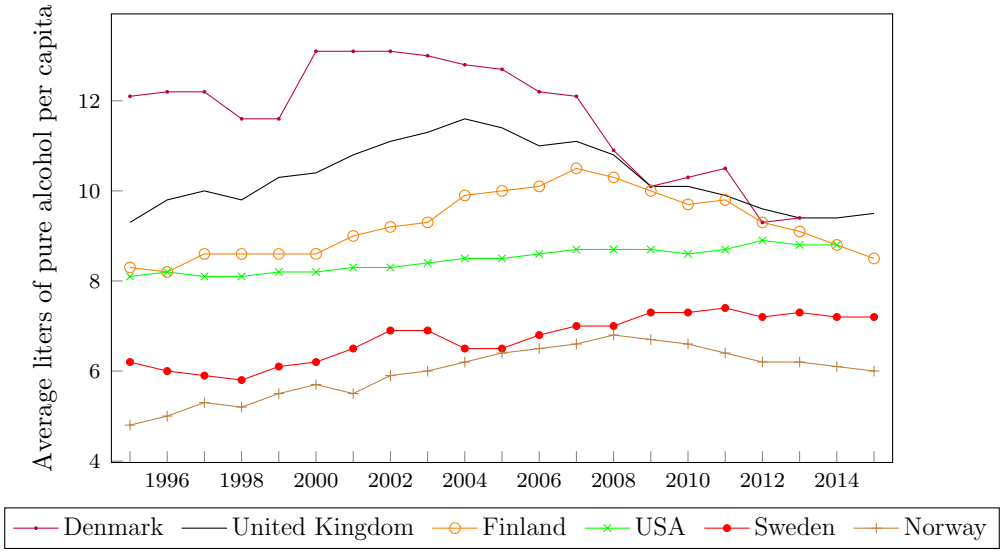


Figure A2. Alcohol Consumption Patterns in Sweden

This figure depicts drinking patterns in Sweden, 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 use 1999-2001 data, the 4,688 households that responded out of 9,000 contacted (3,000 each year).

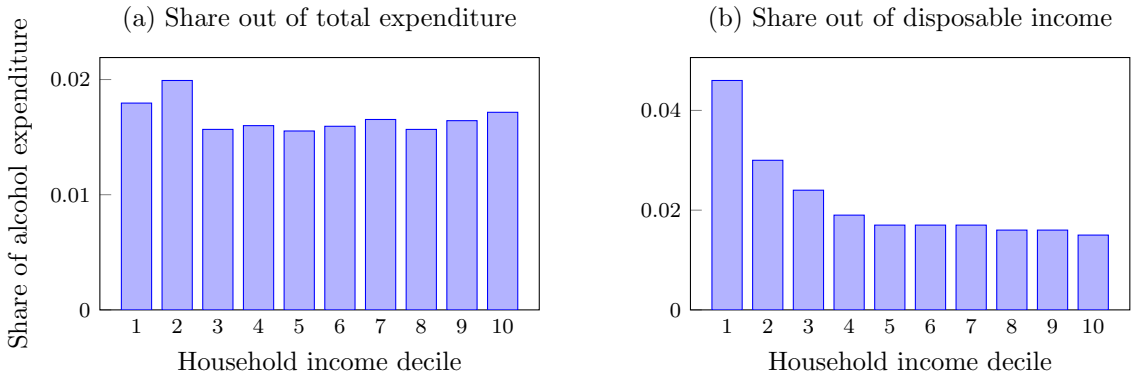


Figure A3. Distribution of Placebo Estimates (DD)

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 (DD) specification for 18-25 year-olds. The CDF charts show that the original DD regression coefficients (represented by the red vertical line) is below the 10^{th} percentile or above the 90^{th} percentile of the distribution, 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. The dashed vertical lines are at zero.

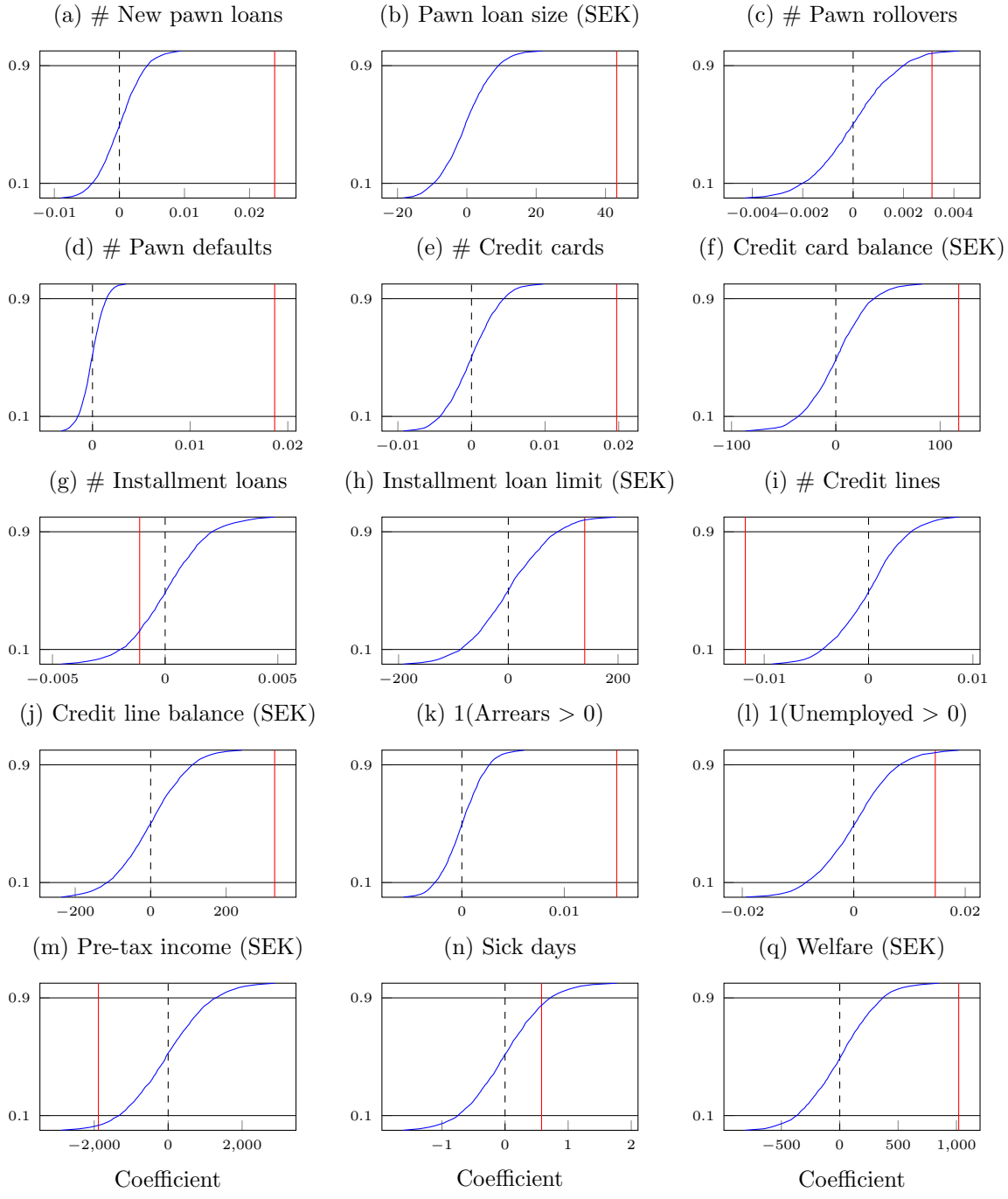
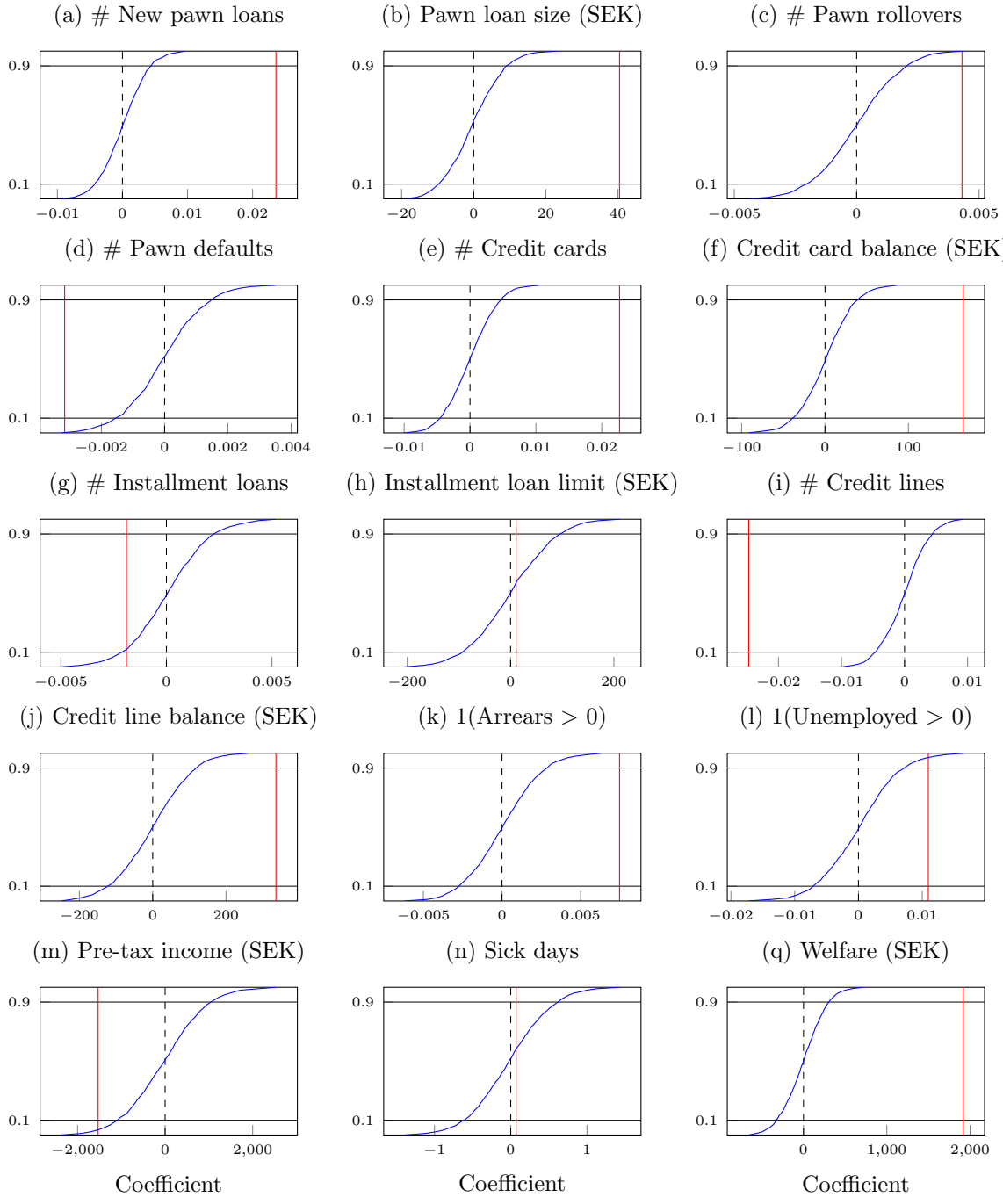


Figure A4. Distribution of Placebo Estimates (DDD)

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 (DDD) specification for 18-25 year-olds. The CDF charts show that the original DDD regression coefficients (represented by the red vertical line) is below the 10^{th} percentile or above the 90^{th} percentile of the distribution, 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. The dashed vertical lines are at zero.



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