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ABSTRACT

Does Changing the Legal Drinking Age Influence Youth Behaviour?

This paper examines the impact of a reduction in the legal drinking age in New Zealand from 20 to 18 on alcohol use, and alcohol-related hospitalisations and vehicular accidents among teenagers. We use both a difference-in-differences approach and a regression discontinuity design (RDD) to examine the impact of the law change. Our main findings are that lowering the legal drinking age did not appear to have led to, on average, an increase in alcohol consumption or binge drinking among 15-17 or 18-19 year-olds. However, there is evidence that the law change led to a significant increase in alcohol-related hospital admission rates for 18-19 year-olds, as well as for 15-17 year-olds. While these increases are large in relative magnitude, they are small in the absolute number of affected teenagers. Finally, we find no evidence for an increase in alcohol-related vehicular accidents at the time of the law change for any teenagers. In an important methodological contribution, we show that one approach commonly used to estimate the impact of changing the legal drinking age on outcomes, an RDD that compares individuals just younger than the drinking age to those just older, has the potential to give misleading results. Overall, our results support the argument that the legal drinking age can be lowered without leading to large increases in detrimental outcomes for youth.

JEL Classification: I18, K42, C25

Keywords: drinking age, alcohol consumption, hospitalization, vehicular accidents, New Zealand

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1. Introduction

This paper examines the impact of a reduction in the legal drinking age in New Zealand from 20 to 18 on alcohol use, alcohol-related hospitalisations and alcohol-related vehicular accidents among teenagers. We focus on both the behaviour of individuals directly affected by the change and those who were unaffected but whose access to alcohol might have changed (e.g., slightly younger individuals). The quasi-experimental nature of the change allows us to identify the causal impact of legal age restrictions on alcohol consumption on a number of youth behaviours. We use two complimentary empirical approaches, difference-in-differences with individuals aged 22-23 used as the control group against which effects on 15-17 and 18-19 year-olds are measured and a regression discontinuity design (RDD), to examine the impact of the law change.

There has been a recent push among policymakers in the US, especially higher education officials, for the federal government to consider lowering the minimum legal drinking age (MLDA) from its current level of 21. In a recent overview paper, Carpenter and Dobkin (2011) argue that “a large and compelling body of empirical evidence [...] shows that [...] setting the minimum legal drinking age at 21 [in the US] clearly reduces alcohol consumption and its major harms (p.134).” Evidence for this statement generally comes from one of two empirical approaches, both extensively reviewed in Carpenter and Dobkin.

The first approach uses state-level time-series data on various outcomes, such as vehicular accidents, alcohol consumption and cause-specific mortality rates, to examine the impact of changes in the MLDA. These studies typically use a difference-in-differences (D-in-D) framework and exploit the fact that, in response to the National Minimum Drinking Age Act of 1984, different US states increased their MLDA to 21 in different years (and some never had a MLDA below 21). This approach has two serious weaknesses. First, these changes occurred at a time of comparatively high and increasing alcohol consumption in the US (WHO 2013) and hence it is unclear that one should expect the impact of an increase in the MLDA that occurred under these conditions to be a good predictor of what would happen now in the US if the MLDA was lowered (i.e., for these estimates to have external validity). Second, as with all D-in-D approaches, one must assume that the law change being examined (here, increases in the MLDA) are exogenous to prior outcomes at the state level. In a situation where alcohol consumption was increasing, one can imagine that states with the largest increases in problems perceived to be related to alcohol consumption might have been the first to increase their MLDA as well as to make other more subtle and difficult to measure changes aimed at reducing these perceived problems. If this was the case, then D-in-D estimates of the impact of increasing the MLDA would likely be biased upwards.[1]

Because of these known shortcomings, a second approach to measuring the impact of MLDAs has become increasingly popular (for example, Carpenter and Dobkin 2009; Yörük and Yörük 2011). This approach uses a regression discontinuity design (RDD) where the running variable is age and the discontinuity occurs at exactly the MLDA (i.e., the day a person turns 21 in the case of the US). In other words, identification of the impact of the MLDA is achieved by comparing outcomes for people slightly younger than the MLDA to those slightly older. If nothing other than legal status changes discretely at the MLDA, then a discrete change in any outcome at that age can plausibly be attributed to the drinking age.

However, there are two serious threats to the validity of this approach, one a threat to internal validity and a second to external validity. The first issue is that this approach assumes by definition that any excessive drinking right after becoming legal represents what younger people would do if they were legal. This rules out that individuals enter a 'party mode' after becoming legal before their behaviour returns to a steady-state that is potentially the same as before becoming legal. Papers using this approach typically recognise this issue and parametrically allow for some amount of a party period (for example, one month in Carpenter and Dobkin). Unfortunately, there is no way to know what the right amount of adjustment time is and, as one allows for more adjustment time, the main assumption of the RDD approach, that nothing else differs between people just before and just after the discontinuity, becomes more tenuous.

The second issue is more subtle. RDD under standard assumptions only provides an estimate of the local average treatment effect (LATE), which in this case is the change in the examined outcome for 21-year-olds whose behaviour changes because of the MLDA (Lee and Lemieux 2010). Hence, given the high prevalence of drinking among individuals younger than 21 in the US, it is quite likely that the RDD approach only identifies the impact of the MLDA on outcomes for very inexperienced drinkers. This suggests that these estimates may have little external validity for judging the overall impact that reducing the MLDA would have on outcomes. A major contribution of this paper, discussed further below, is that the combination of the policy change we examine and the high quality data on

[1] A recent paper by Miron and Tetelbaum (2009) provides evidence for precisely this type of bias.

vehicular accidents we have access to allows us to evaluate whether this approach is likely to provide policy relevant evidence on the average impact of changing a MLDA.

Our paper makes three major contributions to the current literature on the impact of MLDAs. First, by examining a fairly recent policy change in a country, New Zealand, that has remarkably similar drinking habits to the US (according to WHO 2013, total adult alcohol consumption per capita is 9.44 litres of pure alcohol in the US and 9.62 in New Zealand) and a similar MLDA prior to the law change (20 as opposed to 21), we are able to provide arguable the best evidence so far on what might be the impact of lowering the MLDA in the US on a number of outcomes for youth.[2] Second, we highlight the importance of examining multiple outcomes and carefully interpreting relative changes when baseline rates are low. While, as in Conovera and Scrimgeour (2013), we find large relative impacts of the reduction of the MLDA in New Zealand on alcohol-related hospitalisations, these results are in contrast to our findings for alcohol consumption and alcohol-related vehicular accidents, and they translate to a fairly small number of additional hospitalisations in absolute terms. Third, by comparing results from a RDD which uses time before/after the policy change as the running variable and the date of the policy change as the discontinuity to results using the approach discussed above with age as the running variable and the MLDA birthday as the discontinuity, we are able to judge whether the second approach, which is commonly used, is likely to give policy relevant estimates of the impact of changing the MLDA.[3]

Our main findings are that lowering the legal drinking age did not appear to have led to, on average, an increase in alcohol consumption or binge drinking among 15-17 or 18-19 year-olds. However, there is evidence that the law change led to a significant increase in alcohol-related hospital admission rates for 18-19 year-olds, as well as for 15-17 year-olds. While these increases are large in relative magnitude, they are small in the absolute number of affected teenagers. Consistent with our findings for alcohol consumption, we find no

[2]Two prior papers, Conovera and Scrimgeour (2013) and Kypri et al. (2006) also examine the impact of the change in the MLDA in New Zealand on alcohol-related hospitalisations and vehicular accidents, respectively. Our results for hospitalisations are qualitatively similar to Conovera and Scrimgeour but, because we examine multiple outcomes that show very different impacts than those for hospitalisations, our contribution is much broader. We also argue that one needs to be careful in interpreting the results for hospitalisations since baseline rates are very low. Our results for alcohol-related vehicular accidents differ significantly from Kypri et al. which we show is because the methodology they use does not allow for different underlying trends in outcomes for different age-groups.

[3] Conovera and Scrimgeour (2013) also do this in their paper but, unlike us, argue that the two approaches are complimentary. Crucially, because they are only examining hospitalisations and find positive impacts using the ‘correct’ approach, they are not able to easily judge any inherent bias in the typically used approach.

evidence for an increase in alcohol-related vehicular accidents at the time of the law change for any teenagers. Finally, we show that, in our context, using a RDD design to compare individuals just younger than the drinking age to those just older, gives very misleading results on the impact of a change in the MLDA on alcohol-related vehicular accidents. Overall, our results support the argument that the legal drinking age can be lowered without leading to large increases in detrimental outcomes for youth.

Our paper proceeds as follows: in the next section, we provide background on the institutional situation in New Zealand and the reduction in the MLDA that occurred in 1999. In section 3, we discuss the survey data we use to measure alcohol consumption and present the results for a number of outcomes in this domain. In section 4, we then discuss the administrative data used to measure alcohol-related hospitalisations and vehicular accidents, and discuss the results for these outcomes. In section 5, we present the results from estimating the impact of the MLDA on alcohol-related vehicular accidents using the age-based RDD and compare these to the results in section 4. We then conclude.

2. Background

In 1999, the New Zealand Parliament voted by a narrow majority to lower the minimum legal drinking age from 20, where it had stood since 1969, to 18.[4] The impetuous for this law change was the desire to bring the MLDA in line with the general law of majority in New Zealand. This was voted on as a members bill meaning that all MPs were free to vote based on their own conscience and that there were no opinions by political parties on how they should vote. Hence, there was more or less a natural experiment which changed the legal status of 18 and 19 year-olds in regards to alcohol consumption and potentially had spillover effects on individuals just below the new drinking age (e.g. 16 and 17 year-olds).

This change was made as a component of the Sale of Liquor Amendment Act 1999 (SLAA1999) which liberalised alcohol policy in New Zealand along a number of other dimensions. In particular, the SLAA1999 allowed supermarkets to start selling beer, and liquor stores and other retail establishments, including supermarkets, to sell alcohol on

[4] Technically, the same as in the US, the law actually restricts the purchase and public possession of alcohol to those over the threshold age, not consumption by individuals. In the case of New Zealand, it even allows underage individuals to consume alcohol publicly if purchased by their parents. However, to remain consistent with common usage and the international literature we will continue to call this the ‘drinking age’ or MLDA.

Sundays.[5] While our RDD estimates will be contaminated by these additional changes and will measure the impact of the full set of changes brought about by the SLAA1999, our D-in-D estimates will identify the causal impact of the change in MLDA on its own conditional on the assumption that the impact of these other changes is similar for teenagers as for the 22-23 year-olds that serve as our control group.[6] Given our overall finding of little impact of reducing the MLDA on youth outcomes, we do not believe this to be a particularly important issue in the interpretation of our results since, if anything, one could imagine these other policy changes also leading to more consumption for young people with supermarkets and liquor stores almost always the cheapest outlets for purchasing alcohol.

As part of the change, it was agreed that the policy would be reviewed over a period of time. By 2006, there was some discussion in both parliament and the national media that the change had led to more drinking among teenagers and a vote was undertaken on a bill that would have returned the drinking age to 20. This vote failed and thus serves as a potential test of our identification strategy discussed below. There was a second review in 2013 where the idea of a split drinking age of 18 for restaurants and bars (i.e., places with on-site liquor licenses) and 20 for other purchases was proposed. This vote narrowly failed as well. However, other changes were made at this point to reduce access to alcohol but without a particular focus on youth drinking.

3. Impacts on Alcohol Consumption

3.1 Data

The New Zealand Health Survey (NZHS) was fielded in 1996/97, 2002/03 and 2006/07 and provides the most comprehensive information on alcohol consumption over time available in New Zealand (Ministry of Health 1999; 2004; 2008).[7] The NZHS collects representative cross-sectional data on the health status of the residential New Zealand population, and the prevalence of risk and protective factors associated with these health conditions. Each of the surveys involves face-to-face interviews with individuals aged 15 years and older and collected a variety of information on alcohol consumption, as well as data on a number of background characteristics (e.g., gender, ethnicity, residence and material resources).

[5] See http://www.justice.govt.nz/publications/ publications-archived/1999/amendments-to-the-1989-sale-of-liquor-act/publication for a summary of the major changes of the SLAA 1999.

[6] In fact, we can even slightly relax this assumption and allow for age-specific price effects, but in practice this had a very limited effect on our results.

[7] The NZHS was also fielded in 1992/93 but this wave did not collect data on alcohol consumption.

In particular, the following questions (followed by the possible responses) were asked about alcohol consumption consistently in all three waves of the survey: i) Have you had a drink containing alcohol in the last year? – yes / no; ii) How often do you have a drink containing alcohol? – Monthly or less / Up to 4 times a month / Up to 3 times a week / 4 or more times a week; iii) How many drinks containing alcohol do you have on a typical day when you are drinking? – 1 or 2 / 3 or 4 / 5 or 6 / 7 to 9 / 10 or more; iv) How often do you have six or more drinks on one occasion? – Never / Less than monthly / Monthly / Weekly / Daily or almost daily; and v) Have you or someone else been injured as a result of your drinking? No / Yes, but not in the last year / Yes, during the last year.

We use these variables to define continuous measures of i) the frequency of alcohol consumption and ii) the amount of alcohol consumed on a typical day when drinking using the midpoint of each range and the bottom of the range (i.e. 4 times per week and 10 drinks per day) for the highest values.[8] We then multiple these two figures to get an estimate for the number of drinks consumed in a usual month. We also examine the following discrete outcomes; i) whether someone has had a drink in the last year, ii) whether they typically have six or more drinks at least once per week (labelled binge drinking), and iii) whether they have been injured or injured someone else while drinking in the last year.

3.2 Descriptive Evidence

Table 1 shows basic descriptive statistics for the 3,783 individuals aged 15-23 in the NZHS data. We report means and standard errors for several outcomes and key explanatory variables for each survey year. Sample weights are used in all calculations from the NZHS because the survey over-samples minority groups, in particular individuals with Maori ethnicity. Table 1 indicates that more than 80 percent of the youths in the age group 15-23 had at least one drink in the previous year. This number is consistent with other data sources, e.g., Ministry of Health (2009) which is based on a survey solely on alcohol and drug use. Total consumption dropped by about 3 drinks per month, on average, comparing the 96/97 and the 02/03 waves (from about 19.9 to 16.8), and then remained constant in the 06/07 wave. The reduction can be explained by a lower frequency of drinking, as the number of drinks per occasion lightly increased in this time period. About 20 percent of 15-23s are binge drinkers and around 10 percent reported that their drinking lead to someone

[8] We also estimated ordered D-in-D models for these outcomes and discrete choice D-in-D models for the other outcomes with no qualitative impact on the results.

getting injured. In general, drinking behaviour appears fairly stable over the period being examined none-withstanding the law change.

Regarding the background variables, the sample shows about an equal share of age-groups and gender. The largest ethnic group in New Zealand is Pakeha/European (about two thirds), followed by Maori (about 15 percent), Pacific and Asian (each about 6-10 percent). Only about 10 percent of the youths live in a rural environment. The average real household income remained constant at about 43’000 NZ dollars (in 2006 dollars), whereas the mean NZDEP Index of Deprivation (a measure of the socioeconomic status of one’s neighbourhood), measured in deciles with 1 being the least deprived and 10 being the most, increased from the 96/97 to the 02/03 wave and then slightly dropped in 06/07.

Instead of variation over time, Table 2 shows summary statistics of the outcomes by age group. We classify youth into 18-19 year-olds (those directly affected by the policy), the slightly younger 15-17 year-olds (who still cannot legally purchase alcohol, but who might have a better access to it through their older peers), and the slightly older 20-21 and 22-23 year-olds. Initially, we pooled 20-21 and 22-23 year-olds to give us a larger control group, but our RDD estimates for hospitalisations suggested that there might be spillovers on to 20-21 year-olds from legalising alcohol for 18-19 year-olds.

As one would expect, the incidence and frequency of drinking, the number of drinks and total consumption are significantly lower among the 15-17 year-olds (about 13 drinks in total per month on average) compared to their older peers (more than 20 drinks in total per month on average). Looking at the decomposition of total consumption, it should be noted that the frequency of drinking is higher among the 20-23 year-olds than among the 18-19 year-olds, whereas the number of drinks is higher among the 18-19 year-olds, suggesting substantial differences in drinking behavior across age groups. Binge drinking is also most common among 18-19 year-olds and someone being injured because of one’s drinking generally declines with age.

3.3 Difference-in-Differences Estimates

We now turn to estimating the impact of the change in MLDA from 20 to 18 in 1999 on the drinking behaviours of 15-17, 18-19 and 20-21 year-olds relative to changes over this time for 22-23 year-olds. We do this be estimating a difference-in-differences (D-in-D) model for each outcome allowing i) different alcohol consumption for different aged individuals; ii) different time trends in alcohol consumption for all 15-23s (for example, reflecting general trends towards healthier drinking habits); and iii) changes over time in alcohol use for different age-groups.[9] Individuals aged 22-23 are used as the control group against which effects on 15-17, 18-19 and 20-21 year-olds are measured. The key assumption of this D-in-D approach is that changes in outcomes between 1999 and the later years for 22-23 year-olds are what would have occurred for teenagers and younger adults if the drinking age had not been lowered.

More formally, the model we estimate can be written as:

$$y = \alpha + \beta' age + \gamma' time + \delta' age\text{-}group * time + X'\theta + \varepsilon \qquad (1)$$

where y is one of six outcomes, *age* is a full set of age dummies, *time* includes two indicators for waves 02/03 and 06/07, *age-group* includes three indicators for the 15-17 year-olds, the 18-19 year-olds and the 20-21 year-olds, respectively, X is a vector of control variables (in some models), and ε is an idiosyncratic error. Equation (1) is a standard D-in-D model where the parameters α, β, and γ measure the age- and time-specific trends in the outcome, and δ measures the impact of lowering the MLDA from 20 to 18 on the three age groups 15-17, 18-19, 20-21 using the 22-23 year-olds as the reference group.

Table 3 shows the results obtained from estimating equation (1) by OLS for all six outcomes discussed above. The regression is weighted as described before and we calculate heteroskedasticity robust standard errors which allow for arbitrary correlation in the error term within age-groups as recommend by Bertrand et al. (2004). We report two sets of results for each outcome, the raw effect in the basic specification without controls (panel A), and the effect obtained after adding controls for gender, ethnicity, log of household income, urban residence and the deprivation index (panel B). These controls account for changes in sample composition over time that are potentially correlated with drinking behaviours.

While we do not find any evidence of a significant impact of the reduction in the MLDA on outcomes for 18-19 year-olds, standard errors on our estimated impacts, especially for frequency of drinking, number of drinks per occasion and (log) total consumption, are quite large meaning that it is difficult to rule out possible positive impacts on alcohol consumption (i.e., the reduction in MLDA led to increased drinking for teenagers). On the other hand, we

[9] Because we only have survey data for these outcomes, we are not able to also estimate RDD models as we do for alcohol-related hospitalisations and vehicular accidents.

find fairly strong evidence that the reduction in the MLDA led to less alcohol consumption and better drinking behaviours among 15-17 year-olds, which perhaps occurred because the reduction in the drinking age led to increased enforcement (we discuss this further below).

One important shortcoming of this analysis is that because we only have one pre-period observation, we are unable to test for or control for age-specific prior trends in alcohol consumption, which could conceivably be important. For this reason, we now turn to our analysis of the impact of the MLDA on alcohol-related hospitalisations and vehicular accidents. In both cases, we have administrative data on the universe of events over more than a decade. Hence, not only are our estimated impacts quite precise, but we can control for age-specific trends in these outcomes flexibly.

4. Impacts on Alcohol-Related Hospital Admissions and Vehicular Accidents

4.1 New Zealand Hospital Admission Data

In addition to the NZHS, we have access to a dataset compiled by the Ministry of Health on all discharges, including day patients, from public hospitals in New Zealand with an admission date between 1 January 1996 and 31 December 2007 where the age at admission is 15 years or over. Almost all hospitalisations in New Zealand are in public hospitals (Kypri et al., 2006). Our dataset includes an extensive amount of information about medical diagnoses (primary and secondary diagnoses based on the International Classification of Disease (ICD) codes) and symptoms, as well as some limited sociodemographic information about the patient, including their gender, age, and ethnicity (see Ministry of Health 2003a and Conovera and Scrimgeour 2013 for more information).

The diagnostic codes allow us to identify admissions that are directly related to alcohol consumption, such as alcohol misuse and alcohol dependence related admissions. We differentiate between the following types of admissions defined via the primary and secondary diagnoses codes listed in brackets according to the ICD-9 (National Coding Centre 1996): i) Alcohol use disorder (3050), our primary outcome; ii) Alcohol intoxication (3030, 2914); and iii) Alcohol dependence (3039, 2910-3, 2915, 2918-9, 3575, 4255, 5353, 5710-3). The first outcome refers to all cases diagnosed with an excessive use of alcoholic beverages, e.g., after celebrations, with no indication of a dependence syndrome. This includes, for example, hospitalisations related to binge drinking events. The second and third outcomes relate to dependence syndromes in terms of temporary mental disturbances (in ii) or serious impairments (in iii). These codes were chosen based on our discussions with public health researchers working on the area of alcohol-related hospitalisations. We combine this data with population estimates from Statistics New Zealand to calculate admission rates per 10,000 population for each age-group.

The key advantage of this data, apart from being administrative with little measurement error especially in the key variable of age, is that we know the exact date of each admission. This allows us to examine age-specific time-trends in alcohol and related admissions in much greater detail than with the NZHS. On the downside, we only observe hospital admissions, which are obviously the consequence of heavy misuse and hence do not tell us about the behavior of the average teenager as captured by the NZHS.

4.2 Regression Discontinuity Design Estimates for Hospitalisations

The MLDA was reduced from 20 to 18 on December 1, 1999 and the law change took effect immediately. As a consequence, a comparison of hospital admissions for a particular age-group shortly before the policy change to just after should reveal the causal impact of the MLDA (along with the other changes brought about by SLAA1999). Of course, simply taking a single month before and after the policy change would likely overestimate the true policy effect because of heavy drinking of the 18-19 year-olds “celebrating” their new legal drinking status. Moreover, hospital admissions tend to be seasonal and December in New Zealand is the end of the university year, the start of summer and the time for many Christmas parties.

A sensible before/after comparison would therefore smooth alcohol-related admissions before and after the policy change, and then interpret any shift in the locally smoothed average at December 1, 1999 as the causal effect of the reduction in the MLDA on outcomes for the age-group being examined. This is precisely what is done in a sharp regression discontinuity design (RDD). The key identification assumption with this approach is that any discontinuity in an outcome at the time of the law change is not caused by something else changing at exactly that same time. We know of no other policy change that occurred at the same time so believe that this assumption should hold.

More formally, the model we estimate can be written as:

$$y = \alpha + \beta * dt + g^{-}(time) + g^{+}(time) + \varepsilon \qquad (2)$$

where y is any of the three outcomes (hospitalisation due to alcohol use disorder, alcohol intoxication or alcohol dependence), dt is an indicator for after December 1, 1999, and g^- and g^+ are flexible functions of *time*, with *time* being normalized to zero at the policy change. The parameter β gives the causal effect of the reduction in the MLDA because of the quasi-experimental nature of the policy. We estimate this model separately for the four age-groups used in the D-in-D analysis; 15-17, 18-19, 20-21 and 22-23 year-olds. There exist several approaches to estimate (2), one being (semi-) parametric with the g functions specified as polynomial functions, the other being nonparametric with g flexibly estimated from the data, for example through local smoothing methods. We use both approaches and present the results from several specifications below. In particular, we will specify g as a linear function with different slopes before and after the policy change, and contrast these parametric results with a local linear smooth before and after the change.

Figure 1 provides a graphical illustration of the nonparametric approach for the number of admissions coded as alcohol use disorder (ICD-9 code 3050) aggregated on the monthly level per 10’000 people in the relevant population. For the ease of presentation, we plot quarterly averages (marked by the grey dots), although all estimates are based on the monthly data. Data are smoothed using the rule-of-thumb bandwidth (Imbens and Kalyanaraman 2012). We observe a significant shift in the number of alcohol related admissions in December 1999 for the affected group (18-19 year-olds), but also for the slightly younger (15-17 year-olds) and slightly older age-groups (20-21 year-olds). There is no significant shift for the 22-23 year-olds showing that their alcohol related admissions did not change in December 1999. These results suggest that the reduction of the MLDA led to an increase in alcohol-related hospitalisations for 18-19 year-olds as well as for 15-17 year-olds and 20-21 year-olds likely due to spillovers from the 18-19 year-olds to their younger and older peers.

Table 4 shows the point estimates (and bootstrapped standard errors) of the shift, using the bandwidth in Figure 1, which range from about 0.3 to 0.4 additional admissions per 10,000 population for the 15-19 year-olds (significant at the 1% level) and 0.2 additional admissions per 10,000 population for the 20-21 year-olds (significant at the 5% level). If we relate the changes for the 15-19 year-olds to the mean number of admissions prior to the new policy, the effects are considerable, almost doubling the average number of alcohol related admissions.

We checked the sensitivity of the local linear smoothing approach using two-thirds and 1.5 times the rule-of-thumb bandwidth and find little variation in our results. Furthermore, we estimated parametric models using linear functions of time both before and after the policy. These estimates are also almost identical to the local linear regressions for the 15-19 year-olds even after including a set of control variables for gender, ethnicity, month of the year, day of the week and location. For the 20-21 year-olds, the estimated shift becomes 0.27 additional admissions per 10,000 population and significant at the 1% level in the linear specification without controls. However, the linear specifications tend to overestimate the shift in December 1999 due to the highly non-linear development of hospital admissions over time (see Figure 1).

Overall, our results imply that the reduction of the MLDA from 20 to 18 led to a 75-91% increase in alcohol-related hospital admission rates for 18-19 year-olds, as well as a 43-69% increase for 15-17 year-olds and a 49-73% increase for 20-21 year-olds. Our results are qualitatively similar to Conovera and Scrimgeour (2013) who use the same data and identification strategy, but generally our effect sizes are larger and more significant. This likely occurs because we use a much narrower definition of alcohol-related admissions that intentionally excludes more chronic conditions, which we show below are unresponsive to the reduction in the MLDA. Conovera and Scrimgeour by including these chronic conditions in their main measure will naturally find small estimated effects.

Notwithstanding how large these impacts are in relative terms, they imply fairly small increases in absolute terms because of the low admission rates at baseline for these age-groups. Translating the relative impacts using population figures from 1999 implies that the reduction in the MLDA from 20 to 18 led to approximately an additional 2.2-3.4 admissions per month for alcohol use disorder for 15-17 year-olds, 2.1-2.6 per month for 18-19 year-olds and 1.0-1.5 per month for 20-21 year-olds in the immediate aftermath of the law change. Another way to judge the scale of the impacts is to compare them to seasonal changes in admissions rates for common conditions with large seasonality. For example, the mean admission rate for 15-23 year-olds in our data for the flu varies from 0.08 per 10,000 population in January to 1.44 per 10,000 population in July and that for asthma varies from 5.04 per 10,000 population in January to 13.12 per 10,000 population in June. Hence, the seasonal variation in these two conditions is 3 and 20 times larger, respectively, than the change in admissions for alcohol use disorder attributed to the reduced MLDA.

4.3 New Zealand Vehicular Accident Data

We next examine the impact of the reduction in the MLDA on vehicular accidents, in particular those judged by the police to be alcohol-related. In New Zealand, vehicular accidents account for more than half of total fatalities and are the second most common cause of hospitalisation, after pregnancy, for teenagers (Kypri et al. 2002a; 2000b). Figures for the United States are quite similar (Langley and Smeijers 1997) and in both countries alcohol is the most common contributor to serious accidents for this age-group (Connor et al. 2004). During the time period examined in this paper, New Zealanders at age 15 years 6 months could, by passing a driving test, get a restricted license allowing them to drive during the day alone or with passengers who held a full license. A full license could then be obtained after having the restricted license for a minimum of 12 months and passing a more comprehensive driving test. This regime was quite similar to that in many less urban US states.

We were able to obtain from the Ministry of Transport data on every vehicular accident (i.e. cars, trucks and motorcycles) in New Zealand from 1 January 1996 to 31 December 2007 that resulted in an injury (and hence would be unlikely to suffer from underreporting since New Zealand has a fully subsidised public health system). Detailed information is available on the location, time, date and circumstances of the accident, e.g. was it raining, was the driver speeding, etc. Furthermore, we know the driver's gender and exact date of birth and whether the police believed that the accident was alcohol-related. Alcohol involvement can be identified using a 3-digit code that indicates suspected alcohol use and whether a person was given a breathalyzer or blood test to detect alcohol. We code accidents as being alcohol related if the alcohol test results were positive or the police suspected that alcohol was involved and a negative alcohol test was not recorded. Again, we combine this data with population estimates from Statistics New Zealand to calculate admission rates per 10,000 population for each age-group.

4.4 Regression Discontinuity Design Estimates for Vehicular Accidents

We use the exact same RDD approach as for alcohol-related hospitalisations to estimate the impact of the reduction in the MLDA on alcohol-related vehicular accidents.[10] As in Figure 1, Figure 2 shows the development of alcohol-related accidents over time from January 1996

[10] We also investigated looking at the impact on vehicular fatalities involving alcohol, but these are such uncommon events for youth that our estimates were very imprecise.

to December 2007. Again, for the ease of presentation, we aggregate the number of traffic accidents to quarterly averages for each age-group; 15-17, 18-19, 20-21, and 22-23 year-olds marked by the grey dots. We also calculated a local linear smooth based on the monthly averages using a rule-of-thumb bandwidth before and after the drinking age reduction.

The results suggest that there are no immediate shifts in traffic accidents due to the policy change. All graphs are almost smooth in December 1999. The only exception is the slight upward trend in accidents for the 18-19 year-olds in the few months prior to the policy change. Table 5 displays the coefficient associated with this graph shift, which indicates that the reduction in the MLDA led to a 0.28 reduction in alcohol-related accidents per 10,000 people which is weakly significant at the 10% level. However, changing the bandwidth has a substantial impact on this estimate (from a significant coefficient of -0.34 to an insignificant -0.16) such that we conclude that this is not a stable effect. Overall, Table 5 indicates that the reduction in the MLDA had no immediate impact on alcohol-related accidents for any of the four age-groups. This holds for the local linear regression, as well as for parametric linear models that control for the driver's gender, the type of vehicle, month of the year, day of the week, time of the day and location.

Kypri et al. (2006) used similar data from the period of December 1, 1995 to November 30, 2003 to examine the same question. They used a simple D-in-D estimator that compares changes before and after December 1999 for 15-17 and 18-19 year-olds to those for 20-24 year-olds and found significant increases in alcohol-related crashes for both age-groups. However, their estimates did not control for either changes in demographic composition or accident type over time, or for differential trends in alcohol-related accidents for different age-groups. We show in Table 7 below that, if we estimate a similar simple D-in-D model we also get similar results, but once controls for the driver's age, gender, the type of vehicle, month of the year, day of the week, time of the day, and location are added and, in particular, we allow for age-specific time-trends in alcohol-related crashes, the D-in-D results are now consistent with our RDD findings.

4.5 Sensitivity Checks and Impact Heterogeneity

Apart from varying the bandwidth and comparing several specifications, we conducted a series of additional checks regarding the validity of the RD identification that are all available by request from the authors if not presented. First, we extended our calculations to the next older age group, the 24-25 year-olds, and find no evidence for a shift in either alcohol-related hospitalisations or vehicular accidents. Second, the New Zealand parliament approved the change in the legal drinking age in August 1999, four months prior to the effective change. One might suspect that youths adjusted their behavior in anticipation of the new law and that hence any estimated shift in December 1999 underestimates the true policy effect. However, allowing for behaviour to change at the end of August 1999 and dummying out the announcement period yields qualitatively similar results (see Tables 4 and 5, row with announcement period). Third, we use the failed vote in November 2006 on a bill that would have returned the drinking age to 20, discussed in more detail in the background section, as a placebo test and find no evidence of a shift in hospital admissions or traffic accidents at that date.

Fourth, we tested for the possibility that we are picking up some other trends in alcohol consumption that are not actually related to the change in the MLDA. One way to distinguish this is to compare alcohol use disorder related admissions, as in Figure 1 and Table 4, with other alcohol-related hospitalisations that should be unrelated to the law change. Specifically, we looked at admissions related to alcoholic intoxications and alcohol dependence. The former is an acute measure that includes intoxications with ethanol, methanol, 2-propanol and other alcohols. Since they are generally consumed by addicts and other heavy drinkers we would not expect changes in the MLDA to impact these hospitalisations. Nor should we find a change for alcohol dependence which is a chronic condition. We also examine the impact on non-alcohol-related vehicular accidents. Table 6 presents the results from this exercise. While we observe a shift in intoxication admissions, this is only for the 20-21 year-olds, which is likely some artefact of the data. We do not find any shifts in the dependence measure or in non-alcohol-related accidents, as one would expect if our identification strategy is correct.

We also examined separate impacts by gender and ethnicity for these two outcomes, as well as for overall alcohol consumption. While most outcomes vary a good deal across this groups, in general, relative impacts were a similar size and no important heterogeneity in the impact of the changes in the drinking age were found.

4.6 Difference-in-Differences Estimates

As discussed in the background section, since the policy change also increased access to alcohol along with reducing the MLDA, our RDD estimates are estimating the impact of the whole SLAA1999 package. However, if the impact of the other policy components is similar for youth as well as for 22-23 year-olds, then a D-in-D estimator will identify just the impact of the MLDA. Of course, the trade-off with this approach is that the further one gets away from the policy change, the more likely that other age-group specific changes might contaminate the results.

In Table 7, we present the results from estimating D-in-D models similar to equation (1) for alcohol-related hospitalisations and vehicular accidents. We include a full set of age and time dummies, and interact the age-group identifiers for the 15-17, 18-19, and 20-21 year-olds with indicators for one year prior, one year post, 2-3 years post and three years or more post the policy change. This allows us to investigate whether the shifts in alcohol related admissions are persistent, and whether traffic accidents increased more over time in the affected age group than in the reference group of 22-23 year-olds, which looks the case in Figure 2. Moreover, by including the interaction for one year prior to the law change, we can evaluate whether we are picking up some general time trends unrelated to the policy and thus check the validity of our identification strategy.

Columns 1-3 show the results for alcohol-related hospital admissions and columns 4-6 for traffic accidents. In each block, we build up the specification from the basic D-in-D framework described above, to including a set of controls as before, and finally to allow for age-specific time trends. Overall, none of the one year prior interactions are significant, suggesting that we are identifying the impact of the policy change and that the announcement period does not matter for these outcomes.

We find strong and persistent effects in the hospital admissions for the 18-19 year-olds. For the 15-17 year-olds, the effect vanishes after 3 years. For the 20-21 year-olds, the effects are strongest 2-3 years after the policy. A possible explanation for this pattern could be peer group effects and changing group compositions. First, there might be immediate spillovers to peers explaining the significant effects for the 15-17 year-olds, and the weakly positive effects for the 20-21 year-olds in the year after the drinking age had been lowered. In the 2-3 years after period, individuals have become older relative to their age at the policy change, affecting the behavior of their new age-group. Hence, it is not surprising that we find the largest effects for the 18-19 year-olds in the period three years after the policy when the composition of that group has changed such that the former 15-17 years-old from the late 1999 period are included (and possibly other effects than the law change play a role).

For the traffic accidents, our results suggest the possibility of long-term effects for 15-17 and 18-19 year-olds. However, in both cases, the estimated effects are insignificant once we control for age-specific time-trends. Combined with the lack of short-run effects, the results here are consistent with other factors influencing alcohol-related vehicular accidents for teenagers relative to 22-23 year-olds besides the change in the MLDA. They are also consistent with the effects of the MLDA taking longer to materialise, which unfortunately is something that is difficult to identify using the empirical strategies considered in this paper.

5. Does An Age-Based Regression Discontinuity Design Work?

In this section, we estimate the impact of the MLDA in New Zealand using the approach taken in a number of recent papers, including Carpenter and Dobkin (2009) and Yörük and Yörük (2011), namely RDD where the running variable is age and the discontinuity occurs at exactly the MLDA. We do this for both the pre- and post-reform periods with the discontinuity at an individual's 20th birthday in the pre-reform period and 18th birthday in the post-reform period. The regression model estimated here can be written as:

$$y = \alpha + \beta * dMLDA + g\text{-}(age) + g\text{+}(age) + \varepsilon \qquad (3)$$

where y indicates a vehicular accident with alcohol involvement, *dMLDA* is an indicator for age above the MLDA, and g^- and g^+ are flexible functions of *age,* with *age* being normalized to zero at the MLDA. If nothing other than legal status changes discretely at the MLDA, then the parameter β gives the causal effect of changing the MLDA. We follow the same estimation approaches as above in the estimation of (2), i.e., a parametric approach with the g functions specified linear, and the other being nonparametric with g flexibly estimated from the data using local linear regression. The results are reported below.

Figure 3 shows that before the policy change there is little evidence for a shift in vehicular accidents at the MLDA, which is confirmed by the estimates reported in Table 8. However, when looking at the after policy change period 2000-2007, we observe an upward shift of about 0.08 alcohol-related accidents per 10'000 population at the MLDA threshold of age 18.[11] These results are very similar across local linear regressions and the parametric linear models (which also includes controls for gender, type of vehicle, month of the year, day of

[11] Yearly (1996-2007) age-based RDD regressions provide very similar results compared to the pooled age-based RDDs, with slightly less precision due to the smaller number of cases. We therefore confine ourselves to the pooled results, the results of the yearly regressions are available from the authors upon request.

the week, time of the day, and location, plus month of birthday). Moreover, allowing for a 6 months adjustment period after becoming legal (following e.g. Carpenter and Dobkin 2009) has little impact on our estimates. Taken at face value, these results indicate that having the MLDA at 18 increases alcohol-related vehicular accidents by around 25%.

However, given that the results in the previous section, which identify the impact of the MLDA using the policy change itself, show no impact of moving the MLDA to 18, we believe the results here provide strong evidence that an age-based RDD is likely to give misleading evidence on the average impact of changing a MLDA, which is the policy relevant question. As discussed in the introduction, we suspect the main reason that this occurs is because RDD is a LATE estimator and hence here only identifies the impact of the MLDA on outcomes for individuals whose behaviour is changed by being just younger/older than the MLDA. In this context, these will only be rather inexperienced drinkers who are the most likely to be negatively impacted by their newly found access to alcohol. A second, more technical reason, why the results from the two RDD methods might deviate is that the age profile of the number of vehicular accidents with alcohol involvement in Figure 3 is inverse u-shaped with a peak around ages 18-20. Thus, when comparing local smooths just below and above the MLDA, an RDD approach likely picks up this non-linearity which may have little to do with the true impact of the MLDA.

It is interesting that we do not find any evidence for a discontinuity in alcohol-related vehicular accidents at age-20 in the pre-reform period. We suspect that this also relates to the LATE interpretation. Anecdotal evidence suggests that the age-20 MLDA was weakly enforced, hence a RDD focusing on that MLDA identifies changes in behaviour for a small subset of people who live in areas where this is less true, which in New Zealand is likely urban areas. If these individuals are also more responsible drinkers or more experienced or less frequent drivers then we would expect to see smaller or zero effects of the MLDA. Alternatively, perhaps there are no differences here because by age 20 even very inexperienced drinkers in New Zealand are sensible about drinking and driving.

6. Conclusion

In 1999, the New Zealand Parliament voted by a narrow majority to lower the minimum legal drinking age from 20, where it had stood since 1969, to 18. This paper examines the impact of this reduction on alcohol use, alcohol-related hospitalisations and alcohol-related vehicular accidents among teenagers. We focus on both the behaviour of individuals directly affected by the change and those who were unaffected but whose access to alcohol might have changed (e.g., slightly younger individuals). The quasi-experimental nature of the change allows us to identify the causal impact of legal age restrictions on alcohol consumption on a number of youth behaviours. We use two complimentary empirical approaches, difference-in-differences with individuals aged 22-23 used as the control group against which effects on 15-17 and 18-19 year-olds are measured and a regression discontinuity design (RDD) to examine the impact of the law change.

Our paper makes three major contributions to the current literature on the impact of MLDAs. First, by examining a fairly recent policy change in a country, New Zealand, that has similar drinking habits to the US and a similar MLDA prior to the law change we are able to provide arguable the best evidence so far on what might be the impact of lowering the MLDA in the US on a number of outcomes for youth. Second, we highlight the importance of examining multiple outcomes and carefully interpreting relative changes when baseline rates are low. Third, by comparing results from a RDD which uses time before/after the policy change as the running variable and the date of the policy change as the discontinuity to results using the approach discussed above with age as the running variable and the MLDA birthday as the discontinuity, we are able to judge whether the second approach, which is commonly used, is likely to give policy relevant estimates of the impact of changing the MLDA.

Our main findings are that lowering the legal drinking age did not appear to have led to, on average, an increase in alcohol consumption or binge drinking among 15-17 or 18-19 year-olds. However, there is evidence that the law change led to a significant increase in alcohol-related hospital admission rates for 18-19 year-olds, as well as for 15-17 year-olds. While these increases are large in relative magnitude, they are small in the absolute number of affected teenagers. We also find no evidence for an increase in alcohol-related vehicular accidents at the time of the law change for any teenagers. Finally, we show that using a RDD design to compare individuals just younger than the drinking age to those just older, has the potential to give very misleading results on the impact of changing a MLDA.

Overall, our results support the argument being made by groups like Amethyst Initiative and Choose Responsibility (see http://www.choose responsibility.org/proposal/) that the legal drinking age can be lowered without leading to large increases in detrimental outcomes for youth. The current age limit of 21 in the US is higher than in Canada, Mexico and most western European countries. The arguments against lowering the drinking age typically include the idea that even, if a new steady-state with a lower drinking age might be beneficial, the transition to that new steady-state might be very costly. The evidence in our paper from a country with drinking habits very similar to the US suggests that this does not have to be the case.

However, it is important to emphasise that our results for New Zealand are specific to the policy change that occurred there. Our results in section 5 suggest that the previous 20-year-old drinking age might not have been particularly salient for 18-19 year-old New Zealanders and there is strong anecdotal evidence that it was lightly enforced (Casswell and Zhang 1997). In fact, a large component of the SLAA1999 was that, while access to alcohol was to be made less restrictive, legal restrictions were to be enforced more thoroughly. For example, the new law changed the rules relating to liquor licences and bar management, allowing bars to be fined for promoting excessive consumption and increasing fines for selling to minors or people already intoxicated. The law also introduced an ‘evidence-of-age’ regime that incentivises sellers to request photographic identification for proof-of-age.

In a situation like this, it is quite likely that a change in the MLDA from 20 to 18 only impacted a small subset of very law abiding and inexperienced drinkers. This is consistent with our findings of a lack of impact on alcohol consumption for the average drinker and alcohol-related vehicular accidents, but an increase in alcohol-related hospitalisations. Given that many/most US college students have the ability to purchase alcohol (Wechsler et al. 2002), it is quite possible that a reduction in the MLDA in the US to 18 or 19 would have a similar impact as what occurred in New Zealand, with detrimental outcomes only increasing for a small subset of very law abiding and inexperienced drinkers. This would be consistent with the results from the current research using the age-based RDD design, which, as discussed above, under standard assumptions only provides an estimate of the LATE, which in this case is the change in the examined outcome for 21-year-olds whose behaviour changes because of the MLDA. Finding large differences in outcomes for this group of likely very inexperienced drinkers is entirely consistent with our findings for New Zealand.

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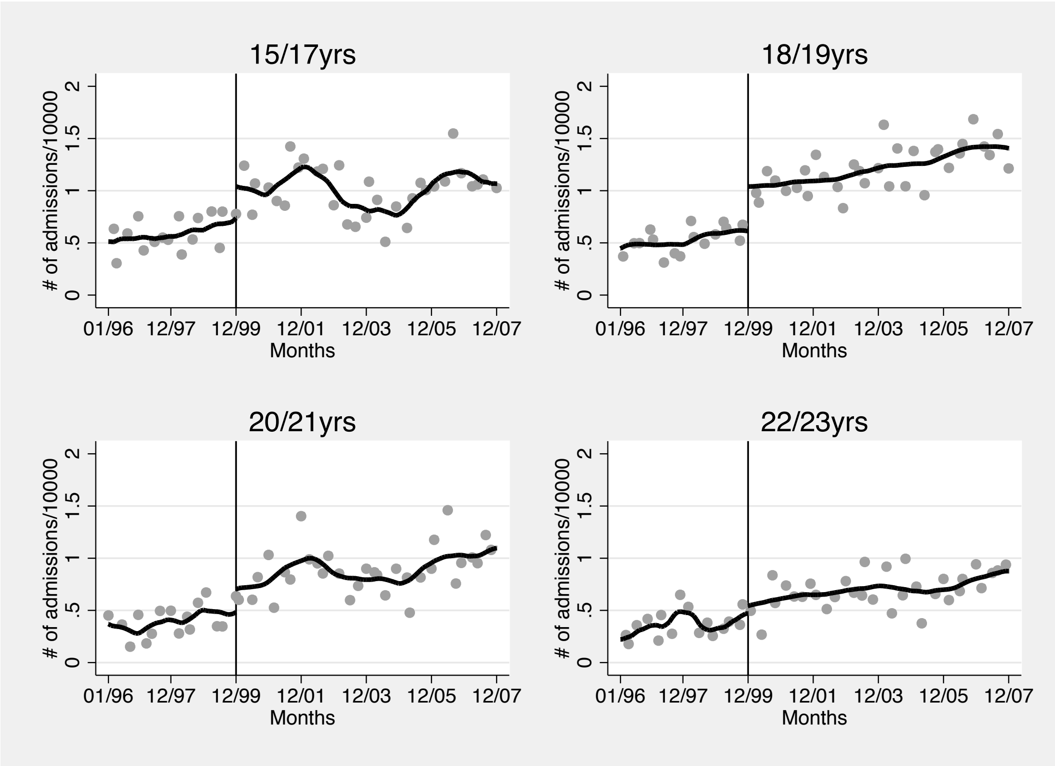
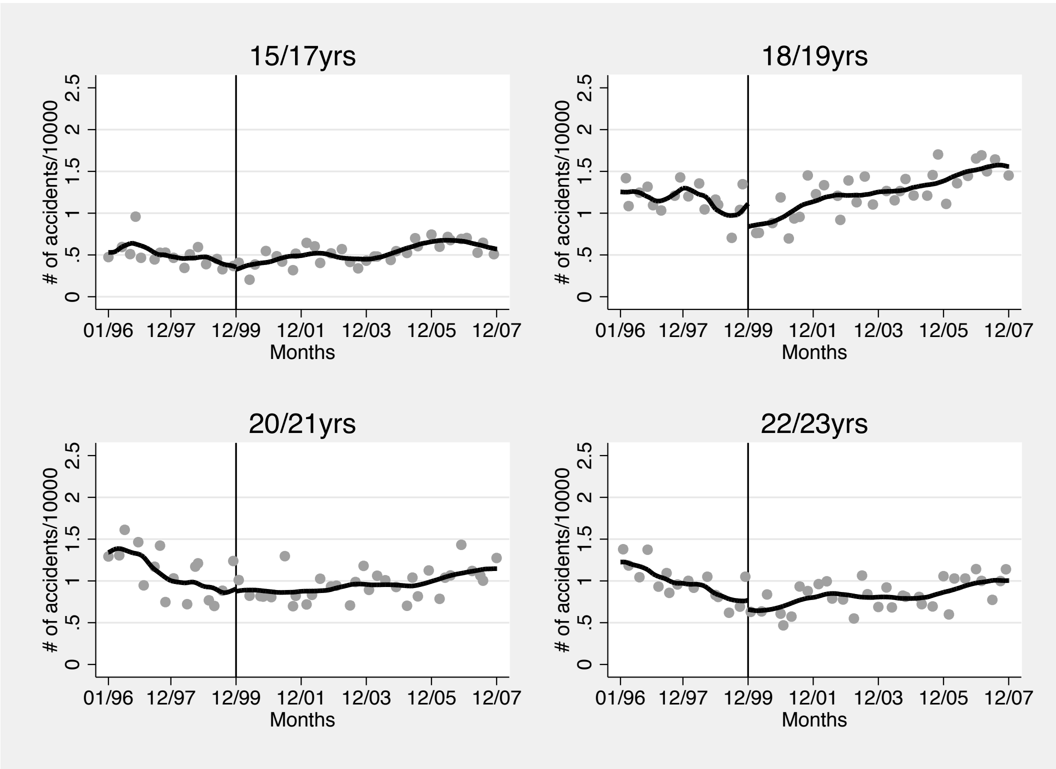
Figures and Tables

Figure 1: Hospital admissions related to alcohol use disorder



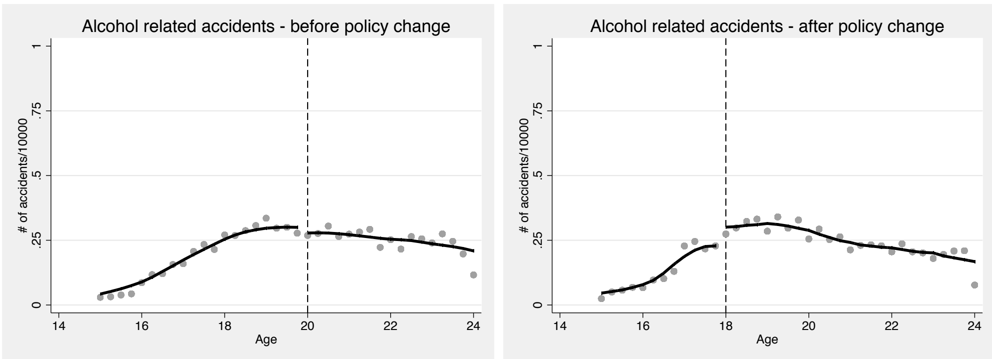
Source: NZ hospital admission data, own calculations. *Notes:* Vertical axes: number of admissions per month per 10,000 in age group population. Admissions related to alcohol use disorder according to ICD V9 diagnoses codes 3050. Reading example: 0.8 in upper left diagram means 8 admissions per 100,000 in age group 15-17yrs in a given month. Reduction in legal drinking age from 20 to 18 in 12/1999 (solid vertical line). Dots mark quarterly averages. Smoothed line based on local linear regression with rule-of-thumb bandwidth.

Figure 2: Vehicular accidents with alcohol involvement



Source: NZ vehicular accidents data, own calculations. *Notes:* Vertical axes: number of vehicular accidents per month per 10,000 people in age group population. Reading example: 1.5 in the upper right diagram means 15 vehicular accidents with alcohol involvement per 100,000 people in the age group 18-19yrs in a given month. Reduction in legal drinking age from 20 to 18 in 12/1999 (solid vertical line). Dots mark quarterly averages. Smoothed line based on local linear regression with rule-of-thumb bandwidth.

Figure 3: Vehicular accidents with alcohol involvement by age



Source: NZ vehicular accidents data, own calculations. *Notes:* Vertical axes: number of vehicular accidents per month per 10,000 people for a given age. Legal drinking age 20 (before policy change, left graph) or 18 (after policy change, right graph) indicated by dashed vertical line. Dots mark averages over pre-policy period (left) or post-policy period (right) for a given age (age in quarters). Smoothed line based on local linear regression with rule-of-thumb bandwidth.

Table 1: New Zealand Health Survey Ages 15-23

	Year 96/97		Year 02/03		Year 06/07	
	mean	se	mean	se	mean	se
A. Drinking and related outcomes						
Drinking alcohol (yes/no)	0.805	(0.013)	0.857	(0.010)	0.834	(0.010)
Frequency of drinking	3.243	(0.127)	3.081	(0.104)	3.136	(0.107)
Number of drinks	4.093	(0.117)	4.262	(0.093)	3.978	(0.087)
Total alcohol consumption	19.95	(1.080)	16.84	(0.696)	16.92	(0.738)
Binge drinking (yes/no)	0.193	(0.013)	0.185	(0.011)	0.193	(0.010)
Injury after drinking (yes/no)	0.102	(0.010)	0.098	(0.008)	0.094	(0.008)
B. Population structure						
Age group 15-17yrs	0.364	(0.016)	0.269	(0.012)	0.392	(0.013)
Age group 18-19yrs	0.175	(0.012)	0.240	(0.012)	0.178	(0.010)
Age group 20-21yrs	0.231	(0.014)	0.239	(0.012)	0.216	(0.011)
Age group 22-23yrs	0.230	(0.014)	0.253	(0.012)	0.213	(0.011)
Female	0.495	(0.016)	0.499	(0.014)	0.502	(0.013)
C. Ethnicity						
Pakeha/European	0.701	(0.015)	0.667	(0.013)	0.595	(0.013)
Maori	0.141	(0.011)	0.165	(0.010)	0.176	(0.010)
Pacific	0.081	(0.009)	0.068	(0.007)	0.078	(0.007)
Asian	0.071	(0.008)	0.092	(0.008)	0.124	(0.009)
Other	0.006	(0.003)	0.008	(0.002)	0.026	(0.004)
D. Further background						
Log(household income)	10.68	(0.021)	10.43	(0.024)	10.68	(0.027)
Urban (as opposed to rural)	0.909	(0.009)	0.876	(0.009)	0.896	(0.008)
Index of deprivation deciles	5.556	(0.100)	5.967	(0.077)	5.788	(0.075)
Number of observations	937		1351		1495	

Source: NZHS, own calculations. *Notes:* Frequency of drinking measured by number of times having at least one drink per month. Number of drinks on a typical day when drinking, zero else. Total alcohol consumption is the product of frequency and number. Binge drinking indicates having 6 or more drinks per occasion at least once per week. Reported numbers are sampling weighted means. Standard errors in parentheses.

Table 2: Outcomes by age group

	Age groups			
	15-17yrs	18-19yrs	20-21yrs	22-23yrs
Drinking alcohol (yes/no)	0.702	0.851	0.871	0.865
	(0.026)	(0.026)	(0.024)	(0.023)
Frequency of drinking	1.935	3.388	4.267	4.150
	(0.170)	(0.255)	(0.308)	(0.283)
Number of drinks	3.224	4.936	4.444	4.404
	(0.194)	(0.268)	(0.252)	(0.228)
Total alcohol consumption	12.91	21.86	25.40	23.63
	(1.681)	(2.016)	(2.583)	(2.353)
Binge drinking (yes/no)	0.133	0.261	0.216	0.212
	(0.019)	(0.032)	(0.029)	(0.027)
Injury after drinking (yes/no)	0.127	0.111	0.073	0.084
	(0.019)	(0.023)	(0.018)	(0.019)
Number of observations	1230	776	849	928

Source: NZHS, own calculations. *Notes:* See Table 1 for a description of variables. Sample means in 1996/97, standard errors (in parentheses).

Table 3: Difference-in-differences results NZHS

	Drinking alcohol	Freq. of drinking	Number of drinks	Log total consump.	Binge drinking	Injury aft. drinking
	(1)	(2)	(3)	(4)	(5)	(6)
A. Without controls						
Age 15-17 × Year 02/03	0.040	0.066	-0.413	-0.021	-0.086*	-0.101***
	(0.041)	(0.429)	(0.376)	(0.148)	(0.043)	(0.033)
× Year 06/07	-0.003	-0.417	-0.498	-0.193	-0.111**	-0.079**
	(0.038)	(0.397)	(0.349)	(0.137)	(0.040)	(0.031)
Age 18-19 × Year 02/03	-0.008	0.805	-0.442	0.100	-0.006	-0.014
	(0.045)	(0.469)	(0.410)	(0.162)	(0.048)	(0.036)
× Year 06/07	0.012	-0.029	-0.305	-0.070	-0.057	-0.026
	(0.045)	(0.472)	(0.412)	(0.162)	(0.048)	(0.037)
Age 20-21 × Year 02/03	-0.049	-1.063**	0.042	-0.185	-0.032	0.009
	(0.043)	(0.453)	(0.395)	(0.156)	(0.046)	(0.035)
× Year 06/07	-0.024	-0.168	-0.149	-0.063	-0.011	-0.015
	(0.042)	(0.444)	(0.387)	(0.153)	(0.045)	(0.034)
B. With controls						
Age 15-17 × Year 02/03	0.013	-0.241	-0.759**	-0.174	-0.109**	-0.110***
	(0.038)	(0.409)	(0.352)	(0.135)	(0.042)	(0.033)
× Year 06/07	-0.052	-0.807**	-1.097***	-0.429***	-0.147***	-0.092***
	(0.035)	(0.379)	(0.327)	(0.126)	(0.039)	(0.031)
Age 18-19 × Year 02/03	-0.030	0.554	-0.603	0.005	-0.020	-0.020
	(0.041)	(0.447)	(0.383)	(0.147)	(0.046)	(0.036)
× Year 06/07	-0.025	-0.301	-0.579	-0.205	-0.074	-0.035
	(0.041)	(0.449)	(0.385)	(0.148)	(0.047)	(0.036)
Age 20-21 × Year 02/03	-0.058	-1.185**	-0.072	-0.242	-0.040	0.004
	(0.040)	(0.432)	(0.370)	(0.142)	(0.045)	(0.035)
× Year 06/07	-0.081**	-0.593	-0.685*	-0.298**	-0.042	-0.030
	(0.039)	(0.423)	(0.363)	(0.139)	(0.044)	(0.034)
Number of observations	3768	3764	3752	3750	3765	3766

Source: NZHS, own calculations. *Notes:* For a description of variables see Table 1. Column 4 uses the log of total consumption plus 1. Each model includes a full set of single age and survey year dummies. Control variables: female, priority stated ethnicity, log of household income, urban, deprivation index. Heteroscedasticity-robust and age-group cluster-adjusted standard errors in parentheses.

*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 4: Shift in hospital admissions in month of legal age reduction

	Age groups			
	15-17yrs	18-19yrs	20-21yrs	22-23yrs
Mean number of admissions before change	0.588	0.514	0.398	0.366
Local linear regression				
Rule-of-thumb bandwidth	0.305***	0.427***	0.218**	0.070
	(0.105)	(0.095)	(0.085)	(0.109)
2/3 × rule-of-thumb bandwidth	0.270**	0.442***	0.237**	-0.082
	(0.120)	(0.108)	(0.103)	(0.126)
3/2 × rule-of-thumb bandwidth	0.253***	0.439***	0.241**	0.083
	(0.120)	(0.108)	(0.103)	(0.126)
with announcement period	0.406***	0.470***	0.288***	0.184*
	(0.108)	(0.135)	(0.109)	(0.106)
Linear model w/o controls	0.334***	0.398***	0.270***	0.127
	(0.086)	(0.087)	(0.099)	(0.078)
Linear model w/ controls	0.338***	0.383***	0.197*	0.186*
	(0.090)	(0.091)	(0.096)	(0.089)

Source: NZ hospital admission data, own calculations. *Notes:* Local linear regression as in Figure 1. With announcement period calculates shift from August to December 1999. Linear model includes a trend before/after the policy, and and indicator for after the policy (reported number). Control variables include gender, ethnicity, month of the year, day of the week, and location. Robust/bootstrapped standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Table 5: Shift in vehicular accidents in month of legal age reduction

	Age groups			
	15-17yrs	18-19yrs	20-21yrs	22-23yrs
Mean number of accidents before change	0.501	1.168	1.096	0.986
Local linear regression				
Rule-of-thumb bandwidth	-0.027	-0.277*	-0.030	-0.112
	(0.084)	(0.165)	(0.145)	(0.126)
2/3 × rule-of-thumb bandwidth	-0.004	-0.338**	-0.134	-0.169
	(0.077)	(0.159)	(0.141)	(0.121)
3/2 × rule-of-thumb bandwidth	0.003	-0.160	-0.036	-0.181**
	(0.062)	(0.114)	(0.094)	(0.073)
with announcement period	-0.121	-0.120	0.127	0.033
	(0.088)	(0.127)	(0.134)	(0.124)
Linear model w/o controls	-0.008	-0.169	0.013	-0.069
	(0.083)	(0.108)	(0.106)	(0.103)
Linear model w/ controls	-0.104	-0.227	-0.039	-0.019
	(0.080)	(0.125)	(0.116)	(0.132)

Source: NZ vehicular accidents data, own calculations. *Notes:* Local linear regression as in Figure 2. With announcement period calculates shift from August to December 1999. Linear models include a trend before/after the policy, and an indicator for after the policy (reported number). Control variables include gender, type of vehicle, month of the year, day of the week, time of the day, and location. Robust/bootstrapped standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Table 6: Shift in alternative outcomes in month of legal age reduction

	Age groups			
	15-17yrs	18-19yrs	20-21yrs	22-23yrs
A. Alcohol intoxication				
Mean number of admissions before change	0.142	0.125	0.126	0.137
Local linear regression	0.023	0.008	0.064**	-0.024
	(0.022)	(0.031)	(0.028)	(0.035)
Linear model w/o controls	-0.019	0.030	0.065*	-0.007
	(0.019)	(0.035)	(0.036)	(0.046)
Linear model w/ controls	-0.014	0.040	0.087**	-0.010
	(0.021)	(0.042)	(0.041)	(0.051)
B. Alcohol dependence				
Mean number of admissions before change	0.040	0.103	0.117	0.145
Local linear regression	-0.008	0.010	0.038	0.039
	(0.058)	(0.070)	(0.063)	(0.069)
Linear model w/o controls	-0.006	0.007	0.032	0.027
	(0.054)	(0.069)	(0.059)	(0.065)
Linear model w/ controls	-0.010	-0.004	-0.044	0.045
	(0.062)	(0.083)	(0.073)	(0.066)
C. Non-alcohol related vehicular accidents				
Mean number of accidents before change	4.386	7.094	6.396	5.357
Local linear regression	-0.389	0.244	-0.191	0.076
	(0.211)	(0.372)	(0.317)	(0.307)
Linear model w/o controls	-0.315	0.376	0.070	0.351
	(0.445)	(0.310)	(0.322)	(0.268)
Linear model w/ controls	-0.336	0.353	-0.095	0.007
	(0.273)	(0.390)	(0.418)	(0.387)

Source: NZ hospital admission and vehicular accidents data, own calculations. *Notes:* Alcohol intoxication includes all admissions with V9 diagnoses codes 2914, 3030; alcohol dependence includes all admissions with V9 diagnoses codes 3039, 2910-3, 2915, 2918-9, 3575, 4255, 5353, 5710-3. Local linear regression with rule-of-thumb bandwidth. Linear models include a trend before and after the policy, and an indicator for after the policy (reported number). Control variables as in Tables 4 and 8. Robust/bootstrapped standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.

Table 7: Difference-in-differences results for hospital admissions and vehicular accidents

	Hospital admissions			Vehicular accidents		
	(1)	(2)	(3)	(4)	(5)	(6)
Age 15-17 × 1yr pre	0.078	0.043	-0.016	0.144	0.101	0.076
	(0.098)	(0.098)	(0.084)	(0.099)	(0.102)	(0.105)
× 1yr post	0.282**	0.260**	0.244**	0.231**	0.113	0.067
	(0.124)	(0.125)	(0.109)	(0.114)	(0.112)	(0.122)
× 2-3yrs post	0.252***	0.226**	0.238**	0.214**	0.126	0.055
	(0.087)	(0.092)	(0.112)	(0.089)	(0.086)	(0.122)
× 3yrs+ post	0.023	0.038	-0.136	0.221***	0.137*	0.011
	(0.070)	(0.084)	(0.163)	(0.072)	(0.071)	(0.165)
Age 18-19 × 1yr pre	0.056	0.020	0.019	0.047	-0.010	-0.024
	(0.094)	(0.099)	(0.091)	(0.134)	(0.130)	(0.130)
× 1yr post	0.361***	0.344***	0.421***	0.053	-0.036	-0.064
	(0.127)	(0.127)	(0.113)	(0.141)	(0.131)	(0.133)
× 2-3yrs post	0.265***	0.221**	0.371***	0.125	0.079	0.037
	(0.094)	(0.099)	(0.126)	(0.112)	(0.108)	(0.119)
× 3yrs+ post	0.431***	0.423***	0.496***	0.342***	0.253***	0.176
	(0.070)	(0.081)	(0.188)	(0.087)	(0.083)	(0.120)
Age 20-21 × 1yr pre	0.104	0.089	0.016	-0.060	-0.113	-0.116
	(0.107)	(0.106)	(0.096)	(0.131)	(0.122)	(0.122)
× 1yr post	0.215	0.195	0.160	0.063	-0.055	-0.068
	(0.132)	(0.123)	(0.110)	(0.135)	(0.133)	(0.133)
× 2-3yrs post	0.255***	0.216**	0.205	-0.030	-0.111	-0.132
	(0.096)	(0.096)	(0.126)	(0.111)	(0.107)	(0.109)
× 3yrs+ post	0.148**	0.145*	-0.063	0.015	-0.046	-0.085
	(0.072)	(0.075)	(0.174)	(0.090)	(0.086)	(0.095)
Controls	no	yes	yes	no	yes	yes
Age specific time trends	no	no	yes	no	no	yes

Source: NZ hospital admission and vehicular accidents data, own calculations. *Notes:* See Tables 4 and 8. Heteroscedasticity-robust and age-group cluster-adjusted standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.5$ * $p < 0.1$.

Table 8: Shift in vehicular accidents at legal drinking age

	Before policy change (1996-1999)	After policy change (2000-2007)
Mean number of accidents just below MLDA	0.287	0.239
Local linear regression		
Rule-of-thumb bandwidth	-0.018	0.076***
	(0.019)	(0.017)
2/3 × rule-of-thumb bandwidth	-0.009	0.065***
	(0.027)	(0.022)
3/2 × rule-of-thumb bandwidth	-0.022	0.076***
	(0.018)	(0.014)
with adjustment period	-0.014	0.085***
	(0.021)	(0.020)
Linear model w/o controls	-0.025	0.099***
	(0.024)	(0.014)
Linear model w/ controls	-0.020	0.090***
	(0.022)	(0.013)

Source: NZ vehicular accidents data, own calculations. *Notes:* Mean number of accidents reported for age just below MLDA as reference. Local linear regression as in Figure 3. Linear models include a linear-in-age function for below and above the legal drinking age, and an indicator for above the legal drinking age (reported number). Control variables include gender, month of birth, type of vehicle, month of the year, day of the week, time of the day, and location. Adjustment period excludes ages MLDA plus 6 months to account for 'party mode' after becoming legal. Robust/bootstrapped standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$.