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ALCOHOL AVAILABILITY AND CRIME: LESSONS FROM LIBERALIZED WEEKEND SALES RESTRICTIONS

by

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Alcohol Availability and Crime:

Lessons from Liberalized Weekend Sales Restrictions*

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ABSTRACT

In February 2000, the Swedish state monopoly alcohol retail company launched a large scale experiment in which all stores in selected counties were allowed to keep open on Saturdays. We assess the effects on crime of this expansion in access to alcohol. To isolate the impact of the experiment from other factors, we compare conviction rates in age cohorts above and below the national drinking age restriction in counties where the experiment had been implemented, and contrast these differences to those in counties that still prohibited weekend alcohol commerce. Our analysis relies on extensive individual conviction data that have been merged to population registers. After demonstrating that Saturday opening of alcohol shops significantly raised alcohol sales, we show that it also increased crime. The increase is confined to crimes committed on Saturdays and is driven by illegal activity among individuals with low ability and among persons with fathers that have completed at least some secondary education. Although the increases in crime and alcohol sales were slightly higher during the initial phase of the experiment, our evidence suggests that both effects persist over time. Our analysis reveals that the social costs linked to the experiment exceed the monetary benefits.

Keywords: Delinquency; Alcohol laws; Substance use;
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1. INTRODUCTION

It is estimated that more than one third of all inmates in US correctional facilities were under the influence of alcohol at the time of the offense (Greenfield 1998). In an effort to combat its deleterious effects, most countries have implemented laws that heavily restrict access to alcohol. Temporal sales restrictions is one of the most frequently used policies. Many US states currently enforce such regulations in terms of prohibitions of alcohol commerce on Sundays. These policies are more commonly known as “blue laws”. In the past decade, several states have however repelled these laws or are in the process of doing so. Proponents argue that abolishing the regulations will expand consumer choice and raise tax revenues. Needless to say, in a welfare analysis such benefits need to be weighed against the potential costs imposed on society by increased crime.

This paper contributes to this policy debate by examining the introduction of liberalized weekend alcohol sales restrictions in Sweden. In February 2000, the state monopoly alcohol retail company granted all stores in six counties to keep open on Saturdays. The reform was designed as an experimental scheme where the explicit goal was to evaluate its social consequences. In practice, this meant that the counties were selected on the basis of variation in certain background characteristics. Since no major adverse effects of the experiment were discovered, Saturday open alcohol shops were implemented nationwide in July 2001. Although our primary objective is to investigate the impact of this large scale experiment on crime, we also consider its effect on alcohol sales.

The strong link between alcohol and crime has been documented by a large literature in several disciplines.[1] The observational findings are supported by experimental evidence showing that alcohol impairs judgment and provokes aggressive behavior

[1] Although the literature is too vast to cover in this paper, Carpenter and Dobkin (2010) and Cook and Moore (2000) provide excellent reviews.

(McClelland et al. 1972). Others argue that alcohol promote crime, not only via pharmacological pathways, but through the context in which it is provided and consumed (Homel, Tomsen and Thommeny 1992). For instance, since alcohol is often enjoyed in group settings, it may increase the number of social contacts, thereby raising the risk of a criminal incident. Alcohol may also encourage criminal activity because of the need to obtain resources necessary for a continued use (Rush, Gliksman and Brook 1986). Besides raising the risk of crime commission, alcohol may also increase the likelihood of victimization. This is the case if impaired decision making implies that individuals place themselves in situations where they are at greater risk of becoming victims (Carpenter and Dobkin 2010).

Despite its policy relevance, there is still limited knowledge of how temporal restrictions on alcohol sales affect crime. Ligon and Thyer (1993) find that Sunday prohibitions of alcohol sales in the US significantly reduced arrests for drunken driving. Olson and Wikström (1982) evaluate the consequences of a similar ban on alcohol commerce on Saturdays in Sweden. They find that crimes related to drunkenness, domestic disturbances and public disturbances fell during the weekend relative to other days of the week after the policy was introduced. Norström and Skog (2005) examine the repeal of the same policy in 2000 and find that it led to an increase in drunk driving, but there was no statistically significant increase in reported assaults.[2] Hough and Hunter (2008) and Humphreys and Eisner (2010) show that voluntary liberalized bar closing hours in the UK had no meaningful effects on crime. Biderman, DeMello and Schneider (2010) provide the perhaps most convincing evidence so far in their study of the consequences of late night alcohol sales restrictions in bars in Sao Paolo. The restrictions were adopted by several municipalities between 2001 and 2004. In contrast to the

[2] Norström and Skog (2005) consider the same policy as the present paper.

abovementioned studies that solely rely on time-series data, Biderman, DeMello and Schneider explore both cross-regional and cross-time variation in the introduction of the policy, which makes it possible to control for fixed unobserved properties of the municipalities. The results show that the policy led to a 10 percent decrease in homicides and assaults.

The existing evidence is complicated by the fact that alcohol regulations are likely to be correlated with unobserved factors also linked to crime. This is especially true for the time-series studies. If changes in alcohol regulations coincide with, for instance, other governmental policies, or with demographic shifts, it is not possible to isolate the causal effect of the policy. Similarly, law enforcement agencies are likely respond to new alcohol laws by reallocating resources. Liberalized alcohol laws may for example lead to a higher demand for policemen patrolling the streets in order to prevent an anticipated surge in crime. This could have offsetting effects on criminal activity. It has also proven difficult to establish direction of causality since many alcohol regulations probably were implemented as a direct consequence of shifts in the crime rate.

Another concern is that few evaluations have been able to investigate the “first-stage” relationship between temporal restrictions on alcohol commerce and alcohol sales.[3] It is not obvious that abolishing weekend sales restrictions will actually increase alcohol commerce. Customers may simply redistribute their weekly purchases over the week with no change in overall consumption. On the other hand, less patient individuals and heavy drinkers may be unable to smooth their consumption in such a way, and may therefore respond to leaner weekend restrictions by increased drinking. Understanding how liberalized sales regulations affect alcohol commerce is of course of key importance when

[3] With exception of the time-series evidence presented in Olson and Wikström (1982) and Norström and Skog (2003, 2005).

trying to assess the social benefits of changes in alcohol laws. Last, because of the use of aggregated data past studies have been unsuccessful in investigating whether the behavioral response to changes in alcohol policy is stronger in some groups of the population. If alcohol laws only affect some individuals it could mask changes in crime at the aggregated level. Identifying these groups may also provide valuable information on how to optimally target crime preventive actions.

To disentangle the effect of Saturday opening of alcohol shops from other aspects we exploit the fact that the experiment was introduced only in a few regions. We also take advantage of another feature of the Swedish alcohol control system: that national law prohibits stores to sell alcohol to individuals under the age of 20. Our empirical strategy is to compare conviction rates in age cohorts above and below the national drinking age restriction in counties where the policy was in place, and to contrast these differences to those in counties that still prohibited alcohol commerce during the weekends.[4] The novelty of this approach is that including underage youths as an additional control group *within* each county across time makes it possible to control for all unobserved factors that may be correlated with the adoption of the policy, as long as they do not affect the relative propensity to engage in crime in closely spaced age cohorts. We are for instance able to account for changes in police effort, provided that it does not differentially affect illegal behavior across age cohorts.

[4] Carpenter and Dobkin (2010) use a cleaver strategy to examine the effect of age based restrictions on alcohol consumption and crime. They exploit the fact that only individuals who have turned 21 are eligible to buy alcohol in the US. The regression discontinuity analysis shows that drinking participation increases sharply at age 21 by about 30 percent. The results further reveal a significant increase in arrest rates for nuisance and violent crimes. More broadly, our paper is related to a series of recent studies using novel research designs to pin down the causal effect of various criminal determinants; see e.g. Card and Dahl (2010); Dahl and DellaVigna (2009); Jacob and Lefgren (2003); Donohue and Levitt (2003); Doyle (2008); Duggan (2001); Lee and McCrary (2009); Deming (2010); Lochner and Moretti (2003); Kling, Ludwig and Katz (2005); Bayer, Hjalmarson and Pozen (2009); Hjalmarson and Lindquist (2010); Adda, McConnel and Rasul (2010); Draca, Machin and Witt (2008); Dustmann and Piil-Damm (2009); Meghir, Palme and Schnabel (2011); Weiner, Lutz and Ludwig (2009).

Our analysis is made possible by extensive individual conviction data that have been merged to administrative registers. The dataset covers the universe of the Swedish population age 16 and above during the period 1985 to 2007, and contains information on crime type as well as offence date. It comprises a range of standard individual characteristics, including parental socioeconomic background. Our analysis focuses on young males. It is well known that male youths account for a disproportionate share of total crime (e.g. Hirschi and Godfredson 1983). By targeting this group we are able to obtain complete records of all individuals' conviction histories, as well as measures of ability taken from compulsory schooling registers.

We make several innovations over the current literature. First and foremost, our research design allows us to identify the effects on crime of liberalized alcohol sales restrictions relying on substantially weaker assumptions than in past studies. Second, the dataset used is by far richer than what previously has been available. In fact, this is the first paper to use individual level data.[5] The data allows us to study several types of crime and to investigate different subgroups of the population. We are especially interested in whether the effect of the policy is stronger in groups usually considered at higher risk of criminal involvement (e.g. worse socioeconomic background, past offenders, low ability). The data also makes it possible to study whether Saturday open alcohol stores simply redistribute crime across different days of the week periods or permanently increases it. Last, drawing on data from multiple sources we are able to document the impact of the experiment on both alcohol sales and alcohol consumption.

Our empirical analysis starts off by investigating how the experiment affected alcohol sales. Exploring regional level panel data to account for unobserved area

[5] Only a few studies in economics and related disciplines have ever used population conviction data merged to administrative data to study criminal behavior. One exception is Hjalmarsson and Lindquist (2010) who use a similar dataset as ours to investigate the intergenerational correlation in crime in Sweden.

heterogeneity, we find robust evidence that the reform increased overall alcohol sales by between 3.7 and 5.3 percent. Survey data further reveal that although weekday alcohol consumption remained unchanged, Saturday consumption grew by 14.3 percent after the experiment was introduced. Moreover, there was no significant change in Saturday drinking among individuals not entitled to buy alcohol in the state monopoly liquor stores.

After having demonstrated that the experiment had real consequences for both alcohol sales and alcohol consumption, we turn to investigating its impact on crime. Although our estimates are too imprecise to detect any statistically significant increase in overall crime, we find that the experiment significantly raised Saturday crime by 18.7 percent. We document even larger increases in illegal behavior among individuals with low ability, and among persons with fathers that have completed at least some secondary education. Just as for alcohol sales, the increase in crime was slightly higher during the initial phase of the experiment. Both effects do however persist over time. We end the analysis by giving some basic back-of-the-envelope calculations trying to assess the social costs and benefits linked to the experiment. Our analysis reveals that the social costs exceed the monetary benefits of increased tax revenues.

The paper unfolds as follows. Section 2 outlines the institutional background surrounding the Swedish alcohol control system and the experimental scheme. In this section we also investigate the effect of the experiment on alcohol sales. Section 3 describes our data and research design. Section 4 presents the results, and Section 5 concludes.

2. INSTITUTIONAL BACKGROUND

2.1 Crime in Sweden

The Swedish crime rate is high in comparison to many other countries. In 2006, the total number of assaults reported to the police per 100,000 inhabitants amounted to 845. The same year, official crime statistics from the US police reveal 787 recorded cases of assaults per 100,000 inhabitants, and the corresponding number for Canada is 738 (Harrendorf et al. 2010). Even though these figures partly reflect differences in the propensity to report crime they are similar across many types of crime. For instance, in 2006 the number of reported burglaries per 100,000 persons was in Sweden 1,094. In the US and in Canada the equivalent numbers were 714 and 680, respectively.

As in most other countries, youths represent the most criminally active age group. Figure 1 plots the share of convicted males in 2005 by age relative to the national conviction rate. A number above (below) one indicates that the share of convicted males for that age group is higher (lower) than the average for all age groups. It is clear that the conviction rate peaks already before age 20, then falls sharply. By age 23 the share of convicted persons has already dropped 25 percent from its peak level.

2.2 Swedish alcohol laws[6]

The use of alcohol is heavily regulated in Sweden. Besides high alcohol taxes, one of the most important control mechanisms is the state monopoly on alcohol retail. The institutional arrangement implies that individuals are only allowed purchase alcohol (spirits, wine and strong beers) over the counter in some of the country's 400 monopoly alcohol retail stores. The stores are distributed all over Sweden and there is virtually one in each municipality. In rural areas where the average distance to a store is longer there are instead retail agents, usually situated in local supermarkets. At a retail agent, customers

[6] This section and section 2.3 draws heavily on Norström and Skog (2003, 2005). We refer to these studies for a more comprehensive treatment of the policy.

can place orders which they collect a few days later. There are about 500 agents. The only type of alcohol that is available to customers over the counter in regular grocery stores is beers with a very low content of alcohol (<2.8%).

The minimum legal age to buy alcohol at the state liquor stores is 20 (since 1969).[7] The age restriction is strictly enforced and cashiers are instructed to require proof of identification from customers that look younger than 25. Purchasing alcohol to underage youths is as in many other countries both unlawful and punishable.

2.3 Saturday opening of alcohol shops

2.3.1 Background

Based on a decision in the Swedish parliament, the state monopoly alcohol retail company granted in February 2000 all shops in 6 out of 21 counties to keep open on Saturdays. The experiment was motivated by a growing consumer demand for increased access to the state liquor stores, which had been closed during the weekends since 1981. The reason for not implementing the reform nationwide was that the government required an initial assessment of the social consequences of liberalizing its weekend sales restrictions. Researchers were directly involved both in designing the experiment and in evaluating it. By selecting counties based on a wide range of structural factors (e.g. size, geographic location, and degree of urbanization) the research team hoped to maximize the external validity of their results. The experimental counties were: Stockholm, Skåne, Norrbotten, Västerbotten, Västernorrland, Jämtland. Figure A.1 provides a map over these regions. Together, these hosted about 3,800,000 inhabitants (almost half of the total Swedish population). No other alcohol policies were significantly changed during the experiment.

[7] It is however legal for youths purchase alcohol in bars when they turn 18.

The evaluation consisted in time-series studies of alcohol sales and various crime and health indicators, both in the experimental areas and in a few control regions believed to resemble the characteristics of the experimental areas (Norström and Skog 2003). The first assessment of the policy revealed a 3.7 percent rise in alcohol sales. The increase was almost exclusively driven by higher sales of beers and spirits. Norström and Skog also considered the effects on crime as measured by the number of assaults reported to the police. They found no statistically significant increase in reported assaults or in any of the health indicators. Although the report clearly stressed that the statistical precision in the crime analysis was not satisfying, the general opinion among policy makers was that the experiment was a success. In the spring of 2001, the Swedish parliament therefore voted in favour of a nationwide introduction, which occurred in July the same year.

Norström and Skog (2005) examined the combined effects of both policy changes and found increases in sales of beer and spirits by about 3.6 percent. Again, there was no statistically significant effect on assaults. The results however showed a significant increase in drunk driving, which the authors claim most likely was due to increased police effort.

2.3.2 Did the experiment really increase alcohol sales?

Despite being carefully executed, the past evaluation of the reform relies only on time-series data, which substantially increases the risk that the results may be driven by other factors. During the 90s and early 00s illegal trade of alcohol increased, so did the number of licensed bars and restaurants. If such factors coincided with the introduction of the policy it is necessary to account for them in the analysis.

Our strategy to deal with confounding factors is to combine both cross-regional and cross-time data. The idea is to compare alcohol sales in counties that had switched to

Saturday open alcohol stores to that in counties that still prohibited alcohol commerce during weekends. We use data covering total alcohol sales for each county and month starting in January 1998 and ending in June 2001. The data was provided by the state monopoly alcohol retail company Systembolaget AB.[8]

Our analysis is based on the following regression model

(1) $$Alcohol\ sales_{ct} = \alpha + \beta Policy_{ct} + \gamma_c + \delta_t + (\theta_t \mathrm{t}) + \varepsilon_{ct}$$

where Alcohol sales_{ct} is the (log) number of liters 100% alcohol sold per person (aged 20 and above) in county *c* and time (month×year) *t*. $\text{Policy}_{\text{ct}}$ is an indicator variable set to unity if the policy was in place in county *c* in time *t*, and zero otherwise. By including county fixed effects (γ_c), the model absorbs all persistent unobserved county characteristics that may be correlated with the timing of the introduction of the policy and with alcohol sales. This could for instance be local preferences for alcohol. In a similar way, the time fixed effects (δ_t) removes national trends in alcohol commerce common for all counties. This way the model effectively sweeps out most potential confounding factors. To avoid problems with cross-border shopping, we follow Norström and Skog (2005) and exclude neighbouring counties from the analysis. This leaves us with a sample of 13 counties observed for 42 consecutive months.

Since the number of cross-sectional units is relatively few there is a risk that conventional standard errors that account for serial correlation at the county level are biased downwards (Bertrand, Dufflo and Mullainathan 2004). We therefore ran Prais-Winsten regressions assuming a county specific AR(1) process. This also allows the error terms to be county specific heteroscedastic, and contemporaneously correlated across

[8] The data was provided unconditional and free of charge.

counties. For the purpose of comparison, we also estimated conventional cluster robust standard errors as well as block bootstrap standard errors. Table A.1 supplies the estimates. It is reassuring that the results from these alternative approaches are basically identical to our preferred model.

Table 1 presents the results. As can be seen in column (1), our baseline estimate shows that Saturday opening of alcohol shops led to a statistically significant increase in alcohol sales. The coefficient suggests that the experiment increased the quantity of alcohol sold by about 3.7 percent. It is intriguing to note that the estimate is basically identical to the time-series evidence presented in Norström and Skog (2005). This finding highlights the successful design of the experimental scheme.

Our empirical strategy requires that the timing of the introduction of Saturday open alcohol shops should not be systematically correlated with unobserved county specific events affecting alcohol sales. We find it unlikely that this assumption would be violated since the counties included in the experiment underwent a time-consuming selection process where the explicit goal was to select regions based on variation in a range of different characteristics. Still, in columns (2) and (3) we test the validity of this assumption.

We start by adding linear county-specific time trends ($\theta_t \mathrm{t}$) to the regressions. This controls for all smoothly evolving county characteristics, regardless whether these are observed or not. In column (2) we can see that this exercise leaves the point estimate virtually unchanged. Last, if the adoption of the policy was truly exogenous we would not expect that future policy affect current sales conditional on current policy. Column (3) presents results from regressions where we added a dummy for whether the policy was in place two quarters in the future. It turns out that the coefficient on future policy is close to zero and statistically insignificant.

The results presented so far suggests that it is fair to treat the introduction of Saturday open alcohol stores as exogenous controlling for county and time effects. Columns (4)-(7) provide some extensions of our analysis. We start by assessing the importance of cross-border spillover effects by including in the regressions the seven counties that were neighbouring the experimental regions. Doing so makes our baseline estimate increase to .053 (.016). The slightly higher coefficient is consistent with a story that alcohol sales fell in neighbouring counties because of increased cross-border shopping induced by the reform. Column (5) exclude the south most situated experimental county: Skåne. Inhabitants in Skåne had already before the experiment the possibility to purchase alcohol on Saturdays by going across the national border into Copenhagen, Denmark. When dropping Skåne our baseline estimate increases somewhat. This suggests that inhabitants in Skåne responded weaker to the experiment. One explanation is that the relatively lower alcohol prices in Denmark still made it profitable to travelling to Copenhagen to purchase alcohol; especially during the weekends when most individuals have time to travel. In columns (6) and (7) we assess the temporal dynamics by investigating whether the increase in alcohol sales was stronger during the initial phase of the experiment. We can see that the increase in alcohol sales was biggest during the first two quarters after the experiment was introduced. Four quarters after the reform had been implemented the magnitude of the estimate has fallen to the same level as for the entire experimental period. The most likely explanation for this is that the reform received large attention in the mass media.

Since the state liquor company is the sole provider of over-the-counter alcoholic beverages, our analysis of alcohol sales should provide a good proxy also for alcohol consumption. However, our results could be biased if the experiment transferred consumption away from illegal procurement of alcohol (e.g. illicit trade or distillation).[9] Another drawback with our data are that we cannot tell whether the experiment increased weekend drinking.

To shed some light on these issues we use data from a survey called ULF (Undersökningen av LevnadsFörhållanden), which asks individuals aged 16 and above about their alcohol habits in the last week prior to the survey date. The survey is conducted by Statistics Sweden and covers a random sample of about 10,000 respondents. Importantly for our purposes is that the respondents are asked to quantify their alcohol consumption in different periods of the week. Due to confidentiality reasons, Statistics Sweden compiled the data on our behalf.

Although the survey contains geographic identifiers, questions on alcohol use are only included for the rounds performed in 1996/97 and 2004/05. Since the policy was adopted nationwide in 2001 we are not able to exploit the regional variation of the experimental scheme. Instead, we compare stated alcohol consumption on weekdays[10] versus Saturdays before and after the reform. Under the assumption that weekday consumption was unaffected by the experiment this approach amounts to a standard difference-in-differences estimator. Of course, the experiment may also have influenced weekday alcohol consumption if, for instance, it decreased weekday queues. Some caution is therefore warranted when interpreting the results from this exercise.

It turns out that the average daily consumption of alcohol expressed in terms of centiliters 100 % alcohol per person at least age 20 remained virtually unchanged between 1996/97 and 2004/05, going from 1.84 to 1.88 centiliters. In contrast, Saturday consumption grew from 2.61 to 2.92 centiliters. Relative to the base this translates into an

[9] To the extent that alcohol and narcotics are substitutes it is also possible that the reform increased the use of illicit drugs. Conversely, if these products are complementary, consumption of narcotics may have decreased.

[10] Monday to Thursday.

increase of about 14.3 percent ((2.92-2.61)/2.61). It is also interesting to note that there was no significant change in Saturday drinking among individuals not entitled to buy alcohol in the state monopoly retail stores. For youths aged 16 to 19, alcohol consumption actually fell slightly from 1.86 to 1.82 centiliters.

In summary, we find robust evidence that the experiment raised alcohol sales in the order of 3.7 to 5.3 percent. Tentative evidence also suggests that the increase was confined to Saturdays, and that it only applied to eligible youths.

The magnitude of this increase is quite large. Still, there are several reasons to expect Saturday open alcohol shops to affect crime over and beyond increased alcohol use. First, the opportunities to commit crime during weekends may be different compared to weekdays. More people may for instance be clustered together in non-job related contexts. Second, the reform may have shifted the venue of consumption away from protected environments, such as bars and restaurants, in favour streets and other public spaces. It is also important to note that the increase in alcohol sales provoked by the experiment seem to have been driven by higher sales of beers and spirits (Norström and Skog 2005). These alcohol types are known to be considerably more strongly associated with criminal activity than wine, for example (Norström 1998). With these facts in mind, we proceed to our analysis of the impact of the experiment on crime.

3. IDENTIFYING THE IMPACT OF SATURDAY OPEN ALCOHOL SHOPS ON CRIME

3.1 Data and sample selections

Our data originate from several administrative registers collected and maintained by Statistics Sweden. The registers contain information on the entire Swedish population age 16 and above each year from 1985 to 2007. These data have been linked to the Swedish conviction register kept by the National Council for Crime Prevention (BRÅ).[11] We obtained complete records of all criminal convictions during the period. The data include information on crime type as well as the sentence ruled by the court, and covers convictions in Swedish district courts (the court of first instance). One conviction may include several crimes and we observe all crimes within a single conviction. Speeding tickets and other minor offenses are not included in the data. In some cases, individuals may be found guilty of a crime without being prosecuted or sentenced in court. This happens if the offender is very young or if (s)he confesses to a less severe crime. Although these cases are handled by the district attorney they are still included in our data.

Even though there is information on the exact date of the offense, there are too few convictions on a given date for us to fully exploit the high frequency nature of the data. A related issue is that the exact day of the crime in some cases is unknown.[12] The specific day in which a break-in occurred is for instance not always clear. In these cases the court assigns a date based on an educated guess, which obviously generates some measurement error in the variable. To alleviate these concerns we study all crime that occurred in a given quarter for which the offender has been convicted. We use the same period of analysis as for alcohol sales, i.e. January 1998 to June 2001. By ending the observation period in June 2001, we allow more than six years between the potential crime and the conviction. Bordering counties are again excluded from the main analysis.

Our population of interest consists of male youths aged 17 to 23. We exclude individuals aged 19 since we want to minimize the risk that individuals not entitled to purchase alcohol at the state liquor stores may have benefitted from the experiment through older friends. 16 year olds are not included since they still are enrolled in

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[11] Only a few previous studies that analyzes crime have used Swedish individual conviction data merged to population registers; see Grönqvist (2011); Hällsten, Sarnecki and Szulkin (2011); Hjalmarsson and Lindquist (2011); Meghir, Palme and Schnabel (2011).
[12] This applies to about 30 percent of all convictions.

compulsory school which means that: (i) we are unable to obtain measures of their school performance; (ii) the characteristics of the group may be very different compared to older cohorts exposed to the experiment. The main advantage of focusing on male youths is that we gain power to our estimations since men in this age group account for a disproportionate number of crimes in the total population. Moreover, the age constraint coupled with the long period for which we have information on crime makes it possible to obtain complete records of all individuals' conviction histories. These restrictions leave us a sample of about 300,000 individuals in each of the 14 quarters under study.

Because of the sheer size of the dataset, and due to the fact that the policy only varies at the aggregate level, we collapse the data into county/quarter×year/age cells. Again, to increase statistical power we define age in two year intervals: 17/18; 20/21 and 22/23. Besides computational convenience, one other benefit of collapsing the data is that this absorbs intra cluster correlation among individuals within each cell which otherwise would tend to underestimate the standard errors (Moulton 1990). Since we are interested in estimating the effect of Saturday open alcohol shops on crime at the individual level, we weight all regressions by the number of observations in each cell to replicate the underlying micro data.

Note that, although we observe each individual's county of residence each year, we have no information on the location of the crime. In most cases however, county of residence will coincide with county of crime.

Our main dependent variable is the overall number of crimes in the cell per 100,000 persons. In some specifications, we also discriminate between violent crimes and property crimes. To investigate aggravated crime we also consider the prison rate, defined as the number of imprisoned individuals per 100,000 persons in the cell. Table A.2 provide exact details of the way these variables have been constructed. Since convictions only represent a subset of all crimes committed, some cells have few crimes reported. In some specifications, we therefore only focus on total crime.

The main advantage of using individual level conviction data is that we can investigate whether the potential effect on crime differs in subgroups of the population. This has not been possible in previous studies which exclusively have relied on aggregated data based on police reports. We center on groups at higher risk of criminal involvement. We stratify individuals according to their compulsory school grade point average (GPA), computed as the percentile rank by year of graduation to account for changes in the grading system over time. Since the data contain an exact link between children and their biological parents we also add information on the father's highest completed level of education. As previously mentioned, we also discriminate between past offenders and individuals with no criminal history. Table A.3 presents descriptive statistics of the variables included in the analysis. As can be seen, the regional characteristics are well-balanced across experimental and control areas. Although the experimental areas exhibit a slight disadvantage in terms of higher crime rates, none of the differences are statistically significant.

Despite the benefits with the data, it should be noted that this paper infers criminal behavior from individuals that have been convicted in court. This generates a concern that the people that had access to Saturday open alcohol shops may be more likely to have been convicted conditional on actually having engaged in crime. Individuals that have consumed alcohol may, for instance, be more careless after having committed a crime, and therefore more likely to get caught. This is a caveat to bear in mind when interpreting the results.[13] Note however that data on self-reported crime would not solve

[13] In their study of the effect of education on crime using arrest data, Lochner and Moretti (2005) raise a similar concern. Using data on self-reported crime they however conclude that for this to be a problem education must substantially alter the probability of being arrested conditional on criminal behaviour.

the problem. It would instead generate problems with recall bias, since subjects that have been drinking are less likely to perfectly remember information about their criminal behavior.

3.2 Research design

To identify the effect of the experiment on crime we exploit the fact that it was introduced in only a few counties. We also take advantage of the national drinking age restriction which prohibits stores to sell alcohol to individuals under the age of 20. This provides a third dimension on which access to the experimental scheme varies. Our strategy is to use this cross-county, cross-time and cross-age variation in access to the experiment in a difference-in-difference-in-difference (DDD) framework by estimating models of the following form

$$Crime_{cta} = \mu + \rho Policy_{cta} + \varphi_{ct} + \psi_{ta} + \omega_{ca} + v_{cta} \quad (2)$$

where $Crime_{cta}$ is the (log) number of crimes per 100,000 individuals in county *c*, time (quarter×year) *t* and age group a [where c×t×a≡13×14×3=546 cells]. $Policy_{cta}$, is a binary variable set to unity if the policy was in place in county *c* in time period *t* and applied to age group a, zero otherwise. The model is very flexible as it provides full nonparametric control for county specific time effects that are common across age groups (φ_{ct}), time-varying age effects (ψ_{ta}) and state specific age effects (ω_{ca}). The advantages of this approach is that we can control for all unobserved factors that may be correlated with the timing of the experiment, as long as these factors do not affect the relative propensity to engage in crime across age cohorts. The model for instance accounts for changes in police effort.

Note that ρ not only identifies the effect of the experiment on crime commission but also on victimization. This is however no problem since it is precisely the parameter of interest for policy makers truing to assess the welfare gains linked to the experiment. A related issue is that there is some risk that our model underestimates the true impact of the experimental scheme on crime. This will happen if the experiment made individuals above the national drinking age more likely to become victims of crime perpetrated by underage youths. Our results should in this case be interpreted as a lower bound of the true effect.

As already mentioned, some cells will have no convicted individuals. In these cases we assign an arbitrary low value before taking the log and control for this in the regressions. This variable is by construction endogenous with respect to the experiment. Still, since the share of empty cells in most part of our analysis is small (only about 2 percent) this is unlikely to constitute a problem.

4. EMPIRICAL ANALYSIS

This section presents the results from our empirical analysis. We start by examining the impact of the experiment on crime throughout the entire week. This provides an estimate of the total effect on crime taking into account any potential temporal displacement effects. We then separate between crimes that occurred during Saturdays and weekdays. We proceed by investigating the temporal dynamics of the experimental impact. The section ends with some back-of-the-envelope calculations of the social costs and benefits linked to the experiment.

4.1 The effect of Saturday open alcohol shops on crime throughout the week

Table 2 provide results for the effect of the experiment on crime throughout the entire week. Each column contains estimates for different types of crime. Panel (i) starts by showing results from regressions only controlling for county, time and age effects (i.e. a differences-in-differences model). As we can see, the experiment has no statistically significant effect on total crime. This finding holds also when looking at violent crime. There is however a statistically significant positive effect on property crime in column (3). The coefficient suggests that the Saturday open liquor stores increased property crime by about 11.6 percent. The estimate is significant at the 10 percent level. There is also a significant positive association between the experiment and the share of individuals in each cell that received prison sentences. The estimate implies that the reform raised the imprisonment rate by about 16 percent.

As discussed earlier, it is likely that the experimental scheme affected the operations of the local law enforcement agencies. Norström and Skog (2005) argue that that increased police surveillance explain why their analysis revealed a significant surge in drunk driving.[14] The results in Adda, McConnell and Rasul (2011) provide further evidence of the importance of relocating police effort. They evaluate a localized experiment in which cannabis possession was depenalized in the UK. Their results clearly suggest that the police devoted more effort towards non-drug related crime. Because of this reason it is difficult to interpret the results from conventional analytical approaches, such as a standard difference-in-differences model, as evidence of the causal impact of the experiment on criminal behaviour. To do this a more flexible model is needed.

Our approach is once again to add male youths below the national drinking age restriction as an additional control group. This allows us to control for county-by-time, county-by-age and age-by-time effects in the regressions. The fixed effects account for

[14] Unfortunately, there are too few offenses in our population of study to include drunk driving in the analysis.

changes in police effort to the extent that these have a similar effect on illicit behavior in different age groups. Our estimation results are displayed in Panel (ii).

As evident, we find no statistically significant effect of the experiment for any of the outcomes. It is however important to note that the coefficients are imprecisely estimated. This uncertainty means that we cannot rule out that the experiment in fact may have brought large effects on crime. Yet, the magnitude of the coefficients is substantially smaller in three out of four regressions compared to the results in Panel (i). One likely explanation is that the experiment provoked more police interventions which led to more individuals being convicted. This result highlights the importance of accounting for changes in police effort when analyzing changes in alcohol or drug policy.

Table 3 presents results for alternative specifications and control groups. It is possible that Saturday opening of alcohol shops did not influence the number of crimes committed, but instead affected the decision of whether at all to participate in criminal activity. To examine the effect on crime at the extensive margin we re-estimated our models using the share of convicted persons in each cell as dependent variable. As can be seen, the results are basically identical to our baseline estimates. This is hardly surprising since few individuals are convicted more than once for crimes committed in a given quarter.

Crime varies substantially both across counties and age. It is also well-known that illicit behavior has a large seasonal component; possibly generated by variation in weather conditions (Jacob, Lefgren and Moretti 2007). Because of this we choose to enter the dependent variable in terms of the natural logarithm. However, since there are no theoretical reasons to prefer a log-linear specification, we also estimated a linear model. It is clear that these estimates are qualitatively similar to our baseline specification. To examine the effect of the experiment on the average county we also ran unweighted regressions. Again, we find no statistically significant effect on crime.

We also tested alternative control groups. Although our research design allows us to estimate the causal effect of the reform on crime under weak assumptions, it is possible that individuals in the control group were affected by the experiment. This is the case if underage youths managed to obtain alcohol from the state liquor stores through their older friends or if criminal activity increases in this group because there are more potential victims under the influence of alcohol. Our estimator will then underestimate the true effect on crime. We therefore included 16/17 year olds as an alternative control group in the regressions. However, none of the estimates are statistically significant and the coefficients reveal no major changes.

It is possible that crime in neighboring areas was affected by the experiment. Recall that our previous analysis revealed that alcohol sales in bordering areas went down because of increased cross-border shopping. Even though two out of four coefficients are found to be negative when including neighboring counties in the analysis, it is clear that the statistical precision is not sufficient to conclude that crime in these regions declined.

Table 4 provides results for different subgroups of the population. Unfortunately, when analyzing smaller parts of the population the potential problem with empty cells grows bigger. In some of these regressions, the share of empty cells increases to 20 percent. This means that the statistical uncertainty increases as well as the risk that our estimator is biased.

Column (1) presents our baseline estimates for the entire sample. Columns (2) and (3) show results for individuals stratified according to their compulsory school performance. There is no statistically significant effect for any of these two groups. Columns (4) and (5) present results for past criminals and individuals with no criminal background. Again, we find no significant estimates. Last, we examine groups separated by father's education. As evident, the experimental scheme increased violent crimes for individuals with fathers that have completed at least some upper secondary education. This finding is not surprising. There is plenty of evidence in the literature that individuals from more affluent socioeconomic backgrounds tend to consume more alcohol (e.g. Bellis et al. 2007). One explanation that has been proposed is that a favorable socioeconomic background implies greater financial resources to purchase alcohol. It is however important to bear in mind that since Table 4 tests many hypotheses, we are likely to come across a few significant estimates by just pure chance.

4.2 Did the reform lead to increased crime on Saturdays?

Although our analysis so far suggests that Saturday open alcohol shops had no significant effect on crime throughout the entire week, it is important to remember that the lack of statistical precision makes it impossible to rule out large effects. Still, it is natural to expect any effect on crime to be biggest on Saturdays. The evidence presented earlier also suggested that the increase in alcohol consumption was confined so Saturdays. To investigate this we separated in the analysis between crimes committed during Saturdays versus weekdays. Since the share of empty cells increases when looking at crimes committed for sub-periods of the week we are only able to perform this analysis for the total crime rate. Our results are presented in Table 5.

We find a statistical significant positive effect of the experiment on total crime. The coefficient implies that the experiment increased total crime on Saturdays by 18.7 percent. This is by all accounts a large effect. In columns (2) through (7) we repeat the analysis for the different subgroups. We find an even bigger effect among individuals with low compulsory schooling grades. For this group, criminal activity increases by more than

21 percent. In contrast, we find no significant effect for individuals who received higher grades than the median. There is no significant effect in the two groups separated by criminal background. There is however once more a significant increase in Saturday crime for individuals with fathers who have completed at least some upper secondary education.

It is interesting to note that all coefficients are larger in magnitude for crimes committed on Saturdays relative to the entire week. It is also remarkable that all estimates for weekday crimes display negative signs. Although the imprecise estimates make this explanation speculative, one reason for the negative coefficients is that the experiment led to a temporal displacement of criminal activity away from weekdays in favor of Saturdays.

4.3 Dynamic effects

Our analysis of alcohol sales revealed slightly higher increases in alcohol commerce during the first two quarters after the experiment had been introduced. We repeated this exercise to investigate if there also was a corresponding initial increase in crime. Since we found that the increase in crime only occurred on Saturdays, we discriminate between Saturday crimes and weekday crimes. In order to avoid problems with empty cells we again focus only on total crime.

Our results are displayed in Table 6. As can be seen in column (1), there is no statistically significant effect of the experiment on crime throughout the entire week. In contrast, column (2) reveals a significant rise in crimes committed on Saturdays. In line with the results for alcohol sales, the increase is largest during the first two quarters of the experiment. After four quarters, the effect has decreased somewhat. Just before the nationwide introduction of the reform, the magnitude of the coefficient has shrunk even further. Still, it constitutes a large effect. We again find negative coefficients for weekday crimes, irrespective of how much time has elapsed since the experiment was launched.

4.4 Cost-benefit analysis

Having shown that the experiment led to increased alcohol sales as well as more crime the obvious next question is whether the monetary benefits generated by increased tax revenues surpass the additional costs imposed on society by higher crime rates. Although the social benefits are fairly easy to measure, estimating the costs is considerably more challenging. Our accounting exercise should therefore only be considered as crude back-of-the-envelope calculations.

Our measure of the social benefits produced by the experiment is given by

$$SB = \hat{\beta} \times QS_{pre}^{reform} \times \bar{T} \qquad (3)$$

where $\hat{\beta}$ is the estimated impact of the experiment on alcohol sales, QS_{pre}^{reform} is the quantity of alcohol sold in the reform areas in the pre-policy period and $\bar{T}$ denotes the average alcohol tax measured in USD per unit alcohol.

Based on information provided by Systembolaget AB we were able to compute the average tax revenues (from alcohol tax and VAT) per liter 100% alcohol to USD 43,5. In 1999, the state monopoly alcohol stores in the experimental areas sold a total of 14,094,336 liters 100% alcohol. This means that the total tax revenues amounted to 14,094,336×43,5 = USD 613,103,616. Plugging in our preferred estimate of the experimental impact on alcohol sales ($\hat{\beta}$ = 0.037) then gives us SB = $\hat{\beta} \times QS_{pre}^{reform} \times \bar{T} =$

0.037×14,094,336×43,5 = 22,684,834. In other words, the monetary benefits produced by the experiment are almost USD 22,7 million.

In a similar way, the estimated social costs from the increase in illegal activity is given by

$$(4) \qquad SC = \hat{\rho} \times O_{pre}^{reform} \times \bar{C}$$

where $\hat{\rho}$ is the estimated effect of the reform on crime, O_{pre}^{reform} is the number of offenses reported to the police in the reform areas in the pre-policy period and $\bar{C}$ is the average cost of an offense.

Recall that our analysis of Saturday crime revealed that $\hat{\rho}$ = 0.187. Our own calculations using data from the conviction register shows that about 16.04 percent of all crimes are committed on Saturdays. From official crime statistics we know that 579,644 crimes were reported to the police in the experimental areas in 1999.[15] It is fair to assume that the distribution of crimes that led to a conviction over the week is the same for crimes reported to the police. Under this assumption we estimate that 92,975 (579,644 ×.1604) crimes were committed on Saturdays in these areas that year. If we impose the additional assumption that the increase in convictions induced by the reform is proportional to the increase in reported crime, our estimate suggests that the experiment raised total crime by 17,386 (92,975×0.187) cases.

Jarl et al. (2006) is the only study we are aware of that tries to estimate the total costs linked to crime in Sweden. Their calculation takes into account both the direct costs of criminal activity (e.g. health care; foregone income; property damage etc.), as well as

[15] See: www.bra.se

the costs of crime prevention (e.g. justice system; insurances etc.). Since they only consider the most common types of crime (violent crime, property crime, vandalism and drunken driving), using their numbers in our calculations are likely to produce downward biased estimates of the true costs of crime. The average costs of a crime reported in Jarl et al. is USD 2,211. This means that $SC = \hat{\rho} \times O_{pre}^{reform} \times \bar{C} = 0.187 \times 92{,}975 \times 2{,}211 = 38{,}441{,}165$.

To summarize, our cost-benefit analysis shows that the social costs generated by the reform surpass the monetary benefits by about SC–SB= 38,1–22,7= USD 15,4 million.[16] Note however that since our estimates are imprecise, there is some uncertainty in these figures. To quantify this uncertainty we also supply estimates on the social costs and benefits produced by the experiment based on the confidence intervals. The upper limit of the 95% confidence interval of $\hat{\beta}$ is 0.07. Repeating the exercise above by plugging in this value gives us SB=0.07× 14,094,336×43,5 = 42,917,253. Here, the experiment actually generates a small welfare gain in the order of USD 4,8 million (42,9–38.1). However, there is even greater uncertainty in the estimated effect on crime. By plugging in the upper limit of the 95 % confidence interval for $\hat{\rho}$ (0.393), we can see that the social costs increases substantially to USD 80,788,116 ($0.393 \times 92{,}975 \times 2{,}211$) resulting in a welfare loss by about USD 58,1 million (22,7–80,8).

5. CONCLUDING REMARKS

Understanding how liberalized weekend alcohol sales restrictions affect both alcohol sales and crime is important for policy makers weighing potential benefits from increased

[16] Note that this exercise assumes that the results from our analysis focusing on male youths can be generalized to the entire population. However, our conclusions would not change if we only consider male youths. This is because male youths account for a similar fraction of crimes committed in the total population as for their use of alcohol compared to the rest of the population (just above 12 percent).

alcohol tax revenues with possible higher crime rates. This paper examines the introduction of a large scale experimental scheme in which the Swedish state monopoly alcohol retail company granted all stores in several counties to keep open on Saturdays. To isolate the impact of the experiment from other factors, we compare conviction rates in age cohorts above and below the national drinking age restriction in counties where the experiment had been implemented, and contrast these differences to those in counties that still prohibited weekend alcohol commerce. Our analysis relies on extensive individual longitudinal conviction data that have been merged to population registers.

The results reveal that the experiment significantly raised alcohols sales by between 3.7 and 5.3 percent. There is also suggestive evidence that the experiment increased alcohol consumption, and that this increase is confined to Saturdays only for individuals entitled to buy alcohol at the state monopoly alcohol retail stores. Our results further show that the experiment significantly increased crimes committed on Saturdays. The effect is especially strong among individuals with low ability, and among persons with fathers that have completed at least some secondary education. Although the increases in crime and alcohol sales were slightly higher during the initial phase of the experiment, the effect persists over time.

Although our results suggests that Saturday opening of alcohol retail stores lead to moderate increases in alcohol sales, the experiment did not imply large increases in crime other than on Saturdays. Still, our cost-benefit analysis reveals that the social costs generated by the experiment exceed the monetary gains from increased tax revenues. Of course, any welfare analysis of similar reforms also needs to consider other possible costs. These include: worse public health, increased early retirement rates and adverse consequences for the next generation (e.g. Nilsson 2008). Estimating these costs is an important avenue for future work.

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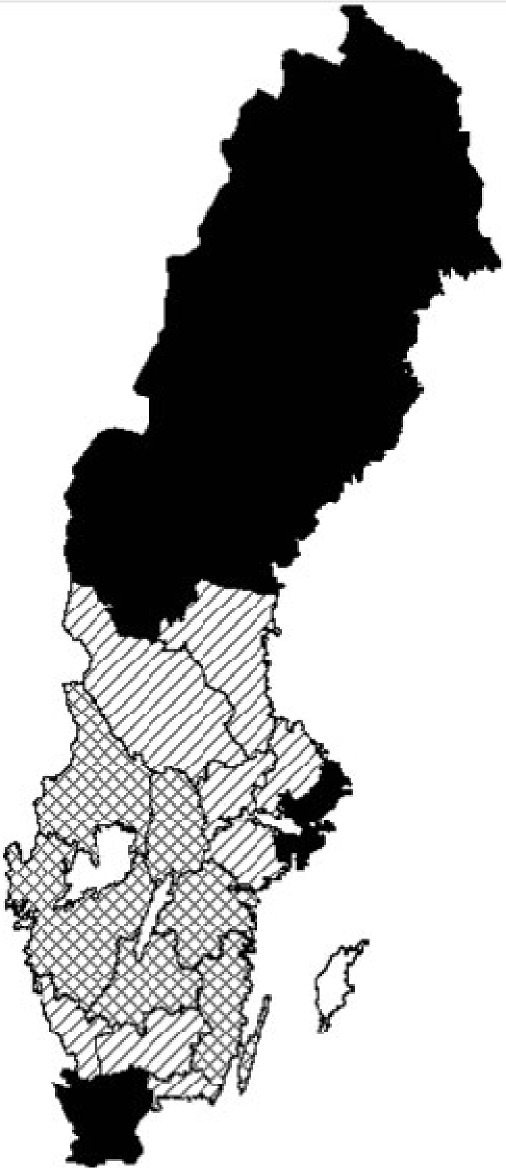

Figure A.1 Experimental areas (black), control areas (crosshatched) and buffer areas (cross-striped). From Nordström and Skog (2005).

Table A.1 OLS estimates of the effect of Saturday open alcohol shops on alcohol sales

	Baseline	Add linear trends	Placebo reform	Add border counties	Drop Skåne	Effect 2 quarters after reform	Effect 4 quarters after reform
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	.037**	.045**	.044**	.041**	.047**	.030**	.029
	(.015)	(.011)	(.016)	(.015)	(.015)	(.012)	(.018)
	[.017]	[.009]	[.017]	[.018]	[.014]	[.012]	[.017]
t+2 quarters			–.004				
			(.011)				
			[.011]				
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month (×year) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (N*T)	546	546	546	882	504	390	468
Mean of (anti-log) dep. var.	.382	.382	.382	.366	.392	.372	.381

Notes: The dependent variable is (log) per capital alcohol sales per capita age 20 and above measured in liters 100% alcohol in each county and year×month. The period of observation is January 1998 to June 2001. Numbers in parenthesis denote standard errors estimated by clustering at the county level. Numbers in brackets denote block bootstrap standard errors estimated by resampling at the county level (100 replications). ** = significant at 5 % * = significant at 10 %.

Table A.2. Definitions of crime categories

Crime type	Explanation	Legal text
Any crime	Any recorded conviction in a criminal trial regardless of type of crime	
Violent crime	The full spectrum of assaults from pushing and shoving that result in no physical harm to murder.	BRB Chapter 3 paragraph 4; BRB Chapter 17 paragraphs 1,2,4,5,10
Property crime	The full spectrum of property crimes from shop-lifting to burglary. Robbery is also included.	BRB Chapter 8
Prison	Sentenced to prison in criminal trial for any type of crime.	

Table A.3. Descriptive statistics, mean (std. dev)

Variable	Counties part of the experimental scheme [c×t×a =252] (1)	Non-bordering control counties [c×t×a =294] (2)
(i) *Crime*		
Total crime per 100,000 persons	2,337	1,986
	(553)	(447)
- Saturdays	458	412
	(151)	(151)
- Weekdays	1,549	1,284
	(403)	(344)
Violent crime per 100,000 persons	383	345
	(134)	(142)
Property crime per 100,000 persons	466	395
	(189)	(199)
Prison rate per 100,000 persons	200	187
	(121)	(116)
(ii) *Background characteristics*		
GPA (pct rank)	44.33	42.42
	(1.92)	(2.75)
Fraction past criminals	.20	.18
	(.05)	(.05)
Fraction with fathers with comp. edu.	.66	.63
	(.04)	(.04)

Notes: The sample includes all Swedish males aged 17/18, and 20 to 23. The period of observation is from January 1998 to June 2001. Descriptive statistics is weighted by the number of individuals in each cell defined by county c, time (month×year) t, and age group $a \in \{17/18; 20/21; 22/23\}$.

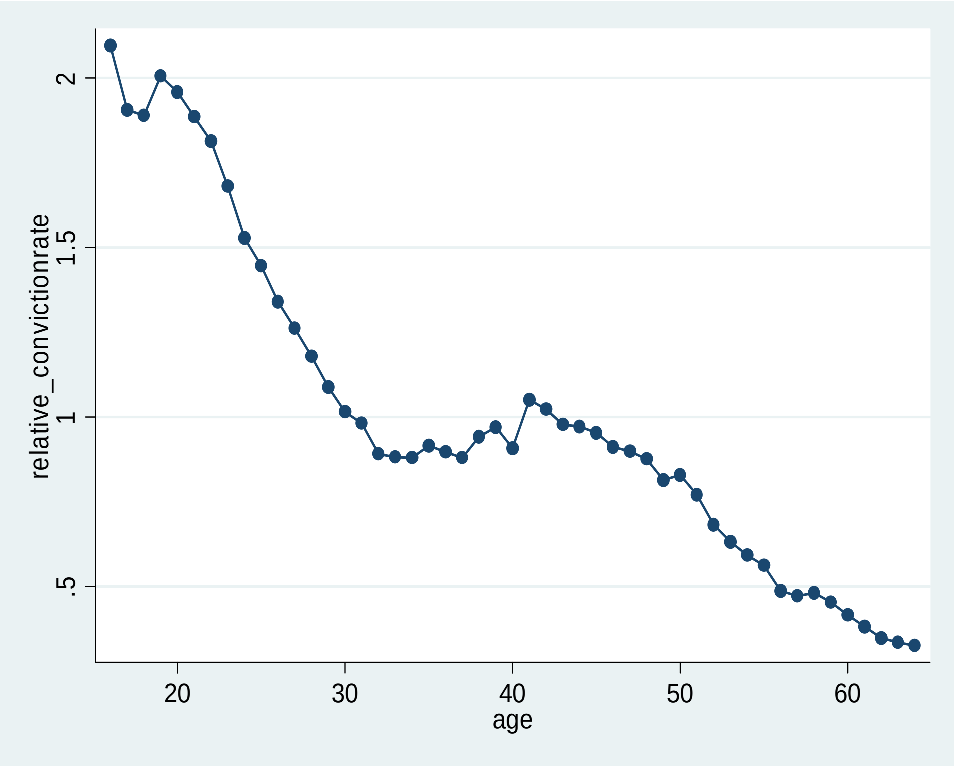

Figure 1. Share of convicted persons for crimes committed in 2005 by age relative to national conviction rates

Table 1 Prais-Winsten regression estimates of the effect of Saturday open alcohol shops on alcohol sales

	Baseline	Add linear trends	Placebo reform	Add border counties	Drop Skåne	Effect 2 quarters after reform	Effect 4 quarters after reform
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Policy	.037**	.050**	.049**	.053**	.052**	.043**	.035*
	(.017)	(.008)	(.009)	(.007)	(.009)	(.005)	(.017)
t+2 quarters			.022				
			(.049)				
County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month (×year) FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations (N*T)	546	546	546	882	504	390	468
Mean of (non-log) dep. var.	.382	.382	.382	.366	.392	.372	.381

Notes: The dependent variable is (log) per capital alcohol sales per capita age 20 and above measured in liters 100% alcohol in each county and year×month. The period of observation is January 1998 to June 2001. Panel corrected standard errors (in parenthesis) are calculated using a Prais-Winsten regression where a county specific AR(1) process is assumed. ** = significant at 5 % * = significant at 10 %.

Table 2. The overall effect of Saturday open alcohol shops on crime

	Dependent variable			
	Total crime rate	Violent crime rate	Property crime rate	Prison rate
	(1)	(2)	(3)	(4)
(i) *DD estimate*	.072	.085	.116*	.160**
	(.055)	(.092)	(.059)	(.056)
County FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
(ii) *DDD estimate*	.011	.129	.072	.077
	(.050)	(.108)	(.085)	(.133)
County×time FE	Yes	Yes	Yes	Yes
County×age FE	Yes	Yes	Yes	Yes
Age×time FE	Yes	Yes	Yes	Yes

Notes: All coefficients are weighted least squares estimates from separate regressions, weighting by the number of observations in the relevant cell. The sample in Panel (i) consists of males aged 20–23. The sample in Panel (ii) consists of males aged 17/18 and 20–23. The dependent variable is the log number of convictions or prison sentences per 100,000 inhabitants for crimes of type *j* committed in county *c*, time (month×year) *t*, and age group $a \in \{17/18; 20/21; 22/23\}$ [c×t×a=13×14×3=546 cells]. All regressions control for empty cells. Panel (i) reports cluster robust standard errors at the county level in parenthesis. Panel (ii) reports conventional heteroscedasticity robust standard errors. ** = significant at 5 % * = significant at 10 % .

Table 3. Alternative specifications and control groups

	Total crime rate (1)	Violent crime rate (2)	Property crime rate (3)	Prison rate (4)	
Baseline estimate	.011 (.050)	.129 (.108)	.072 (.085)	.077 (.133)	
(i) *Change in specification*					
• Dep. var.: Conviction rate	–.023 (.042)	.092 (.097)	.072 (.081)	N/A	
• Linear model (coeff.×100)	–.012 (.105)	.043 (.039)	.016 (.036)	.029 (.018)	
• Unweighted model	.010 (.079)	–.015 (.155)	.112 (.122)	.057 (.158)	
(ii) *Change in control group*					
• Males aged 16/17	.002 (.048)	.032 (.110)	.010 (.091)	–.065 (.189)	
• Including bordering counties	.007 (.045)	.047 (.099)	–.018 (.077)	–.027 (.112)	
County×time FE	Yes	Yes	Yes	Yes	Yes
County×age FE	Yes	Yes	Yes	Yes	Yes
Age×time FE	Yes	Yes	Yes	Yes	Yes

Notes: All coefficients are weighted least squares estimates from separate regressions, weighting by the number of observations in the relevant cell. The sample consists of males aged 17/18 and 20–23. The dependent variable is the log number of convictions or prison sentences per 100,000 inhabitants for crimes of type j committed in county c, time (month×year) t, and age group $a \in \{17/18; 20/21; 22/23\}$ [c×t×a≡13×14×3=546 cells]. All regressions control for empty cells. Robust standard errors in parenthesis. ** = significant at 5 % * = significant at 10 % .

Table 4. The effect of Saturday open alcohol shops on crime in subgroups of the population

	Entire sample (cf. Table 2) (1)	GPA below median (2)	GPA at least median (3)	Criminal past (4)	No criminal past (5)	Father comp. school (6)	Father more than comp. school (7)
Total crime rate	.011	.011	.031	.014	-.090	-.036	.051
	(.050)	(.061)	(.120)	(.072)	(.088)	(.074)	(.068)
Violent crime rate	.129	.085	.214	.182	-.017	.028	.357**
	(.108)	(.127)	(.239)	(.136)	(.176)	(.145)	(.139)
Property crime rate	072	.116	-.214	.080	-.067	.092	.099
	(.085)	(.099)	(.209)	(.115)	(.175)	(.131)	(.135)
Prison rate	.077	.200	-.212	.056	.091	.152	-.001
	(.133)	(.162)	(.203)	(.147)	(.262)	(.184)	(.197)
County×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All coefficients are weighted least squares estimates from separate regressions, weighting by the number of observations in the relevant cell. The sample consists of males aged 17/18 and 20–23. The dependent variable is the log number of convictions or prison sentences per 100,000 inhabitants for crimes of type j committed in county c, time (month×year) t, and age group $a \in \{17/18; 20/21; 22/23\}$ [c×t×a≡13×14×3=546 cells]. All regressions control for empty cells. Robust standard errors in parenthesis. ** = significant at 5 % * = significant at 10 % .

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Table 5. The effect of Saturday open alcohol shops on total crime by period of the week

	Entire sample	GPA below median	GPA at least median	Criminal past	No criminal past	Father comp. school	Father more than comp. school
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Entire week (cf Table 4)	.011	.011	.031	.014	-.090	-.036	.051
	(.050)	(.061)	(.120)	(.072)	(.088)	(.074)	(.068)
Saturdays	.187*	.215*	.054	.138	.107	.123	.212*
	(.105)	(.119)	(.220)	(.138)	(.166)	(147)	(.124)
Weekdays	-.045	-.050	-.023	-.058	-.087	-.070	-.011
	(.056)	(.069)	(.125)	(.082)	(.108)	(.084)	(.087)
County×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County×Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age×Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All coefficients are weighted least squares estimates from separate regressions, weighting by the number of observations in the relevant cell. The sample consists of males aged 17/18 and 20–23. The dependent variable is the log number of convictions per 100,000 inhabitants for any type of crime committed in county c, time (month×year) t, and age group $a \in \{17/18; 20/21; 22/23\}$ [c×t×a≡13×14×3=546 cells]. All regressions control for empty cells. Robust standard errors in parenthesis. ** = significant at 5 % * = significant at 10 % .

Table 6. Dynamic effects of Saturday open alcohol shops on total crime

	Period of the week		
	Entire week (1)	Saturdays (2)	Weekdays (3)
Elapsed time since introduction			
2 quarters	.035	.273**	-.028
	(.084)	(.133)	(.086)
4 quarters	.004	.241**	-.068
	(.056)	(.109)	(.064)
6 quarters	.011	.187*	-.045
	(.050)	(.105)	(.056)
County×Time FE	Yes	Yes	Yes
County×Age FE	Yes	Yes	Yes
Age×Time FE	Yes	Yes	Yes

Notes: All coefficients are weighted least squares estimates from separate regressions, weighting by the number of observations in the relevant cell. The sample consists of males aged 17/18 and 20–23. The dependent variable is the log number of convictions per 100,000 inhabitants for any type of crime committed in county *c*, time (month×year) *t*, and age group $a \in \{17/18;\ 20/21;\ 22/23\}$ [c×t×a≡13×14×3=546 cells]. All regressions control for empty cells. Robust standard errors in parenthesis. ** = significant at 5 % * = significant at 10 % .