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

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Abstract

We exploit changes in minimum legal alcohol purchasing ages in Denmark in order to estimate effects on short- and long-term health outcomes, as well as on human capital formation. Employing a difference-in-differences approach for immediate outcomes and a "regression kink design" for long-term outcomes, we bring comprehensive evidence on the health and education effects of three reforms, which affected alcohol availability along different dimensions and 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 short-term effects of the first and third reforms in terms of reducing injuries and alcohol-related conditions, and some long-term effects of the first reform in terms of reducing injuries and increasing the probability of obtaining a high-school degree. We find, however, no effects of the second reform and little impact of any of the reforms on mortality. Effects of spirits (reform 3) are driven by males, and there is no consistent evidence on differential impacts by socioeconomic status.

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

Jel-Classification: I12; I18

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1. Introduction

Alcohol is one of the most serious threats to population health. In terms of disability-adjusted life years (DALYs), it is believed to be one of the top-three risk factors in the world, with effects comparable to those of high blood pressure and tobacco (Lim et al. 2012). Among youths and young adults, alcohol is even number one. There is widespread belief that alcohol consumption in young ages is 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). Consequently, 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.[1]

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 spurious relationship with alcohol consumption may arise.

Moreover, even if the (average) effects of alcohol on health and other outcomes were known, these would 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

[1] Normatively, 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 the youths' consumption behavior more concerning.

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 (for a review, see Wagenaar and Toomey 2002).

When the US 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., 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 the 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). Using more comprehensive data than most studies, Dee (1999) finds that estimated effects of higher MLDAs depend on whether state-fixed effects are controlled for, and Ruhm (1996) 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.

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 can be estimated.

Gruenewald et al. (2015) find that a recent lowering of the MLDA in New Zeeland from 20 to 18 had substantial impacts on drinking, and Kypri et al. (2006) document effects on traffic crashes. 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), or traffic crashes (Callaghan et al. 2014). Results suggest positive 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 the US). In addition, 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.

In this study, we apply two different identification strategies to consider short- and long-term effects of adolescent alcohol availability, exploiting a series of reforms that influenced alcohol availability at different age margins and in terms of different alcohol content. Looking at short-term outcomes, we use a difference-in-differences methodology and exploit both the introduction and changes in MLDAs. Parts of the analysis exploits only age groups close around the age thresholds changed or implemented, and we consider the possibility of lagged effects, representing habit formation, dependence, or other dynamics. For a long-term analysis, we make a methodological contribution to the literature on alcohol access by applying the recently developed regression kink (RK) design (Card et al. 2015). Here, we exploit time-of-birth thresholds generated by the policy changes, and the nonlinearities in months exposed to alcohol access induced by these.

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. Since effects of alcohol on human capital are of particular policy concern, we additionally consider obtaining an academic high school degree. Our paper adds to previous literature by considering the context of Denmark, a country with an extraordinarily liberal youth alcohol culture, and where children of all ages for a long time were allowed to purchase alcohol in stores. In 1998, an age limit of 15 was implemented, and this was later increased to 16 (and subsequently to 18 for stronger beverages). 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.

Our results suggest substantial reductions in both injuries and alcohol-related conditions when the MLDA of 15 was implemented. According to our main regressions, injuries dropped by 6 percent and alcohol-related conditions by 27 in the affected age groups. We find no effects of the increase of the MLDA to 16, however, and we hypothesize that this is due to the culture, which generally accepts alcohol consumption from age 15 and where parents may even provide their children with alcohol. The 18-year-limit for buying stronger beverages (spirits) appears to have had effects on both injuries and alcohol-related conditions, but only among males. In general, we find no evidence of differential effects by family SES. Estimates for long-term outcomes indicate that individuals that were less able to buy alcohol in youth due to the introduction of the MLDA ended up with fewer injuries in young adulthood, and may have been more likely to obtain an academic education.

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[2], 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

[2] 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.

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 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.[3]

In November 1997, the National Board of Health conducted a survey to investigate alcohol consumption among Danish youth in grades 5-10 (essentially those aged 12-17). Partly in order to evaluate the effects of the 1998 reform, this survey was then repeated in the same calendar month in subsequent years. Results, reported 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 between 1997 and 1998, giving credibility to the idea of a causal effect. 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. 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

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

impacts on the ability of 15-year-olds to purchase alcohol in stores. However, consumption itself was less affected. There was a slight reduction in the probability of having had three or more drinks in the last month among those aged 15, as compared to increases or no changes in the adjacent age groups. However, for the probability of having had one or two drinks, trends appear very similar for 15-year-olds as for other age groups. Similar conclusions can also be drawn from surveys administered by the National Health Board (Sundhedsstyrelsen 2010). A possible explanation for the seemingly weak effect of this reform on consumption is that those aged 15 had parents and peers willing to offer them 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; 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. The age of criminal responsibility is 15; crimes committed at younger age are recorded but not punishable.

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 different sources. Our analyses exploit all individuals between 12 and 20 years of age.

We make use of the Patient Register, which includes 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 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. We also obtain information on own schooling, and focus on secondary schooling since individuals may not have finished their potential tertiary education at the time of the study. We distinguish between individuals with academic high school education, and those with either vocational high school education or no secondary education.

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.

2.3 Method

Investigating effects on short-term outcomes, our analysis exploits differences across time and across age groups in terms of whether alcohol can be legally purchased. We first calculate the number of hospital visits that each individual has, at a certain age as expressed in months. Based on the date at the time the individual entered this monthly age, we create cells based on age (in months) and time (in months), and calculate average outcomes within these cells.[4] The following type of difference-in-differences regression is run.

$$(1)\ y_{at} = Prohibited_{at} + Age_a + Time_t + \varepsilon_{at}$$

In this equation, y is the average number of visits in the age*time cell at. $Prohibited_{at}$ is the treatment dummy, indicating that the individual is not allowed to buy alcohol at off-premises. Indeed, this variable equals one for individuals aged 14 or younger after the first reform, and for individuals aged 15 or younger after the second.[5] We use dummies to control for each age in months, and time in months. Standard errors are clustered at the monthly level.

Depending on the specification, we limit attention to either to two years before and two years after each policy change, or to one year before and one year after. In some specifications, we also limit attention to age groups right around the age threshold that is affected. Exploiting

[4] By aggregating data, we take into account correlations between outcomes occurring within the same cell (here, outcomes occurring during the same month, for individuals born during the same month). Results based on individual data are, however, very similar.

[5] We delete observations where the month of observation overlaps with the reform, since the *Prohibited* indicator is not well-defined for all individuals here.

the third policy change, a corresponding regression is run, but with *Prohibited* indicating that the individual is not allowed to buy stronger alcoholic beverages (spirits). Since at most two years around a reform are used, there is no overlap across reforms.

Several hospital outcomes are studied. First, we examine effects on injuries, which reflect risk-taking. We then focus attention on alcohol-related conditions, i.e., effects that immediately follow from alcohol consumption, including 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 follow Callaghan et al. (2013) and consider hospital visits due to appendicitis as a placebo check, since appendicitis is a condition for which we would not expect any effects of alcohol (Sauerland et al. 2004).[6]

Following this analysis of short-term (immediate) effects of alcohol access, we subsequently consider potential 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.

We here apply an RK design, exploiting the fact that the number of months an individual (at a certain age) has been prohibited from buying alcohol develops linearly with birth time, but with a reform-induced change in the slope at certain birth time points. In particular, for an individual born up until June 1983, there are no months during which alcohol could not be purchased. This is because the first reform is only launched in 1998 and individuals born up to June 1983 have already exceeded the 15-year age limit by the time of its implementation. As a result of the first reform, however, individuals born in July 1983, have up to one month (July 1998) during which they are not allowed to buy alcohol. Furthermore, individuals born in August 1983 have two months (July-August 1995), and so on, in a fashion that depends linearly on birth time.

While the third reform, in 2011, is too recent to be exploited in an analysis of long-term outcomes, the second reform, in 2004, gives rise to two cutoffs: June/July 1988 and June/July 1989. Individuals born up to June 1988 can buy alcohol from their 15th birthday and onwards. Those born in July 1988 can still buy alcohol from their 15th birthday, but not for some part of

[6] The ICD10 codes for the conditions we study are: S-T, except T51 (injuries, poisonings, and external causes, except alcohol poisonings); F10 and T51 (psychological or psychomotoric effects of alcohol and alcohol poisoning); and K35-K37 (appendicitis).

July 2004, prior to their 16th birthday. Similarly, those born in August 1988 have up to two months prior to their 16th birthday when alcohol cannot be purchased, and so on.

For the reasons just described, the number of months an individual can purchase alcohol varies linearly with birth time up to June 1989, with individuals born one month later having one month less during which alcohol can be bought prior to the 16th birthday. However, from birth times of July 1989 and onwards, everyone can buy alcohol only from age 16, giving rise to a change of the slope in the relationship between birth time and months where alcohol purchases are possible.[7]

Exploiting the cutoff induced by the first reform ("cutoff 1"), we consider individuals' total number of hospital visits, due to certain conditions, between age 18 and 28. We also examine if an individual has died by age 28. Finally, we ask whether an individual has completed academic high school by age 21 (most individuals undergoing academic high school would have finished already by age 19 or 20). For the cutoffs induced by the second reform ("cutoff 2a" and "cutoff 2b"), we carry out a similar analysis but we consider hospital visits only up to age 23 and 22, respectively, and mortality up to these ages.

Collapsing outcomes at the day-of-birth level, regressions of the equation (2) type are run. The underlying assumption is that, around the relevant birth time cutoffs, outcomes develop according to certain trends. On top of this, the time an individual has been allowed to buy alcohol (or equivalently, not allowed) has a potential impact on outcomes.

$$(2)\ y_c = \alpha + \beta_1(BTime_c - T) + \beta_2 D_c(BTime_c - T) + \gamma D_c + \varepsilon_c$$

Here, c is index for birth time and y is the average number of visits among individuals born this day. $BTime_c - T$ is the number of days from the cutoff date (e.g., July 1, 1983) that the individual was born, and D_c is an indicator assuming the value of one if the individual was born after the cutoff. In some specifications, we also add terms that are quadratic in $BTime_c - T$, to capture nonlinear trends influencing the outcome. The interest lies in whether the coefficient β_2 is significant, as this would suggest an impact of the exposure to alcohol. When exploiting the cutoff 2b, we are also interested in whether γ is different from zero, as this would suggest

[7] Of course, the reform in 1998 is also relevant for individuals born around the 1988 and 1989 cutoffs. However, this reform has the same linear impact on the number of months an individual can buy alcohol on both sides of the cutoffs. Moreover, these cohorts were only 8-10 years of age in the summer of 1998, ages at which alcohol is hardly ever bought.

an effect of the youngest age at which the individual can purchase alcohol in adolescence (15 or 16). Individuals born right before this threshold could start purchasing alcohol at age 15, whereas those born right after could only start purchasing at age 16.

We use mean square error optimal bandwidths (Calonico et al. 2014) and as a robustness exercise we try bandwidths that are either half or twice as large as these.[8] However, when exploiting cutoff 2a we at most include individuals born up to 12 months after it, and when exploiting cutoff 2b we at most include individuals born up to 12 months before it, since the data window would otherwise include more than one cutoff. Observations around cutoffs are weighted triangularly, thus giving more weight to observations that are close to the cutoff.

3. Results

3.1 Graphical evidence on short-term results

We here show graphs displaying patterns of hospital visits due to injuries as well as alcohol-related conditions around the relevant reforms. Beginning with injuries, Figure 1 (panels a-c) shows visits at different ages two years before and two years after each of the three reforms. Following the first reform, individuals below age 15 display a clear drop in hospital visits due to injuries. Among individuals aged 13 and 14, for example, the average number of monthly injury-related visits is around 26 per thousand individuals prior to the reform. After the reform, this number reduces to 23, a reduction by roughly ten percent. Among children aged twelve, the number of injury-related visits is smaller and the impact of the reform appears less substantial, in line with the idea that this age group consumed little alcohol even before the policy change. Among individuals aged 15 to 20, injuries also tended to move downward, but much less than in the age group affected by the policy change. Compared to the age groups 16-20, however, the drop among 15-year-olds is more pronounced, perhaps as a result of social interaction or of a greater awareness among parents (and children) of the risks associated with alcohol consumption in early teenage years. Our results for the first reform line up with Møller (2002), who established reduced drinking after the implementation of the reform, especially among children in grades 5-7.

The potential impacts of the second reform, raising the age limit from 15 to 16, are then

[8] In general, the choice of bandwidth involves a trade-off between accuracy and efficiency, where a larger bandwidth may lead to more bias. To account for this, one can apply the bias-corrected, robust confidence interval RK estimator, described by Calonico et al. (2014). Results, available upon request, are similar to the conventional estimates.

illustrated in panel b. Here, it is difficult to discern any impact of the policy change. There is a clear drop in injuries among 15-year-olds, but similar drops can be seen for most other age groups as well. These results are also rather expected, as previous literature (Jørgensen et al. 2006; Sundhedsstyrelsen 2010) indicate little impacts on alcohol consumption.

Possible effects of the third reform are shown in panel c. Here, we may expect a reduction in injuries among 16- and 17-year-olds, as the limit for buying sprits was raised from 16 to 18. While the figure shows that the injury rate clearly reduced in these age groups (from about 23 to 21 per thousand individuals and month), there are reductions in other ages too, and it is not entirely clear whether the drop among individuals affected by the reform is larger than the drop among other individuals.

Panels d-f display the development of hospital visits due to alcohol-related conditions around the respective reforms. These events are much less common, with rates only about a hundredth of those of injuries. Nevertheless, there is some evidence that the first and third reforms had impacts on these rare 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 third reform, rates appear to have decreased somewhat among 16-year-olds, whereas among older individuals that were unaffected by the reform, an increase can be seen. Again, no effect of the second reform appears to be present.

In Appendix Figure 1, we display the corresponding graphs for our additional outcomes: mortality and appendicitis (the placebo outcome). In the young ages that we study, mortality rates are low, and monthly averages fluctuate heavily due to the few occurrences. It is hard to detect any mortality impacts of the reforms. Hospitalizations due to appendicitis are more common but there also appears to be no evidence that availability of alcohol, as induced by the reforms, would be related to the occurrence of these.

3.2 Short-term results: Main regressions

We now consider regression results for the same outcomes as above. Throughout, we display results based on different parts of the sample: 1) a specification including two years before and two years after the reform, using all ages 12-20; 2) a specification including only one year before and one year after the reform, using all ages 12-20; 3) specifications including two years before and two years after the reform, using the age groups (in years) immediately around age limits(s) that were changed by the reform; 4) specifications including only one year before and one year after the reform, using the age groups (in years) immediately around age

limits(s) that were changed by the reform; and 5) A specification similar to 1 but where we also allow for the possibility of a lagged effect, i.e., the effect of having been able to purchase alcohol one year ago. The lagged effect captures the possibility of effects such as learning, habit formation, and dependence. In general, the specifications (2)-(4) can be seen as sensitivity checks where we, on the one hand, try to make treatment and control groups more similar to each other but, on the other, exploit less variation. Additionally, peer effects may be more of an issue in specifications where control individuals are limited to those that are close in age. For example, if treated 14-year-olds reduce their drinking, 15-year-olds may do so as well. As a result, specifications which only include control individuals that are close in age may generate lower bound estimates.

Table 1 reports the results for injuries. Our first specification suggests an effect of -1.5 injuries per month and thousand individuals when alcohol cannot be purchased, an effect that is significant at the 1 percent level, and corresponds to roughly 6 percent of the average in any age group. Indeed, the figure presented before suggested that injuries among the treated were around 10 percent fewer after the reform, but part of this difference can be explained by trends because non-treated exhibited a reduction as well.

In the second specification, we limit ourselves to one year before and one year after the reform, in order to reduce the potential influence of differential trends. We find an effect of -1.4, which is very similar to the one found before, but whereas we previously obtained statistical significance at the 1 percent level, we only have significance at the 10 percent level. Including individuals only at the ages around the threshold (14- and 15-year-olds) also produces quite similar but smaller estimates, as displayed in column 3 and 4. However, the estimate itself turns insignificant in column 4, where attention is limited to one year before and one year after the policy change. Finally, model 5 provides no evidence that availability of alcohol 12 months earlier would have a direct impact, suggesting that dependence or habit formation is not important for this outcome.

In panel B, we provide the corresponding results pertaining to the second reform. In line with the graphical evidence presented before, there is little evidence that injuries were affected, and all estimates are statistically insignificant. Moreover, all confidence intervals are quite tight around zero and, by applying a t-test, an effect of the size indicated in panel A can generally be ruled out.

Panel C displays the results based on reform three. Model 1, using observations up to two years before and two years after the reform, suggests an effect of -0.5 and this is significant at the 10 percent level. This corresponds to about 2-2.5 percent of average injuries per month.

The effect is clearly smaller than the one obtained for reform one, but not distinguishable from estimates based on reform two. When we limit the sample (models 2-4) we find no significant effects, however, but the power is also lower. Again, there is no evidence of a lagged impact.

We then consider effects on alcohol-related conditions in Table 2. Like for injuries, our first specification suggests that alcohol availability is related to more harmful outcomes, with an estimate that is significant at the 5 percent level. We find that being prohibited from buying alcohol reduces alcohol-related conditions by 0.049 per month and 1,000 individuals, an effect that corresponds to 27 percent of the average of 0.18 in ages below 15. Other models yield lower precision, turning estimates insignificant. When we add a lagged indicator, we find that the overall effect is driven by alcohol availability one year earlier, pointing at the importance of habit formation and dependence. The existence of a lagged effect contrasts with the previous results for injuries, perhaps because of differences in compliers' relationships with alcohol, where individuals that become alcohol dependent are particularly likely to experience intoxication and alcohol poisoning in the long run, but may be able to avoid injuries.

Again, there is no evidence of an impact of the second reform but instead of the third. Being unable to purchase spirits is found to reduce alcohol-related conditions by 0.044, which corresponds to about 8 percent of the average among 16- and 17-year-olds. Interestingly, adding a lagged indicator produces an estimate for lagged availability that has the opposite sign, but roughly the same size. The result suggests that the availability of spirits only has an impact during the first year and that after this initial burst of alcohol-related conditions, individuals are able to drink spirits somewhat more responsibly (even though injuries still appear to be affected).

Appendix Table 1 displays results for mortality. There is essentially no evidence of impacts on this outcome, and all coefficients but one (the lagged effect of reform 3) are statistically insignificant. It should be noted, however, that mortality is very low in these ages, making effects harder to detect. In Appendix Table 2, we show results for our placebo outcome appendicitis. Again, there is virtually no evidence of any effects, but likely due to chance, one estimate (reform 1, model 4) is significant at the 5 percent level.

3.3 Interactions with 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. In this way, we shed light on the distributional effects of restrictions on alcohol availability, and in particularly whether MLDAs could improve outcomes among the socioeconomically

disadvantaged. There are several reasons why differential effects might emerge. First, in families of higher income, there may be more resources available for 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 by home production. We can only speculate about whether such behavior varies by socioeconomic status.

In Denmark, alcohol consumption has been rather similar across the genders and the responses to the first reform on alcohol consumption appear similar as well (Møller 2002). 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 3, 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 create cells based on combinations of age (in month) and time (in month) and proceed as in the main analysis, but with four parallel regressions.

We find limited evidence that effects vary by parental income. For the effects of the first reform on injuries, estimates are similar and statistically significant for all quartiles but the second. The estimate for quartile two is just slightly smaller and statistically indistinguishable.[9] For reform 3, we fail to obtain statistical significance in any quartile, but all estimates are similar to the overall effect and statistically indistinguishable from each other.

[9] We determine whether two estimates are statistically different by applying a t-test of the following form: $(b_1 - b_2)/\sqrt{s_1^2 + s_2^2}$.

For alcohol-related conditions, significant effects of the first reform only arise in the third and fourth quartile, and the one in the fourth quartile is significantly larger than in the second. The second reform (whose overall effect was insignificant) produces an unexpected positive effect, but this might be falsely significant, a result which would be unsurprising given the large number of regressions performed. The third reform significantly reduces alcohol-related conditions only in quartile two.

Having found no consistent evidence of differential effects by economic resources, we then in Table 4 consider effects by 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 clear evidence that effects would differ across socioeconomic groups, as measured by education. For injuries and the first reform, effects are very similar across groups when stratifying either by mother's or father's education. For the second reform, effects are always insignificant. When considering the third reform, however, we find that the effect is only significant when the father has no more than primary schooling, and this estimate is significantly larger than the case where the father has tertiary schooling.

The effects of the first reform on alcohol-related conditions are only significant when the father has tertiary education or when the mother has secondary education, and the latter effect is clearly larger than when the mother has primary education only. In contrast to the results for injuries, effects of the third reform on alcohol-related conditions are only significant when either the mother or father has tertiary education. In the case of the father, this significant effect is clearly larger than the effect among those with lower education.[10]

In Table 5, we look at effects by gender. The effects of the first reform on injuries is very similar for males and females. However, the third reform, affecting spirits consumption in older age groups, has clearly stronger effects on men, and no significant effect on women. Finally,

[10] In Tables 3-4, we included all individuals between age 12 and 20, and this was partly done to ensure comparability with our main results. 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 available may in fact represent some (unknown) weighted sum of parental and own income. We therefore instead exclude individuals aged 18 and older, and re-run the analyses. The results based on this exercise, displayed in Appendix Tables 3-4, are rather similar to the ones above.

in panels D-F concerning alcohol-related conditions, reform one and three have significant effects on men only, but the estimates are not significantly larger than the ones for women.

3.4 Long-term results

We now turn to asking if the length of the time span that an individual has been prohibited from purchasing alcohol also has a long-term impact on health and human capital accumulation, an issue that we explore using an RK design. First, in Figure 2, we display the development of injuries as well as alcohol-related conditions around the cutoffs previously described. A linear polynomial is fit, using the optimal bandwidth. As shown in Figure 2a, individuals born further to the right of the first cutoff tend to experience fewer injuries at age 18-28, as indicated by change in the slope of the regression line. In other words, the longer an individual was prohibited from purchasing alcohol after the first reform was implemented and when they were not yet 15 years of age, the less damaging consequences in the long run would be. The pattern is not entirely clear, however, and it appears to depend on the choice of bandwidth. For the other cutoffs or for alcohol-related conditions, it is more difficult to discern any changes in the slopes of the regression lines. There is, however, a relatively large upward jump in the *level* of alcohol-related conditions after the cutoff 2b, a finding that we return to when carrying out the regressions below. In Figure 3, we show results for education, indicating that individuals born longer after cutoffs 1 and 2b have a higher chance of finishing academic high school.

In Tables 6-8, we then report results for the regressions examining long-term effects on the same outcomes as shown in the figures. There is, indeed, an effect of the first cutoff on injuries when using the optimal bandwidth. The estimate from a specification based on a linear polynomial suggests that for every month an individual is prohibited from purchasing alcohol, injuries at age 18-28 reduce by 0.1, an effect that corresponds to about 4 percent of the average. Using a quadratic polynomial makes the effect larger but less precisely estimated. There is, however, no significant evidence of an effect when using bandwidths that are larger or smaller than the optimal one and, in general, no evidence that the other cutoffs would have had an impact.

In addition to the number of months prohibited from buying alcohol, it is possible that the exact age in adolescence at which the individual was first able to buy alcohol has an impact on long-term outcomes. We can exploit this issue by applying a standard regression discontinuity (RD) design around cutoff 2b, as this generates a jump from 15 to 16, in terms of the earliest age in adolescent that the individual could purchase alcohol. However, we find no evidence of

an effect on injuries when applying RD design.

In line with the graphical evidence presented before, there is no evidence that the number of months prohibited from buying alcohol would have had an effect on alcohol-related conditions in the long run. As Figure 2f also suggests, however, there is some evidence that delaying the potential alcohol debut from 15 to 16 influences long-term alcohol-related conditions. The effect is significant in three out of the six specifications and is, somewhat surprisingly, positive. For education, four specifications suggest that the first cutoff generated a higher propensity of finishing academic high school among those with a lower ability to buy alcohol, but there is also one specification generating a significant result in the opposite direction.

As before, we have considered effects on mortality and, as a placebo outcome, appendicitis. The results are placed in Appendix Tables 5-6 and in Appendix Figure 2. We find one significant effect of the cutoff 2a on appendicitis and several of cutoff 2b, indicating that there may be other trends changing around the same points in time. This would have been concerning, had we found strong effects of the same cutoffs on other outcomes.

4. Conclusions

Previous research on the impacts of MLDA legislation originates mostly from North America. These studies focus on a limited set of outcomes (mostly traffic crashes), often lack sufficient power, and have limited external validity due to the selected samples. There is scarce evidence from settings outside of these, 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 strong assumption of no adjustment effects when alcohol becomes available. Further, these studies only estimate local treatment effects only around a certain threshold (21 years).

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 measured by the probability of completing academic high school. Thus, we bring evidence on both short- and on long-term outcomes. The target group of the reforms in the Danish case is youths aged 15-18, enabling us to provide estimates for a younger age interval than what previous studies have been able to consider.

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. Exploiting both difference-in-differences and regression kink approaches, our results show substantial reductions in both injuries and alcohol-related conditions when the minimum purchase age of 15 was implemented, with injuries dropping by 6 percent and alcohol-related conditions by 27 percent in the affected age groups. We find no effects of the second reform where the minimum purchase age was subsequently increased to 16. One explanation for this lack of finding may be the Danish drinking culture, where an alcohol debut from age 15 is generally accepted and where parents may even provide their children alcohol. The third reform, raising the age for buying stronger beverages (spirits) to age 18, appears to have had effects on both injuries and alcohol-related conditions, but only among males. However, the effects are much weaker than those of the first reform, again pointing at the importance of the introduction of the MLDA rather than its adjustment. Estimates for long-term outcomes indicate that individuals who were less able to buy alcohol in youth due to the introduction of the MLDA ended up with fewer injuries in young adulthood, and may have been more likely to obtain an academic education.

The short-term effects of reform one on injuries and alcohol-related conditions are similar but somewhat larger than those of Callaghan et al. (2013), who find a 16 percent increase in alcohol-related conditions and a 3 percent increase in a broad class of injuries when individuals became old enough to buy alcohol. 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. For alcohol-related conditions, we find that substantial adjustment effects appear to operate after individuals become eligible to buy alcohol, casting doubt on studies only comparing outcomes within a limited interval around an age eligibility threshold. We can also compare our results on alcohol-related hospitalizations with those of Marcus and Siedler (2015), who exploit a German reform that prohibited alcohol purchases only during certain hours. The effect of the introduction of the Danish MLDA appears several times larger, as their estimate suggests an effect of around 7 percent among those aged 15-19 years.

Denmark is a country with an extraordinarily liberal alcohol culture, where children historically have been able to purchase alcohol at stores without restrictions, and where many children below the subsequently implemented MLDA were still been able to obtain alcohol from various sources. Thus, the effects of restrictions on alcohol purchase in Denmark possibly provide a lower bound on what can be expected elsewhere. Using similar data and methods as

this study, researchers should try to explore whether in other countries even stronger effects are found on injury, harm, and human capital formation when MLDAs were changed or implemented.

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Table 1: Injuries (monthly per 1,000 individuals)

	(1)	(2)	(3a)	(4a)	(3b)	(4b)	(5)
Panel A: Reform 1							
Prohibited	-1.524***	-1.373*	-1.045***	-0.879			-1.302***
	(0.484)	(0.718)	(0.367)	(0.605)			(0.461)
Prohibited_1							-0.261
							(0.585)
Panel B: Reform 2							
Prohibited	0.063	-0.082	0.038	0.460	0.576	0.082	0.116
	(0.331)	(0.485)	(0.279)	(0.400)	(0.405)	(0.618)	(0.339)
Prohibited_1							-0.563
							(0.441)
Panel C: Reform 3							
Prohibited	-0.492*	-0.276	-0.385	-0.093	-0.209	-0.146	-0.494*
	(0.271)	(0.242)	(0.315)	(0.371)	(0.343)	(0.481)	(0.271)
Prohibited_1							-0.014
							(0.318)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation 1.
Specifications in column (1) exploit two years before and after each policy change
Specifications in column (2) exploit one year before and one year after each policy change
Specifications in columns (3) exploit two years before and two years after each policy change but only include age groups (in years) right at the relevant threshold
Specifications in columns (4) exploit one year before and one year after each policy change but only include age groups (in years) right at the relevant threshold
Specification (5) is similar to specification (1), but adds a treatment indicator lagged by one year
In panel B, specifications 3a and 4a refer to the 14/15 age threshold whereas specifications 3b and 4b refer to the 15/16 threshold
In panel C, specifications 3a and 4a refer to the 15/16 age threshold whereas 3b and 4b refer to the 18/19 threshold

Table 2: Alcohol-related conditions (monthly per 1,000 individuals)

	(1)	(2)	(3a)	(4a)	(3b)	(4b)	(5)
Panel A: Reform 1							
Prohibited	-0.049**	-0.033	-0.033	-0.113			-0.026
	(0.023)	(0.033)	(0.058)	(0.074)			(0.025)
Prohibited_1							-0.049*
							(0.028)
Panel B: Reform 2							
Prohibited	0.009	-0.003	-0.013	-0.014	-0.007	-0.009	0.007
	(0.037)	(0.059)	(0.037)	(0.058)	(0.049)	(0.076)	(0.038)
Prohibited_1							-0.012
							(0.040)
Panel C: Reform 3							
Prohibited	-0.044*	-0.056	-0.026	-0.041	-0.065	-0.064	-0.054**
	(0.025)	(0.039)	(0.033)	(0.042)	(0.044)	(0.054)	(0.024)
Prohibited_1							0.061*
							(0.033)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation 1.
Specifications in column (1) exploit two years before and after each policy change
Specifications in column (2) exploit one year before and one year after each policy change
Specifications in columns (3) exploit two years before and two years after each policy change but only include age groups (in years) right at the relevant threshold
Specifications in columns (4) exploit one year before and one year after each policy change but only include age groups (in years) right at the relevant threshold
Specification (5) is similar to specification (1), but adds a treatment indicator lagged by one year
In panel B, specifications 3a and 4a refer to the 14/15 threshold whereas specifications 3b and 4b refer to the 15/16 threshold
In panel C, specifications 3a and 4a refer to the 15/16 threshold whereas 3b and 4b refer to the 18/19 threshold

Table 3: Results by parental income (monthly per 1,000 individuals)

	First quartile	Second quartile	Third quartile	Fourth quartile
Panel A: Injuries, reform 1				
Prohibited	-1.702***	-1.029	-1.711***	-1.112**
	(0.632)	(0.615)	(0.614)	(0.537)
Panel B: Injuries, reform 2				
Prohibited	0.257	0.248	-0.621	0.200
	(0.602)	(0.570)	(0.677)	(0.677)
Panel C: Injuries, reform 3				
Prohibited	-0.230	-0.501	-0.281	-0.817
	(0.491)	(0.420)	(0.421)	(0.489)
Panel D: Alcohol-related conditions, reform 1				
Prohibited	-0.070	0.012	-0.080*	-0.114***
	(0.055)	(0.051)	(0.040)	(0.039)
Panel E: Alcohol-related conditions, reform 2				
Prohibited	-0.051	-0.038	-0.090	0.137**
	(0.094)	(0.074)	(0.070)	(0.065)
Panel F: Alcohol-related conditions, reform 3				
Prohibited	-0.081	-0.132**	0.041	-0.025
	(0.072)	(0.056)	(0.052)	(0.071)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications exploiting two years before and after each policy change.

Table 4: Results by parental education (monthly per 1,000 individuals)

	Father primary	Father secondary	Father tertiary	Mother primary	Mother secondary	Mother tertiary
Panel A: Injuries, reform 1						
Prohibited	-1.325**	-1.596***	-1.853***	-1.340**	-1.573***	-1.600***
	(0.537)	(0.579)	(0.528)	(0.528)	(0.505)	(0.573)
Panel B: Injuries, reform 2						
Prohibited	-0.015	0.054	0.386	-0.515	0.012	0.562
	(0.547)	(0.454)	(0.558)	(0.418)	(0.388)	(0.536)
Panel C: Injuries, reform 3						
Prohibited	-1.059**	-0.479	0.173	-0.545	-0.502	-0.229
	(0.428)	(0.322)	(0.353)	(0.385)	(0.301)	(0.386)
Panel D: Alcohol-related conditions, reform 1						
Prohibited	-0.024	-0.045	-0.083*	-0.007	-0.074***	-0.055
	(0.037)	(0.027)	(0.043)	(0.039)	(0.027)	(0.039)
Panel E: Alcohol-related conditions, reform 2						
Prohibited	-0.019	0.001	0.021	0.014	-0.004	0.070
	(0.069)	(0.051)	(0.066)	(0.062)	(0.050)	(0.060)
Panel F: Alcohol-related conditions, reform 3						
Prohibited	-0.035	-0.002	-0.154***	-0.063	-0.015	-0.081*
	(0.046)	(0.033)	(0.044)	(0.045)	(0.036)	(0.047)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications exploiting two years before and after each policy change.

Table 5: Results by gender (monthly per 1,000 individuals)

	Male	Female
Panel A: Injuries, reform 1		
Prohibited	-1.525***	-1.541**
	(0.498)	(0.579)
Panel B: Injuries, reform 2		
Prohibited	-0.064	0.238
	(0.408)	(0.426)
Panel C: Injuries, reform 3		
Prohibited	-0.889**	-0.074
	(0.337)	(0.295)
Panel D: Alcohol-related conditions, reform 1		
Prohibited	-0.068**	-0.029
	(0.029)	(0.026)
Panel E: Alcohol-related conditions, reform 2		
Prohibited	-0.006	0.027
	(0.050)	(0.054)
Panel F: Alcohol-related conditions, reform 3		
Prohibited	-0.065*	-0.020
	(0.035)	(0.032)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications exploiting two years before and after each policy change.

Table 6: Long-term effects on injuries

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reform 1 (cutoff at the birth date of July 1, 1983)						
Months	-0.110**	-0.022	0.021	-0.210*	-0.057	0.148
prohibited	(0.046)	(0.019)	(0.141)	(0.107)	(0.043)	(0.327)
Bandwidth	3.987	7.974	1.994	5.431	10.862	2.716
Panel B: Reform 2, cutoff 2a (cutoff at the birth date of July 1, 1988)						
Months	-0.018	-0.012*	-0.058	-0.044	-0.010	-0.148
prohibited	(0.018)	(0.007)	(0.056)	(0.049)	(0.019)	(0.151)
Bandwidth	5.552	11.104	2.776	6.922	13.844	3.461
Panel C: Reform 2, cutoff 2b (cutoff at the birth date of July 1, 1989)						
Months	0.011	0.008	-0.003	0.005	0.026*	-0.024
prohibited	(0.017)	(0.006)	(0.041)	(0.032)	(0.014)	(0.075)
Bandwidth	5.057	10.114	2.528	8.499	16.998	4.250
Panel D: Reform 2, cutoff 2b: being able to purchase at age 16 rather than at age 15 for the first time						
Age	0.047	0.010	-0.012	0.059	0.030	-0.049
allowed	(0.037)	(0.027)	(0.045)	(0.042)	(0.034)	(0.054)
Bandwidth	5.941	11.882	2.970	8.729	17.458	4.364

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (2), with specifications in columns (1) - (3) using linear trends below and above the cutoffs, whereas specifications in columns (4) - (6) use quadratic trends below and above the cutoffs. In columns (1) and (4) we use optimal bandwidths according to the criterion by Calonico et al. (2014). Specifications in columns (2) and (5) use twice as large, whereas specifications in columns (3) and (6) use half as large bandwidths. Panels A-C exploit the number of months an individual is prohibited from buying alcohol, using an RK design, whereas panel D applies an RD design to investigate the potential role of the youngest age at which the individual was allowed to purchase alcohol. In panel A, we consider the number of incidents between age 18 and 28, in panel B the number of incidents between age 18 and 23, and in panels C-D the number of incidents between age 18 and 22. Bandwidths displayed in specifications (2) and (5) are sometimes cut (in one direction) to avoid overlaps with other cutoffs. Specifically, bandwidths above the *cutoff* can be no higher than 12 when exploiting the second cutoff and bandwidths below the *cutoff* can be no higher than 12 when exploiting the third cutoff.

Table 7: Long-term effects on alcohol-related conditions

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reform 1 (cutoff at the birth date of July 1, 1983)						
Months	-0.002	-0.000	-0.007	-0.001	-0.000	-0.012
prohibited	(0.005)	(0.002)	(0.013)	(0.008)	(0.003)	(0.022)
Bandwidth	3.759	7.518	1.880	6.444	12.888	3.222
Panel B: Reform 2, cutoff 2a (cutoff at the birth date of July 1, 1988)						
Months	0.001	0.000	-0.009	0.001	0.001	-0.021
prohibited	(0.004)	(0.001)	(0.009)	(0.010)	(0.003)	(0.020)
Bandwidth	4.097	8.194	2.048	5.828	11.556	2.914
Panel C: Reform 2, cutoff 2b (cutoff at the birth date of July 1, 1989)						
Months	-0.001	-0.000	-0.008	-0.003	-0.001	-0.017
prohibited	(0.003)	(0.001)	(0.008)	(0.006)	(0.002)	(0.014)
Bandwidth	3.815	7.630	1.908	6.324	12.648	3.162
Panel D: Reform 2, cutoff 2b: being able to purchase at age 16 rather than at age 15 for the first time						
Age	0.008	0.003	0.016**	0.011*	0.005	0.024***
allowed	(0.005)	(0.003)	(0.007)	(0.006)	(0.004)	(0.009)
Bandwidth	4.587	9.174	2.294	6.882	13.764	3.441

Standard errors in parentheses

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

Regressions are run according to Equation (2), with specifications in columns (1) - (3) using linear trends below and above the cutoffs, whereas specifications in columns (4) - (6) use quadratic trends below and above the cutoffs. In columns (1) and (4) we use optimal bandwidths according to the criterion by Calonico et al. (2014). Specifications in columns (2) and (5) use twice as large, whereas specifications in columns (3) and (6) use half as large bandwidths. Panels A-C exploit the number of months an individual is prohibited from buying alcohol, using an RK design, whereas panel D applies an RD design to investigate the potential role of the youngest age at which the individual was allowed to purchase alcohol. In panel A, we consider the number of incidents between age 18 and 28, in panel B the number of incidents between age 18 and 23, and in panels C-D the number of incidents between age 18 and 22. Bandwidths displayed in specifications (2) and (5) are sometimes cut (in one direction) to avoid overlaps with other cutoffs. Specifically, bandwidths above the cutoff can be no higher than 12 when exploiting the second cutoff and bandwidths below the cutoff can be no higher than 12 when exploiting the third cutoff.

Table 8: Long-term effects on the probability of obtaining an academic high school degree

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reform 1 (cutoff at the birth date of July 1, 1983)						
Months	0.010*	-0.006***	0.039**	0.047**	0.004	0.142***
prohibited	(0.006)	(0.002)	(0.018)	(0.019)	(0.006)	(0.055)
Bandwidth	4.067	8.134	2.034	4.616	9.323	2.308
Panel B: Reform 2, cutoff 2a (cutoff at the birth date of July 1, 1988)						
Months	-0.000	-0.001	-0.002	0.001	-0.006	-0.016
prohibited	(0.008)	(0.003)	(0.020)	(0.013)	(0.004)	(0.034)
Bandwidth	2.926	5.852	1.463	5.087	10.174	2.544
Panel C: Reform 2, cutoff 2b (cutoff at the birth date of July 1, 1989)						
Months	0.007	-0.002	0.023	0.021	-0.001	0.046
prohibited	(0.006)	(0.002)	(0.015)	(0.013)	(0.005)	(0.034)
Bandwidth	3.660	7.320	1.830	4.976	9.952	2.488
Panel D: Reform 2, cutoff 2b: being able to purchase at age 16 rather than at age 15 for the first time						
Age	0.001	-0.005	-0.012	-0.015	0.007	-0.014
allowed	(0.010)	(0.006)	(0.014)	(0.014)	(0.010)	(0.019)
Bandwidth	4.373	8.746	2.186	4.350	8.700	2.175

Standard errors in parentheses

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

Regressions are run according to Equation (2), with specifications in columns (1) - (3) using linear trends below and above the cutoffs, whereas specifications in columns (4) - (6) use quadratic trends below and above the cutoffs. In columns (1) and (4) we use optimal bandwidths according to the criterion by Calonico et al. (2014). Specifications in columns (2) and (5) use twice as large, whereas specifications in columns (3) and (6) use half as large bandwidths. Panels A-C exploit the number of months an individual is prohibited from buying alcohol, using an RK design, whereas panel D applies an RD design to investigate the potential role of the youngest age at which the individual was allowed to purchase alcohol. The outcome is an indicator for having an academic high school degree by age 21.

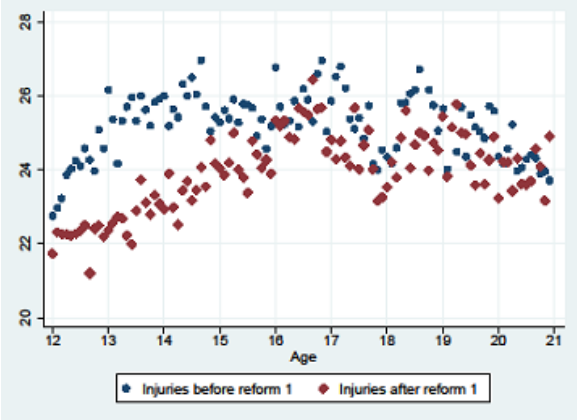

Figure 1a: Injuries around reform 1 (July 1, 1998)

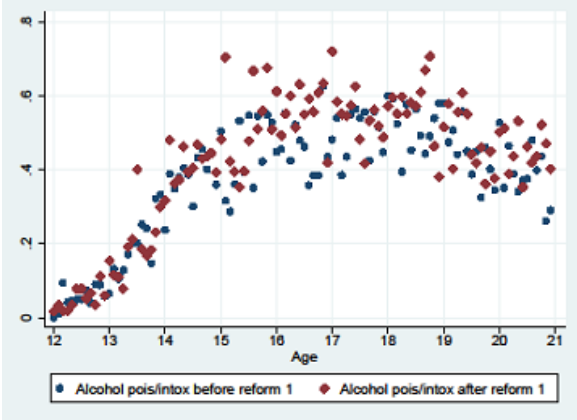

Figure 1d: Alcohol-related conditions around reform 1 (July 1, 1998)

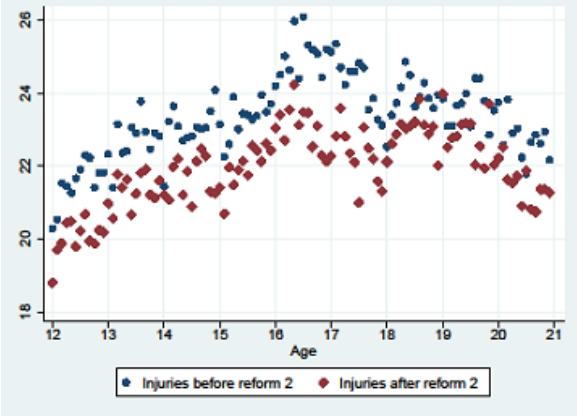

Figure 1b: Injuries around reform 2 (July 1, 2004)

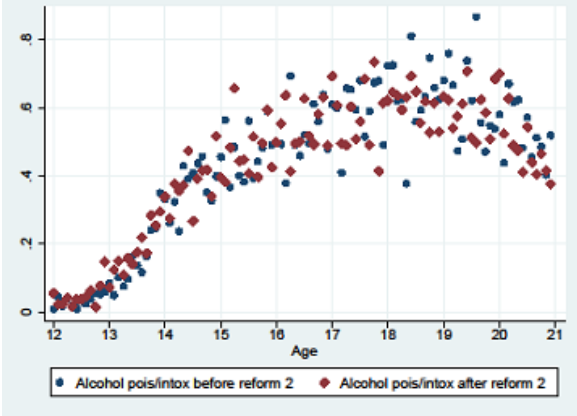

Figure 1e: Alcohol-related conditions around reform 2 (July 1, 2004)

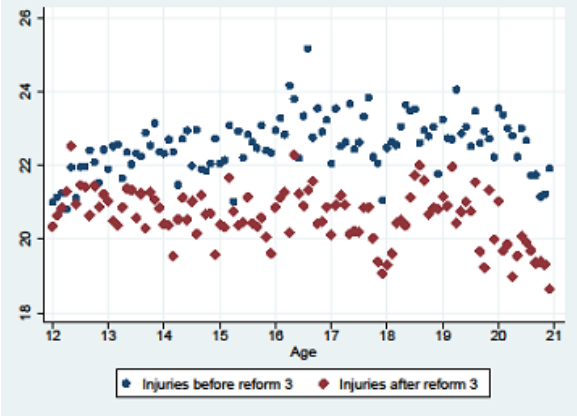

Figure 1c: Injuries around reform 3 (March 7, 2011)

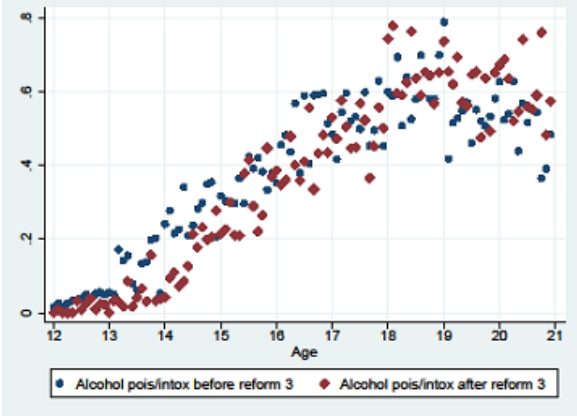

Figure 1f: Alcohol-related conditions around reform 3 (March 7, 2011)

Figure 1: Injuries (excluding alcohol poisoning) and alcohol-related conditions two years before and two years after each reform. Monthly rates per 1,000 individuals.

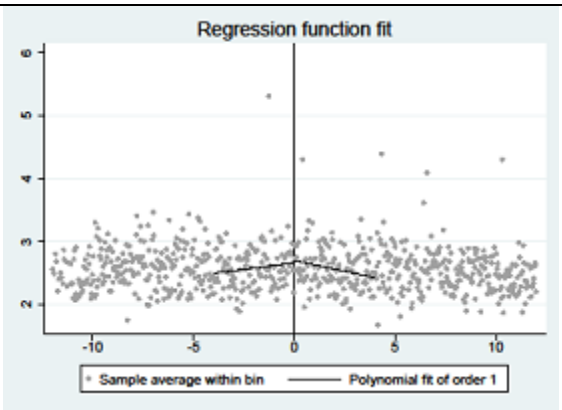

Figure 2a: Injuries around cutoff 1 (July 1, 1983)

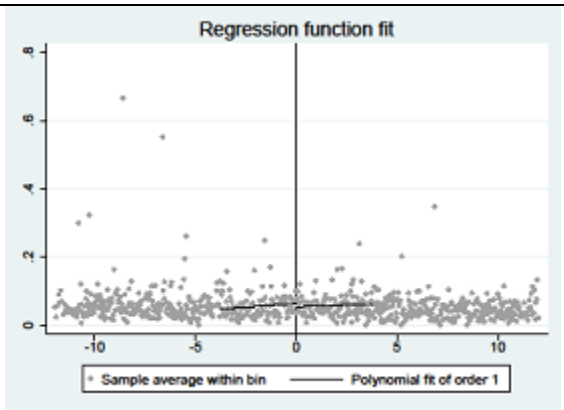

Figure 2d: Alcohol-related conditions around cutoff 1 (July 1, 1983)

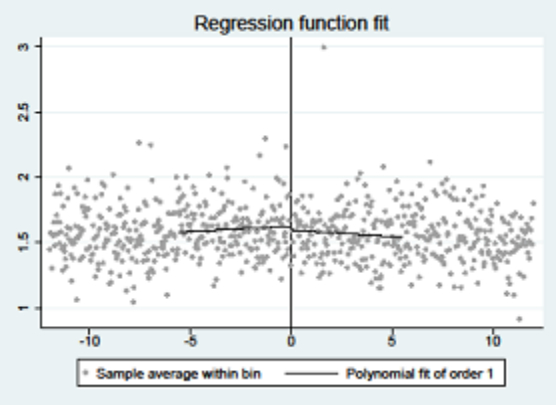

Figure 2b: Injuries around cutoff 2a (July 1, 1988)

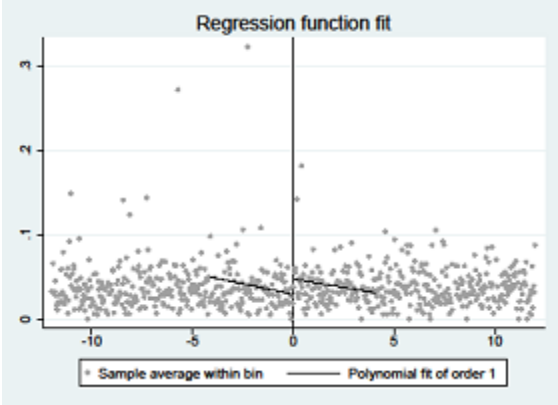

Figure 2e: Alcohol-related conditions around cutoff 2a (July 1, 1988)

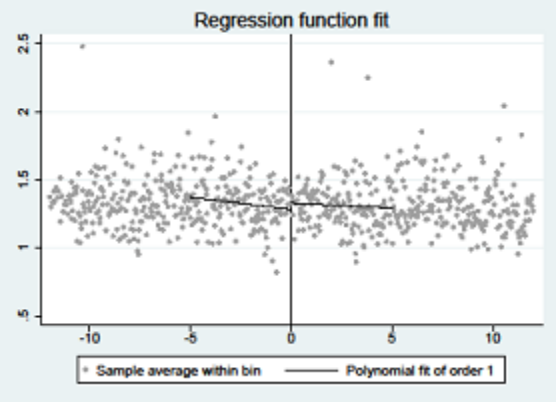

Figure 2c: Injuries around cutoff 2b (July 1, 1989)

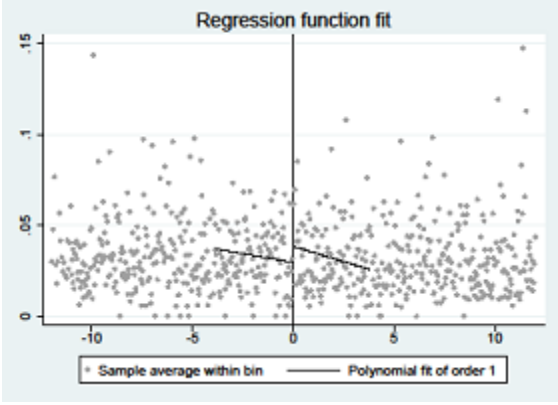

Figure 2f: Alcohol-related conditions around cutoff 2b (July 1, 1989)

Figure 2: Injuries (excluding alcohol poisoning) and alcohol-related conditions among individuals with different birth dates around the cutoffs generated by reform 1 and 2. Distances from cutoffs are expressed in months. Exploiting cutoff 1, we consider the number of incidents in ages 18-28, exploiting cutoff 2a, we consider the number of incidents in ages 18-23, and exploiting cutoff 2b, we consider the number of incidents in ages 18-22.

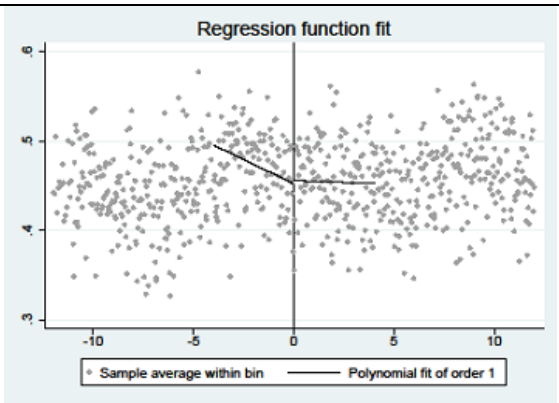

Figure 3a: Academic high school around cutoff 1 (July 1, 1983)

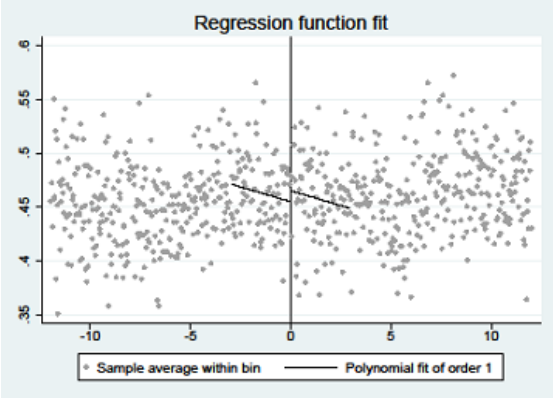

Figure 3b: Academic high school around cutoff 2a (July 1, 1988)

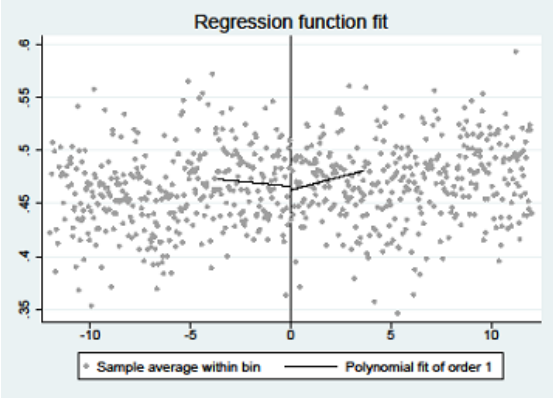

Figure 3c: Academic high school around cutoff 2b (July 1, 1989)

Figure 3: Share finishing academic high school among individuals with different birth dates around the cutoffs generated by reform 1 and 2. Distances from cutoffs are expressed in months. Exploiting cutoff 1, we consider the number of incidents in ages 18-28, exploiting cutoff 2a, we consider the number of incidents in ages 18-23, and exploiting cutoff 2b, we consider the number of incidents in ages 18-22.

Appendix Table 1: Mortality (monthly per 1,000 individuals)

	(1)	(2)	(3a)	(4a)	(3b)	(4b)	(5)
Panel A: Reform 1							
Prohibited	0.003	0.007	0.002	0.008			0.002
	(0.004)	(0.006)	(0.008)	(0.012)			(0.005)
Prohibited_1							0.000
							(0.005)
Panel B: Reform 2							
Prohibited	-0.001	0.008	0.000	0.011	-0.007	0.000	-0.001
	(0.004)	(0.005)	(0.007)	(0.008)	(0.007)	(0.007)	(0.004)
Prohibited_1							0.002
							(0.008)
Panel C: Reform 3							
Prohibited	-0.000	0.002	-0.004	-0.002	0.005	-0.001	0.002
	(0.004)	(0.006)	(0.006)	(0.007)	(0.009)	(0.012)	(0.004)
Prohibited_1							-0.010**
							(0.005)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications in column (1) exploiting two years before and after each policy change and specifications in column (2) exploiting one year before and one year after each policy change. Specifications in columns (3) exploit two years before and two years after each policy change but only include age groups (in years) right at the relevant threshold whereas specifications in columns (4) exploit one year before and one year after each policy change but only include age groups (in years) right at the relevant threshold. Specification (5) in similar to specification (1), but adds a treatment indicator lagged by one year. In panel B, specifications 3a and 4a refer to the 14/15 age threshold whereas specifications 3b and 4b refer to the 15/16 threshold. In panel C, specifications 3a and 4a refer to the 15/16 age threshold whereas 3b and 4b refer to the 18/19 threshold.

Appendix Table 2: Appendicitis

	(1)	(2)	(3a)	(4a)	(3b)	(4b)	(5)
Panel A: Reform 1							
Prohibited	-0.007	-0.011	-0.052	-0.095**			-0.002
	(0.014)	(0.015)	(0.033)	(0.042)			(0.014)
Prohibited_1							-0.018
							(0.018)
Panel B: Reform 2							
Prohibited	0.020	0.034	0.036	0.035	0.002	0.018	0.020
	(0.019)	(0.030)	(0.024)	(0.040)	(0.025)	(0.040)	(0.019)
Prohibited_1							0.013
							(0.024)
Panel C: Reform 3							
Prohibited	-0.015	-0.019	-0.021	0.011	-0.037	-0.074	-0.015
	(0.013)	(0.022)	(0.021)	(0.030)	(0.029)	(0.048)	(0.013)
Prohibited_1							0.005
							(0.048)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications in column (1) exploiting two years before and after each policy change and specifications in column (2) exploiting one year before and one year after each policy change. Specifications in columns (3) exploit two years before and two years after each policy change but only include age groups (in years) right at the relevant threshold whereas specifications in columns (4) exploit one year before and one year after each policy change but only include age groups (in years) right at the relevant threshold. Specification (5) in similar to specification (1), but adds a treatment indicator lagged by one year. In panel B, specifications 3a and 4a refer to the 14/15 age threshold whereas specifications 3b and 4b refer to the 15/16 threshold. In panel C, specifications 3a and 4a refer to the 15/16 age threshold whereas 3b and 4b refer to the 18/19 threshold.

Appendix Table 3: Results by parental income, excluding individuals aged 18 and older (monthly per 1,000 individuals)

	First quartile	Second quartile	Third quartile	Fourth quartile
Panel A: Injuries, reform 1				
Prohibited	-1.807***	-1.008*	-1.466**	-0.982*
	(0.567)	(0.563)	(0.620)	(0.528)
Panel B: Injuries, reform 2				
Prohibited	0.459	0.451	-0.374	0.473
	(0.614)	(0.565)	(0.710)	(0.612)
Panel C: Injuries, reform 3				
Prohibited	-0.637	-1.061*	-0.700	-1.273**
	(0.564)	(0.539)	(0.480)	(0.574)
Panel D: Alcohol intoxication and poisoning, reform 1				
Prohibited	-0.068	-0.005	-0.080	-0.139***
	(0.059)	(0.059)	(0.050)	(0.048)
Panel E: Alcohol intoxication and poisoning, reform 2				
Prohibited	-0.065	-0.054	-0.111	0.129*
	(0.091)	(0.072)	(0.068)	(0.068)
Panel F: Alcohol intoxication and poisoning, reform 3				
Prohibited	-0.014	-0.073	0.098*	0.039
	(0.070)	(0.057)	(0.055)	(0.070)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications exploiting two years before and after each policy change.

Appendix Table 4: Results by parental education, excluding individuals aged 18 and older (monthly per 1,000 individuals)

	Father primary	Father secondary	Father tertiary	Mother primary	Mother secondary	Mother tertiary
Panel A: Injuries, reform 1						
Prohibited	-1.055*	-1.249**	-2.059***	-0.827	-1.616***	-1.595***
	(0.554)	(0.499)	(0.439)	(0.495)	(0.454)	(0.480)
Panel B: Injuries, reform 2						
Prohibited	0.209	0.287	0.683	-0.288	0.149	0.983*
	(0.536)	(0.408)	(0.568)	(0.387)	(0.354)	(0.512)
Panel C: Injuries, reform 3						
Prohibited	-1.059**	-0.479	0.173	-0.545	-0.502	-0.229
	(0.428)	(0.322)	(0.353)	(0.385)	(0.301)	(0.386)
Panel D: Alcohol-related conditions, reform 1						
Prohibited	-0.033	-0.075**	-0.062	-0.020	-0.107***	-0.047
	(0.047)	(0.033)	(0.054)	(0.045)	(0.035)	(0.045)
Panel E: Alcohol-related conditions, reform 2						
Prohibited	-0.032	-0.008	0.000	-0.013	-0.016	0.056
	(0.069)	(0.049)	(0.064)	(0.058)	(0.051)	(0.059)
Panel F: Alcohol-related conditions, reform 3						
Prohibited	0.008	0.064*	-0.069*	-0.080	0.032	-0.004
	(0.043)	(0.036)	(0.040)	(0.048)	(0.036)	(0.044)

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (1), with specifications exploiting two years before and after each policy change.

Appendix Table 5: Long-term effects on mortality

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reform 1 (cutoff at the birth date of July 1, 1983)						
Months	0.000	0.000	0.001	0.001	0.000	0.004
prohibited	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.004)
Bandwidth	5.500	11.000	2.750	6.709	13.418	3.354
Panel B: Reform 2, cutoff 2a (cutoff at the birth date of July 1, 1988)						
Months	0.000	0.000	0.002	0.000	0.000	0.003
prohibited	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.002)
Bandwidth	3.898	7.796	1.949	7.185	14.370	3.592
Panel C: Reform 2, cutoff 2b (cutoff at the birth date of July 1, 1988)						
Months	-0.000	-0.000	-0.000	-0.001	-0.000	-0.001
prohibited	(0.001)	(0.000)	(0.002)	(0.001)	(0.000)	(0.002)
Bandwidth	3.466	6.932	1.733	7.835	15.670	3.918
Panel D: Reform 2, cutoff 2b: being able to purchase at age 16 rather than at age 15 for the first time						
Age	-0.002*	-0.001	-0.001	-0.002*	-0.001	-0.001
allowed	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Bandwidth	4.028	8.056	2.014	8.325	16.650	4.162

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (2), with specifications in columns (1) - (3) using linear trends below and above the cutoffs, whereas specifications in columns (4) - (6) use quadratic trends below and above the cutoffs. In columns (1) and (4) we use optimal bandwidths according to the criterion by Calonico et al. (2014). Specifications in columns (2) and (5) use twice as large, whereas specifications in columns (3) and (6) use half as large bandwidths. Panels A-C exploit the number of months an individual is prohibited from buying alcohol, using an RK design, whereas panel D applies an RD design to investigate the potential role of the youngest age at which the individual was allowed to purchase alcohol. In panel A, we consider the number of incidents between age 18 and 28, in panel B the number of incidents between age 18 and 23, and in panels C-D the number of incidents between age 18 and 22. Bandwidths displayed in specifications (2) and (5) are sometimes cut (in one direction) to avoid overlaps with other cutoffs. Specifically, bandwidths above the cutoff can be no higher than 12 when exploiting the second cutoff and bandwidths below the cutoff can be no higher than 12 when exploiting the third cutoff.

Appendix Table 6: Long-term effects on appendicitis

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reform 1 (cutoff at the birth date of July 1, 1983)						
Months	0.001	0.000	0.001	0.001	0.001	0.005
prohibited	(0.001)	(0.000)	(0.003)	(0.003)	(0.001)	(0.008)
Bandwidth	6.061	12.122	3.030	7.165	14.330	3.582
Panel B: Reform 2, cutoff 2a (cutoff at the birth date of July 1, 1988)						
Months	0.002*	0.000	0.004	0.005	0.002	0.001
prohibited	(0.001)	(0.000)	(0.003)	(0.004)	(0.001)	(0.009)
Bandwidth	4.568	9.136	2.284	5.149	10.298	2.574
Panel C: Reform 2, cutoff 2b (cutoff at the birth date of July 1, 1988)						
Months	0.002*	0.001**	0.002	0.003*	0.001**	0.003
prohibited	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.003)
Bandwidth	4.712	9.424	2.356	8.000	16.000	4.000
Panel D: Reform 2, cutoff 2b: being able to purchase at age 16 rather than at age 15 for the first time						
Age	-0.001	-0.001	-0.000	-0.001	-0.001	-0.001
prohibited	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)
Bandwidth	6.358	12.716	3.179	8.643	17.286	4.322

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Regressions are run according to Equation (2), with specifications in columns (1) - (3) using linear trends below and above the cutoffs, whereas specifications in columns (4) - (6) use quadratic trends below and above the cutoffs. In columns (1) and (4) we use optimal bandwidths according to the criterion by Calonico et al. (2014). Specifications in columns (2) and (5) use twice as large, whereas specifications in columns (3) and (6) use half as large bandwidths. Panels A-C exploit the number of months an individual is prohibited from buying alcohol, using an RK design, whereas panel D applies an RD design to investigate the potential role of the youngest age at which the individual was allowed to purchase alcohol. In panel A, we consider the number of incidents between age 18 and 28, in panel B the number of incidents between age 18 and 23, and in panels C-D the number of incidents between age 18 and 22. Bandwidths displayed in specifications (2) and (5) are sometimes cut (in one direction) to avoid overlaps with other cutoffs. Specifically, bandwidths above the cutoff can be no higher than 12 when exploiting the second cutoff and bandwidths below the cutoff can be no higher than 12 when exploiting the third cutoff.

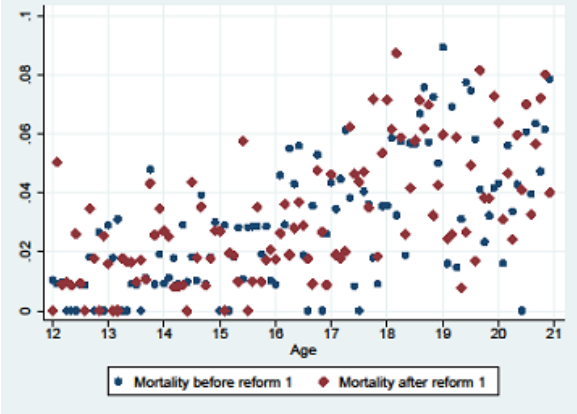

Figure 1a: Mortality around reform 1 (July 1, 1998)

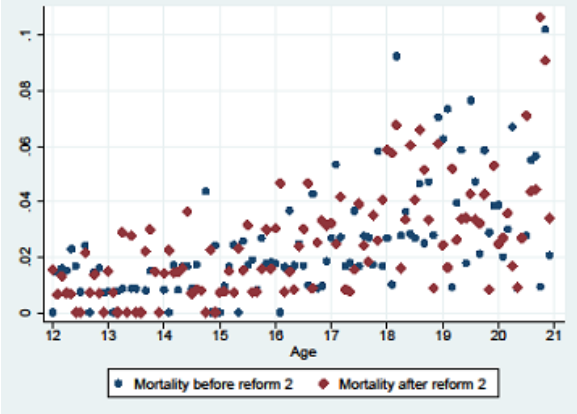

Figure 1b: Mortality around reform 2 (July 1, 2004

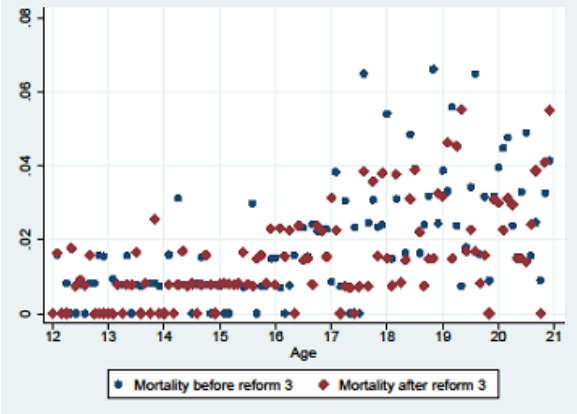

Figure 1c: Mortality around reform 3 (March 7, 2011)

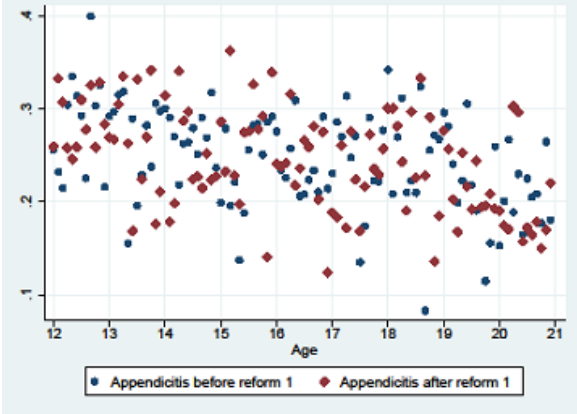

Figure 1d: Appendicitis around reform 1 (July 1, 1998)

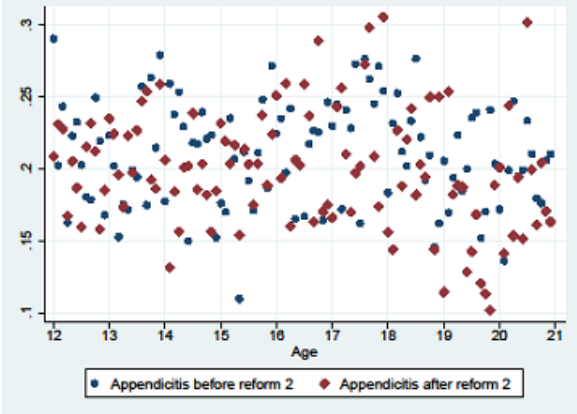

Figure 1e: Appendicitis around reform 2 (July 1, 2004)

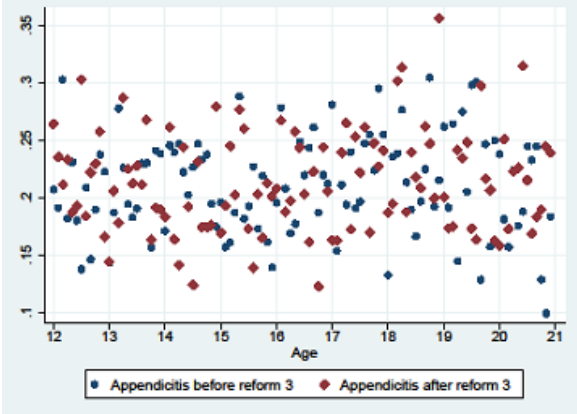

Figure 1f: Appendicitis around reform 3 (March 7, 2011)

Appendix Figure 1: Mortality and appendicitis two years before and two years after each reform. Monthly rates per 1,000 individuals.

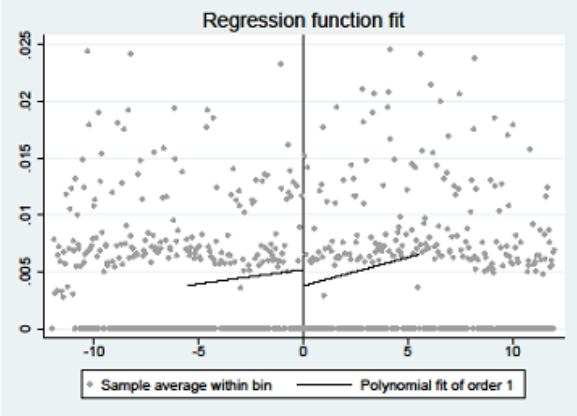

Figure 2a: Mortality around cutoff 1 (July 1, 1983)

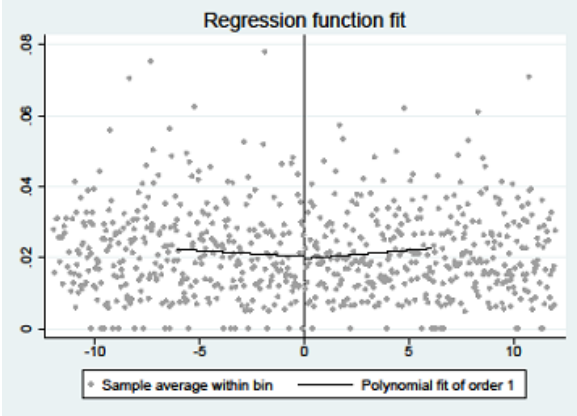

Figure 2d: Appendicitis around cutoff 1 (July 1, 1983)

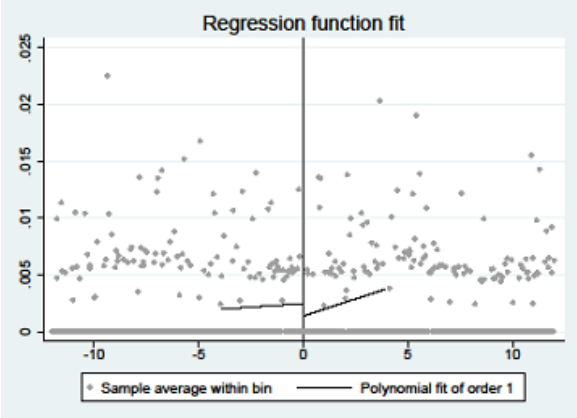

Figure 2b: Mortality around cutoff 2a (July 1, 1988)

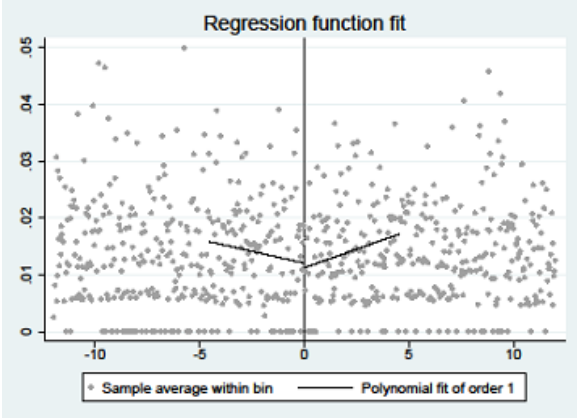

Figure 2e: Appendicitis around cutoff 2a (July 1, 1988)

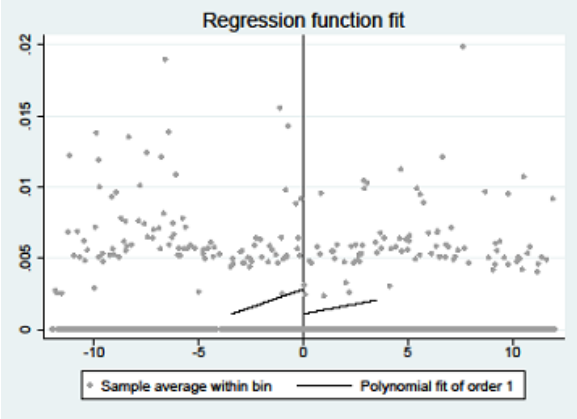

Figure 2c: Mortality around cutoff 2b (July 1, 1989)

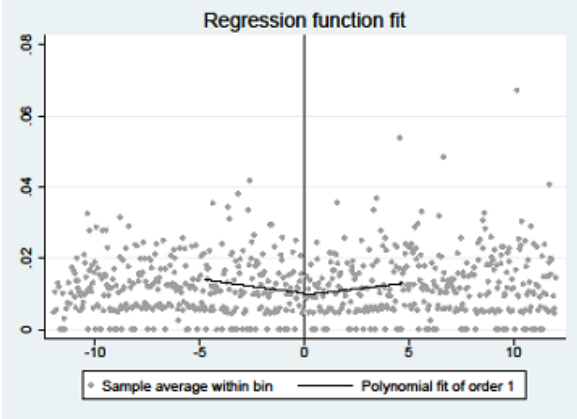

Figure 2f: Appendicitis around cutoff 2b (July 1, 1989)

Appendix Figure 2: Mortality and appendicitis individuals with different birth dates around the cutoffs generated by reform 1 and 2. Distances from cutoffs are expressed in months. Exploiting cutoff 1, we consider the number of incidents in ages 18-28, exploiting cutoff 2a, we consider the number of incidents in ages 18-23, and exploiting cutoff 2b, we consider the number of incidents in ages 18-22.

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