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DISCUSSION PAPER SERIES

IZA DP No. 13401

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ISSN: 2365-9793

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ABSTRACT

Legal Drinking, Injury and Harm: Evidence from the Introduction and Modifications of Age Limits in Denmark

Alcohol is considered one of the most serious threats to population health, and to mitigate its negative consequences, most countries have implemented policies such as minimum legal drinking ages (MLDAs). Denmark, a country with an exceptionally liberal youth alcohol culture, introduced a minimum age for purchasing alcoholic beverages as late as in 1998, prohibiting those below 15 to buy alcohol. Previous studies from the U.S. and a few other contexts have provided substantial evidence that MLDA legislations influence outcomes such as car accidents, but there is little evidence from Europe. Moreover, there is limited evidence for injuries other than those due to vehicle accidents. We exploit the introduction and changes in the MLDA in Denmark to estimate effects on all classes of injuries, as well as alcohol-related outcomes such as intoxication and poisoning. We bring comprehensive evidence on the effects of a total of three reforms, which affected alcohol availability along different margins – 1) establishing an off-premise alcohol purchase age of 15 (1998), 2) raising the off-premise alcohol purchase age to 16 (2004), and 3) increasing the purchase age of beverages exceeding 16.5% in alcohol content from 16 to 18 (2011). Our findings show significant impacts of all the three reforms on injuries. We find that girls responded more to two first two reforms influencing alcohol availability, whereas boys responded more to the last reform, influencing availability of strong liquor. On the other hand, no consistent differences were found across different socioeconomic groups, perhaps reflecting similar patterns of drinking.

JEL Classification: H00, I00, I12

Keywords: alcohol, minimum legal drinking ages, Difference-in-Differences, Denmark

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Introduction

Alcohol is considered one of the most serious threats to population health. In terms of disability-adjusted life years (DALYs), it has consistently been ranked as one of the top risk factors in both the West and in the world as a whole, with effects comparable to those of high blood pressure, tobacco, and child malnutrition [1-2]. Among youths and young adults, alcohol has even been ranked as number one [1]. Alcohol consumption in young ages is believed to be particularly damaging because of the development of the brain and other organs, and because youth have a heightened propensity for risk-taking [3]. Hence, access to alcohol for youth is restricted throughout the world. Most countries have, for example, implemented Minimum Legal Drinking Age (MLDA) legislations, prohibiting individuals below a certain age to purchase and/or consume alcohol. It is a crucial policy concern whether these legislations can reduce youth alcohol consumption and associated harms. However, evidence on associated harms is still limited as previous studies are only based on the US and a few other contexts, and since only few studies have considered injuries in general, rather than motor vehicle accidents.

In this paper, we bring evidence on the effects of changes of legislation limiting youth alcohol access in Denmark. Our setting, which has not been studied in previous papers, is a rather interesting one because of its liberal alcohol culture with high incidence of drinking at early ages.[1] These features coupled with the availability of rich registry data on patient records enables us to reliably identify the effects of curtailing early drinking, focusing on injuries and related outcomes. We exploit a series of reforms, which influenced access to alcohol at different age margins and in terms of different alcohol content. Specifically, we exploit both the introduction and two changes in the MLDAs, using a difference-in-differences setup where outcomes are compared over time as well as across age groups. Since the age margins of study are lower than those examined by previous literature, they provide interesting clues as to what can happen if alcohol availability is manipulated at ages below 18.

Much evidence on the effects of MLDAs has been obtained from the US, but also Canada and more recently New Zealand. However, there is virtually no evidence from Europe (for a review, see [5]). In the US, after Prohibition ended in 1933, MLDAs of 21 were implemented by almost all states. In the 1970s, however, along with reductions in the voting age, many states lowered these minimum ages to 18, 19, or 20. A number of studies exploit those lowerings as natural experiments and look at alcohol sales, consumption (e.g., [6-8]), or traffic crashes (e.g., [9-

[1] There is previous evidence on effects of alcohol policy on Denmark (e.g., [4]), but there has been no investigation of the impacts of MLDAs on health-related outcomes.

12]). Typically, these studies report positive effects. Following these findings and after pressure from the federal government, policies began to reverse so that, by 1988, all states in the US had again introduced an MLDA of 21. Studies based on these changes to stricter legislation provide evidence of reductions in consumption, sales, and traffic crashes (e.g., [13-16]).

Using survey data more comprehensive than what most of the studies in the literature had used, Dee [17] finds that estimated effects of higher MLDAs depend heavily on whether state-fixed effects are controlled for, however. Ruhm [18], using state-level data over the time period 1982-1988, finds that the effects of increases in MLDAs on traffic crashes even become insignificant when accounting for other changes in legislations that influenced highway fatalities. Uncertainty thus remains as to whether these legislations have actually had an impact.

Several studies have exploited a recent lowering of the MLDA in New Zealand from 20 to 18. For example, Gruenewald et al. [19] find substantial impacts on drinking, Kypri et al. [20] document effects on traffic crashes, and Conover and Scrimgeur [21] document effects on alcohol-related hospitalizations. Some of these studies were questioned by Boes and Stillmand [22], who find significant effects on alcohol-related hospitalizations but not on traffic crashes or even on average alcohol consumption when accounting for time effects. Further, Marcus and Siedler [23] study a policy introduced in Southern Germany, which banned off-premise sales of alcoholic beverages between 10 pm and 5 am. Although this reform was not specifically targeted at young individuals, the authors find effects on alcohol-related hospitalizations only among adolescents and young adults.

Other studies do not exploit policy changes but instead compare outcomes below and above the MLDA threshold, looking at mortality [24], hospitalizations due to injuries and other causes [25], traffic crashes [26-27], or risky sex or sexually transmitted disease [28-29]. Except for the last of these classes of outcomes, results generally suggest adverse effects when individuals reached the age of eligibility. One limitation of these studies is that individuals may become eligible for more than just buying alcohol at a certain age (e.g., individuals aged 21 and above can buy handguns in parts of the US). Furthermore, effects are only estimated at a certain age threshold, and behaviors around this threshold may not be representative of behaviors at other ages. For alcohol consumption, there may be an adjustment process so that consumption is extraordinarily high for a limited period of time after the individual is given the permission to drink – or, alternatively, that it takes some time for effects to set in if individuals need to form a habit of drinking.

Method

Setting

Denmark is known for having a liberal alcohol culture – especially so among youth. Danish parents put few restrictions on their children's alcohol intake, and traditionally alcohol consumption is seen as acceptable after the confirmation at age 14 or 15. Not only is consumption widespread, but youth also drink large amounts [30-32].

The European School Survey Project on Alcohol and Other Drugs (ESPAD) has surveyed 15-16-year-olds on their alcohol consumption since the 1990s, and it has consistently reported Denmark to be one of the European countries with the highest consumption. In 1995, for example, Danish youth ranked clearly highest in terms of "lifetime use of any alcoholic beverage 40 times or more" and "use of any alcoholic beverage 20 times or more during the last 12 months" [30]. In 2015, the same survey asked individuals about "any lifetime use," "30-day use," and "intoxication in the last 30 days." Denmark was number one in all but the first measure, and no country comes close in terms of intoxication; 32% of Danish youth have reportedly been intoxicated in the last month, as compared to 13% in Europe on average and 10% in the US [32].

In 1997, there was an intense public debate in Denmark about youth alcohol consumption. This was spurred by a fear that the introduction of so-called alcopops (a mixture of alcohol and soda) would increase consumption levels, but the debate was also influenced by the newly published results from the previously mentioned ESPAD. In Denmark at this time, age limits did not exist at off-premises (grocery stores, kiosks, gas stations etc.), although there was an age limit of 18 at on-premises such as restaurants and bars. Following the debate, an off-premise 15-year age limit for purchasing any beverage with an alcoholic content of 1.2% or more was decided to be implemented by *July 1, 1998*. Specifically, the law prohibited sales of alcohol (shops breaking the law become subject to fines) and although drinking itself was not prohibited we refer to the law as a Minimum Legal Drinking Age (MLDA) for convenience. The fines amounted to DKK 1,000–2,000 (ca USD 140–280) during the initial period and were then raised to DKK 5,000–10,000 in 2004, depending on the number of instances of illegal sales.

In November 1997, the National Board of Health conducted a survey to investigate alcohol consumption among Danish youth in grades 5–10 (those aged roughly 12–17). This survey was then repeated in the same calendar month in subsequent years. Results, reported in the correlational study by Møller [33], suggest a general trend downwards in alcohol consumption in the years following the reform, but consumption dropped especially among individuals below 15. Between 1997 and 2001, the probability of drinking alcohol in the last month decreased by

36% among those in grades 5 to 7 (12 to 14 years of age), as compared to a reduction by 17% among those in grades 8 to 10. Still, after the reform, a significant portion of individuals below age 15 were able to access alcohol, either through parents, friends, or other sources, or because the law was being ignored by stores.

Concerns about youth alcohol consumption remained, and on *July 1, 2004*, the age limit was increased from 15 to 16. Results of evaluations by the Danish National Center for Alcohol Research [34] suggest clear impacts on the ability of 15-year-olds to purchase alcohol in stores, with a probability of having bought alcohol in a store during the past month dropping by about almost half, from an initial level of more than 40%. However, consumption itself was less affected, possibly with a reduction in drinking over the past month by 7% (5 percentage points) when compared to other age groups. Similar conclusions can be drawn from surveys administered by the National Health Board [31].

Further raising the general age limit for buying alcohol was not perceived as realistic; however, from *March 7, 2011*, another limit was put into place, implying that an individual had to be at least 18 years of age, rather than 16, in order to purchase beverages with an alcohol content of 16.5% or more. With this decision, policy makers wanted to send a signal that spirits consumption among youth is not acceptable, and high schools were urged to revise their alcohol policies with this in mind [35]. While there appears to be no evaluation of the effects of this reform on the specific age groups affected, alcohol consumption and problem drinking has decreased in the broader age group of 16-24-year-olds between 2010 and 2013 [36].

Summarizing the three policy changes discussed above, Figure 1 illustrates alcohol policy in Denmark over the time period of study.

< Figure 1 about here >

Data

Our study exploits information from multiple national registers held by Statistics Denmark. We utilize the Population Register, which has information on all individuals in the country. Information includes the date of birth and the sex of the person, as well as a personal identifier, which allows us to link data from other sources. Our analyses exploit individuals between 12 and 20 years of age.

We make use of the Patient Register, which contains data on all hospital visits in the country between 1995 and 2013. There is information on the date of visit, including both the start and end date of visit if the visit lasted multiple days. There is also information on diagnoses assigned at the visit, coded according to the ICD-10 classification scheme. Both in- and outpatient visits are recorded. Our main outcome, which we will refer to as "injuries" essentially corresponds to the broad class of "injuries and poisonings" (S-T in the ICD-10 classification). However, we remove "alcohol poisonings" (T51) from this group and instead place it in a different category, which we refer to as "alcohol-related conditions." In addition, we let this group include those consequences of alcohol that are classified among mental conditions (F10); these are essentially alcohol intoxications with behavioral rather than physical effects. In principle, it also includes alcohol dependence syndrome and similar diagnoses, but these are rare among youth. Finally as a third outcome, we exploit the Death Register to measure consequences of alcohol in terms of mortality.

Family members can be linked, and through the Taxation and Education registers we obtain information on parents' highest educational attainment. Indicators for completed secondary and tertiary education are created.

In some cases, an individual has two or more hospital visits, which overlap in time. This could happen because the individual was admitted to several departments or for other administrative reasons. In our analysis, we combine overlapping visits (of the type/diagnosis we are interested in), and only count them as one.

Descriptive statistics are shown in Table 1. Descriptives are split according to time period, with intervals determined by the reforms. As can be seen, numbers tend to reduce somewhat over time. Injuries is by far the most common health event, occurring in 250-300 individuals per 1000 each year. In contrast, only 4–5 alcohol poisonings or intoxications occur among 1000 individuals, and only around 0.2–0.3 cases of death. We thus expect lower power to detect effects on those outcomes and our main focus will therefore lie on the injury category.

< Table 1 about here >

Empirical model

We estimate equations where the outcome is measured at the same time as alcohol access. The analysis exploits differences over time and across age groups. Specifically, we estimate the following difference-in-difference (DiD) regression.

(1) $$y_{it} = \alpha + \beta Prohibited_{it} + A_{it} + T_t + \varepsilon_{it}$$

In this equation, y is the number of events of a certain type (in the main analysis, hospitalizations due to "injuries") for individual i in year t. α is the constant term, $Prohibited_{it}$ is the treatment indicator, assuming the value 1 if the individual was prohibited from buying alcohol at off-premises during the entire year and 0 if the individual was allowed to buy alcohol during the entire year. It may also assume a value between 0 and 1 if the individual was prohibited from buying alcohol during some part of the year; it then reflects this share of a year. (We also run a sensitivity check where the treatment indicator is set to one if an individual was treated for at least some part of the year, and alternatively, as zero if an individual was not treated for the entire year.) A_{it} is a vector of age fixed effects, indicating the individual's age in years at the end of the year, T_t is a vector of time fixed effects, also expressed in years and ε_{it} is the idiosyncratic error term varying between individuals and over time. β is the treatment effect of interest. We make use of all available years (1995–2013) without exploiting more than one reform in the same regression, implying that we use 1995–2003, 1999–2010, and 2005–2013 for the different reforms, respectively. The estimations include up to nine observations per individual (if all the individual's ages between 12 and 20 fall within the year interval of interest) and we cluster standard errors at the individual level.

In a DiD setup, it is important to make treatment and control groups as similar to each other as possible. In our case, treatment and control groups are represented by different ages, and treatment varies depending on time. We take two alternative strategies to achieve comparability of treatment and control groups. First, we add age-group-specific linear or quadratic time trends, where "age group" refers to the specific age in years at the end of the year. This accounts for any changes over time that develop according to these polynomials. Second, we focus on narrower ranges of years, so that there is little time for differences across treatment and control groups to develop. In particular, we restrict attention to the reform year plus two full years before and two full years after (i.e., five years around the reform), or to the reform year plus one full year before and one full year after (i.e., three years around the reform). We also explicitly test for the presence of pre-treatment trends by estimating regressions where belonging to the treatment group (e.g., being less than 15 years of age) is interacted with a linear time trend in pre-treatment periods.

Results

Graphical evidence of reform impacts

Before turning to our econometric analysis of potential consequences of alcohol access, we display the patterns of injuries around the three different reforms; see Figure 2. The average number of events per year are reported by age group during four different time intervals: 1995–1997 (before reform 1), 1999–2003 (between reform 1 and reform 2), 2005–2010 (between reform 2 and reform 3), and 2011–2013 (after reform 3). Since outcomes are measured at the calendar year level, the age of the individual always assumes two different values within an observation. For this reason, we plot outcomes in the middle of two tick marks (e.g., observations where individuals are 14 for some part of the year and 15 for some part of the year are plotted in between 14 and 15).

Overall and in line with our descriptive statistics previously shown, there is a declining trend in injuries over time. After the first reform, in 1998, injuries dropped in all age groups, but clearly more so among those below age 15, indicating an impact of the reform. Indeed, the drop is substantial both among those below 15 during the entire year (those to the left of “14”) and those who were 15 for some part of the year (those in between “14” and “15”). Our graphical results for the first reform appear consistent with Møller [33], who – as previously discussed – established reduced drinking after the implementation of the first reform, especially among children in grades 5–7.

< Figure 2 about here >

After the second reform, 15-year-olds were prohibited from buying alcohol. The figure shows substantial drops in injuries among those aged 15 for some part of the year (14-15-year-olds and 15-16-year-olds). However, there are also substantial drops in several other age groups, such as among those aged 16–17. Potentially, this could reflect spill-overs or peer effects due to the reform, or alternatively some trends or other efforts to combat youth drinking.

Reform 3 is expected to have influenced 16- and 17-year-olds, who became ineligible to buy spirits. Indeed, the figure shows substantial drops in injuries in these age groups (those between 16 and 18). At the same time, there are substantial drops in several other age groups. In particular, those above age 18 face large drops in their injuries, perhaps reflecting underlying trends. As noted, our empirical specifications will attempt to account for such underlying trends that may influence the outcomes.

Similar to Figure 2, Figures A1 and A2 display alcohol-related conditions and mortality in different age groups over time. While these events are much rarer than injuries, there is some evidence that the incidence of alcohol-related conditions was affected by the reforms. Around the first reform, the number of alcohol-related conditions were more or less unchanged among individuals below 15. Among older individuals, however, there was a clear tendency towards an increase, suggesting an impact of the reform that counteracts an underlying trend. After the second reform, incidences dropped among those aged 15 (those between “14” and “16” in the figure), in line with our expectations. There was also the expected drop after the third reform, although an even larger drop may be seen among 14-15-year-olds. While not entirely clear, the patterns for mortality are also partly in line with our expectations, with drops among those below 15 after the first reform, those aged 15 after the second reform and those aged 16–17 after the third reform.

Main results

We then turn to the regression results, where Table 2 reports the main results for injuries. Throughout, we use outcomes multiplied by 1000, so that estimates can be interpreted as yearly effects per 1000 individuals. In Table 2, column (1) reports results based on the specification in equation (1), where no trends are included. To account for potential trends operating over the time period of interest, we then add linear age-specific time trends in column (2). In column (3), we add quadratic age-specific time trends. In columns (4) and (5), attention is restricted to fewer years around the reforms, another way of mitigating the potential influence of non-parallel trends, since given that such trends are continuous, they will have less impact over a shorter time period.

< Table 2 about here >

Our first specification suggests an effect of -14 injuries per year and thousand individuals when alcohol cannot legally be purchased, an effect that is significant at the 1% level. In relative terms, it corresponds to roughly 5% of the average number of injuries in any age group. The effect is somewhat smaller than the drop shown among those below age 15 in Figure 2, a result that can be explained by a drop in the control groups as well.

In column (2) of Table 2, we added age-specific linear trends to the specification in column (1). As can be seen, the estimate becomes somewhat larger as compared to column (1), where non-parallel trends were not taken into account. Moreover, it increases even further, to about -20 when quadratic trends are used. Indeed, our estimates from simpler models do not appear to be artefacts of underlying trends, but if anything, the trends are working in the direction opposite to the reform. Restricting attention to fewer years around the reform reinforces this conclusion; we find estimates of -19 both in a model including five years around the reform and in a model including three years. In sum, the estimate is quite stable across specifications and suggests a 5–7% reduction in injuries when purchases of alcohol became illegal.

In panel B, we provide the corresponding results pertaining to the second reform. In a model with no trends, the estimate is similar to what we obtained for reform 1. Adding trends yields estimates that are largely similar but smaller. In a model with linear trends it equals -11 and with quadratic trends it equals -10, corresponding to 3-4% of the baseline effect among 15-year-olds. A similar estimate arises when restricting attention to five years around the reform, but it reduces to -7 and becomes statistically insignificant when restriction attention to only three years. However, the precision is lower in this case.

Panel C displays the results based on reform 3. Column 1 suggests an impact of -17. As for reform 2, the estimate in column 1 is reduced when accounting for trends, but a relatively large and strongly significant effect remains, amounting to a negative of 12. In relative terms, the results suggest a reduction in injuries of about 3% when spirits purchase became illegal for 16-17-year olds.

We consider effects on alcohol-related conditions in Appendix Table A1. As for injuries, our first specification suggests that alcohol availability is related to more harmful outcomes, with an estimate that is significant at the 1% level. The estimate suggests that being prohibited from buying alcohol reduces alcohol-related conditions by 1.2 per year and 1,000 individuals, an effect that corresponds to 70% of the average of 1.8 in ages 12–14. However, the estimate becomes smaller and statistically insignificant when accounting for trends, either with a quadratic birth-year-specific trend, or by restricting attention to three years around the reform. For reform 2 or 3, there is no evidence of an effect on alcohol-related conditions.

Appendix Table A2 displays results for mortality. There is little evidence of impacts on this outcome. All coefficients but two are statistically insignificant and provide some evidence that mortality was reduced. However, mortality is very low in these ages.

Robustness

One of the key assumptions in a DiD framework is that treatment and control groups exhibit similar trends. In our analysis, some specifications accounted for the possibility of trends and suggested that results were relatively unaffected. To further shed light on whether any trends are present, we add pre-reform treatment-group-specific trends and replicate the results in columns (1) of Tables 2, A1, and A2. We report the estimates for the trends and examine their size and statistical significance. If the trends turn out significant, this suggests that outcomes of treatment and control groups do not develop in parallel. We use linear trends and focus on pre-reform trends rather than post-reform trends since the latter could potentially be influenced by treatment. Belonging to the treatment group is defined based on age at the end of the year.

The results with pre-treatment trends are shown in Appendix Table A3. As can be seen, the pre-trends are never significantly different from zero. The estimate, however, is quite large for the first reform and injuries, pointing at an increase in injuries in the treatment group over time. This adverse development in the treatment group could lead to underestimates of the beneficial impacts of the reform, especially if a similar trend were to continue into the post-treatment period. Indeed, the coefficient on the treatment indicator becomes somewhat larger when trends are accounted for. On the whole, however, results are unaffected.

In order to examine the sensitivity of our main results with respect to the definition of the treatment indicator, Appendix Tables A4 and A5 present results using treatment indicators that are always defined as either zero or one; in Table A4 the treatment indicator assumes the value of one if the individual is treated for at least some part of the year and in Table A5 it assumes the value of zero if the individual is treated only for some part of the year. Results point in the same direction as before but are smaller and estimated with less precision. This makes sense as we introduce measurement error and lends credibility to the interpretation that our results are driven by effects of alcohol availability rather than some underlying trends that may operate at the age level.

Heterogeneity

We next examine whether effects of alcohol availability on injuries differ by family SES or by gender. In this way, we shed light on the distributional effects of restrictions on alcohol availability, and in particular whether MLDAs could improve outcomes among the socioeconomically disadvantaged. There may be several reasons why differential effects could emerge, such as differences in resources or attitudes towards alcohol. Notably, however, alcohol consumption is rather independent of SES in Denmark, and there are no clear differences across education groups [37].

Alcohol consumption has also been rather similar across genders in Denmark and the responses to the first and second reforms on alcohol consumption appear similar for boys and girls as well [33-34]. Boys, however, are known to be more-risk taking than girls in general (e.g., [38-39]), which could potentially lead to a larger effect on outcomes like injuries, although it is also possible that boys are more risk-taking regardless of alcohol, so that there might be more "room for adjustment" among girls, as these do not take so much risks in the absence of alcohol.

We first consider effects by parental education in Table 3. In general, there is no evidence that effects would differ across socioeconomic groups, as measured by education. No consistent patterns can be seen, and according to t-tests for differences in coefficient sizes, estimates are all statistically indistinguishable. For example, estimates suggest that the responses to the first reform are largest when parents are low-educated, but that the responses to the last reform are largest when parents are high-educated, but, again, these differences are not statistically significant.

< Table 3 about here >

In Table 4, we then look at effects by gender. We obtain evidence of large differences across males and females, with the injuries of females responding three times as much to the first and second reform. However, males respond more to the reform pertaining to spirits, with a more than three times as large response as that among women. This could reflect gender differences in the attitudes towards different types of alcohol. Differences are strongly significant.

< Table 4 about here >

Discussion

Our results show substantial reductions in injuries when the minimum purchase age of 15 was introduced, with a relative effect of around 6%. Relating this to [33], these results suggest that injuries drop by about a third of a percent per percent reduction in monthly alcohol consumption. Our results also suggest impacts of the second and third reforms, albeit with somewhat smaller effects – perhaps reductions in injuries by about 4%. Again, a very simple comparison with survey evidence on the impacts of alcohol consumption around reform 2 suggests reductions in injuries by about half a percent per percent reduction in consumption during a month. Thus, the smaller effects on injuries of reform 2 compared to reform 1 seem to be due to a smaller effect on consumption. One explanation for this may be the Danish drinking culture, where alcohol consumption from age 15 is generally accepted and where parents may provide their children with alcohol. Youths also enter high school at age 15 or 16, and may be more able to obtain alcohol through peers there, even if not legally allowed to buy at stores.

Our results do not appear to be driven by trends. Effects are relatively unchanged when controlling for trends and we find no statistical evidence of differential pre-reform trends across treatment and control groups. We acknowledge, however, that trends can come in many forms and may not necessarily be picked up by low-order polynomials. Given our graphical evidence, the most convincing result appear to be the one for the first reform and injuries, whereas other results, albeit significant in the case of injuries, are less clear.

In general, we find no clear evidence on impacts on alcohol-related conditions (alcohol poisonings and intoxications) or on mortality. However, these results are much rarer and power is lower. The effects of the reforms that we found for injuries are similar but larger than those of Callaghan [25] et al., who find a 3% increase in a broad class of injuries when individuals became old enough to buy alcohol. However, Callaghan also report large and significant effects on alcohol-related conditions. Their study has higher power to detect outcomes of the latter type, but is also based on a different context with a different alcohol culture. Moreover, since it only considers outcomes close to the MLDA threshold, it may reflect more temporary processes. Another related paper is Marcus and Siedler [23], who exploit a German reform that prohibited alcohol purchases only during certain hours. Their estimate suggests an effect of around 7% on alcohol-related conditions among those aged 15-19 years. In our data, we cannot rule out effects of these magnitudes on alcohol-related conditions. In any case, our results provide no evidence that MLDAs would be ineffective, despite the fact that compliance with the legislation was far from perfect and many Danish kids obtain alcohol also when not legally allowed to. Therefore, they may provide lower bounds on the effects that may occurred when MLDAs have been changed or implemented elsewhere.

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Table 1: Descriptive statistics

Outcome	1995–1998	1998–2004	2004–2011	2011–2013
Injuries	294.900	284.482	267.485	245.232
Alcohol-related	4.259	4.717	4.717	3.869
Mortality	0.346	0.299	0.235	0.174
Observations	1,430,536	2,122,246	3,629,339	1,272,702

Note: The data include individuals whenever their age falls between 12 and 20 within the time interval of interest. Injuries includes all injuries and poisonings except alcohol poisonings, and alcohol-related conditions include alcohol intoxication and poisoning. All outcomes are measured per person and year (i.e., the unit is person-year) and are multiplied by 1000.

Table 2: Effects on injuries (yearly per 1,000 individuals)

	(1)	(2)	(3)	(4)	(5)
Panel A: Reform 1 (1998)					
	1995–2003	1995–2003	1995–2003	1996–2000	1997–1999
Prohibited	-14.492***	-17.274***	-20.331***	-19.136***	-19.131***
	(2.185)	(3.578)	(3.866)	(2.817)	(3.574)
Observations	3,203,017	3,203,017	3,203,017	1,755,468	1,049,543
Panel B: Reform 2 (2004)					
	1999–2010	1999–2010	1999–2010	2002–2006	2003–2005
Prohibited	-14.703***	-10.968***	-10.314**	-10.482*	-7.366
	(2.482)	(3.302)	(3.328)	(4.244)	(5.781)
Observations	4,609,484	4,609,484	4,609,484	1,895,459	1,136,850
Panel C: Reform 3 (2011)					
	2005–2013	2005–2013	2005–2013	2009–2013	2010–2012
Prohibited	-17.347***	-12.053***	-12.049***	-13.082***	-12.619***
	(2.223)	(3.223)	(3.488)	(3.059)	(3.973)
Observations	3,730,196	3,730,196	3,730,196	2,117,456	1,273,314
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Regressions are estimated according to Equation 1, in some cases with linear or quadratic age-specific trends added.

Table 3: Effects on injuries by parental education (yearly per 1,000 individuals)

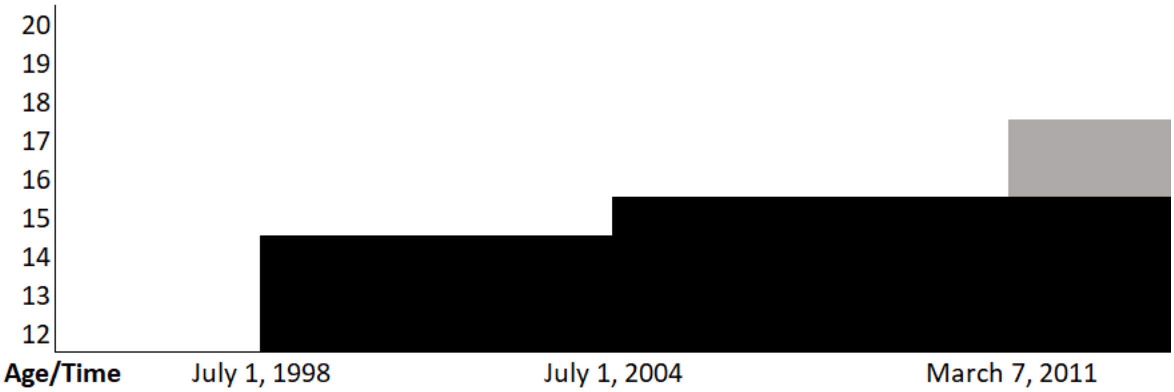
	Father primary	Father secondary	Father tertiary	Mother primary	Mother secondary	Mother tertiary
Panel A: Reform 1						
Prohibited	-17.144***	-13.407***	-15.401***	-20.668***	-12.649***	-11.649**
	(4.400)	(0.201)	(4.414)	(3.879)	(3.279)	(4.245)
Observations	742,964	1,422,819	721,194	941,417	1,367,208	789,060
Panel B: Reform 2						
Prohibited	-11.952*	-14.700***	-14.459**	-14.374**	-11.357**	-20.883***
	(5.371)	(3.713)	(5.276)	(4.653)	(3.829)	(5.057)
Observations	1,068,041	2,050,731	1,037,664	1,356,333	1,964,547	1,136,825
Panel C: Reform 3						
Prohibited	-15.513***	-17.082***	-20.682***	-15.671***	-16.934***	-20.852***
	(4.521)	(3.413)	(4.488)	(4.017)	(3.474)	(4.356)
Observations	862,394	1,660,985	840,391	1,097,987	1,589,357	920,370

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation (1).

Table 4: Effects on injuries by gender (yearly per 1,000 individuals)

	Male	Female
Panel A: Reform 1		
Prohibited	-7.321*	-22.177***
	(3.144)	(3.006)
Observations	1,637,723	1,565,294
Panel B: Reform 2		
Prohibited	-8.493*	-21.068***
	(3.627)	(3.361)
Observations	2,357,966	2,251,518
Panel C: Reform 3		
Prohibited	-26.674***	-8.039
	(3.106)	(3.176)
Observations	1,909,698	1,820,498

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation (1).



■ = prohibited from buying alcoholic beverages in general; ■ = prohibited from buying spirits.

Figure 1: Illustration of the reforms.

Figure 2: Injuries per 1,000 persons and year in different policy periods. “Before reform 1” represents 1995-1997, “between reform 1 and 2” 1999–2002, “between reform 2 and 3” 2005–2010, and “after reform 3” 2012–2013.

Appendix

Table A1: Effects on alcohol poisonings and intoxications (yearly per 1,000 individuals)

	(1)	(2)	(3)	(4)	(5)
Panel A: Reform 1 (1998)					
	1995–2003	1995–2003	1995–2003	1996–2000	1997–1999
Prohibited	-1.236***	-0.302	-0.127	-0.956***	-0.146
	(0.191)	(0.370)	(0.415)	(0.265)	(0.352)
Observations	3,203,017	3,203,017	3,203,017	1,755,468	1,049,543
Panel B: Reform 2 (2004)					
	1999–2010	1999–2010	1999–2010	2002–2006	2003–2005
Prohibited	-0.432	0.766	0.763	0.335	0.426
	(0.309)	(0.411)	(0.414)	(0.502)	(0.661)
Observations	4,609,484	4,609,484	4,609,484	1,895,459	1,136,850
Panel C: Reform 3 (2011)					
	2005–2013	2005–2013	2005–2013	2009–2013	2010–2012
Prohibited	-0.257	-0.086	-0.139	-0.311	-0.645
	(0.257)	(0.378)	(0.427)	(0.335)	(0.456)
Observations	3,730,196	3,730,196	3,730,196	2,117,456	1,273,314
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation 1, in some cases with linear or quadratic age-specific trends added.

Table A2: Effects on mortality (yearly per 1,000 individuals)

	(1)	(2)	(3)	(4)	(5)
Panel A: Reform 1 (1998)					
	1995–2003	1995–2003	1995–2003	1996–2000	1997–1999
Prohibited	0.045	0.035	0.021	-0.009	0.075
	(0.041)	(0.074)	(0.083)	(0.059)	(0.082)
Observations	3,203,017	3,203,017	3,203,017	1,755,468	1,049,543
Panel B: Reform 2 (2004)					
	1999–2010	1999–2010	1999–2010	2002–2006	2003–2005
Prohibited	-0.032	-0.096	-0.103	-0.159*	-0.187*
	(0.049)	(0.064)	(0.065)	(0.073)	(0.092)
Observations	4,609,484	4,609,484	4,609,484	1,895,459	1,136,850
Panel C: Reform 3 (2011)					
	2005–2013	2005–2013	2005–2013	2009–2013	2010–2012
Prohibited	-0.102*	-0.027	-0.073	-0.056	-0.003
	(0.040)	(0.059)	(0.068)	(0.049)	(0.059)
Observations	3,730,196	3,730,196	3,730,196	2,117,456	1,273,314
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation 1, in some cases with linear or quadratic age-specific trends added.

Table A3: Including treatment-group-specific pre-treatment trends (yearly per 1,000 individuals)

	Injuries	Alcohol-related conditions	Mortality
Panel A: Reform 1 (1998)			
	1995–2003	1995–2003	1995–2003
Prohibited	-17.925***	-1.177***	0.010
	(2.961)	(0.305)	(0.063)
Pre-trend	2.025	-0.034	0.021
	(1.231)	(0.122)	(0.027)
Observations	3,203,017	3,203,017	3,203,017
Panel B: Reform 2 (2004)			
	1999–2010	1999–2010	1999–2010
Prohibited	-15.797***	-0.153	-0.039
	(2.675)	(0.345)	(0.054)
Pre-trend	0.641	-0.164	0.004
	(0.664)	(0.087)	(0.014)
Observations	4,609,484	4,609,484	4,609,484
Panel C: Reform 3 (2011)			
	2005–2013	2005–2013	2005–2013
Prohibited	-16.977***	0.144	-0.093*
	(2.569)	(0.314)	(0.047)
Pre-trend	-0.133	-0.144*	-0.003
	(0.492)	(0.063)	(0.010)
Observations	3,730,196	3,730,196	3,730,196

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation 1 but with an added triple interaction between belonging to the treatment group, pre-reform period, and year relative to the reform year. The triple interaction coefficient is being reported as "pre-trend". An individual is defined as "belonging to the treatment group" if being less than 15 years of age for reform 1, being 15 years for reform 2, and being 16-17 years of age for reform 3 (ages defined at the end of the year).

Table A4: Effects on injuries (yearly per 1,000 individuals), with treatment defined as 1 if the individual was treated for some part of the year

	(1)	(2)	(3)	(4)	(5)
Panel A: Reform 1 (1998)					
	1995–2003	1995–2003	1995–2003	1996–2000	1997–1999
Prohibited	-11.051***	-9.480**	-13.701***	-12.994***	-12.516***
	(2.011)	(3.117)	(3.531)	(2.392)	(2.851)
Observations	3,203,017	3,203,017	3,203,017	1,755,468	1,049,543
Panel B: Reform 2 (2004)					
	1999–2010	1999–2010	1999–2010	2002–2006	2003–2005
Prohibited	-11.313***	-4.305	-4.537	5.703	-4.044
	(2.084)	(4.118)	(4.251)	(3.333)	(4.257)
Observations	4,609,484	4,609,484	4,609,484	1,895,459	1,136,850
Panel C: Reform 3 (2011)					
	2005–2013	2005–2013	2005–2013	2009–2013	2010–2012
Prohibited	-14.812***	-8.362*	-7.011	-11.201***	-10.288**
	(1.964)	(3.468)	(4.270)	(2.590)	(3.297)
Observations	3,730,196	3,730,196	3,730,196	2,117,456	1,273,314
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation 1, in some cases with linear or quadratic age-specific trends added.

Table A5: Short-term effects on injuries (yearly per 1,000 individuals), with treatment defined as 0 if the individual was treated only for some part of the year

	(1)	(2)	(3)	(4)	(5)
Panel A: Reform 1 (1998)					
	1995–2003	1995–2003	1995–2003	1996–2000	1997–1999
Prohibited	-13.125***	-16.917***	-16.696***	-16.417***	-14.582***
	(1.993)	(3.492)	(3.596)	(2.517)	(3.064)
Observations	3,203,017	3,203,017	3,203,017	1,755,468	1,049,543
Panel B: Reform 2 (2004)					
	1999–2010	1999–2010	1999–2010	2002–2006	2003–2005
Prohibited	-3.769	-5.380	-3.884	-5.180	-4.742
	(2.031)	(4.024)	(4.148)	(3.209)	(4.111)
Observations	4,609,484	4,609,484	4,609,484	1,895,459	1,136,850
Panel C: Reform 3 (2011)					
	2005–2013	2005–2013	2005–2013	2009–2013	2010–2012
Prohibited	-8.350***	-3.387	-1.281	-6.415*	-6.103
	(2.156)	(3.463)	(4.693)	(2.678)	(3.435)
Observations	3,730,196	3,730,196	3,730,196	2,117,456	1,273,314
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$
Regressions are estimated according to Equation 1, in some cases with linear or quadratic age-specific trends added.

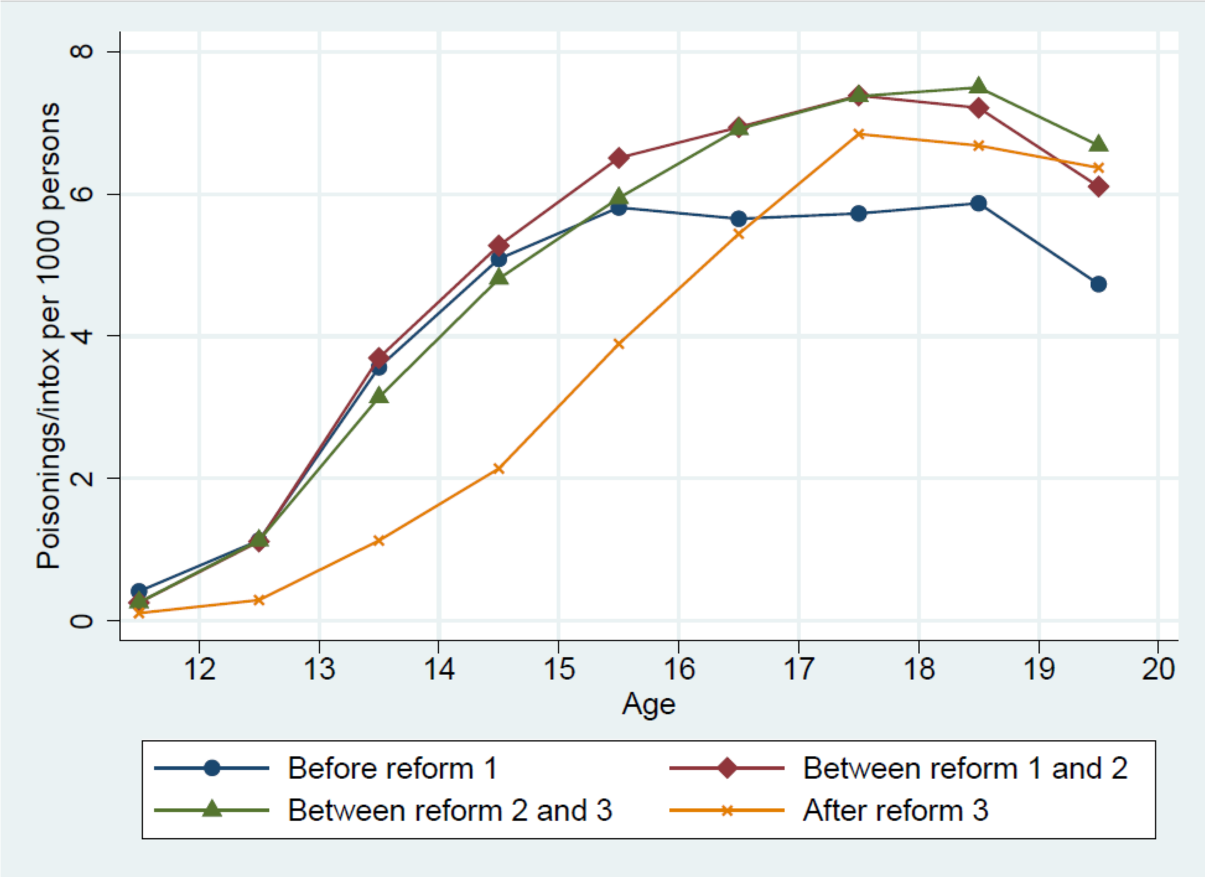


Figure A1: Alcohol poisonings and intoxications per 1,000 persons and year in different policy periods. “Before reform 1” represents 1995–1997, “between reform 1 and 2” 1999–2002, “between reform 2 and 3” 2005–2010, and “after reform 3” 2012–2013.

Figure A2: Deaths per 1,000 persons and year in different policy periods. "Before reform 1" represents 1995–1997, "between reform 1 and 2" 1999–2002, "between reform 2 and 3" 2005–2010, and "after reform 3" 2012–2013.