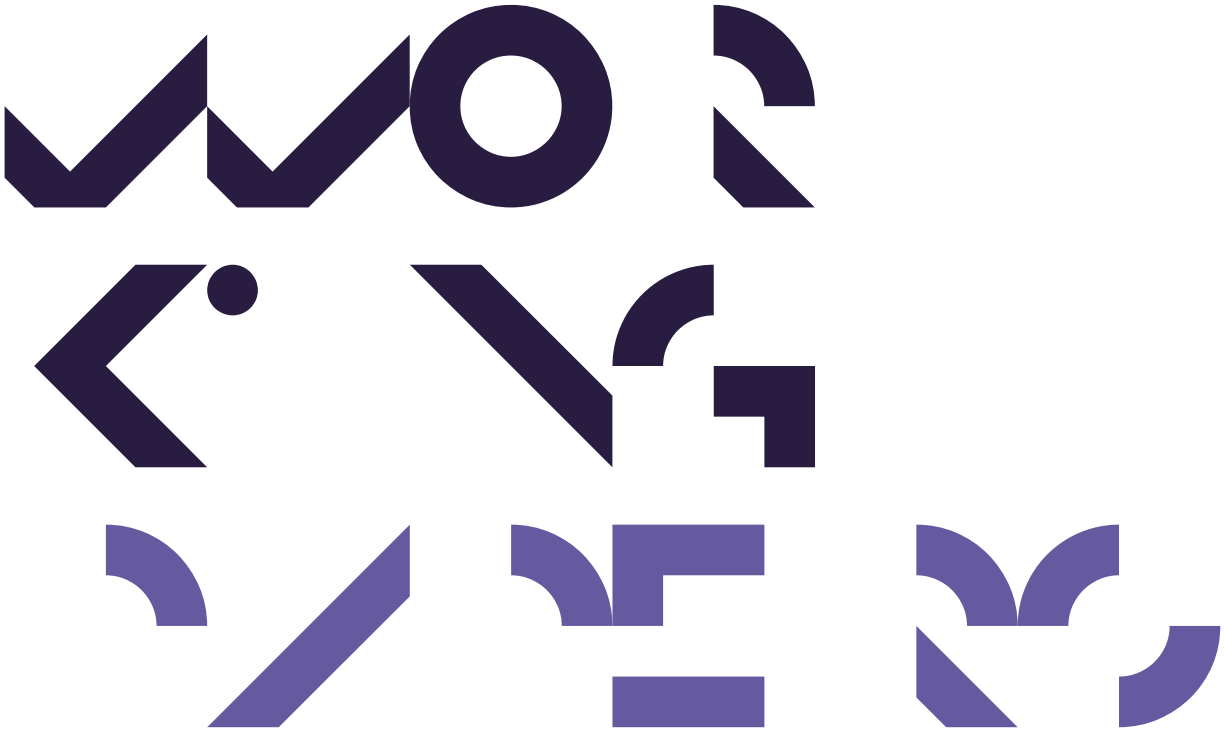

Economics Working Papers

2017-3

Short- and Long-Term Effects of Adolescent Alcohol Access: Evidence from Denmark

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Short- and Long-Term Effects of Adolescent Alcohol Access: Evidence from Denmark

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April 2018

Abstract

We exploit changes in minimum legal alcohol purchasing ages in Denmark to estimate effects on health, as well as on human capital formation. In contrast to previous literature on minimum legal drinking ages, we do not only consider outcomes in the short run, but also several years down the road. Employing a difference-in-differences approach, we bring comprehensive evidence on the effects 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 on injuries in both the short and long run. Effects on rarer outcomes (alcohol poisonings and intoxications, and mortality) are mostly insignificant, and there is no clear evidence that educational attainment would be affected.

Keywords: minimum legal drinking ages; injuries; alcohol-related conditions; difference-in-differences

Acknowledgements: Anton Nilsson acknowledges support from Handelsbanken's Research Foundation (grant number W2014-0468:1). We thank Abdu Kedir Seid, who was also a coauthor of a previous version of the paper.

1. Introduction

Alcohol is 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 (Lim et al. 2012; GBD 2016). Among youths and young adults, alcohol has even been ranked as number one (Lim et al. 2012). 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 (Jetha and Segalowitz 2012). 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 have the ability to reduce youth alcohol consumption and associated harms, and whether additional benefits such as increased human capital accumulation may arise as well.

For several reasons, however, knowledge about the consequences of access to alcohol is limited. Although certain health problems, such as liver damage (Duryee et al. 2004) are directly related to alcohol through biological mechanisms, a correlation between alcohol and external injuries could just as well reflect unobserved factors. For example, Dave and Saffer (2008) show that demand for alcohol is negatively related to risk aversion. Since many expressions of risk-taking are likely to cause injuries, a positive relationship between injuries and alcohol consumption may just as well be spurious.

Further, even if the (average) effects of alcohol on health and other outcomes were known, these may not be very informative about the effects of legislated access to alcohol, as determined by MLDAs or other policies. This is because some individuals will decide not to drink (or not to drink very much) even when legally allowed to, and some individuals will be able to obtain alcohol even when they are not allowed to. Thus, any legislation will only affect some share of the population. The effect of the law will depend on how large this share is, but also on the characteristics of individuals in the compliant subpopulation. If, for example, the damaging consequences of alcohol were limited to those who hardly drink anyway and/or to those who are able to obtain alcohol anyway, laws restricting access to alcohol would be of little benefit. Only if these laws restrict a large enough group of potential problem drinkers from obtaining alcohol, they can be viewed as successful.

A number of studies evaluate the effects of alcohol policies to prevent youths or young

adults from obtaining alcohol. We focus here on MLDAs, which are the most widely implemented measures to reduce alcohol consumption. Almost all evidence on this type of policy comes from the US and, to a smaller extent, Canada, with virtually no evidence from Europe (for a review, see Wagenaar and Toomey 2002). In addition, all evidence in the current literature focuses on short-term effects, although it is a policy question of major interest whether the consequences of the age of first use of alcohol are of long-term nature. The contribution of this paper, therefore, is to provide some of the first estimates based on full population data from the Danish registers of the longer-term effects of being prohibited from alcohol access at younger ages.

When Prohibition ended in 1933, MLDAs of 21 were implemented by almost all states in the US. 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., Smart 1977; McFadden and Wechsler 1979; Wagenaar 1982), or traffic crashes (e.g., Douglass et al. 1974; Williams et al. 1975; Wagenaar 1983; Cook and Tauchen 1984). 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., Thiel 1985; DuMouchel et al. 1987; Wilkinson 1987; O'Malley and Wagenaar 1991). On the other hand, using survey data that is more comprehensive than what most of the studies in the literature have used, Dee (1999) finds that estimated effects of higher MLDAs depend on whether state-fixed effects are controlled for, and Ruhm (1996), 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.

For other outcomes and other countries, evidence is scarce. A few US studies consider health and social problems such as suicides, homicides, vandalism, and injuries (Wagenaar and Toomey 2002). Effects have pointed in the direction of fewer problems when drinking ages are higher but perhaps due to small samples and insufficient power, estimates are most often insignificant. One relatively recent study by Carpenter and Dobkin (2011) uses comprehensive data all the way from the 1970s to the 90s, and documents effects on (different types of) mortality. As noted by the authors, no comprehensive and age-specific US data on injuries is available from the 70s and 80s, so that effects of MLDA changes could be estimated.

Several papers have also exploited a recent lowering of the MLDA in New Zeeland from 20 to 18. Gruenewald et al. (2015) find substantial impacts on drinking, Kypri et al. (2006) document effects on traffic crashes, and Conovera and Scrimgeour (2013) document effects on alcohol-related hospitalizations. Some of these studies were questioned by Boes and Stillman (2013), who find significant effects on alcohol-related hospitalizations but not on traffic crashes or even on average alcohol consumption when accounting for time effects. Marcus and Siedler (2015) study a policy introduced in Southern Germany, which banned off-premise sales of alcoholic beverages between 10 pm and 5 am. Although the 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 outcomes such as mortality (Carpenter and Dobkin 2009), hospitalizations (Callaghan et al. 2013), traffic crashes (Callaghan et al. 2014; Lindo et al. 2015), and student performance (Carrell et al. 2011; Lindo et al. 2011). Results generally suggest adverse effects on these outcomes when individuals reach 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 are allowed to buy handguns in parts of the US). More importantly, however, effects are only estimated in a certain age interval around the MLDA, and behaviors around this threshold may not be representative of behaviors at other ages. For alcohol consumption in particular, 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.

The fact that previous studies have not considered long-term consequences of MLDAs on health or socioeconomic outcomes such as education is a major limitation of the literature, as alcohol is generally assumed to have long-term effects in young individuals. More correlational studies have provided ample evidence that heavy alcohol-consumption is not only negatively correlated with health but also education (e.g., Cook and Moore 1993; Yamada 1996; Renna 2007) and income (e.g., Renna 2007), although moderate alcohol consumption is often found to have beneficial effects. As for another quasi-experimental identification strategy, some studies (Lee 2003; Böckerman et al. 2017) have used comparisons of twins to eliminate genetic and shared environmental confounders, and found the same patterns as the more correlational studies. Even if we were to interpret results from these causally, the implications for alcohol policy are not straightforward, however, as such policy could both increase heavy drinking

(with very adverse consequences) and moderate drinking (with potentially beneficial consequences).

Economists have long addressed issues surrounding alcohol consumption also from a theoretical perspective. While the traditional approach has been to assume perfect rationality and foresight (Becker 1988), economic models with hyperbolic discounting or other types of bounded rationality suggest that individuals regret previous decisions and that there is room for policies to protect individuals from their own actions (Gruber and Köszegi 2001). Much of the public debate presumes that rationality is more limited among youth than among adults, making youth consumption more concerning. Our study makes no attempt to evaluate the appropriateness of this presumption but we note that a focus on the alcohol availability of youth is of particular interest to policy makers, the public, and in light of the potential long-term effects.

In this paper, we consider short- and long-term effects of adolescent alcohol availability. We exploit a series of reforms in Denmark, which influenced access to alcohol at different age margins and in terms of different alcohol content. We exploit both the introduction and changes in MLDAs in a difference-in-differences (DiD) framework, where outcomes are compared over time as well as across age groups, where some were affected by the policy changes and some were not.

We consider the effects of MLDAs on rarely exploited health outcomes, using full-population data. In particular, by making use of hospital records, we examine effects on a broad class of injuries, as well as alcohol intoxication and poisoning (henceforth, alcohol-related conditions). We also consider mortality. In addition, we measure possible long-term consequences on the same dimensions as well as on educational attainment, both in terms of obtaining secondary and tertiary education. Secondary education is interesting not only in itself, but also because alcohol consumption is known to be high at Danish high schools, so that if individuals obtain more secondary education due to the reforms, they may in turn drink more and have worse outcomes later on – potentially counteracting the direct effects of these reforms.

In general, considering the context of Denmark is an additional contribution to the literature, as this country has an extraordinarily liberal youth alcohol culture, and children of all ages were for a long time allowed to purchase alcohol in stores. In 1998, an age limit of 15 was eventually implemented, and this was later increased to 16 (and subsequently to 18 for stronger beverages). These are the reforms we exploit for identification. In contrast to previous studies, we are thus able to not only consider changes in MLDAs, but also their introduction, as well as effects along younger age margins. Thanks to detailed socioeconomic information, we can address

heterogeneity with respect to parental education and income.

The rest of the paper proceeds as follows. Section 2 provides a description of the data and methods employed by our study. In section 3, the results (both for short- and long-term outcomes) are presented. Finally, section 4 provides a concluding discussion.

2. Data and methods

2.1 Institutional setting

Denmark is known for having a liberal alcohol culture, and 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[1], at age 14 or 15. Alcohol is generally considered an important base of social activities among the youth (Demant and Krarup 2013). Not only is consumption widespread, but youth also tend to drink large amounts. 85 percent of adolescents find it acceptable to drink in order to get drunk, according to surveys from the Danish National Board of Health (Sundhedsstyrelsen 2008).

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" (Hibell et al. 1997). In 2015, the same survey asked individuals about "any lifetime use," "30-day use," and "intoxication in the last 30 days." Only for the first of these measures was Denmark not number one (instead shared a second place), and no country comes close in terms of intoxication; 32 percent of Danish youth have reportedly been intoxicated in the last month, as compared to 13 percent in Europe on average and 10 percent in the US (Kraus et al. 2016).

In 1997, there was intense public debate in Denmark about youth alcohol consumption. This was spurred by a fear that the introduction of so-called alcopops (essentially a mixture of alcohol and soda) would increase consumption levels. In addition, the debate was influenced by the results from the previously mentioned ESPAD. It was noted that virtually all comparable

[1] In the Danish Lutheran church, confirmation is where 14- or 15-year-olds reaffirm their Christian faith, which was pledged at baptism. In 2016, 68% of this age-group took part in the ceremony (Danish Church Ministry, http://www.km.dk/folkekirken/kirkestatistik/konfirmerede/) although, increasingly, children are choosing "nonfirmation," a secular coming-of-age party without the church ceremony.

countries had MLDAs or minimum alcohol purchase ages. In Denmark, although there was an age limit of 18 at on-premises such as restaurants and bars, age limits did not exist at off-premises (grocery stores, kiosks, gas stations etc.). Following the intense debate, an off-premise 15-year age limit for purchasing any beverage with an alcoholic content of 1.2 percent or more was decided to be implemented by July 1, 1998.[2]

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). Partly with the aim to evaluate the effects of the 1998 reform, this survey was then repeated in the same calendar month in subsequent years. Results, reported in the correlational study by Møller (2002), 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 percent 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. Most of the drop occurred already in the year after the reform, giving credibility to the idea of a causal effect. Still, after the reform, a significant portion of individuals below age 15 was able to access alcohol, either through parents, friends, or other sources, or because the law was being ignored by the stores.

Concerns about youth alcohol consumption remained, and on July 1, 2004, the age limit was increased from 15 to 16. The Danish National Center for Alcohol Research distributed surveys to youth between 13 and 16 years of age in May and June 2004 and 2005 to evaluate the impacts of this reform (Jørgensen et al. 2006). Results of the evaluations 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 a half, from an initial level of more than 40 percent. However, consumption itself was less affected, possibly with a reduction in drinking over the past month by 7 percent (5 percentage points) when compared to other age groups. A similar conclusion can also be drawn from surveys administered by the National Health Board (Sundhedsstyrelsen 2010). A possible explanation for the seemingly weaker effect of this reform on consumption is that those aged 15 had parents and peers willing to provide them with alcoholic beverages, as drinking at this age had long been considered normal in Denmark.

Further raising the age limit for buying alcohol in general was not perceived as realistic;

[2] For simplicity, we refer to the Danish legislation as an MLDA one, even though the policy implies a restriction on sales and purchases rather than actual drinking.

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 buy beverages with an alcohol content of 16.5 percent 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 (DR 2010). 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 (Sundhedsstyrelsen 2014).

Notably, the legal driving age limit in Denmark is 18. In contrast to studies based on countries such as the US, the implemented MLDAs are thus lower than the driving age, and effects are less likely to operate through traffic crashes.

2.2 Data

Our study exploits information from multiple national registers held by Statistics Denmark. We utilize the Population Register, which has information on the date of birth and the sex of the person. This register, like the others, also includes a personal identifier, which allows us to link data from other sources. Our analyses of short-term outcomes exploit all individuals between 12 and 20 years of age, whereas we include individuals aged 15-28 when considering long-term effects of reform 1, and 16-22 when considering long-term effects of reform 2.

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 several days. There is also information on diagnoses assigned at the visit, coded with the ICD-10 classification. Both inpatient and outpatient (specialist) visits are included. For another measure of health, we exploit the Death Register.

Family members can be linked, and through the Taxation and Education registers we obtain information on total parental income and 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.

We display descriptive statistics in Table 1. Descriptives are split according to time period, where we restrict attention to different intervals when considering different reforms, in order to only include one reform at a time. As can be seen, numbers are similar across the periods.

Injuries are 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 0.2-0.3 cases of death.

2.3 Method

Investigating effects on short-term outcomes, our analysis exploits differences over time and across age groups in terms of whether alcohol can be legally purchased. The following DiD regression is run.

(1) $y_{it} = Prohibited_{it} + Age_{it} + Time_t + \varepsilon_{it}$

In this equation, *y* is the number of events (such as hospitalizations due to injuries) for individual *i* in year *t*. $Prohibited_{it}$ is the treatment indicator, assuming the value 1 if the individual was prohibited from buying alcohol at off-premises during the entire year; it may also assume a value between 0 and 1 if the individual was prohibited from buying alcohol during some part of the year. Age_{it} is a vector of age dummies, indicating the individual's age in years at the end of the year, and $Time_t$ is a vector of time dummies, also expressed in years. Standard errors are clustered 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 age groups, 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. This accounts for any changes over time that develops according to these polynomials. Second, we focus on a narrower range of years, so that there is little time for differences across treatment and control groups to develop. Specifically, in our initial analysis, we use all available years (1995-2013) without exploiting more than one reform in the same regression, which means that we exploit 1995-2003, 1999-2010, and 2005-2013 for the different reforms, respectively. In our subsequent regressions, we then 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).

Two different hospital outcomes are studied. First, we examine effects on injuries. The category includes poisonings, but we exclude alcohol poisonings, as we want to study direct consequences of alcohol separately. We then consider alcohol-related conditions, i.e., effects

that immediately follow from alcohol consumption, including both poisonings and intoxications. Other alcohol-related hospital visits (e.g., liver conditions) are almost nonexistent in young ages and are therefore not included. Finally, we examine mortality.

Following this analysis of short-term (immediate) effects of alcohol access, we subsequently consider more long-term consequences. Such consequences could, for example, arise because individuals who are allowed to obtain more alcohol in youth get into a habit of drinking more (or possibly less, if individuals realize the negative consequences of alcohol consumption). This could then have secondary impacts, including human capital accumulation. Reform 3 is too recent to study long-term consequences, but for reforms 1 and 2, the following regressions are run:

(2) $y_{it} = Prohibited\ Age\ 12-14_{it} + Age_{it} + Time_t + \varepsilon_{it}$

(3) $y_{it} = Prohibited\ Age\ 15_{it} + Age_{it} + Time_t + \varepsilon_{it}$

Outcomes included include the ones we study in the analysis of short-term outcomes, but we additionally consider two measures of education: obtaining a secondary degree, and obtaining a tertiary degree.[3] Individuals are included up to the point when such a degree is obtained.[4] The treatment variables represent whether individuals were prohibited from buying alcohol at age 12-14 or at age 15, respectively, and are allowed to assume values between 0 and 1. In this analysis, treatment is entirely determined by birth cohort. As above, we use specifications with linear or quadratic age-specific time trends, and also specifications where we restrict attention to fewer cohorts around the changes.[5] Standard errors are clustered at the

[3] While our data also contains information on final grades in grade 9, analyzing effects on these does not fit into our DiD framework, where outcomes are compared across both time and age groups.

[4] Moreover, we restrict attention to ages 19-22 when considering secondary education and ages 21-28 when considering tertiary education, so that we include the very ages when there is some variation in these outcomes. Only cohorts where everyone can be examined throughout the ages of interest are included, and – as previously mentioned – we additionally make sure to not include other cohorts so that more than one reform is included in the same regression.

[5] In the analysis of short-term outcomes, the specifications where we restrict attention to only a few outcome years do not parametrically control for time trends, since these are alternative ways of taking trends into account. In the long-term analysis, however, outcomes are always studied over an extended number of years. Some specifications restrict attention to a few *birth* years around the threshold, but since the outcomes are not restricted to a few years, we still control for time trends parametrically.

individual level.

3. Results

3.1 Short run: Graphical evidence

We here show graphs displaying patterns of hospital visits around the relevant reforms, separately for the three health outcomes of interest: injuries, alcohol-related conditions, and mortality. Beginning with injuries, Figure 1a displays average outcomes by age group for the different time periods around the reforms: 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). In Figure 1b, we then display differences in the outcome across time periods for the respective age group. Overall, there has been a declining trend in injuries, from about 0.30 per year and individual in the 1995-1997 period, to 0.23 in the 2011-2013 period. After the first reform, in 1998, injuries dropped in all age groups, but much more among those below age 15. Note that the ages indicated in the graph are ages at the end of the year, which means that the individual was one year younger for some part of the year. Indeed, and as may would expect, the drop is substantial both among those below and those at the age of 15, as measured by age at the end of the year. Our graphical results for the first reform line up with Møller (2002), who established reduced drinking after the implementation of the first reform, especially among children in grades 5-7.

After the second reform, 15-year-olds were prohibited from buying alcohol. The figure shows substantial drops in injuries among 15- and 16-year-olds, where the latter were 15 for some part of the year, but there are also comparable drops in several other age groups. This is the case for 17-year-olds, for example, whereas the drop is less pronounced among those further away from age 15.

Reform 3 is supposed to have influenced 16- and 17-year-olds, who became ineligible to buy spirits. Indeed, the figure shows substantial drops in injuries in these ages, but there are also drops in other age groups. In particular, those above age 18 have face drops in their injuries, perhaps reflecting underlying trends.

Figure 2 then displays alcohol-related conditions in different age groups and over time, where panel A again focuses on levels and panel B on changes. Compared to injuries, these events are much less common, with rates only about a hundredth of those of injuries. Nevertheless, there is some evidence that the reforms had impacts on these events. Around the first reform, the number of alcohol-related conditions was rather constant among individuals

below 15. Among older individuals, however, there was a tendency towards an increase, suggesting an impact of the reform. After the second reform, occurrences dropped among 15- and 16-year-olds (who were 15 for some part of the year), in line with our expectations. In other groups, there were little changes, with the exception of 14-year-olds, who also faced a decline (potentially a peer effect). Following the third reform, the expected drop among 16- and 17-year-olds (including 18-year-olds who were 17 for some part of the year) is present, but the drop is even larger among 15-year-olds, suggesting that there are other mechanisms operating.

The prevalence of mortality in our data is even lower, only a tenth of that of alcohol-related conditions. As shown in Figure 3, there are few clear patterns. One exception is the development after reform 3, where there is a drop among those above 16, but if the development were just due to the reform, we would also have seen a drop among those aged or 16 at the end of the year, with little impact on those above 18.

3.2 Short-term results: Main regressions

We now consider regression results for the same outcomes as displayed in the graphs. Throughout our regressions, we use outcomes multiplied by 1000, so that estimates can be interpreted as yearly effects per 1000 individuals. In our main tables on short-term results, column (3) reports results for regressions based on quadratic trends, with results being viewed as more credible than those the simpler models (1), where no trends are included, and (2), where linear trends are included. In columns (4) and (5), attention is restricted to years close to the reform year, another way of taking care of the parallel trends assumption. If results line up regardless of whether trends are accounted for in a parametric or non-parametric way (i.e., restricting attention to fewer years), there is greater reason to have faith in the conclusions.

Table 2 reports the results for injuries. 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 percent level. In relative terms, it corresponds to roughly 6 percent of the average in any age group. The effect is somewhat smaller than the drop shown among those below age 15 in Figure 1, a fact that can be explained by a drop in the control groups as well.

In the second specification, linear age-specific trends are used. The estimate now becomes somewhat larger, and 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 rather are the trends 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 6 percent 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, but the estimate becomes smaller when adding trends. In a model with quadratic trends it equals -10, corresponding to 3-4 percent 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.

Panel C displays the results based on reform three. Model 1 suggests an impact of -17. As for reform 2, the estimate is reduced when accounting for trends, but a relatively large and strongly significant effect remains, amounting to a negative of 12-13. In relative terms, the results suggest a reduction in injuries of about 3 percent when spirits became illegal to purchase for 16-17-year olds.

We then consider effects on alcohol-related conditions in Table 3. 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 percent 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 percent of the average of 1.8 in ages below 15. However, the estimate becomes smaller and insignificant when accounting for trends, either with a quadratic birth-year-specific trend, or by restricting attention to three years around the reform.

For the second reform we find a significant effect on alcohol-related conditions only when accounting for trends; the estimate is positive and about 0.8. This is a counterintuitive result, which could potentially be explained by substitution into more dangerous types of alcohol. However, in contrast to other significant results presented so far, this is only significant at the 10 percent level, and given the large number of specifications run it is not unlikely to be a false positive. For reform 3, there is no evidence of an effect on alcohol-related conditions.

Table 4 displays results for mortality. There is little evidence of impacts on this outcome, and all coefficients but three are statistically insignificant and provide some evidence that mortality was reduced. It should be noted, however, that mortality is very low in these ages, making effects harder to detect.

3.3 Effects by SES and gender

We next examine whether effects of alcohol availability on injuries or alcohol-related conditions differ by measures of family SES (parental income or education) or by gender. 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 are several reasons why differential effects might emerge. First, in families with higher income, there may be more resources available for a child to spend on alcohol. On the other hand, individuals from families of lower socioeconomic status, whether lower income or lower education, may experience more stress or lack of control (e.g., Link et al. 1993; Adler and Newman 2002; Adler and Snibbe 2003), or may be less forward-looking (Fuchs 1982). These factors may, in turn, increase the propensity for unhealthy behavior such as problem drinking (Newcomb and Harlow 1986; Vasse et al. 1998; Debbie and Jeffery 2003). While studies have typically found that individuals of higher SES drink more often, evidence points at more problem drinking and more intense drinking among individuals of lower SES (e.g., Berggren and Sutton 1999; Droomers et al. 1999; van Oers et al. 1999; Huure et al. 2003; Heckley et al. 2016). For this reason, we might expect that those from lower socioeconomic status backgrounds respond more to the availability of alcohol in terms of injuries or alcohol-related problems. Another issue, however, is that availability of alcohol is not restricted in the same way for everyone. Although purchases in stores are formally illegal, some individuals may be able to obtain alcohol from stores by pretending they are older than the age limit, or may obtain alcohol through parents, friends, the black market, or even by home production (rare). We can only speculate about whether such behavior and possibilities vary by socioeconomic status.

In Denmark, alcohol consumption has been rather similar across the genders and the responses to the first and second reforms on alcohol consumption appear similar as well (Møller 2002; Jørgensen et al. 2006). Boys, however, are known to be more-risk taking than girls in general (e.g., Byrnes et al. 1999; Croson and Gneezy 2009), and it is thus conceivable that stronger effects on injuries or on alcohol-related conditions would arise.

First, in Table A1, we stratify the analysis by family income. To be precise, we sum the income of the mother and father, and create year-specific quartiles based on this sum. For each income quartile, we then proceed as in the main analysis, but with four parallel regressions. We only consider models corresponding to column (3) in previous tables, where we include a broad range of years and quadratic trends.

We find no strong evidence that effects would vary by parental income. For the effects of the first reform on injuries, the estimate is largest for those in the second quartile. There is no clear gradient across the income quartiles, and no differences are significantly different each other. For reform 2 and 3, estimates are only significant in the second and fourth income

quartiles, but in general there are no significant differences across groups.

For alcohol-related conditions, there are no significant effects of reform 1 or 3. The second reform produces significant, and as before, positive, effects in the first and third income quartiles.

We then in Table A2 consider effects by parental education. Indeed, more education is typically related to higher incomes, but education may also be related to patterns of thinking and decision-making (Cutler and Lleras-Muney 2006), which were not picked up (much) when the analyses stratified by income. We stratify by father's education (primary, secondary, and tertiary) and then by mother's education (primary, secondary, and tertiary).

In general, there is again no evidence that effects would differ across socioeconomic groups, as measured by education. Estimates are all statistically indistinguishable. The effects on alcohol-related conditions are only significant for reform 2 when the father has secondary education or the mother has primary education, and for reform 3 when the father has primary education. In short, there are no clear patterns.[6]

In Table A3, we look at effects by gender. The effects on injuries are similar for males and females. Effects on alcohol-related conditions are only significant for the second reform and for women, where the positive coefficient estimate shows up.

3.4 Long-term results

We now turn to asking if the age at which an individual was allowed to buy alcohol in early teenage years influences long-term health and human capital accumulation. First, in Table 5, we collect results for injuries, alcohol-related conditions, and mortality. We look at effects of reform 1 and 2, whereas, as mentioned, reform 3 is not exploited as it occurred too recently. Four different types of models are run: a model with no trends, a model with linear birth-year-specific trends, a model with quadratic birth-year-specific trends, and a model with quadratic birth-year-specific trends as well as a restriction to only four birth cohorts around the threshold.

As in our short-term analysis, we find strong evidence of an effect of the first reform on injuries. Also as before, the estimate becomes larger when taking care of trends. In our most

[6] In our regressions, we have always included all individuals in the age interval 12-20. One problem with including the older individuals in this age span, however, is that parental income or education may be poor indications of resources or other circumstances facing the individual. We could replace parental income by own income or use parental income plus own income, but these options are problematic as well, because the resources actually available may in fact represent some (unknown) weighted sum of parental and own income. We have therefore instead excluded individuals aged 18 and older, and re-run the analyses. The results based on this exercise, displayed are rather similar to the ones above (results available upon request).

credible specifications, we find strongly significant effects, amounting to reductions in injuries by about 50 per person and year, corresponding to a reduction by about 20 percent.

There is little evidence of impacts of the second reform; all estimates are statistically insignificant. Indeed, it is conceivable that early alcohol debut is more problematic in terms of long-term behavior and that it makes less difference whether the individual begins to drink at 15 or 16. However, power is somewhat low and it cannot be ruled out that the second reform has important effects. This is also the case for all estimates on alcohol related conditions, both for reform 1 and 2, and all estimates for mortality, which are all statistically insignificant but confidence intervals relatively wide.

Lastly, in Table 6, we consider long-term educational outcomes. We find inconclusive evidence that being prohibited from buying alcohol would have an influence on the completion of secondary or tertiary schooling. For the effects of the first reform on secondary schooling, our simplest specification suggests an increase in the likelihood of completing secondary education by 11 per 1000 individuals and years. However, the estimate switches sign when adding linear birth-year-specific trends, and becomes statistically insignificant when adding quadratic trends. For the effects of the second reform on the probability of completing secondary education, effects are positive and significant with no trends or with linear trends, but turn insignificant when adding quadratic trends. The estimates for the first reform and tertiary education switch back and forth between being positive or negative, and turn insignificant when restricting attention to only the four cohorts that are right at the threshold. In sum, there is no evidence that restricted ability to obtain alcohol would influence educational attainment.

4. Conclusion

Previous research on the impacts of MLDA legislation originates mostly from North America, focuses on a limited set of outcomes, and only short-term ones. There is scarce evidence from other settings, on wider ranges of outcomes and based on population-wide data. A few recent studies (e.g., Carpenter and Dobkin 2009; Callaghan et al. 2013) that base their analyses on comparisons of individuals right below and right above the MLDA have been able to exploit large population datasets but have to make the assumption of no adjustment effects when alcohol becomes available. Further, these studies only estimate local treatment effects only around a certain threshold (21 years). A drawback not only of these studies but of the entire MLDA literature is also that effects have only been examined in the very short run.

Our paper brings new evidence on the effects of MLDAs on rarely observed health and

human capital outcomes using population-wide register data from Denmark. By linking Population Registers to records from the Patient, Education, and other registers, we investigate effects of not just changes but also the introduction of MLDAs on injuries, alcohol-related conditions, as well as on human capital formation as measured by the probability of obtaining secondary and tertiary education. In contrast to previous literature, we bring evidence not only on short- but also long-term outcomes.

We analyze the impact of three reforms – 1) Establishing a minimum alcohol purchase age of 15 in 1998, 2) Increasing this minimum age to 16 in 2004, and 3) Increasing this age to 18 for purchase of stronger beverages in 2011. Notably, the target groups of the reforms in the Danish case are youths at much lower ages than those in studies based on the US, allowing us to examine effects on different margins.

Exploiting a DiD approach, our results show substantial reductions in injuries when the minimum purchase age of 15 was implemented, with a relative effect of 6 percent. Relating this to Møller (2002), 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 percent. 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. The smaller effects on injuries of reform 2 compared to reform 1 does not seem to be due a smaller effect of consumption on injuries, but rather that consumption was less affected. 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. Children also start 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.

In general, we find no clear evidence on impacts on alcohol-related conditions (alcohol poisonings and intoxications) or mortality. However, these results are much rarer and power is lower. For long-term outcomes, we find that the first reform had strong impacts on injuries between age 15 and 28, whereas the second reform has no significant effect. This is again in line with the larger consumption response to the first reform, perhaps combined with a larger impact of earlier consumption on long-term habits. Nevertheless, we find no evidence that alcohol would interfere with learning so that educational attainment would be affected.

The short-term effects of the reforms on injuries are similar but larger than those of Callaghan et al. (2013), who find a 3 percent increase in a broad class of injuries when individuals became old enough to buy alcohol. However, Callaghan et al. also report significant

effects on alcohol-related conditions. It is unclear to what extent their results are influenced by a temporary boost in alcohol consumption after the eligibility threshold is reached, or how effects would be amplified by habit formation. Another related paper is Marcus and Siedler (2015), who exploit a German reform that prohibited alcohol purchases only during certain hours. Their estimate suggests an effect of around 7 percent 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 obtained alcohol also when not legally allowed to.

Denmark is a country with an extraordinarily liberal alcohol culture, where children for decades were able to purchase alcohol at stores without restrictions, and where many children below the subsequently implemented MLDA were still able to obtain alcohol from various sources. Thus, the effects of restrictions on alcohol purchase in Denmark pertaining to injuries, alcohol-related conditions and human capital formation should provide lower bounds on the effects when MLDAs are or have been changed or implemented elsewhere.

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Table 1: Descriptive statistics

	Reform 1	Reform 2	Reform 3
Panel A: Analysis of short-term outcomes	1995-2003	1999-2010	2005-2013
Injuries	288.933	273.622	259.328
Alcohol-related	4.585	4.836	4.496
Mortality	0.320	0.264	0.217
Parental income	322,117	321,996	321,911
Parental income missing	0.008	0.009	0.008
Father primary education	0.257	0.257	0.256
Father secondary education	0.492	0.493	0.494
Father tertiary education	0.249	0.249	0.250
Father's education missing	0.098	0.098	0.098
Mother primary education	0.303	0.304	0.304
Mother secondary education	0.440	0.439	0.440
Mother tertiary education	0.254	0.254	0.255
Mother's education missing	0.031	0.031	0.031
Observations	3,203,017	4,609,484	3,730,196
Panel B: Analysis of long-term outcomes			
Injuries, age 15-28/16-22 (reform 1/2)	246.210	268.834	
Alcohol-related, age 15-28/16-22 (reform 1/2)	4.757	6.612	
Mortality, age 15-28/16-22 (reform 1/2)	0.476	0.385	
Observations	6,197,673	2,489,718	
Secondary education, age 19-22	182.708	174.726	
Observations	1,617,473	1,161,681	
Tertiary education, age 21-28	99.647	-	
Observations	2,990,994	-	

Note: Panel A includes individuals aged 12-20. Injuries includes all injuries and poisonings except alcohol poisonings, and alcohol-related conditions include alcohol intoxication and poisoning. Injuries, alcohol-related conditions, and mortality are multiplied by 1000 and measured on a yearly basis. Parental income is measured in DKK per year. In panel B, educational outcomes are only observed up to the point when the degree in question is obtained. To enable observation of all individuals in a given regression through the same ages, for reform 1, we include individuals born 1974-1985 when considering health-related outcomes, individuals born 1976-first half of 1988 when considering secondary education, and individuals born 1974-1985 when considering tertiary education. For reform 2, we include individuals born second half of 1983-1991 when considering health-related outcomes, and individuals born second half of 1983-1991 when considering educational outcomes.

Table 2: Short-term 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)
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)
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)
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation 1.

Table 3: Short-term 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)
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)
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)
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation 1.

Table 4: Short-term 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)
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)
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)
Linear trends		X			
Quadratic trends			X		

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation 1.

Table 5: Long-term effects on health (yearly per 1,000 individuals)

	(1)	(2)	(3)	(4)
Panel A: Injuries, reform 1				
Prohibited	-3.390	-35.354***	-44.543***	-55.075**
	(5.079)	(12.658)	(14.616)	(21.523)
Panel B: Injuries, reform 2				
Prohibited	-3.080	0.207	-0.595	11.779
	(3.106)	3.967	(4.239)	(11.065)
Panel C: Alcohol-related conditions, reform 1				
Prohibited	-0.683	0.045	-1.052	-0.954
	(0.548)	(1.283)	(1.439)	(2.568)
Panel D: Alcohol-related conditions, reform 2				
Prohibited	0.138	0.583	0.455	0.952
	(0.359)	(0.431)	(0.478)	(1.161)
Panel E: Mortality, reform 1				
Prohibited	0.015	0.071	-0.039	-0.617
	(0.087)	(0.208)	(0.231)	(0.404)
Panel F: Mortality, reform 2				
Prohibited	0.072	0.068	0.076	-0.034
	(0.057)	(0.069)	(0.075)	(0.212)
Linear trends		X		
Quadratic trends			X	X
Only 4 cohorts				X

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equations (2) and (3), with controls for age-specific quadratic time trends.

Table 6: Long-term effects on education (yearly per 1,000 individuals)

	(1)	(2)	(3)	(4)
Panel A: Secondary education, reform 1				
Prohibited	10.751***	-5.243*	3.668	0.033
	(2.212)	(2.918)	(2.984)	(8.064)
Panel B: Secondary education, reform 2				
Prohibited	8.840***	4.233**	2.518	0.930
	(1.743)	(1.987)	(2.174)	(1.557)
Panel C: Tertiary education, reform 1				
Prohibited	-19.082***	26.842***	-13.142***	-0.255
	(1.213)	(1.812)	(3.083)	(7.135)
Linear trends		X		
Quadratic trends			X	X
Only 4 cohorts				X

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regressions are run according to Equations (2) and (3), with controls for age-specific quadratic time trends.

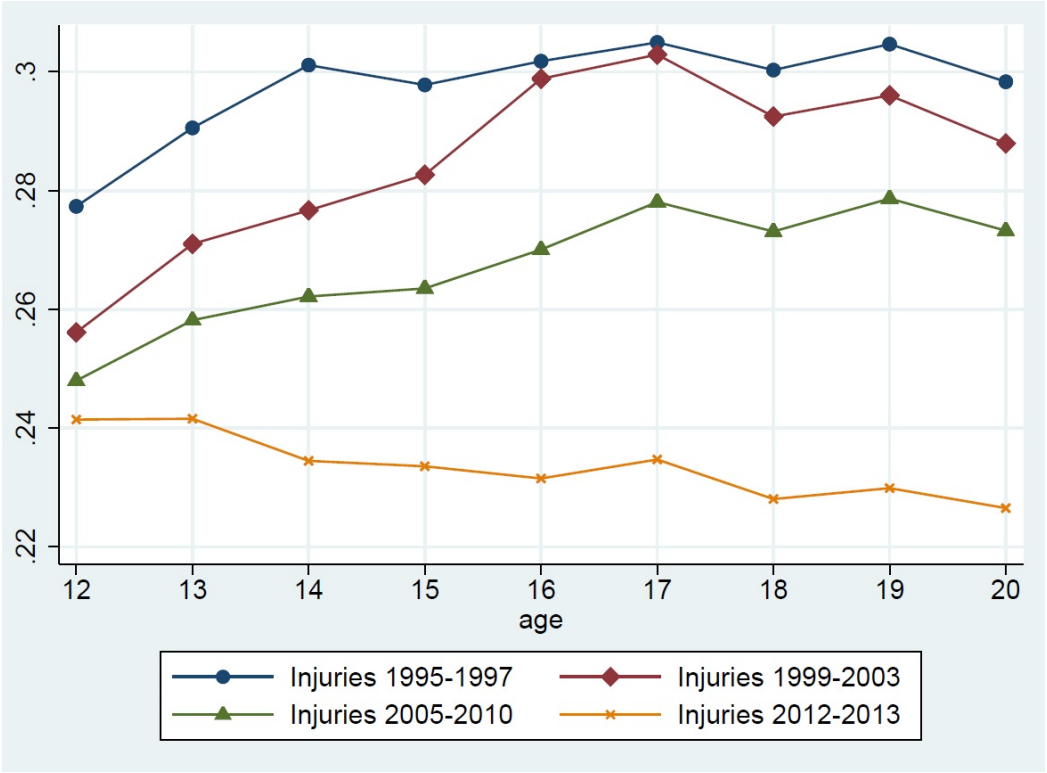

Figure 1a: Injuries (excluding alcohol poisoning) in different policy periods.

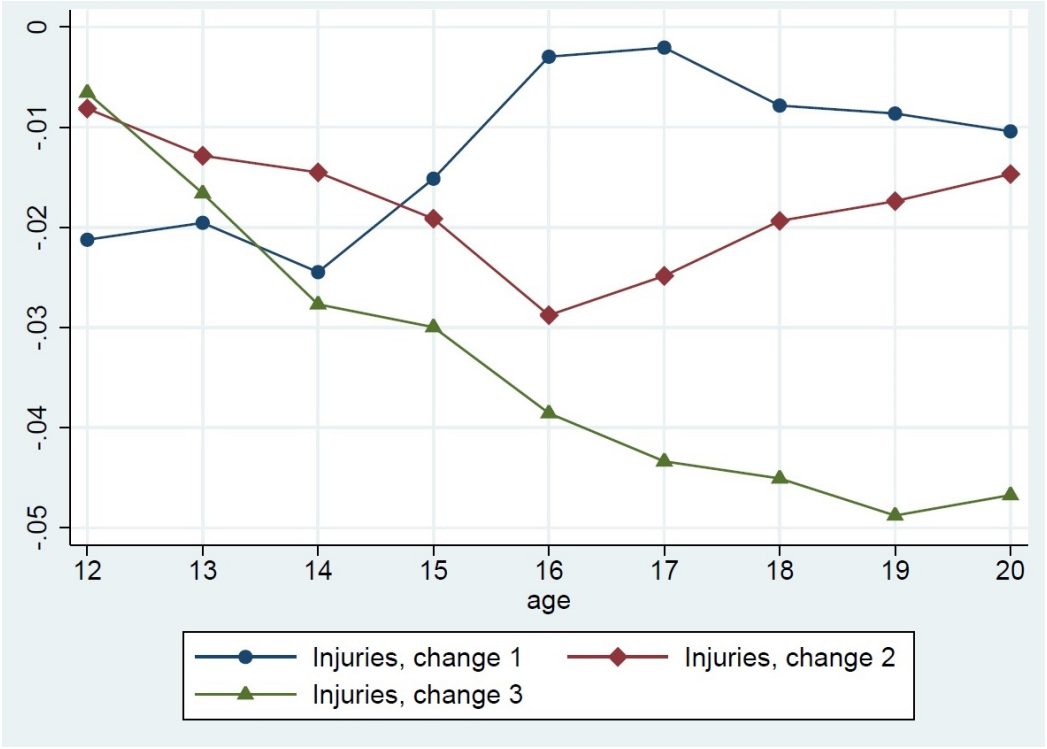

Figure 1b: Differences in injuries (excluding alcohol poisoning) across policy periods.

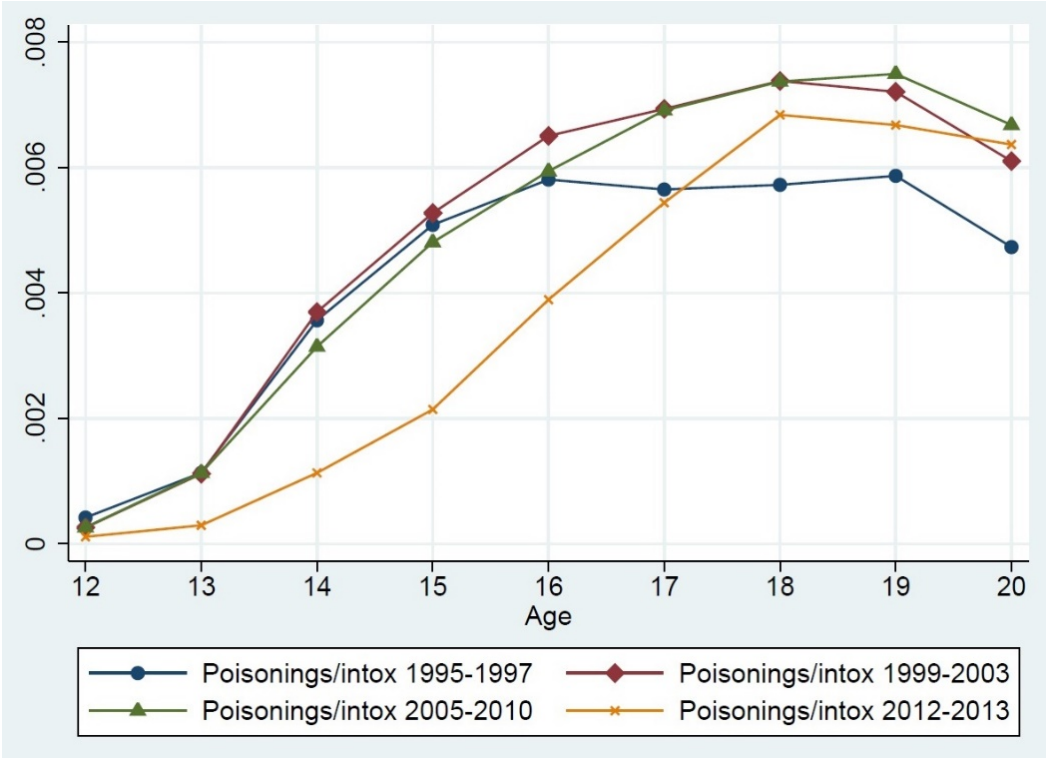

Figure 2a: Alcohol intoxications and poisonings in different policy periods.

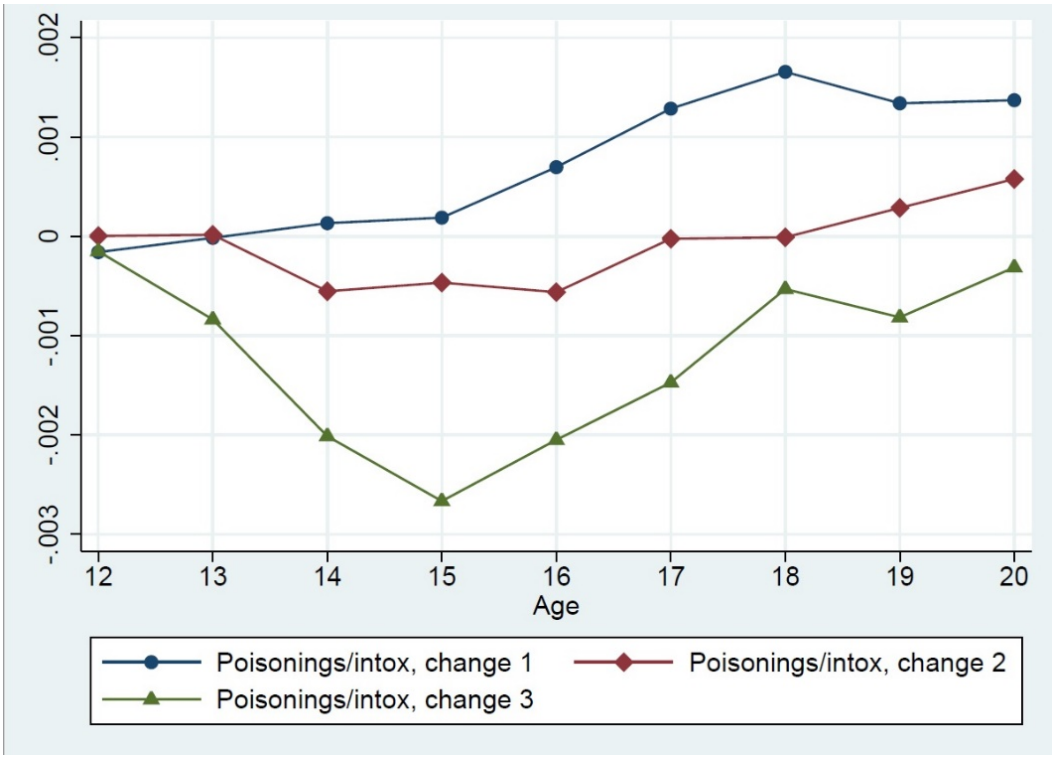

Figure 2a: Changes in alcohol intoxications and poisonings across policy periods.

Figure 3a: Mortality in different policy periods.

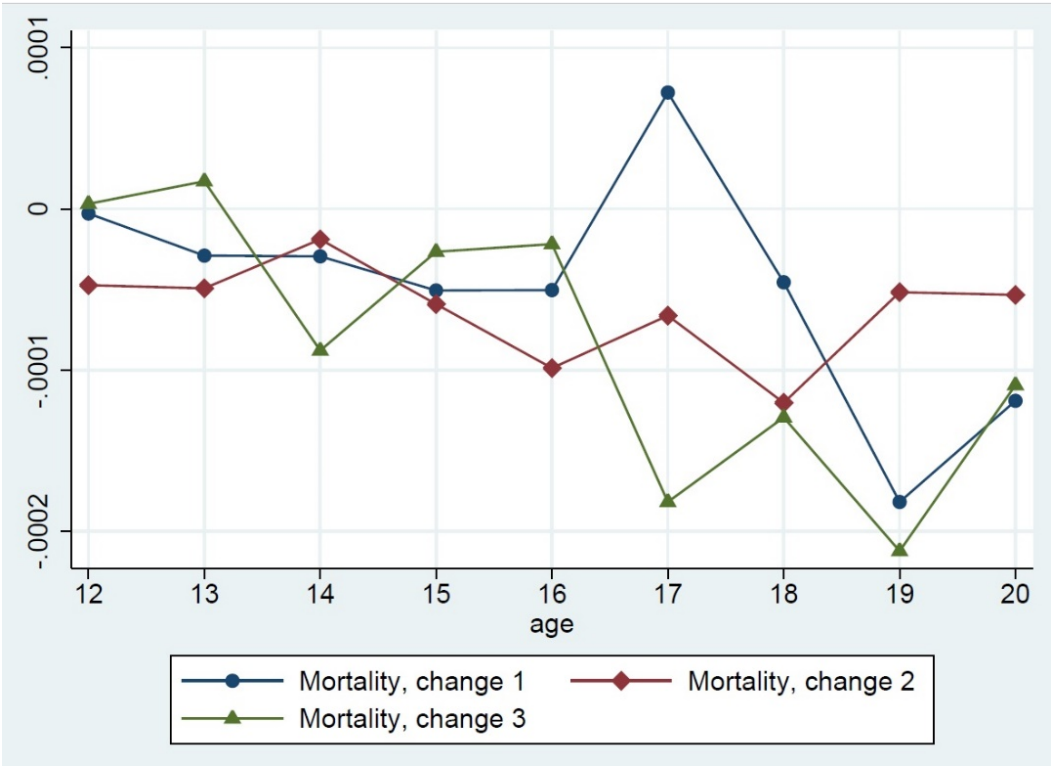

Figure 3b: Changes in mortality across policy periods.

Table A1: Results by parental income (yearly per 1,000 individuals)

	First quartile	Second quartile	Third quartile	Fourth quartile
Panel A: Injuries, reform 1				
Prohibited	-19.392***	-27.611***	-17.026**	-21.199***
	(6.930)	(9.274)	(7.888)	(7.822)
Panel B: Injuries, reform 2				
Prohibited	0.596	-28.360***	-4.470	-16.411**
	(5.961)	(7.930)	(6.893)	(6.730)
Panel C: Injuries, reform 3				
Prohibited	-3.192	-20.383**	-7.815	-21.127***
	(6.259)	(8.043)	(7.189)	(6.822)
Panel D: Alcohol-related conditions, reform 1				
Prohibited	0.284	-1.373	-0.770	0.917
	(0.733)	(1.001)	(0.846)	(0.822)
Panel E: Alcohol-related conditions, reform 2				
Prohibited	1.484**	-1.142	1.727**	-0.068
	(0.745)	(1.030)	(0.814)	(0.818)
Panel F: Alcohol-related conditions, reform 3				
Prohibited	-0.117	-0.163	0.245	-0.821
	(0.743)	(1.054)	(0.856)	(0.852)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with controls for age-specific quadratic time trends.

Table A2: Results by parental education (yearly per 1,000 individuals)

	Father primary	Father secondary	Father tertiary	Mother primary	Mother secondary	Mother tertiary
Panel A: Injuries, reform 1						
Prohibited	-28.773***	-14.412***	-24.164***	-24.164***	-15.688***	-25.437***
	(8.195)	(5.839)	(8.152)	(7.116)	(6.015)	(7.845)
Panel B: Injuries, reform 2						
Prohibited	-12.401*	-4.890	-12.674*	-0.377	-11.911**	-15.130**
	(7.189)	(4.963)	(7.043)	(6.169)	(5.157)	(6.754)
Panel C: Injuries, reform 3						
Prohibited	-8.755	-13.569**	-10.418	-6.594	-10.855**	-18.581***
	(7.179)	(5.412)	(7.136)	(6.367)	(5.505)	(6.967)
Panel D: Alcohol-related conditions, reform 1						
Prohibited	0.359	0.254	-0.646	0.001	0.123	-0.599
	(0.883)	(0.602)	(0.910)	(0.755)	(0.644)	(0.844)
Panel E: Alcohol-related conditions, reform 2						
Prohibited	0.283	1.452**	0.634	1.706**	0.692	-0.073
	(0.909)	(0.599)	(0.882)	(0.790)	(0.633)	(0.817)
Panel F: Alcohol-related conditions, reform 3						
Prohibited	-1.790*	0.506	-0.106	0.096	0.179	-1.031
	(0.922)	(0.629)	(0.905)	(0.790)	(0.654)	(0.850)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with controls for age-specific quadratic time trends.

Table A3: Results by gender (yearly per 1,000 individuals)

	Male	Female
Panel A: Injuries, reform 1		
Prohibited	-18.308***	-22.886***
	(5.670)	(5.216)
Panel B: Injuries, reform 2		
Prohibited	-9.510*	-10.575**
	(4.887)	(4.479)
Panel C: Injuries, reform 3		
Prohibited	-12.161**	-11.975**
	(4.932)	(4.917)
Panel D: Alcohol-related conditions, reform 1		
Prohibited	-0.339	0.094
	(0.607)	(0.564)
Panel E: Alcohol-related conditions, reform 2		
Prohibited	0.199	1.373**
	(0.597)	(0.573)
Panel F: Alcohol-related conditions, reform 3		
Prohibited	-0.141	-0.137
	(0.621)	(0.585)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with controls for age-specific quadratic time trends.

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