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ABSTRACT

You Drink, You Drive, You Die? The Dynamics of Youth Risk Taking in Response to a Change in the Legal Drinking Age*

This paper exploits the reduction in the legal drinking age in New Zealand from 20 to 18 to study the dynamics of youth risk taking. Using administrative data on the universe of road accidents over a fifteen year period spanning the law change, we undertake three complimentary analyses to examine the dynamics of alcohol-related and total vehicular accidents among youth. First, using an event history approach, we find no evidence that changing the drinking age from 20 to 18 led to more vehicular accidents or alcohol-related accidents among teens. This is true both in the short-run following the law change and when examining cumulative accidents for the affected cohorts. Next, using an age-based regression discontinuity design (RDD), we find that accidents do increase after one's 18th birthday, but this appears to be a short-run phenomenon. Finally, estimating flexible parametric regression models suggests that reducing the drinking age led to a decline in risky driving by youth who were already 15 at the time of the change but had no longer-run impacts. Overall, our results support the argument that the legal drinking age can be lowered without increasing detrimental outcomes for youth and call into question previous studies that have made policy recommendations by extrapolating from results identified using age-based RDDs.

JEL Classification: I18, K42, C25

Keywords: drinking age, vehicular accidents, regression discontinuity design, dynamics, New Zealand

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1. Introduction

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. However, 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)." Because of the lack of recent policy variation in the MLDA in the US and other OECD countries, the majority of studies discussed in this overview paper use an age-based regression discontinuity design (RDD) to examine how becoming a legal drinker affects a variety of outcomes (for example, Carpenter and Dobkin 2009, 2015; Crost and Guerrero 2012; Crost and Rees 2013; Yörük and Yörük 2011). This approach identifies the impact of the MLDA by comparing outcomes for people slightly younger than 21 to those slightly older and arguing that since nothing other than legal status changes discretely at age 21, then a discrete change in any outcome at this age can plausibly be attributed to a person becoming a legal drinker.

However, ex-ante it is unclear whether the findings from an RDD approach are informative for evaluating the impact of a potential change in the MLDA (or any age-based policy). By design, a standard RDD model cannot identify whether a change at the discontinuity, here a person's 21st birthday, persists as one moves away from this point.[1] Hence, in applications examining an age discontinuity, it is not possible to identify whether a change in outcomes that occur at the threshold (here the MLDA) persists at other ages. This particular distinction is crucial for evaluating the impact of changing the MLDA (or any age-based policy). For example, even if one interprets previous RDD results as providing compelling evidence that reducing the drinking age from 21 to 18 leads to worse outcomes among 18-year-olds, it is impossible to rule out that at the same time outcomes may have improved at other ages, perhaps because it is better to 'learn' how to drink responsibly at a younger age when people are less independent. Overall, without knowing the dynamic effects of a change in MLDA (or any age-based policy), it is impossible to evaluate it normatively.

[1] Angrist and Rokkanen (2015) discuss various approaches for estimating treatment effects away from the threshold in RDD models. Our dynamic analysis discussed below is in the spirit of the parametric approaches that they discuss, but requires weaker assumptions because we also gain identification from observing a change in the threshold (i.e. the MLDA).

This paper addresses this gap in the literature by exploiting the reduction in the MLDA in New Zealand from 20 to 18 that occurred in 1999 to examine the dynamics of youth risk taking. Using administrative data on the universe of road accidents over a fifteen year period spanning the law change, we undertake three complimentary analyses to examine the dynamics of alcohol-related and total vehicular accidents among youth. We first use an event history approach to examine how accident rates changed in the short-run for different age-groups of youth with the change in drinking age. This approach produces causal estimates of the short-run impact of the drinking age as long as any change in outcomes soon after the law change is not caused by other interventions or changes that happened around the same time. Next, we use an age-based regression discontinuity design (RDD) to examine how accident rates change for different cohorts of youth at the point of becoming a legal drinker. This identifies the short-run 'legalization' effect, which other researchers have then suggested is a permanent change and hence equivalent to the long-run treatment effect of changing the MLDA. Finally, we estimate flexible parametric models of the relationship between age and vehicular accidents for different cohorts of youth who faced different legal drinking ages. Combining these analyses allows us to examine the dynamic response of youth to a change in the MLDA and evaluate both the short-run and longer-run impacts of this change.

Overall, we find no evidence that changing the drinking age from 20 to 18 led to more vehicular accidents or alcohol-related accidents among teens. This is true both in the short-run following the law change and when examining cumulative accidents for the affected cohorts. We find that accidents do increase after one's 18th birthday, but this appears to be a short-run phenomenon. Finally, our parametric regression models suggest that reducing the drinking age led to a <u>decline</u> in risky driving by youth who were already 15 at the time of the law change but had no longer-run impacts on youth risky driving among younger cohorts. We speculate that this occurred because of the extensive public discussion about the drinking age change that took place in New Zealand after the change was made and because teens are likely to be particularly focused on the near future. We also present supportive evidence from infrequent health surveys showing a similar pattern for alcohol consumption among different youth cohorts. Our results support the argument that the legal drinking age can be lowered without leading to increases in detrimental outcomes for youth and call into question previous studies that have made policy recommendations by extrapolating from results identified using age-based RDDs.

This paper makes a major contribution to the literature as it is the first to our knowledge that looks at the dynamic impacts of a change in the MLDA.[2] As discussed above and shown in more detail later in the paper, this is crucial for properly evaluating the impact of a prospective change in the MLDA and likely other age-based policies such as youth minimum wages. We also believe that our findings are informative for the active policy debate on this topic in the United States. Importantly, New Zealand is similar to the United States along three key dimensions in the nexus of youth alcohol usage and youth driving. First, New Zealand 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). Second, in both countries, 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; Langley and Smeijers 1997), and alcohol is the most common contributor to serious accidents for this age-group (Connor et al. 2004). Finally, during the time period examined in this paper, New Zealanders could start the process of getting a driver's license at 15.5 years-old which is quite similar to the age that youth can start to drive in many US states. Hence, in both countries it is fairly standard for youth to start driving prior to being able to legally purchase alcohol under all policy regimes implemented or being considered.

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 administrative data used to measure vehicular accidents. In section 4, we present the results from the three complimentary analyses briefly discussed above. In section 5, we present supportive results on youth alcohol consumption and discuss our findings more broadly. We then conclude in section 6.

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.[3] The impetuous for this law change

[2] Conover and Scrimgeour (2013) use both an event history analysis and age-based RDD to examine the impact of the change in the MLDA in New Zealand on alcohol-related hospitalisations. However, they do not attempt to estimate a dynamic model as we do and hence they can only examine the short-run impacts of the policy change over time and at the point of becoming a legal drinker.

[3] 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. However, to remain consistent with common usage and the international literature we will continue to call this the 'drinking age' or MLDA.

was a 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, this can be interpreted as 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 Sundays.[4] While our event history estimates will be contaminated by these additional changes and will measure the impact of the full set of changes brought about by the SLAA1999, 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. 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, which would bias us towards finding larger effects of the change in MLDA.

As part of the change, it was agreed that the policy would be reviewed over a period of time. For this reason, there has been fairly constant media coverage on the topic for extensive periods. By 2006, there was 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 narrowly failed.[5] 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. Other changes were made at this point to reduce access to alcohol but without a particular focus on youth drinking, for example reductions in opening hours for bars.

During the time period examined in this paper, the rules around both drivers licenses and drinking and driving remained constant. New Zealanders at age 15 years and 6 months could,

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[4] 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.

[5] Boes and Stillman (2013), which examines the short-run impact of the change in drinking age on a number of outcomes, also examines the impact of this failed law change and shows it has no impact on alcohol consumption, vehicular accidents or alcohol-related hospitalizations.

by passing a written driving test and a short practical exam, 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. Hence, this regime was quite similar to that in many US states and the majority of New Zealanders start the process of becoming a legal driver before reaching either the old or new legal drinking age. The legal BAC limit for driving in New Zealand during the period examined in this study was 80 milligrams of alcohol per 100 millilitres of blood for adults aged 20 years and older and 30 milligrams per 100 millilitres of blood for drivers younger than 20 years. Importantly, the rules around drinking and driving did not change when the MLDA was modified.

3. Data

We obtained from the Ministry of Transport data on every vehicular accident (i.e. cars, trucks and motorcycles) in New Zealand from 1 January 1996 to 30 June 2011 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, type of vehicle, number of passengers and the 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 breathalyser 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.[6]

For our descriptive statistics and parametric regression models, we use the estimated resident population by age, gender and quarter of year produced by Statistics New Zealand to calculate accident rates per 10,000 population for each age-group.[7] An estimate of the at-risk population denominator is not needed for either the event history or RDD analyses since the population is fairly constant over the short time periods examined using these approaches.

[6] The data manager for the Ministry of Transport recommended coding alcohol involvement in this manner are results are not sensitive to alternative choices such as just having failed an alcohol test.

[7] We also have information on the number of youth in different age bins with new drivers licenses on a monthly basis starting in July 2001. Our results are not sensitive to using these figures to calculate an alternative denominator of the at-risk population in the later time periods.

We focus on three outcomes throughout the paper: i) total number of accidents; ii) number of alcohol related accidents; and iii) the proportion of total accidents that are alcohol related.[8] Each of these measures has strengths and weaknesses. The number of alcohol related accidents is a direct measure of whether youth are more likely to drink and drive. However, it might understate the overall impact of a change in access to alcohol if increased access leads to more risky driving behaviour in general and not just an increase in drinking and driving. One concern with looking at either the absolute number of accidents or per capita accident rates is that there might be secular improvements in both road and vehicle quality that lead to differential reductions in accidents for different aged youth. Under the assumption that there are no spillovers from alcohol access to non-alcohol related accidents and that secular improvements impact both types of accidents proportionately, examining the proportion of accidents that are alcohol related gives an unbiased measure of the impact of access to alcohol on drinking and driving.

Figure 1 plots each of these outcomes over time for three age-groups, i) 15-17 year-olds who are potentially indirectly impacted by the change in drinking age through easier access; ii) 18-19 year-olds who were directly impacted by the change in the drinking age from 20 to 18; and iii) 20-21 year-olds who were not impacted by the policy change and hence provide an ideal control group for evaluating the impact of the change in MLDA. Also, indicated on these graphs by two dashed vertical lines are the announcement date of the policy change on August 31, 1999 and the implementation date of December 1, 1999. Because of the noisiness of the data, we aggregate accidents to the monthly level and each of the plots is smoothed using a local linear regression with a bandwidth of four months. The figures for total number of accidents and number of alcohol related accidents are per 10,000 population.

Total and alcohol related accident rates declined steadily for all three age-groups from the beginning of our sample in January 1996 to around June 2000 before trending back up until around January 2007 and then declining steadily for the rest of our data window. The data for the proportion of accidents that are alcohol related looks fairly constant over time for all three age-groups with perhaps a slight increase over time for each age-group. Table 1 shows the same information but aggregated at the weekly level over the entire time period split into before and after the policy change. Here again, there is very little evidence of a change in any

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

of the three outcomes for either 15-17 year-olds or 18-19 year-olds, with perhaps some indication of a small decrease in alcohol related accidents and the proportion alcohol related for 20-21 year-olds.

Kypri et al. (2006; 2016) and Huckle and Parker (2014) use this same data to estimate difference-in-differences models of the impact of the drinking age change on accidents in New Zealand and find that the reduction in the MLDA lead to more alcohol related accidents for 18-19 year-olds and perhaps for 15-17 year-olds in both the short- and longer-run. None of these papers include any control variables, age-specific trends or allow for different pre-existing age differences in accident rates. In Boes and Stillman (2013) we show that these results are highly sensitive to how the difference-in-differences model is specified and that once we include control variables and allow for age-specific accident rates and trends, we do not find any evidence of short-run impacts of the change in the MLDA on either 15-17 or 18-19 year-olds.

Evidence for longer-run impacts is more mixed and depends crucially on whether one includes data for after 2008 when accident rates show large declines for youth. This speaks more generally to why a difference-in-differences approach is not suitable for estimating long-run policy effects without carefully controlling for other potential time-varying explanations for changes in the outcome. For example, the data in Figure 1 strongly suggests that business cycles are correlated with accident rates and that this correlation differs by age-group.

Because of these issues with a difference-in-differences modelling approach and because we have ideal data for doing both an event history and regression discontinuity design along with a more parametric regression approach, we proceed in the next section by undertaking the three complimentary analyses discussed in the introduction. First, we use an event history approach to examine how accident rates changed in the short-run for different age-groups of youth with the change in drinking age. Next, we use an age-based regression discontinuity design (RDD) to examine how accident rates change for different cohorts of youth at the point of becoming a legal drinker. Finally, we estimate flexible parametric models of the relationship between age and vehicular accidents for different cohorts of youth. Combining these analyses allows us to examine the dynamic response of youth to a change in the MLDA and evaluate both the short-run and longer-run impacts of this change.

4. Main Analysis and Results

4.1 Event History Estimates of the Short-run Impacts of Changing the MLDA

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 vehicular accidents for a particular age-group shortly before the policy change to just after should reveal the causal short-run 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 drinking among 18-19 year-olds "celebrating" their new legal drinking status. Moreover, road accidents 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 our three outcomes 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 This is precisely what is done in an event history study. The key identification assumption with this approach is that any change 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 relevant 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:

$$y = \alpha + \beta * dt + g^{-}(time) + g^{+}(time) + \varepsilon \qquad (1)$$

where y is one of the three outcomes (total number of accidents, number of alcohol related accidents, the proportion of total accidents that are alcohol related), 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 three age-groups used above; 15-17, 18-19 and 20-21. There exist several approaches to estimating (1), 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 below. In particular, we will specify g as a step-wise linear function with steps at different points before and after the policy change, and contrast these parametric results with a local linear smooth before and after the change.

Figure 2 shows the results from the nonparametric approach for the three outcomes discussed above aggregated at a weekly level. Because we are only examining a short time period, the at-risk population should be nearly constant and hence our focus here is on the number of accidents in absolute terms and the proportion that are alcohol related. In each graph, the dots represent the number (or proportion) of accidents for a particular age-group in a particular week relative to December 1, 1999. We examine outcomes in the 25 weeks after the policy change compared with the 25 weeks prior. Because of the potential importance of seasonality in accidents, we first use data for the year prior to the policy change to remove both week of the year and day of the week effects.[9] We also show two dashed vertical lines for the announcement date of the policy change on August 31, 1999 and the implementation date of December 1, 1999. The solid line and the associated confidence interval are then calculated using local linear regression estimates separately on each side of the policy change with a rule-of-thumb bandwidth (Imbens and Kalyanaraman 2012).

The results show that there are no immediate shifts in traffic accidents due to the policy change. All graphs are almost smooth in December 1999 with no indications of any discontinuity in outcomes. Table 2 displays the coefficient associated with the corresponding parametric analysis. Here, the data are examined on a daily basis with outcomes in the announcement period and three post-reform periods (0-29 days, 30-89 days and 90-183 days) compared to outcomes in the 183 days prior to the policy change minus the announcement period. Again, data from the previous year is used to control for week of the year effects, but otherwise does not contribute to the estimates. We also control for day of the week effects and age effects within age-groups. As in the non-parametric analysis, we find no evidence that reducing the MLDA from 20 to 18 in December 1999 led to a short-run increase in accidents, alcohol-related accidents or the proportion of accidents that are alcohol related for either 15-17 or 18-19 year-olds. If anything, there is weak evidence for a very short-run (in the 30-89 days after the policy change) decline in alcohol-related accidents for 18-19 year-olds.[10]

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[9] In practice, we use the weekly data from one and a half years before and half a year after the policy change and regress each of the three outcomes on week of the year and day of the week fixed effects per age group. From these regressions, we extract the residuals, normalized around the mean of the outcome over the two-year period. These residuals are then shown in the plots. Our estimates are qualitatively unaffected by this adjustment.

[10] These results are robust to dropping the control for the announcement period, for further controlling for information about the accident, such as the driver's gender and the location, and for defining the post-reform period in finer or broader grouping. We also examined separate impacts by gender and find no significant impacts for either men or women.

4.2 Age-Based RDD Estimates of the Short-run Impacts of Becoming a Legal Drinker

Next, we follow the approach in the papers discussed above to examine the impact of reaching the legal drinking age. Specifically, we estimate a regression discontinuity design (RDD) where the running variable is age and the discontinuity occurs at exactly the MLDA. The regression model estimated here can be written as:

$$y = \alpha + \beta * dMLDA + g\text{-}(age) + g\text{+}(age) + \varepsilon \qquad (2)$$

where y is one of the three outcomes (total number of accidents, number of alcohol related accidents, the proportion of total accidents that are alcohol related), *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. We use data from +/- 183 days around each individual's 18th and 20th birthday to examine changes in outcomes for three separate time periods: i) prior to the change in drinking age; ii) the first six years after the change; iii) the remaining six year period. Again, we do this both using a non-parametric analysis with g flexibly estimated from the data using local linear regression and a regression analysis, which further controls for birthday effects (none, just an individual's birthday, +/- 7 days, +/- 30 days around each birthday) and for a linear trend in accidents before and after one's birthday.[11]

Figure 3, as in Figure 2, shows the results from the nonparametric approach for the three outcomes aggregated at a weekly level. Again, because we are only examining a short time period, the at-risk population should be nearly constant and hence our focus here is on the number of accidents in absolute terms and the proportion that are alcohol related. In each graph, the dots represent the number (or proportion) of accidents for a particular week relative to a person's 18th birthday in a particular time period. In the years 1996-1998, turning 18 had no impact on access to alcohol, so unsurprisingly there is no evidence of a discontinuity in any of the outcomes. However, in the first six years after the change of the drinking age to 18, we find evidence for an increase in both total accidents and alcohol-related accidents in the 25 weeks after a person's birthday relative to the 25 weeks prior. This is similar to the previous findings in the literature for the US and Canada. Interestingly, when we repeat the analysis for the later years in our data (from 2006-2011), we no longer find any evidence for a discontinuity in any outcome. We suspect this relates to the intensive media coverage in this later period

[11] Birthday effects are potentially quite important for estimating an age-based RDD of the impact of becoming a legal drinker since one can imagine that the days just after becoming a legal drinker include a lot of celebrations.

around the possibility of returning the drinking age to 20 and/or the general decline in accident rates that is found for all groups of youth in this period likely because of the economic slowdown taking place at the same time.[12]

Figure 4 shows the same results for the RDD around the 20th birthday in each period. In the 1996-1998 period, this was the MLDA and we see some weak evidence of an increase in alcohol-related accidents around one's birthday. However, this increase is insignificant both here and in the parametric analysis. Since we only have three years of data prior to the policy change, we might not have enough power to identify a discontinuity, or it could just be the case that alcohol-related accidents did not increase around the old MLDA. Interestingly, in the later periods, we see some evidence for a decrease in alcohol-related accidents after an individual's 20th birthday. In New Zealand, the 20th birthday is traditionally seen as the start of 'adulthood' and most individuals have a large party to celebrate reaching this milestone. These results suggest that perhaps individuals start behaving more maturely as well at this age, which could also explain why alcohol-related accidents did not increase significant at 20 when it was the MLDA.

Table 3 shows the results from the parametric RDD models which use data on a daily basis and include controls for linear trends in accidents before and after the birthday. We present the results from four specifications where the only variation is how birthday effects are treated. We estimate these models excluding either no days, just the birthday, one week before and after the birthday and 30 days before and after the birthday. Confirming the results in Figure 3, we find that accidents increase by around 16% after an individual turns 18 in the years 2000-2005. This increase occurs for total accidents, but not alcohol-related accidents but could still be attributed to riskier behaviours that occur because of increased access to alcohol. This finding is not sensitive to controlling for traditional birthday effects (e.g. one day or one week), but we no longer find an impact on total accidents if we drop the month before/after the birthday. This suggests that what we are seeing here is a short-run impact directly related to having the birthday per se and not a longer-run change in behaviour. Our next analysis will look much closer at this point. As in Figure 4, we also find a significant decline in alcohol-related accidents at the 20th birthday in the years 2000-2005. This is also robust to controlling

[12] We have also estimated the RDD effects on a year by year basis but no interesting patterns emerge besides the disappearing of the discontinuity from 2006 and on.

for birthday effects although loses significance when we drop the month before/after the birthday.

4.3 Accident Dynamic by Age and Cohort

We now use a parametric approach to examine how each of the three outcomes vary by age and for different cohorts of youth who experienced different MLDAs. We define four cohorts for all the analysis in this section. Individuals in the oldest cohort, called 'cohort 20+', were already age 20 when the drinking age was changed in December 1999 and hence were unaffected by the change (and also did not know that it would happen) and can be thought of as a counterfactual group for evaluating the impact that the change had on the other three cohorts. Individuals in the second oldest cohort, called 'cohort 18-20', were between 18 and 20 when the law change occurred and hence on December 1st 1999 went overnight from being illegal to purchase alcohol to being completely legal. This is the group on which our event history analysis focuses. Individuals in the next oldest cohort, called 'cohort 15-18', were between 15 and 18 at the time the MLDA was changed and hence became legal drinkers on their 18th birthday. Most people in this group were already experienced drivers and many would have been drinking as well when the law changed. Individuals in the youngest cohort we examine, called 'cohort 15-', were less than 15 at the time that the law changed. Like the previous cohort they also became legal drinkers on their 18th birthday, but for this group, the change occurred before they were driving or likely had much experience with alcohol.

In Figure 5, we begin by presenting the raw data on both accident rates and cumulative accidents per 10,000 population for these four cohorts of youth starting at age 16 and continuing until age 24.[13] Rates are per month and smoothed over 4-month periods. The age pattern is similar for all four cohorts, accident rates increase sharply from 16 to 18.5 and then decline slowly. This is true for both total accidents and alcohol-related accidents, but the initial increase with age is much steeper for alcohol-related accidents. For this reason, the proportion of accidents that are alcohol-related also increases sharply between 16 and 18.5 and is then steady between 15 and 20 percent until age 24.

Comparing total accidents by age across cohorts, we see a distinct pattern. Accident rates are highest for the 20+ cohort until age 21. The two cohorts that either were directly affected by the policy change (cohort 18-20) or already were established drivers (and maybe drinkers)

[13] There are very few accidents for 15 year-olds drivers and we do not see the oldest cohort at this age.

when it happened (cohort 15-18) both have much lower accident rates than the 20+ cohort. This lower accident rate persists for the 18-20 cohort until they are age 22, and for the 15-18 cohort until they are age 20. After these ages, both of these cohorts have very similar accident rates to the 20+ cohort at all ages. Hence, as can be seen in the bottom-left graph, these two cohorts experience fewer cumulative accidents during their youth. For example, until age 20 the 20+ cohort has accumulated approximately 33 accidents per 10,000 population, compared to 29 accidents for the 18-20 and 15-18 cohorts. By age 18, the 18-20 and 15-18 cohorts have experienced approximately 11 percent fewer accidents than the 20+ cohort and this difference persists until age 24. This evidence suggests that lowering the MLDA actually led to less risky behaviours at least on the roads for the most directly affected cohorts.

Turning to the youngest cohort in our sample, accident rates are slightly lower than for the 20+ cohort until around 18.5 but then slightly higher at later ages. For this cohort, the cumulative accidents are nearly identical to those experienced by the 20+ cohort at all ages. This suggests that as the policy change has gotten less salient, youth risky behaviour on the roads has returned to what appears to be a stable age pattern.

The figures for alcohol-related accidents tell a nearly identical story especially in terms of cumulative accidents but with a few subtle differences in the time pattern. For the 18-20 cohort, alcohol-related accidents occurred at a higher rate from age 16 to 17 than for the other cohorts. This is in the period prior to when the change in the MLDA was even being discussed so is most likely driven just by randomness during an age with quite low accident rates. On the other hand, alcohol-related accident rates remain lower than for other cohorts at all older ages and hence there is an even larger gap in the cumulative alcohol-related accidents experienced by this cohort compared to the oldest cohort than the gap in total accidents. Until age 20, the 20+ cohort has accumulated approximately 5.3 alcohol-related accidents per 10,000 population, compared to 4.6 for the 18-20 cohort. Relative to the 20+, cumulative alcohol-related accidents are approximately 13 percent less for the 18-20 cohort by age 18 and that gap remain until age 24. Alcohol-related accident rates are also lower at all ages for the 15-18 cohort so this group also has significantly less cumulative alcohol-related accidents at all ages than the 20+ cohort. On the other hand, again the pattern for the 15- cohort is nearly identical to that for the 20+ cohort.

Interestingly, the ratio of alcohol-related to total accidents is quite stable across cohorts at all ages, except for the higher rate of alcohol-related accidents seen for the 18-20 cohort at age

16 to 17. There is also some evidence that by age 21, the proportion of accidents that are alcohol-related stabilises at a lower rate for the 15-18 and 18-20 cohorts relative to the 20+ and 15- cohorts. However, looking at the proportion of accidents that are alcohol-related in cumulative terms shows that this has barely changed over time with around 10% of cumulative accidents being alcohol related at age 16 and this increasing steadily over time until reaching 20% at age 20 and remaining at that rate until age 24.

We next estimate a simple regression model that is designed to capture the pattern seen in the raw data but allow us to also control for other potentially important factors determining accident rates, such as age-specific seasonal patterns or secular changes in road safety. Specifically, we estimate the following regression model:

$$y = \alpha + \sum_{j}^{4} \beta^{j,0} age^{j} + \sum_{c}^{3} \sum_{j}^{4} \beta^{j,c} age^{j} * (cohort = c) + \delta X + \varepsilon \tag{3}$$

where y is one of the three outcomes: (1) total number of accidents per 10'000 population in the respective age/cohort, (2) total number of alcohol related accidents per 10'000 population in the respective age/cohort, (3) proportion of alcohol related accidents relative to total number of accidents and control variables include a polynomial function of degree 4 in person age, with the main effect for cohort 20+, interactions of the polynomial terms with indicators for the cohorts 18-20, 15-18, and 15-, and controls for age-specific seasonality and time trends, and cohort effects. Data for this regression model is aggregated to the crash month and person age in months level and standard errors are clustered on the monthly level.

Table 4 presents the coefficients on the cohort dummies and the age effects for each cohort. The top row of Figure 6 graphs the fitted age profiles from these regressions. Comparing Figure 6 to Figure 5 shows that the parametric model with a 4th order polynomial does a good job fitting the age patterns in the raw data. As can be seen in both the table and the graphs, the age profiles for the 18-20 and 15-18 cohort are significantly different from that for the 20+ cohort. Specifically, total accident rates and alcohol-related accident rates at younger ages are lower for these two cohorts. On the other hand, the pattern for the 15- cohort is nearly identical to that found for the 20+ cohort for both outcomes, although with a statistically significant difference for total accidents, presumably due to the differences at higher ages. Similar to what is seen in the raw data, no significant differences are found between cohorts in the age profiles of the proportion of accidents that are alcohol related.

The bottom graphs in Figure 6 show the estimated marginal effects of the impact of an extra month of age on each outcome. As seen in the raw data, total accident rates increase sharply with age up until around age 18.5 after which marginal effects are approximately zero for all cohorts. The age pattern of this increase is similar for all cohorts, however, marginal effects are lower at younger ages for the 18-20 cohort. For this cohort in particular, the relationship between age and total accident rates is much flatter, which explains why cumulative accident rates are lower for this cohort. If decreasing the MLDA from 20 to 18 led to more risky driving by youth in this age range, one would expect to find the exact opposite result for this cohort, i.e. there should be a larger increase in accident rates with age than for other cohorts. A similar pattern is found when looking at alcohol-related accidents, and hence, we find no evidence that the reduced drinking age led to more risky behaviour on the roads for this cohort. In fact, we find evidence that their behaviour was relatively safer than for the previous cohort, who faced a higher drinking age, at the same ages. Interestingly, for the youngest cohort in our data, the age-accident profile is nearly identical to the older cohort suggesting that the age-accident profile is stable over time regardless of the MLDA.

5. Impacts on Alcohol Consumption

Our findings above clearly show that reducing the legal drinking age did not led to longer-run changes in the age-accident pattern for youth cohorts. While this is an important finding in its own right as vehicular accidents account for more than half of total fatalities and are the second most common cause of hospitalisation, after pregnancy, for teenagers, our analysis so far cannot identify whether this reflects that changing the MLDA had no longer-run impact on youth drinking or whether there was instead a change in drinking behaviours but no corresponding increase in vehicular accidents.[14] Distinguishing between these two possibilities is important for interpreting our results and making policy recommendations.

Unlike for vehicular accidents, administrative data does not exist in New Zealand for alcohol consumption. However, the New Zealand Health Survey (NZHS) collects representative cross-sectional data using face-to-face interviews with individuals aged 15 years and older on the health status of the residential New Zealand population, and the prevalence of risk and protective factors associated with these health conditions, and included among these risk

[14] Supporting the second possibility, Lindo et al. (forthcoming) using an age-RDD approach find that, in the Australian state of New South Wales, alcohol consumption increases when youth become legal drinkers but there was no corresponding change in vehicular accidents. They argue that this occurs because there is a strong focus on reducing teen drinking and driving in that state.

factors is a variety of information on alcohol consumption (Ministry of Health 1999; 2004; 2008). While the NZHS provides the most comprehensive information on alcohol consumption over time available in New Zealand, it is unfortunately only fielded infrequently, during the sample period covered by our data on vehicular accidents, specifically in 1996/97, 2002/03 and 2006/07.[15]

In Figure 7, we present graphs analogous to the top row in Figure 5 for three measures of alcohol consumption: (1) the percent of individuals at a particular age who drank alcohol in the last year; (2) the average number of days per month in which individuals at a particular age drank alcohol in the last year (including not drinking); and (3) the average number of drinks consumed by individuals at a particular age who do drink in a typically drinking session.[16] These graphs show this information from age 18 to 24 for cohort 20+ and from age 15 to 22 for cohort 15-. Unfortunately, because of the timing of the NZHS, we only have data for these age ranges for these cohorts and a couple of age points for the 15-18 and 18-20 cohorts. Hence, we can only look at the longer-run effects of the change in the MLDA and not the transition dynamics in this analysis.

The results here are striking. The age pattern for all three outcomes are nearly identical for the two cohort with the exception of a slightly higher proportion of the 15- cohort reporting that they have drunk any alcohol in the last year. This is true even though the age pattern of these outcomes is non-linear. For example, the frequency of consumption increases steeply with age from only 1 day per month for 15 year-olds to nearly 4.5 days per month for 20 year-olds and then slightly declines before stabilizing at 4 days per month. Consumption per session also steeply increases from 2 drinks per session for 15 year-olds to around 5 drinks per session for 19 year-olds and then slightly declines before stabilizing at 4.5 drinks per session. These

[15] The NZHS was also fielded in 1992/93 but this wave did not collect data on alcohol consumption. The survey switched to an annual basis but with a smaller sample size in 2011/2012 after the end of our data on vehicular accidents. We do not currently have access to this data.

[16] 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. We 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.

results provide strong suggestive evidence that changing the MLDA had no longer-run impact on youth drinking behaviour along with no impact on youth risky 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 exploits this change to examine the dynamics of youth risk taking. Using administrative data on the universe of road accidents over a fifteen year period spanning the law change, we undertake three complimentary analyses. First, we use an event history approach to examine how accident rates changed in the short-run for different age-groups of youth with the change in drinking age. Next, we use an age-based regression discontinuity design (RDD) to examine how accident rates change for different cohorts of youth at the point of becoming a legal drinker. Finally, we estimate flexible parametric models of the relationship between age and vehicular accidents for different cohorts of youth who faced different legal drinking ages. Combining these analyses allows us to examine the dynamic response of youth to a change in the MLDA and evaluate both the short-run and longer-run impacts of this change.

Overall, we find no evidence that changing the drinking age from 20 to 18 led to more vehicular accidents or alcohol-related accidents among teens. This is true both in the short-run following the law change and when examining cumulative accidents for the affected cohorts. We find that accidents do increase after one's 18th birthday, but this appears to be a short-run phenomenon. Finally, our parametric regression models suggest that reducing the drinking age led to a <u>decline</u> in risky driving by youth who were already 15 at the time of the law change but had no longer-run impacts on youth risky driving among younger cohorts. We speculate that this occurred because of the extensive public discussion about the drinking age change that took place and because teens are likely to be particularly focused on the near future. We also present supportive evidence from infrequent health surveys showing a similar pattern for alcohol consumption among different youth cohorts. Our results support the argument that the legal drinking age can be lowered without leading to increases in detrimental outcomes for youth and call into question previous studies that have made policy recommendations by extrapolating from results identified using age-based RDDs.

Overall, our results support the argument being made by groups like Amethyst Initiative and Choose Responsibility (see http://www.chooseresponsibility.org/proposal/) that the legal drinking age can be lowered without leading to 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.

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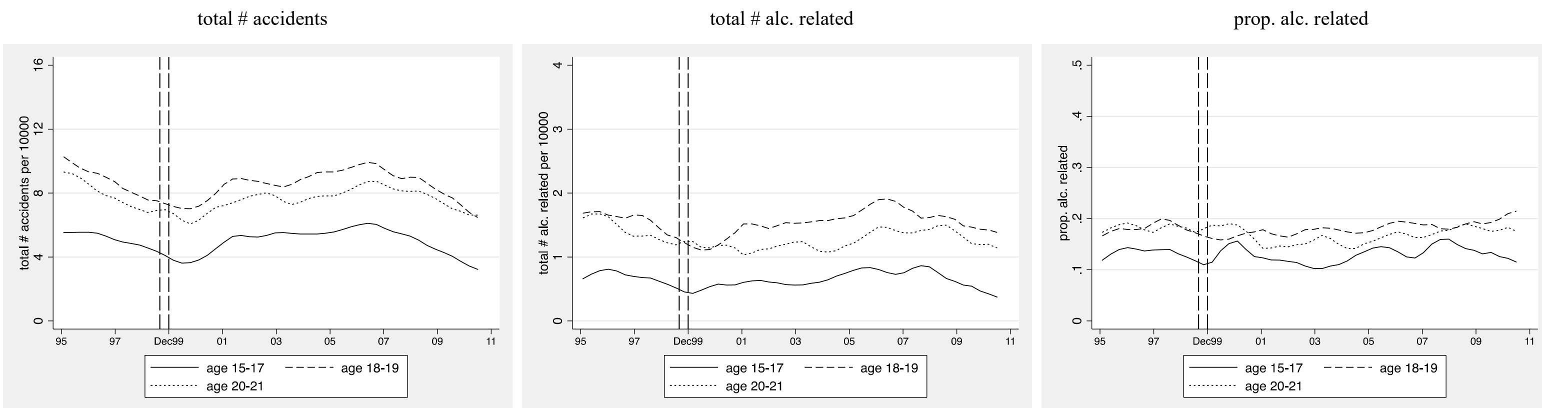
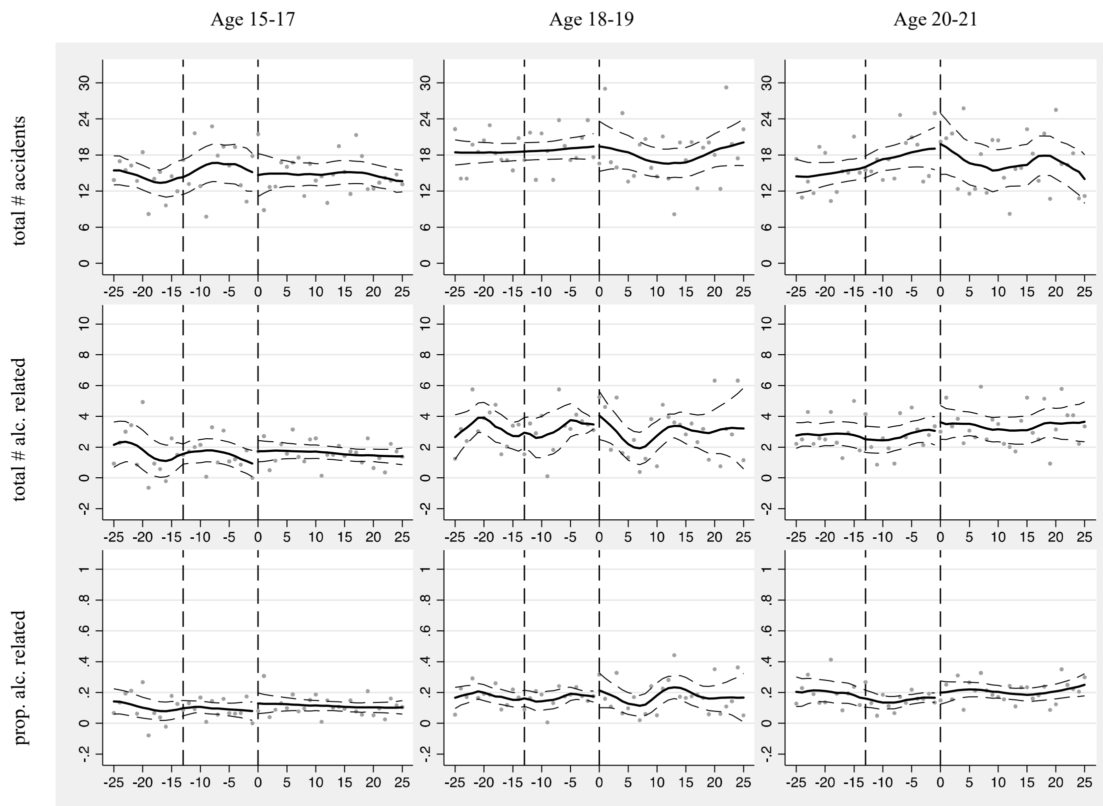
Figures

Figure 1: Development of accidents over time by age group



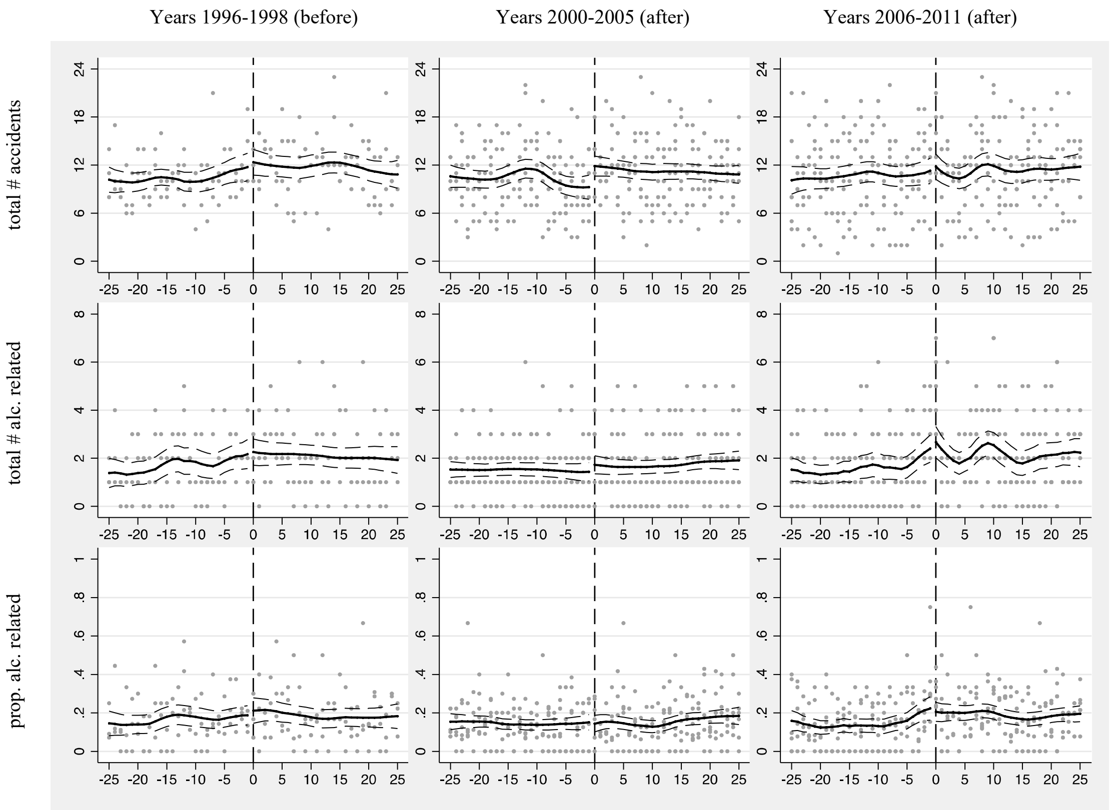
Source: NZ vehicular accidents data, own calculations. *Notes:* Data are aggregated on a monthly level (horizontal axis), i.e., total number of (alcohol related) accidents per age group and month per 10,000 population. Raw data are smoothed with local linear regression (bandwidth = 4 months). In each graph, right vertical line indicates time of policy change (Dec 1, 1999), left vertical line indicates date of announcement (Aug 31, 1999). Proportion of alcohol related accidents is relative to total number of accidents.

Figure 2: Event history analysis for the change in the MLDA (-/+ 6 months)



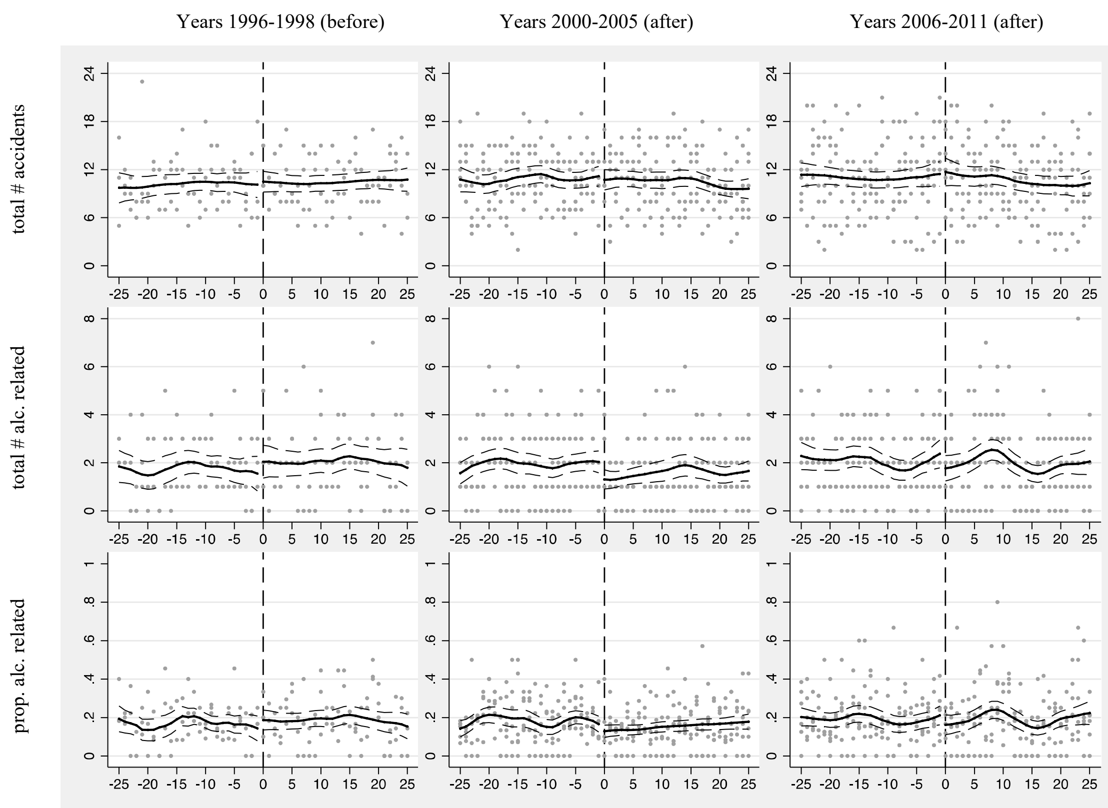
Source: NZ vehicular accidents data, own calculations. *Notes:* Data are aggregated on a weekly level (horizontal axis), i.e., total number of (alcohol related) accidents per age group and week half a year before and after the date of the policy change, adjusted for week of the year and day of the week effects (using an additional pre-policy year). Right vertical line indicates time of policy change (Dec 1, 1999), left vertical line indicates date of announcement (Aug 31, 1999). Proportion of alcohol related accidents is relative to total number of accidents.

Figure 3: Age-based RDD around 18th birthday



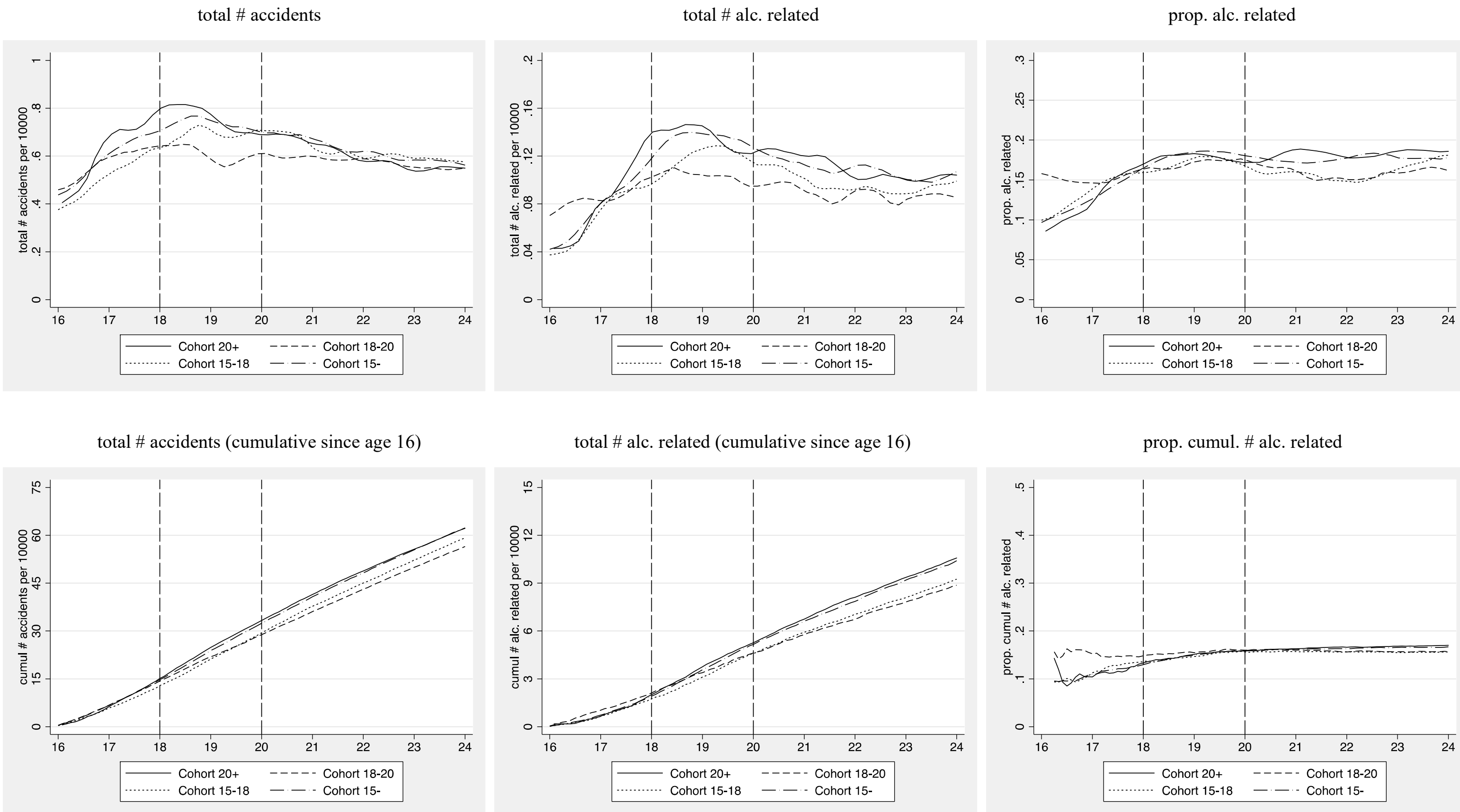
Source: NZ vehicular accidents data, own calculations. *Notes:* Data are aggregated on a weekly level (horizontal axis), i.e., total number of (alcohol related) accidents per week. In each graph, vertical line indicates 18th birthday. Proportion of alcohol related accidents is relative to total number of accidents.

Figure 4: Age-based RDD around 20th birthday



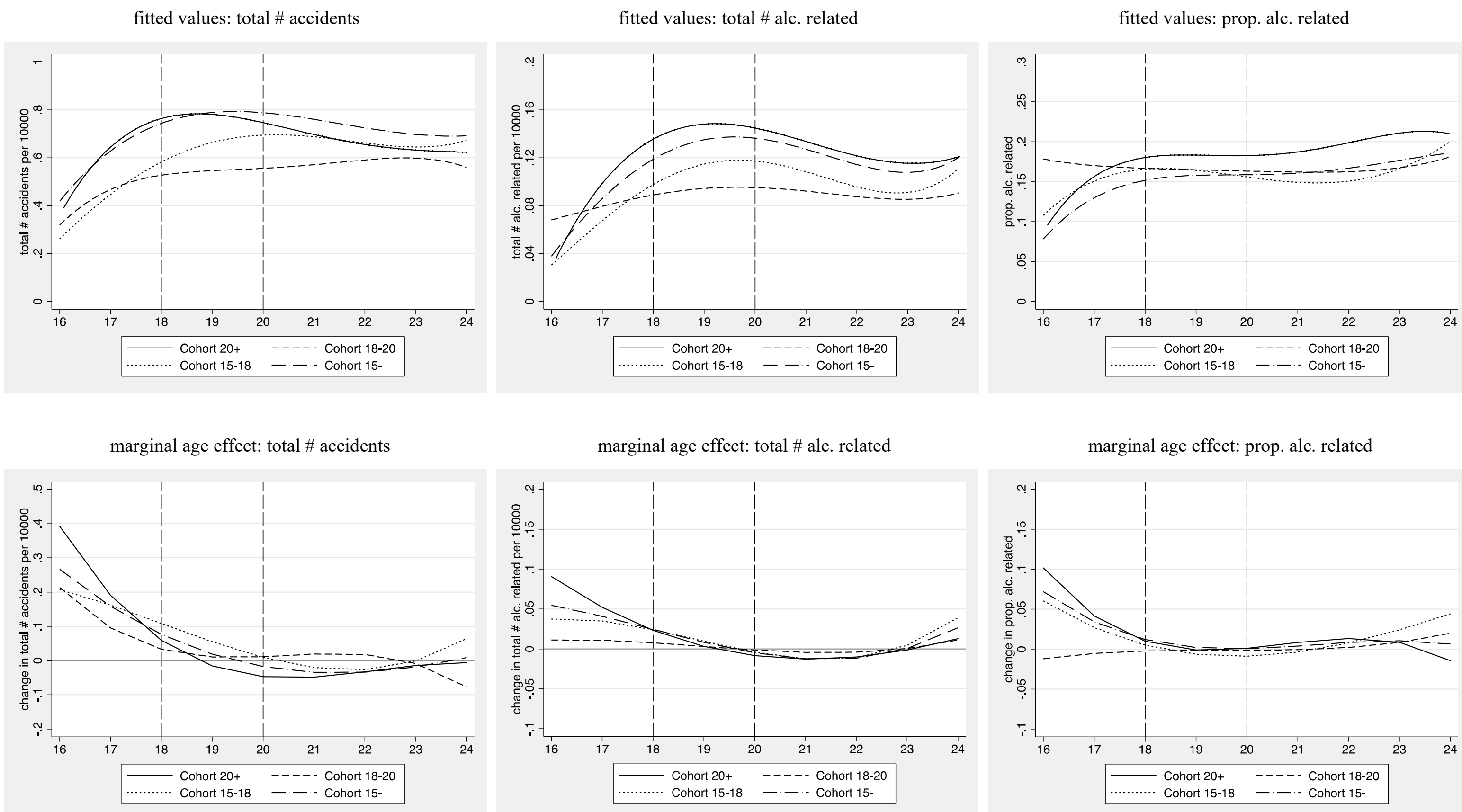
Source: NZ vehicular accidents data, own calculations. *Notes:* Data are aggregated on a weekly level (horizontal axis), i.e., total number of (alcohol related) accidents per week. In each graph, vertical line indicates 20th birthday. Proportion of alcohol related accidents is relative to total number of accidents.

Figure 5: Distribution of accidents over age range 16-24 by cohort



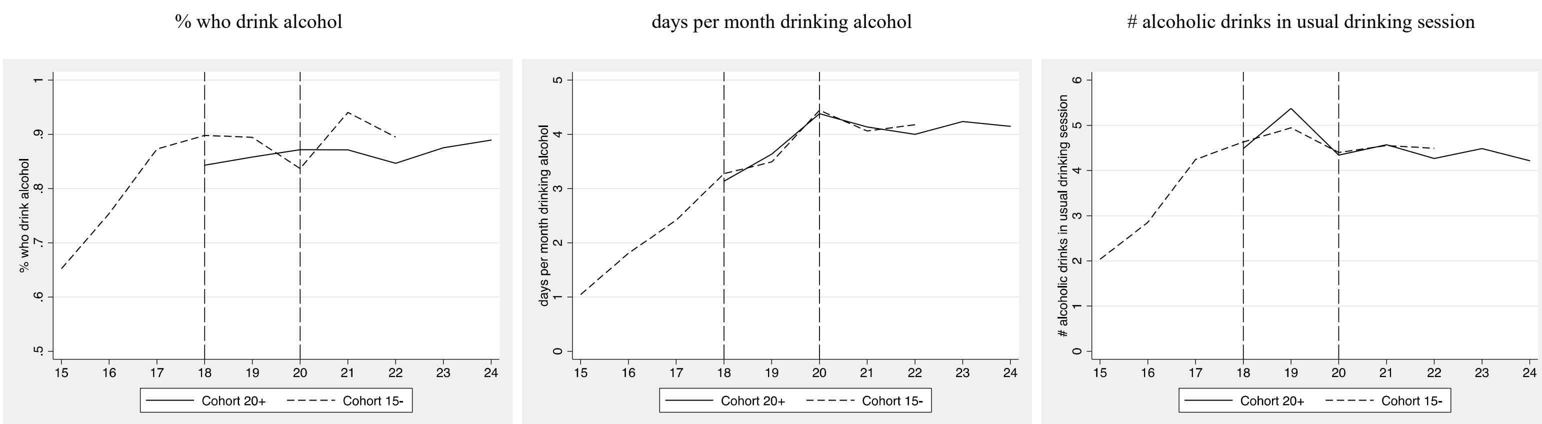
Source: NZ vehicular accidents data, own calculations. *Notes:* Cohorts are defined relative to their age at the policy change, e.g., cohort 18-20 are individuals aged 18-20 at the date of the policy change (likewise for the other cohorts). Horizontal axis: exact person age in months. Vertical axis top panel: total number of (alcohol related) accidents per age month and 10000 population. Proportion of alcohol related accidents is relative to total number of accidents. Top panel smoothed over 4-month periods. Bottom right graph shows proportion of cumulative number of alcohol related accidents (bottom middle graph) relative to cumulative number of accidents (bottom left graph).

Figure 6: Fitted values and marginal age effects in parametric cohort regressions



Source: NZ vehicular accidents data, own calculations. *Notes:* Graphs show fitted values and marginal effects of a monthly increase in the exact person age for the total number of (alcohol related) accidents per 10000 population in age group and the proportion of alcohol related accidents relative to total number of accidents, based on the results of the parametric cohort regressions in Table 6. Cohorts are defined relative to their age at the policy change, e.g., cohort 18-20 are individuals aged 18-20 at the date of the policy change (likewise for the other cohorts). Horizontal axis: exact person age in months.

Figure 7: Distribution of alcohol consumption variables in NZHS data over age range 15-24 by cohort



Source: NZ Health Survey data, own calculations. *Notes:* Cohorts are defined relative to their age at the policy change, e.g., cohort 20+ are individuals aged 20+ at the date of the policy change (likewise for the 15-). Cohorts 15-18 and 18-19 are excluded from the graph because the NZHS was only conducted every 5 years in the relevant time frame and therefore insufficient information is available for the calculations for these two cohorts. Horizontal axis: person age in years. Vertical axis: percent of individuals who drink alcohol (left), number of days per month drinking alcohol (middle), and number of drinks in a usual drinking session by person age in years (right).

Tables

Table 1: Descriptive statistics of outcome variables

	Before Policy Change (01/96-11/99)			After policy change (12/99-06/11)		
	Age 15-17	Age 18-19	Age 20-21	Age 15-17	Age 18-19	Age 20-21
Total # accidents	1.168	1.994	1.795	1.147	1.965	1.747
	(0.308)	(0.517)	(0.482)	(0.343)	(0.491)	(0.465)
Total # alc. related	0.155	0.360	0.324	0.147	0.353	0.288
	(0.099)	(0.180)	(0.179)	(0.096)	(0.175)	(0.154)
Proportion alc. related	0.133	0.182	0.182	0.131	0.181	0.166
Number of observations	*203*	*203*	*203*	*602*	*602*	*602*

Source: NZ vehicular accidents data, own calculations. *Notes:* Data are aggregated on a weekly level, i.e., total number of (alcohol related) accidents per 10000 population in age group and week before/after the policy change (Dec 1, 1999). Standard deviations in parentheses. Proportion of alcohol related accidents is relative to total number of accidents.

Table 2: Event history analysis for the change in the MLDA

	Age 15-17	Age 18-19	Age 20-21
A. Total number of accidents			
Mean value 6 months before policy change	*2.208*	*2.612*	*2.311*
Announcement period	0.4886	0.1780	0.6180*
	(0.3455)	(0.3548)	(0.3255)
0-29 days after change in MLDA	0.0342	0.4685	0.8516*
	(0.4834)	(0.4965)	(0.5079)
30-89 days after change	0.1890	-0.2345	0.0389
	(0.4016)	(0.4182)	(0.3888)
90-183 days after change	0.0230	0.0958	0.3303
	(0.3327)	(0.3498)	(0.3229)
Number of observations	*732*	*732*	*732*
B. Total number of alcohol related accidents			
Mean value 6 months before policy change	*0.246*	*0.514*	*0.421*
Announcement period	-0.0383	-0.0359	-0.0882
	(0.1113)	(0.1406)	(0.1378)
0-29 days after change in MLDA	-0.0678	0.1404	0.1415
	(0.1465)	(0.2006)	(0.1905)
30-89 days after change	0.0467	-0.2607*	-0.0286
	(0.1233)	(0.1515)	(0.1500)
90-183 days after change	-0.0388	-0.0251	0.1708
	(0.0991)	(0.1266)	(0.1259)
Number of observations	*732*	*732*	*732*
C. Proportion of alcohol related accidents			
Mean value 6 months before policy change	*0.112*	*0.203*	*0.165*
Announcement period	-0.0054	-0.0551	-0.0602
	(0.0515)	(0.0554)	(0.0587)
0-29 days after change in MLDA	-0.0231	0.0122	-0.0440
	(0.0713)	(0.0692)	(0.0751)
30-89 days after change	-0.0154	-0.1033*	0.0120
	(0.0555)	(0.0608)	(0.0630)
90-183 days after change	-0.0365	-0.0276	0.0847
	(0.0579)	(0.0505)	(0.0576)
Number of observations	*640*	*671*	*653*

Source: NZ vehicular accidents data, own calculations. *Notes:* Data are on a daily basis, i.e., total number of (alcohol related) accidents per age group and day. Data are used for a time frame of 548 days before and 183 days after the date of the policy change (Dec 1, 1999). Proportions are relative to the total number of accidents for days with positive number of accidents. Mean values of variables six months before the policy change in italics. Regressions control for week of the year effects (including an indicator for the previous year), and day of the week effects. Robust standard errors in parentheses. Significance levels: * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3: Age-based RDD effects of changing the MLDA

	Age 18			Age 20		
	1996-98	2000-05	2006-11	1996-98	2000-05	2006-11
A. Total number of accidents						
Mean value 6 months before birthday	*1.487*	*1.483*	*1.533*	*1.454*	*1.552*	*1.577*
No birthday effects	0.128	0.256**	-0.0287	-0.0608	0.00154	0.0380
	(0.157)	(0.106)	(0.113)	(0.146)	(0.108)	(0.115)
Incl. birthday effects	0.0994	0.231**	-0.0716	-0.0671	0.0314	0.0392
	(0.157)	(0.107)	(0.115)	(0.146)	(0.108)	(0.115)
Incl. birthday effects (+/- 7)	0.179	0.232**	-0.116	-0.0459	0.0313	0.0707
	(0.166)	(0.115)	(0.120)	(0.152)	(0.114)	(0.120)
Incl. birthday effects (+/- 30)	0.0836	0.0232	-0.0556	-0.255	0.127	0.121
	(0.217)	(0.145)	(0.156)	(0.189)	(0.141)	(0.152)
Number of observations	*1095*	*2190*	*2190*	*1095*	*2190*	*2190*
B. Total number of alcohol related accidents						
Mean value 6 months before birthday	*0.242*	*0.212*	*0.232*	*0.258*	*0.278*	*0.298*
No birthday effects	0.0165	0.00718	0.0335	0.0560	-0.0838**	0.0116
	(0.0610)	(0.0400)	(0.0491)	(0.0662)	(0.0417)	(0.0464)
Incl. birthday effects	0.0154	-0.0227	0.00628	0.0517	-0.0871**	-0.00233
	(0.0617)	(0.0399)	(0.0495)	(0.0637)	(0.0418)	(0.0463)
Incl. birthday effects (+/- 7)	0.0185	-0.0148	-0.0246	0.0543	-0.0925**	0.0425
	(0.0667)	(0.0424)	(0.0501)	(0.0672)	(0.0448)	(0.0483)
Incl. birthday effects (+/- 30)	-0.0105	-0.0185	-0.0187	0.0551	-0.0697	0.130*
	(0.0897)	(0.0560)	(0.0642)	(0.0851)	(0.0611)	(0.0668)
Number of observations	*1095*	*2190*	*2190*	*1095*	*2190*	*2190*
C. Proportion of alcohol related accidents						
Mean value 6 months before birthday	*0.166*	*0.146*	*0.154*	*0.176*	*0.179*	*0.180*
No birthday effects	-0.0185	-0.0171	0.0231	0.0544	-0.0525*	0.0123
	(0.0407)	(0.0282)	(0.0314)	(0.0437)	(0.0287)	(0.0295)
Incl. birthday effects	-0.0190	-0.0309	0.0148	0.0591	-0.0601**	-0.00098
	(0.0412)	(0.0284)	(0.0316)	(0.0436)	(0.0289)	(0.0295)
Incl. birthday effects (+/- 7)	-0.0313	-0.0226	-0.0125	0.0540	-0.0633**	0.0253
	(0.0432)	(0.0296)	(0.0329)	(0.0451)	(0.0309)	(0.0315)
Incl. birthday effects (+/- 30)	-0.0222	-0.0117	-0.00307	0.0968	-0.0404	0.0570
	(0.0560)	(0.0390)	(0.0438)	(0.0606)	(0.0410)	(0.0412)
Number of observations	*863*	*1711*	*1671*	*848*	*1686*	*1695*

Source: NZ vehicular accidents data, own calculations. *Notes:* Reported numbers are age-based RDD effects with different controls for birthday effects (none, just birthday, +/- 7, +/- 30 days). Data are on a daily basis, i.e., total number of (alcohol related) accidents per day half a year around birthday (18th or 20th). Proportions are relative to the total number of accidents. Mean values in year before birthday in italics. Regressions control for linear trend in accidents before and after the birthday (excluding birthday effects). Robust standard errors in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01

Table 4: Regression estimates of accident dynamics by cohort

	Total # accidents		Total # alc. related		Prop. alc. related	
	mean=0.6284		*mean=0.1037*		*mean=0.1643*	
	Estimate	SE	Estimate	SE	Estimate	SE
Cohort 20+ (*reference*)						
Cohort 18-20	-0.0970***	(0.0175)	-0.0326***	(0.0062)	-0.0187*	(0.0102)
Cohort 15-18	0.0370*	(0.0200)	-0.0102*	(0.0060)	-0.0223**	(0.0096)
Cohort 15-	0.1450***	(0.0250)	0.0094	(0.0080)	-0.0243**	(0.0118)
Person age	-0.0360**	(0.0181)	-0.00360	(0.00697)	-0.00111	(0.0107)
x cohort 18-20	0.0448***	(0.0139)	0.00438	(0.00530)	-0.00039	(0.00897)
x cohort 15-18	0.0666***	(0.0144)	0.00606	(0.00467)	-0.00754	(0.00808)
x cohort 15-	0.0339***	(0.0115)	0.00478	(0.00407)	0.00159	(0.00715)
Person age squared	-0.0154***	(0.00528)	-0.0059***	(0.00220)	0.00118	(0.00371)
x cohort 18-20	0.0162***	(0.00526)	0.00360*	(0.00213)	-0.00119	(0.00400)
x cohort 15-18	-0.00757	(0.00513)	-0.00109	(0.00178)	-0.00238	(0.00334)
x cohort 15-	-0.00259	(0.00399)	-0.00080	(0.00177)	-0.00184	(0.00315)
Person age cubic	0.00612***	(0.00171)	0.00131**	(0.000577)	0.00153	(0.00114)
x cohort 18-20	-0.00357*	(0.00185)	-0.00118*	(0.000652)	-0.00154	(0.00126)
x cohort 15-18	-0.00415**	(0.00177)	-0.000807	(0.000557)	-0.000141	(0.00113)
x cohort 15-	-0.00269*	(0.00157)	-0.000592	(0.000520)	-0.000420	(0.00106)
Person age quartic	-0.000557	(0.000400)	-0.0000287	(0.000145)	-0.000320	(0.000279)
x cohort 18-20	-0.000122	(0.000473)	0.0000904	(0.000184)	0.000380	(0.000347)
x cohort 15-18	0.000886*	(0.000454)	0.000217	(0.000152)	0.000263	(0.000306)
x cohort 15-	0.000459	(0.000394)	0.000143	(0.000155)	0.000168	(0.000298)
F-test difference in age effects to cohort 20+						
cohort 18-20	$F = 15.83$	$p < 0.001$	$F = 6.23$	$p < 0.001$	$F = 1.96$	$p = 0.102$
cohort 15-18	$F = 16.14$	$p < 0.001$	$F = 2.92$	$p = 0.023$	$F = 1.51$	$p = 0.203$
cohort 15-	$F = 5.95$	$p < 0.001$	$F = 0.45$	$p = 0.769$	$F = 0.15$	$p = 0.964$
Number of observations	*17488*		*17488*		*17488*	

Source: NZ vehicular accidents data, own calculations. *Notes:* Data are aggregated on the crash month and person age (in months) level. Dependent variables: (1) total number of accidents per 10'000 population in the respective age group, (2) total number of alcohol related accidents per 10'000 population in the respective age group, (3) proportion of alcohol related accidents relative to total number of accidents. All models include a polynomial function of degree 4 in person age, with the main effect for cohort 20+, and interactions of the polynomial terms with indicators for the cohorts 18-20, 15-18, and 15-. Cohorts are defined relative to their age at the policy change, e.g., cohort 18-20 are individuals aged 18-20 at the date of the policy change (likewise for the other cohorts). All models allow for age-specific seasonality, time trends and cohort effects. Standard errors clustered on the monthly level in parentheses. Significance levels: * p<0.1, ** p<0.05, *** p<0.01