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Health Consequences of Easier Access to Alcohol: New Zealand Evidence [*]

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Abstract

We evaluate the health effects of a reduction in New Zealand's minimum legal purchase age for alcohol. Difference-in-differences (DD) estimates show a substantial increase in alcohol-related hospitalizations among those newly eligible to purchase liquor, around 24.6% (s.e.=5.5%) for males and 22% (s.e.=8.1%) for females. There is less evidence of an effect among ineligible younger cohorts. There is little evidence of alcohol either complementing or substituting for drugs. We do not find evidence that earlier access to alcohol is associated with learning from experience. We also present regression discontinuity estimates, but emphasize DD estimates since in a simulation of a rational addiction model DD estimates are closer than regression discontinuity estimates to the policy's true effect.

JEL Classification: I12, I18, J13

Keywords: alcohol, minimum purchase age, youth, health, hospitalizations, New Zealand.

1 Introduction

Alcohol consumption is enjoyable for most adults, but it is also risky as excess consumption can lead to antisocial behavior and health problems. These risks are especially salient when it comes to young people who may be more physiologically sensitive or psychologically immature (Cook, 2007). Almost all countries legally prohibit the purchase or public consumption of alcohol for those below some threshold age. Most OECD countries have a drinking age of eighteen, though several western European countries maintain a drinking age of sixteen.[1] A lower threshold age has both benefits, since alcohol consumption is enjoyable, and costs.

Our paper estimates some of the health consequences of access to alcohol among young people. We use difference-in-differences (DD) and regression discontinuity (RD) methods to examine New Zealand data on hospitalizations over the period in which the minimum legal age for purchasing alcohol was reduced from twenty years to eighteen. We use those over twenty as our primary comparison group since the lower purchase age should not have had much impact on this group and any changes we see in it can proxy for broader social influences that affect eighteen and nineteen year olds too. We construct two RD estimates, one based on comparing eighteen and nineteen year olds just before and just after the law change, and the other based on comparing outcomes for those just below and just above the threshold age.

More specifically, we examine the following questions. First, what was the effect of reducing the minimum purchase age on hospitalizations of eighteen and nineteen year-olds? This is the group that became legally entitled to purchase liquor. Second, how were those under the age of eighteen affected? Since law enforcement is imperfect, it is important to consider the consequences for those ostensibly not affected by the law. Third, are negative health consequences for young people due to their youth or their inexperience? If it is inexperience, then the argument for a higher minimum

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[1] http://www.icap.org/PolicyIssues/YoungPeoplesDrinking/tabid/108/Default.aspx.

purchase age is more limited. In principle, our setting permits us to distinguish between effects of age and experience, exploiting the variation in experience across cohorts induced by the lowering of the minimum purchase age. Fourth, did changes in the liquor laws affect drug-related health problems as suggested by the substitution hypothesis (DiNardo and Lemieux, 2001).

We find a significant increase in hospitalizations as a consequence of passage of the Sale of Liquor Amendment Act (1999). Among eighteen and nineteen year old males difference-in-differences estimates indicate a 24.6% (s.e.=5.5%) increase in alcohol-related hospitalizations. For females in the same age group, the estimated effect is 22% (s.e.=8.1%). We find some evidence of an increase in hospitalizations for males younger than eighteen, but this appears to be driven mostly by changes in relative cohort size over time. Using RD to compare eighteen and nineteen year olds just before and just after the law change shows larger effects. Regression discontinuity evidence suggests that neither younger age groups nor older age groups are affected. We do not find evidence that the lower drinking age reduced the prevalence of drug-related problems. In fact we find some evidence of an increase in drug-related hospitalizations, though this evidence is somewhat fragile, as we discuss below.

Using two different measures of experience, we find evidence that experience with alcohol does not have benefits for those close to the age of legal eligibility. With one measure, after conditioning on age, each year above the minimum purchase age is associated with around 2.9% (s.e.=2.3%) higher hospitalization rates, on average, for males according to our main specification. The point estimate is small compared with the effect of attaining legal eligibility. This result is broadly consistent with estimates using an alternative measure of experience, the cumulated prior alcohol-related hospitalizations for a cohort, though the estimates for males are statistically significant with that measure. For females we find no statistically significant evidence of an experience effect.

We simulate a basic rational addiction model (Becker and Murphy, 1988) to provide a framework

for interpreting the different magnitudes obtained across the DD and RD models. We simulate time paths for different cohorts' age-consumption profiles and subject the model to a change in the minimum purchase age. In the model consumption changes only slightly when a youth becomes old enough to consume legally. By contrast, when the law changes there is a large increase in consumption for those newly eligible. A difference-in-difference estimate using the simulated data is in between the two regression discontinuity estimates and is close to the steady-state effect of the policy change. From the perspective of the rational addiction model, the DD estimates are more reliable guides to the true results of the policy.

For several reasons, our estimates could be thought of as a lower bound on the cost of the law change. We do not measure all long-term health consequences such as alcoholism, nor do we estimate externalities due to automobile accidents for example. More generally, there are benefits and non-health costs that we do not study, so our results are only a partial assessment of the merits of a lower drinking age.

This paper gives some historical background about alcohol consumption and legislation in New Zealand as well as a review of the literature on the effects of minimum purchase age laws in section 2. Section 3 discusses our data and empirical approach. Section 4 presents our main results. Section 5 discusses the model as a framework for interpreting our empirical results.

2 Related Literature

2.1 Historical Background

Belich (1996) notes that New Zealanders in the 19th century were very young, very male, very transient, and very often drunk. Excessive drinking and associated public disorder and crime were seen as significant social problems of the day. In response, New Zealand's parliament passed around 50 laws between 1880 and 1920 restricting the consumption of alcohol (Belich, 2001). A temperance movement gained majority support for legal prohibition in referenda, but not the super-majority

required to pass.

In 1910, the legal drinking age was raised from eighteen to twenty-one. A 1917 law prohibited liquor sales after six o'clock p.m.[2] In 1969, the minimum purchase age was lowered from twenty-one to twenty. Continuing this liberalization, many restrictions on liquor licensing were eased in 1989, and in 1999 the Sale of Liquor Amendment Act lowered the minimum purchase age from twenty to eighteen.[3] Our paper studies the health consequences of this lower minimum purchase age.

The Sale of Liquor Amendment Act introduced a variety of other changes to the existing law. Among these, supermarkets were permitted to sell beer, and off-licence proprietors became eligible to trade on Sundays, changes that expanded alcohol availability.[4] Since the law altered regulations governing supply and demand for liquor, one might be concerned that liquor prices changed as a result of the law. To the extent that eighteen and nineteen year olds have different price elasticities of demand from those in our comparison group, our estimates conflate the effect of a lower purchase age and changed liquor prices. However, data from Statistics New Zealand do not support this concern. The price series for beer, wine, and spirits do not show signs of a break around the passage of the law.

A dominant narrative around the passage of the Sale of Liquor Amendment Act was that access to alcohol would be less restricted but that any restrictions would be enforced more thoroughly. Hence, the new law changed the rules relating to liquor licences and bar management, providing for fines for promoting excessive consumption, and increasing fines for selling to minors or supplying people already intoxicated. The law also introduced an evidence-of-age regime that encourages

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[2]This gave rise to the so-called 'six o'clock swill' in which men would rush from their workplace to the public bar and consume a large amount of liquor before driving home.

[3]Technically, the law restricts the purchase and public possession of liquor to those over the threshold age. It does not directly restrict consumption by those below the age. Therefore, we prefer the term minimum purchase age over drinking age. Rules in the United States similarly place restrictions primarily on the purchase and public possession of liquor.

[4]The following website from New Zealand's Ministry of Justice has a summary of the major changes of the SLAA 1999: http://www.justice.govt.nz/publications/publications-archived/1999/amendments-to-the-1989-sale-of-liquor-act/publication.

sellers to request photographic identification for proof-of-age purposes, since the reasonable belief, based on identification, that a patron was of legal age could be used as a defense against the charge of supplying a minor.

An objection to our empirical strategy is that the effect of the minimum purchase age depends on the nature of the evidence-of-age regime, so the effect of a change in the MPA is larger when credible identification is generally required. We contend that our control group, in their early twenties, are likely to be affected by the change in the evidence-of-age regime and that the difference-in-differences approach will remove this component of the change in outcomes for the treated group. Furthermore, we note that the older the control group is, the less affected it is by requirements to show identification, so the estimated effect on the treated group should be smaller when we use older age groups for the control group. As we discuss in section 4, the opposite is true.

Since 1999, debate has continued about the appropriate MPA. Harms associated with youth drinking, such as public intoxication, unwanted sexual encounters, and alcohol-induced violence, have promoted interest in returning the minimum purchase age to twenty. In 2010, the New Zealand Law Commission, a body of lawyers that advises the government on legislation, produced an extensive report (New Zealand Law Commission, 2010) proposing legal reforms regarding liquor sales and consumption that endorsed a minimum purchase age of twenty. Due to concern that the lower MPA has had larger negative health and development consequences than expected, the New Zealand government proposed a partial reversal of the MPA reduction in the 1999 law (Alcohol Reform Bill, 2010). Legislation eventually passed in 2012 (Sale and Supply of Alcohol Bill, 2010; Local Government (Alcohol Reform) Bill, 2010; Summary Offenses (Alcohol Reform) Bill, 2010) rejected a higher minimum purchase age, substantial increases in excise taxes for alcohol, and minimum prices. These laws did provide for stricter conditions for obtaining a liquor licence, weaker conditions for losing a licence, more local input into regulations governing liquor sales, and

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additional rules relating to the supply of minors, so they aim to restrict alcohol availability, but not as much as recommended by the Law Commission. Christoffel (2006) has more detail on the history of alcohol control in New Zealand.

In the United States, many states reduced drinking ages in the 1960s and 1970s. The National Minimum Drinking Age Act 1984 forced all states to have a drinking age of twenty-one. The Amethyst Initiative[5] campaign, supported by a large number of higher education executives, has sought to renew public debate about the drinking age, to some extent discounting the evidence from studies evaluating the law changes of the 1980s (Dee, 1999; Dee and Evans, 2001; Wagenaar and Toomey, 2002). Some in the United Kingdom have advocated a higher drinking age to curb a variety of undesirable behavior (Gerard, 2007). Arguments for a lower legal purchase age in the United States in the 1960s and 1970s were similar to arguments in New Zealand during the 1990s[6]: eighteen year olds were deemed old enough to vote, old enough to serve in the military, and marry, so should be permitted to purchase alcohol as adults. The Amethyst Initiative's campaign could be justified by a view that inexperience rather than immaturity determines alcohol-related problems, so that younger people should be permitted to drink and benefit from learning-by-doing. A lower drinking age might increase health problems for the youngest eligible drinkers but reduce problems among older cohorts.[7]

2.2   Effects of Minimum Purchase Age

Many studies have used staggered changes in the MPA in the U.S.A. in the 1970s and 1980s to estimate the effects of the minimum purchase age using difference-in-differences. Wagenaar and

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[5]The Amethyst Initiative is an attempt by leaders of U.S. higher education institutions, starting in 2008, to generate public policy debate around youth drinking. The Initiative issued a statement, signed by over 100 college and university presidents as of August 2010, asserting that "a culture of dangerous, clandestine 'binge-drinking'–often conducted off-campus–has developed", among other problems. The Initiative appears to support a lower drinking age.

[6]See Cook, 2007; see also Gerard, 2007, on the U.K. context.

[7]Dee and Evans (2001) present findings on this issue from the period of rising minimum purchase ages in the 1980s in the United States.

Toomey (2002) survey studies of the effects of the minimum purchase age on the behavior of young people in the United States and Canada. The studies generally find that a higher drinking age tends to reduce alcohol consumption and alcohol-related harms. These findings are confirmed in Dee (1999), who emphasizes the importance of state level heterogeneity (using fixed effects in panel data models). He shows that a higher minimum purchase age lowers reported drinking and binge drinking.

Carpenter and Dobkin (2009) use the minimum purchase age laws to estimate the effect of alcohol consumption on mortality. Instead of relying on a change in the legal purchase age, they use the discontinuity in eligibility to purchase alcohol when a person turns twenty-one as an instrument for consumption. Using U.S. National Health Interview Survey data, Carpenter and Dobkin (2009) note that there is a jump in the consumption of alcohol among American youths at age twenty-one among those who are already drinkers. Frequency of consumption and heavy drinking increases about 15% to 20% at age twenty-one in Carpenter and Dobkin's data.[8] Also at age twenty-one, there is a jump in alcohol-related morbidity and mortality.

In our data we have the exact date of birth of those admitted to hospital. Since New Zealand's population is much smaller than that of the United States, we have fewer observations and so less power than Carpenter and Dobkin to estimate an effect of alcohol availability on health outcomes. In our favor, we have data for a period during which the age of eligibility changes. In principle this allows us to test whether it is the legal eligibility to purchase alcohol or perhaps some other age-related status that influences a person's behavior.

The higher drinking age in the 1980s in the United States reduced traffic fatalities among young people, but a lower drinking age in the current context might not lead to much higher traffic fatalities since drunk driving is less socially acceptable now. New Zealand has had a similarly large

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[8]Yörük and Yörük (2011) apply a similar strategy with National Longitudinal Survey of Youth data. In addition to a higher rate of consumption among those already drinking, they find that the probability of drinking increases at the minimum legal age.

decline in youth traffic fatalities and the social acceptability of drunk driving, so the effects of a change in the MPA in New Zealand might be informative about the potential effects of a similar change in the United States.

The lower minimum purchase age could affect health through changing the quantity of consumption or the venue. From a policy perspective the ultimate interest is in the overall health effect of the law change, rather than the effect of alcohol consumption or location of consumption. Wilkins et al. (2002), Habgood et al. (2001), and Huckle et al. (2011) report on surveys showing an increase in consumption of alcoholic beverages by New Zealanders during this time, especially young people and the increase was associated with a movement toward relatively more consumption of hard liquor.

Habgood et al. show that in 1995 consumption for twenty to twenty-four year old males was higher than for eighteen to nineteen year old males but the difference essentially disappeared in 2000 after the Sale of Liquor Amendment Act passed, consistent with the view that the law change stimulated consumption in younger age groups. Binge drinking among males over twenty generally fell between 1995 and 2000, while for eighteen and nineteen year olds it rose.[9] Huckle et al. (2011) conclude that alcohol consumption generally rose between 1995 and 2000 but was roughly stable between 2000 and 2004. For example, among eighteen and nineteen year old males, the fraction of drinking occasions that involved bingeing rose from 51% in 1995 to 61% in 2000, falling to 59% in 2004.

According to results in Huckle et al. (2011), the number of drinking days for 18-19 year old males rose around 20% - 25%, relative to 20-24 year olds, between 1995 and 2000-2004.[10] This is about the same magnitude as the increase in hospitalizations we estimate due to the law change. For

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[9]Rates of binge drinking appear to be similar in New Zealand and the United States. See Johnston et al. (2011) and Habgood et al. (2001). Also, in New Zealand as in the United States, most young people start drinking prior to attaining the age of legal eligibility, with around 70% of 12th graders in the United States and seventeen year olds in New Zealand reporting having consumed alcohol (Wilkins et al., 2002).

[10]The samples for these surveys in New Zealand are small. Based on the text of Huckle et al., the numbers for 18-19 year old males come from around 50 respondents in 2000 and around 150 in 2004, probably fewer in 1995.

18-19 year old females, the increase in number of days drinking is around 12% to 18%, relative to 20-24 year olds, a bit smaller than the change we report in hospitalizations. Taking these numbers together suggests an elasticity of hospitalizations with respect to drinking days of around one. The number of days with heavy drinking increases by more than the number of drinking days for the eighteen and nineteen year old groups, suggesting a smaller elasticity of hospitalization with respect to bingeing. These elasticities are larger, but a similar order of magnitude to what Carpenter and Dobkin (2009) report. They find that a one percent increase in days drinking is associated with a 0.4 percent increase in mortality risk (a different outcome measure) in the United States.

Other studies have addressed the effects of the law change by looking at independently measured outcomes. Kypri et al. (2006) find a surge in injuries from traffic accidents among fifteen to nineteen year olds when compared with with twenty to twenty-four year olds. They find large increases in incidents for young women especially (a 51% increase in alcohol-involved crashes among eighteen and nineteen year old women). Huckle et al. (2006) show an increase in drunk driving prosecutions after 1999 among eighteen and nineteen year olds.[11]

Kypri et al. (2009) study the effect of the law change on hospitalizations for alcohol poisoning. They focus on a single diagnosis, rather than the broader set we use, and find that it is hard to construct a powerful test for the effect of the law change. Our study improves on theirs because it does not restrict focus to a single diagnosis code for alcohol involvement, and because we use more years before and after the law change. Everitt and Jones (2002) study intoxication among patients admitted to Auckland Hospital's emergency department and find a substantial increase (50%) in the fraction of eighteen and nineteen year olds appearing intoxicated, a smaller increase in fifteen to seventeen year old intoxication propensity, with little change in intoxication of those

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[11]Huckle et al. find a change in the rate of increase over time in excess breath-alcohol while driving for eighteen and nineteen year olds, but they do not find a jump in the level of incidents upon passage of the Sale of Liquor Amendment Act.

twenty and over.[12] However, central Auckland's emergency department may not represent well the rest of New Zealand. For example, there could have been an increase in the proportion of young people drinking in central Auckland, in proximity to both bars and the hospital so that the finding of an increase in hospital admissions in this area might not represent the overall effect of the law change.

We are able to look at similar statistics but we have a longer period after the law change so that we can obtain clearer estimates of underlying trends. Figure 1 shows that alcohol-related hospitalizations in New Zealand have been increasing over time, so it is crucial to account properly for trends. In addition, we use both regression discontinuity and difference-in-difference methods that use distinct features of the data and environment to identify the effects we estimate. Furthermore, we use economic theory to inform our views of which methods are likely to be most useful in evaluating the law change's health consequences. We also study the role of experience in potentially mitigating the risks of alcohol use, an issue that others have largely neglected (Dee and Evans (2001) aside).

3   Data

Our data is from the New Zealand Health Information Service and covers all hospitalizations in public hospitals for people up to 30 years old between 1993 and 2006. Almost all hospitalizations were in public hospitals during this period (Kypri et al., 2006). Our dataset includes a large amount of information about medical diagnoses (including International Classification of Disease (ICD) codes) and symptoms, as well as some personal information regarding individual characteristics. This information includes the person's date of birth as well as the date of the incident. In some specifications we combine these data with cohort population estimates from Statistics New Zealand

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[12]See also Lash (2004), who reports that alcohol-related hospital admissions of fifteen to nineteen year olds rose as a fraction of all alcohol-related admissions comparing the period before 2000 with 2000.

to estimate models with incident rates as dependent variables. According to summary statistics for sixteen to twenty-three year olds in Tables 1 women are more likely to be hospitalized than men, due mostly to childbearing. Consistent with New Zealand's general ethnicity distribution, most of the people are of European origin followed by Maori. Approximately 2% of all hospitalizations for the ages we study are related to each of alcohol or drugs.

Figure 1 shows time series of alcohol and drug related hospitalizations for males and females. Hospitalizations have risen substantially over time. The graphs show similar increases in hospitalizations for men and women. However, alcohol and drug hospitalizations are substantially higher for men. In appendix A we report descriptions of the most common alcohol and drug related conditions identified in our sample and the percent of cases they account for. The cases we flag could have alcohol as a contributing factor. For example, one frequent diagnosis is "personal history of harmful use of alcohol" which could include cases of a person who incurred a fracture and was affected by alcohol at the time of injury.[13] Common diagnoses due to drugs relate to cannabis, opioids or amphetamines use.

Starting in 1999 there was a change in the way emergency department cases were reported. Prior to 1999 only cases that resulted in a hospital admission would be recorded. From 1999 one District Health Board (DHB),[14] began recording emergency room presentations even if they did not result in hospitalization and over the next few years other DHBs followed suit. This may have contributed to the upward trend in hospitalizations, but there was a pre-existing upward trend during the 1990s which continued well after 2000. Because this change applies equally to all patients it should not affect the difference-in-differences estimates. Since the change in reporting occurs gradually, we do not believe it exerts a major influence on our regression discontinuity estimates. Unfortunately, we cannot determine whether someone first presented at an emergency department in our data, but

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[13]Source: http://www.health.govt.nz/nz-health-statistics/classification-and-terminology/using-icd-10-am-achi-and-acs/coding-queries/coding-query-database/coding-query-database/alcohol-coding-0.

[14]District Health Boards are government agencies that fund or provide health care services in a region.

analysis omitting very short hospital stays (more likely associated with emergency departments), are consistent with the ones we report in our main findings.

Figure 2 shows the age profile of hospitalization incidents and provides some evidence on the importance of learning or experience in mitigating alcohol-related health problems. In particular, after age twenty there is a general decline in alcohol-related hospitalizations as age increases, even as drug-related hospitalizations remain high and stable. This figure also documents the steep increase in hospitalizations among teenagers. From the perspective of econometric identification, it is important to note that this graph does not allow us to separate an experience effect from and age effect (which would be unchanged by legislation). We discuss later our strategy to separately estimate experience and age effects.

3.1   Empirical Approach

We first estimate the health effects of the Sale of Liquor Amendment Act using difference-in-differences. We do not have a separate geographical area that was not subject to the law change. Instead we use people who are twenty to twenty three years as a control group and produce separate estimates of the effect on eighteen and nineteen year olds (the newly eligible group) as well as sixteen and seventeen year olds. We considered a range of control groups defined by all those twenty or over but below some upper limit. Our control group is selected on the basis of how variable the difference between the treated and control groups' alcohol hospitalizations were in the pre-period.[15] We also discuss the effects of using different control groups.

Our difference-in-differences (DD) effects come from estimating the following regression sepa-

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[15]We measure the distance between treatment and potential control group as follows. The control group is from age 20 to a maximum age, which ranges from 20 to 30. Separately for males and females, for treatment and control, we calculate the average log alcohol-related hospitalizations for each of the seven years in the pre-period. We compute the difference between treatment and control for this measure in each year and take the standard deviation, then average across males and females. The maximum age that results in the lowest such standard deviation is the one we use for our preferred control group.

rately for males and females

$$\log(y_{at}) = \gamma_t + \gamma_a + \beta_1(18or19_a \times Post1999_t) + \beta_2(16or17_a \times Post1999_t) + \epsilon_{at} \qquad (1)$$

where the unit of observation is an age in years (a) and year (t) group. $Post1999_t$ is a dummy equal to one if the year is after the law change, and $18or19_a$ is a dummy equal to one only for eighteen and nineteen year olds.[16] The difference-in-differences effect is β_1. We obtain a separate treatment effect (β_2) for the sixteen and seventeen year old group by including the $16or17_a$ dummy interacted with the $Post1999_t$ dummy. Our DD regressions use data from seven years before and seven years after the law change and observations for those aged from sixteen to twenty-three years.

One might be concerned that the reduction in the minimum purchase age was offset by other policies, such as more extensive campaigns against driving while intoxicated, or by higher alcohol prices. Our identifying assumptions in equation (1) include the notion that such policies or price changes affect twenty to twenty-three year olds just as much as they affect eighteen and nineteen year olds.[17]

The outcome variable in our regression equations is (the log of) a count of hospitalizations with an alcohol-related diagnosis code, or in other regressions a drug-related diagnosis code. Given the count nature of the variable, we also estimate Poisson regression models. In a further cut of the data, we define y to be the number of incidents per thousand people in the cohort, using population estimates from Statistics New Zealand, to account for varying cohort sizes. In a final version of our dependent variable we use alcohol-related hospitalizations as a fraction of total hospitalizations to account for variation in the overall propensity to be hospitalized for any cause.

Since there is a clear separation, based on age, of who can legally purchase liquor and who cannot at any given point in time, and over time there is a clear moment at which eighteen and nineteen

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[16]We define years so that a year runs from 1 December to 30 November. For example, 2000 (year zero in our graphs) in our dataset is 1 December 1999 to 30 November 2000.

[17]We noted in section 2.1 that alcohol prices do not seem to have been affected by the law change.

year olds became eligible to legally purchase liquor (1 December 1999), we also use regression discontinuity (RD) methods. The RD estimates are estimated separately for men and women using

$$y_i = \beta_0 + \beta_1 D_i + f_1(x_i|x_i \geq 0) + f_2(x_i|x_i < 0) + \beta_z z_i + \eta_i \tag{2}$$

where the unit of observation could be calendar time (days relative to 1 December 1999) or age (days relative to attaining legal eligibility), x is the forcing variable in the RD design defined so the point of discontinuity is at zero, and D_i is a dummy variable indicating that $x_i \geq 0$. The functions f_1 and f_2 are polynomial functions of the forcing variable, and in some cases we include additional covariates z. The RD effect is β_1.

The first set of RD estimates use samples restricted to certain age groups. The functions f_1 and f_2 then depend on the date (the forcing variable) relative to the date of the law change. We also include in our regression day of the week dummies and a dummy for the New Year holiday period. We estimate the equation for eighteen and nineteen year olds, then for twenty to twenty-three year olds and sixteen and seventeen year olds separately. We report results for men and women separately. We estimate basic linear models, Poisson regressions that allow for frequent zeros in our dependent variable, and local linear regression. We also allowed for different bandwidths, report robust standard errors, and cluster the results at the running variable level. The RD estimate is identified by the contrast between outcomes before and after the law change for the same age-group. We call this the law change discontinuity.

The second set of RD estimates contrasts those just older with those just younger than the MPA (we call this the age-based discontinuity), as in Carpenter and Dobkin (2009). In this case, f_1 and f_2 depend on the age in days relative to the MPA. We also include year fixed effects in the regression. As we discuss below, these estimates are smaller than the DD results and the date-based RD results. We again report results for different models and bandwidths, use robust

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standard errors, and cluster at the running variable level.

The RD and DD estimates use the same underlying data. They give different weight to various contrasts that are possible within the dataset. The DD estimates are based on a broader set of the data. The RD approaches each use a single difference contrast but only in a neighborhood of some eligibility threshold where it is presumed that the populations either side of the threshold are comparable.

4 Results

4.1 Difference in Differences

Figures 3 (for males) and 4 (for females) show the time series of alcohol-related hospitalizations among three age groups: sixteen and seventeen year olds, eighteen and nineteen year olds, and twenty to twenty-three year olds. Over time there has been a clear upward trend in the prevalence of such incidents. Some of this may be related to changes in reporting, such as changes in the way emergency department admissions are recorded in the Health Information Service database, though these changes are unlikely to account for the gradual increase in hospitalizations. The graphs suggest a common, ongoing upward trend in alcohol-related hospitalizations among the groups. The difference-in-differences specification we use allows for common year-specific components.

The trend rate of hospitalization for those in the eighteen and nineteen year old age group moved from being at or below the level for twenty to twenty-three year olds to being generally higher in the period since the Sale of Liquor Amendment Act was implemented. This is evidence for an effect of the law on health outcomes for this age group. There is less clear evidence that the health outcomes among sixteen and seventeen year olds were also adversely affected, suggesting the law did not substantially alter the accessibility of alcohol for that age group relative to those twenty years and over.

Table 2 shows difference-in-difference estimates of the effects of lowering the MPA. Columns

(1) and (2) report estimates when the dependent variable is the log of hospitalizations for the group. Columns (3) and (4) report estimates from the Poisson regression model. Columns (5) and (6) report estimates when the dependent variable is the log of incidents per thousand members of the population cohort. Finally, columns (7) and (8) report estimates when the dependent variable is alcohol-related hospitalizations as a proportion of all hospitalizations for the relevant age-sex-year group. Each specification presents estimates for males and females separately, and includes estimates for sixteen and seventeen year olds.[18] We also report results using 20-23 year olds as the control group (Panel A), and 20-21 year olds as the control group (Panel B).

Across these various estimation procedures our results are consistent. The Sale of Liquor Amendment Act appears to have caused an increase in alcohol-related hospitalizations, especially for eighteen and nineteen year old males. We estimate the effect for males at 24.6% in the log-linear model. The effect is identical in the Poisson model. When we account for cohort size variation the estimated effects fall somewhat, down to 18.8% for males, and to around 16.2% for females (from 22% in the log-linear specification). In the proportions model, the estimated increase for males is about one-fifth of the initial proportion, while for women the increase is around one-quarter of the initial proportion.[19]

We report two methods for computing standard errors. First, we report heteroskedasticity-robust standard errors. Second, we report standard errors from collapsing the data into a single pre-period observation and single post-period observation for each age group, as recommended by Bertrand et al. (2004) for dealing with a small number of clusters. These standard errors using collapsed data are similar to those obtained when using cluster-corrected standard errors, and are generally smaller than the robust standard errors. To be conservative, we generally refer to the robust standard errors in the text.

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[18]Alternative specifications incorporating different forms of year and age controls show consistent results.

[19]Among eighteen and nineteen year olds, alcohol-related hospitalizations are around 1.4% of all cases for women and around 5.3% of all cases for men.

We see no strong evidence that the law change affected younger cohorts. In one case (using proportion of all hospitalizations) there appears to have been a reduction in hospitalizations of sixteen and seventeen year old males, while in other cases there appears to be a positive effect. When we account for cohort size, there is no effect for males or females.

Those younger than the minimum purchase age occasionally get hospitalized even though they cannot purchase liquor legally. One hypothesis suggests that underage drinkers only go to hospital for major problems. According to this hypothesis, once eighteen and nineteen year olds are legal, more will go to hospital even if there are not more alcohol-related health problems. Our finding that young males are hospitalized more frequently after the law change could represent a form of self-selection, and many of the additional cases would have minor alcohol involvement.

There are several pieces of evidence that work against this view. First, we can restrict attention to cases in which an alcohol-related diagnosis is the first diagnosis code recorded on an individual's hospital record. The order of diagnosis codes tends to reflect the importance of the diagnosis. If the additional hospitalizations we have identified were primarily due to self-selection, there should be a smaller increase, or no increase at all, in the number of cases where alcohol is listed in the first diagnosis code. However, for cases with alcohol on the first diagnosis code, we find a larger increase for eighteen and nineteen year old males than in our base results. Second, one might expect hospital stays to become shorter for eighteen and nineteen year old males after the law change if more mild cases were being admitted than before. However the duration of hospital stays does not show any such decrease. Third, using data from the United States National Survey on Drug Use and Health from 2002 to 2010, we note that conditional on reporting alcohol dependence, those under the legal purchase age are actually more likely to have received medical care for drug or alcohol dependence in the past year, not less.

In Panel B of Table 2 we present results from the same regression but using twenty and twenty-

one year olds as the control group. With this control group, the estimates are generally smaller in magnitude, especially for females. For females the results are generally not statistically significant. We prefer the twenty to twenty-three year olds as a control group because in the pre-period the trend among this larger group is closer to the trend of the treated group we examine. If we use twenty to thirty year olds as the control group, we obtain estimates that are somewhat larger still, but as with the twenty and twenty-one year old control group, the pre-trend does not follow that of the treated group as closely.

Our results are more modest than those of Everitt and Jones (2002) who argue that hospitalizations among eighteen and nineteen year olds increased around 50%, and that sixteen and seventeen year olds also experienced a higher rate of alcohol-related hospital admissions. Their results may be larger because of the specific location of the hospital they study (an emergency department in central Auckland), whereas we are using what is close to the population of hospitalizations in New Zealand.[20] In addition, they consider a shorter period of time, and some of their results may reflect changing relative cohort sizes or the increasing trend in alcohol-related health problems that need not be due to the Sale of Liquor Amendment Act.

4.2 Substitution of Drugs for Alcohol

Young people may respond to a relaxation of alcohol restrictions by substituting toward alcohol and away from other drugs. DiNardo and Lemieux (2001) find that during the 1980s, U.S. states that increased the minimum legal drinking age saw decreases in teen alcohol consumption along with similarly-sized increases in drug consumption. Crost and Guerrero (2012) find support for substitution using regression discontinuity evidence using National Survey of Drug Use and Health data. Anderson and Rees (2011) argue that relaxed laws against using marijuana in the United

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[20]We can estimate our model separately on a subset of relatively urban regions and a subsample of relatively rural regions, based on the District Health Board in which the hospital is located. When we do so, we find almost no difference in the estimated effect of the law change.

States have led to lower alcohol consumption. We examine a possible implication of the substitution hypothesis in our data and report results in Table 3.[21] We estimate equation (1) but use drug-related hospitalizations as our outcome variable. As before, and unlike DiNardo and Lemieux (2001), we do not have a geographically separate comparison group, but we use older cohorts as the control group.

Overall, we see little evidence that the increase in alcohol-related harms documented above is countered by any decrease in drug-related harms. There is some evidence that drug-related hospitalizations increased among young women as a result of the law change. In theory, this could be evidence of consumption complementarity, or perhaps easing access to alcohol makes illicit drug markets more accessible too, or as a third alternative, additional alcohol could make it more likely that drug consumption leads to serious health problems requiring hospitalization (see Robbe, 1998, for evidence along these lines).

Setting aside possible explanations for greater drug-related problems, the empirical evidence is fragile, driven in part by cohort size, outliers, and the choice of control group. When we account for cohort sizes, or for the general upward trend in hospitalizations overall, the estimated increases among eighteen and nineteen year old females, and among sixteen and seventeen year old females too, are smaller and less statistically significant. When we use twenty and twenty-one year olds as the control group, the estimated effects are even further diminished, so our results here are somewhat sensitive to the control group used, more so than for alcohol. In addition, if we omit the year 1995 the estimated effects for females drop substantially and are no longer statistically significant.

Our results suggest that any increase in the minimum legal purchase age in New Zealand need

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[21]The substitution hypothesis in DiNardo and Lemieux relates to consumption. While we do not observe consumption, we do observe adverse health outcomes that result in hospitalization, which is likely to be correlated with consumption. Alternatively, alcohol and drugs could be complements, as suggested by Saffer and Chaloupka (1999) or Williams et al. (2004) for example.

not be accompanied by an unintended surge in drug-related health problems. Further, at least relative to the debate about drugs and alcohol, the estimated effects for sixteen and seventeen year old females are something of a puzzle, since we do not see any significant change in alcohol-related health problems in this group.

4.3   Age vs Experience

While young people tend to have many alcohol-related problems, this could be because they are young or because they are inexperienced with alcohol. If inexperience is a problem, then raising the minimum age will just shift the age at which alcohol causes health problems. If it is being young that causes problems, then raising the minimum age will not cause larger problems later on. The change of the minimum legal purchase age allows us to estimate whether there are benefits from experience.

We construct two proxies for experience. First, we use the number of years since attaining legal eligibility. For any age group below the MPA we set experience to zero.[22] While many people consume while underage, and so are accumulating experience, the rate of accumulation increases after crossing over the MPA. Our second measure of experience is the cumulative number of alcohol-related hospitalizations for a cohort in prior years, divided by 100. This measure allows for accumulation of experience prior to reaching the minimum purchase age. We add these experience measures to equation (1), estimated using data for sixteen to twenty-three year olds, and report results in Table 4. These regressions also include age and year fixed effects as in Table 2.

The results do not indicate benefits from experience. In the specification with log hospitalizations as the dependent variable, the coefficient on experience shows that an extra year of experience increases hospitalizations for males by around 2.9% (s.e.=0.023) on average. This is a small effect

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[22]For example, nineteen year olds in 2003 have 1.5 years of experience since this age group includes those with just over one year and those with almost two years since the MPA. Nineteen year olds in 2000 have half a year of experience, since in 1999, when eighteen, they were below the MPA.

relative to the estimated 24.6% effect of attaining legal eligibility. It is also small relative to the standard error. When we use the cumulative hospitalizations measure of experience, we also find a small positive estimate, and it is statistically significant for males. The coefficients in Panel B of Table 4 imply that one hundred additional prior cases for a cohort – about one year's worth of cases in the 1990s – increases the current incidence of alcohol-related hospitalizations by about 3.5% for males. While this is a significant effect it could be explained as a cohort effect since later-born cohorts have higher hospitalizations when young and when old.

A corollary of the view that accumulated experience lowers the risk of alcohol use is that the standard difference in differences approach will overstate the effect of the law change. This happens because the control group after the lowering of the MPA has more experience than the control group before the law change, so the law change lowers the morbidity of the control group, contributing to an overestimate of the effect on the treated group. This suggests a DD estimator using only one year after the law change (when the control group has not yet realized this experience benefit) will yield a smaller estimated effect on eighteen and nineteen year olds. In our data, the reverse is true.

Further evidence that twenty and twenty-one year olds do not benefit from additional experience due to the law change is suggested by estimating an effect for this group using an older group (e.g. 25-30 year olds) as a control. According to this approach there was an increase in hospitalization rates for the twenty and twenty-one year old group, not a decrease as predicted by the experience hypothesis.

In summary, using different experience measures, we do not detect evidence for benefits from experience in the data. Alternative approaches and data may be able to cast more light on this subtle issue.

4.4 Regression Discontinuity

Our regression discontinuity results are represented graphically in Figures 5 and 6. The figures show hospitalizations for bins of size one month during the year before legal eligibility (or before the law change) and the year after legal eligibility (or after the law change). The lines superimposed are fitted values from a regression using day-level data. The unit of observation in our regressions is an age in days for a particular year and sex (e.g, males, eighteen years and 201 days, in 1997).

Table 5 shows our regression discontinuity estimates. The first column is for a parametric version of equation (2) using one year's data from either side of the law change.[23] The third column repeats this but using 90 days from either side. The second column is a local linear regression with a rule-of-thumb bandwidth following Fan and Gijbels (1996) and McCrary and Royer (2011). The fourth column gives non-parametric estimates using local linear regression with the Imbens and Kalyanaraman (2012) bandwidth. The fifth column is from a Poisson regression applied to the same sample as in column (1). The parametric models reported include a linear function of the distance from the law change on either side of the threshold. We select a linear model over higher order polynomials using the Bayesian Information Criterion (BIC) model selection criterion. Qualitatively, the results tend not to be sensitive to the degree of the polynomial we use. Quantitatively the effects are much larger when we use a higher-order polynomial.

The Fan and Gijbels bandwidth is usually around four months worth of data, whereas the Imbens and Kalyanaraman bandwidth tends to be quite small, using data from three to four weeks either side of the law change, consistent with Malamud and Pop-Eleches (2011)'s observation that the Imbens and Kalyanaraman bandwidth tends to be narrow relative to other bandwidths. The results are qualitatively consistent with our difference-in-difference estimates – positive and significant effects for eighteen and nineteen year old males, but no statistically significant effect

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[23]With the observational unit at the day level there are many zeros, so we do not estimate log specifications.

estimated for those younger or older. Quantitatively, the RD point estimates are larger.[24]

We report estimates using the age-based discontinuity in Table 6. According to the parametric estimates in this table, the effects for men and women tend to be smaller than when we use the DD approach or the RD approach using the law change for the discontinuity, but are generally consistent with the previous DD and RD results in showing a significant increase of hospitalizations for men. The local linear estimates produce much larger effects. For the parametric regressions, the results are sensitive to changes in the order of the polynomial function of age included in the regression. For the local linear regressions, decreasing the bandwidth increases the estimated effect.

5   Interpretation

5.1   Rational Addiction with Experience

We present an augmented rational addiction model (Becker and Murphy, 1988) that incorporates possible experience benefits and use it to interpret the various regression discontinuity and difference-in-difference estimates we present. In the model, a forward-looking agent chooses between consumption of a generic good c and an addictive good a (i.e., alcohol). Utility is quasi-linear in c, and quadratic in a and a stock of experience with the addictive good s. Period utility is given by

$$\tilde{u}_t = c_t + \alpha_a a_t + \frac{\alpha_{aa}}{2} a_t^2 + \alpha_s s_t + \frac{\alpha_{ss}}{2} s_t^2 + \alpha_{as} a_t s_t + \alpha_h h_t \tag{3}$$

where

$$h_t \equiv \frac{e^{\beta + \beta a_t - \beta_s s_t}}{1 + e^{\beta + \beta a_t - \beta_s s_t}}$$

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[24]We reestimate our regression discontinuity model under the assumption that the law change happens on 1 December 1998 and find no consistent effect across all estimations of such a law change (which did not actually occur). Similarly, if we use 1 December 2000 as the date of the law change, we find no effect.

is the probability of being hospitalized and α_h is negative. Using a second-order Taylor-series expansion around $a = s = 0$ we can approximate h as

$$h_t \approx p + \beta_a p(1-p)a_t - \beta_s p(1-p)s_t + \frac{1}{2}\beta_a^2 p(1-p)(1-2p)a_t^2 + \frac{1}{2}\beta_s^2 p(1-p)(1-2p)s_t^2 - \beta_a\beta_s p(1-p)(1-2p)$$

where p is the probability of hospitalization at $a = s = 0$. With $\beta_a > 0$ and $\beta_s > 0$ greater consumption increases the probability of hospitalization, while more experience lowers the probability of hospitalization. We assume that $\alpha_h < 0$ so that hospitalization lowers utility. When we substitute this approximation for h back into equation (3) we obtain u.

The agent maximizes

$$\sum_{\tau=t}^{\infty} \beta^{\tau-t} u_\tau$$

subject to $y = c_t + p_t a_t$ and $s_{t+1} = (1-d)(s_t + a_t)$, $\quad s_{16} = 0$.

We assume that $\alpha_a + \alpha_h\beta_a p(1-p) > 0$, $\alpha_{aa} < 0$, and $\alpha_{as} > 0$ so that there is diminishing marginal utility in consumption of alcohol. Since $\alpha_{as} > 0$, greater past consumption raises the current marginal utility of consuming alcohol. In addition, the experience stock has independent effects on current utility. Normally in the rational addiction model, we would assume that $\alpha_s < 0$ and $\alpha_{ss} < 0$, so past consumption lowers current utility, increasingly so as past experience rises. In this augmented version of the model, past consumption has benefits because it lowers the probability of hospitalization. This means the sign of $\frac{\partial u}{\partial s}$ could be positive or negative. Past experience could either add to or detract from current utility. Either way, $\frac{\partial^2 u}{\partial s^2} < 0$ so that the utility kernel remains concave in a and s. Beneficial effects of past experience on current health risk provide an additional interpretation for the behavior of individuals in the rational addiction model, without substantially changing the predictions of the model.

The initial condition ($s_{16} = 0$) is that the individual has zero experience at age sixteen. Periods in our simulation are three months. The stock of experience is backward-looking, because the

marginal utility of consumption today (and therefore the future stock of experience) is affected by past consumption, and forward-looking because the individual knows that current actions will affect future choices.[25]

We simulate the model with a perfectly foreseen decrease in the price of alcohol representing the removal of potential punishment for illegal procurement. Figure 7 shows the time path of the variable a in the model in two cases, with an MPA of eighteen and and MPA of twenty. As expected, consumption tends to be higher for those eligible to consume legally. However as an individual approaches the MPA consumption first drops then jumps up before gradually falling to its steady state. Comparing the periods immediately before and immediately after becoming eligible we see a drop in consumption even though consumption is generally higher for those above the legal purchase age. Individuals may want to consume more before they cross the legal purchase age because this adds to their stock of experience and hence to the marginal utility of consumption in a period when they face lower prices. The figure reveals that consumption is higher among those above the minimum legal purchase age, but the extent of the increase is unrelated to the change in consumption around the threshold. For substantially higher habit depreciation rates, the discontinuity at the MPA is closer to the long-run effect.

Figure 8 shows the model analog to Figures 3 and 4. In particular, it shows average consumption in three age groups (always ineligible, newly eligible, always eligible) leading up to and after the law change, which our simulation assumes is unforeseen. The always eligible (twenty to twenty-three year olds) and always ineligible (sixteen and seventeen year olds) groups show minor variations after the law change. These differences are due to the change in timing of consumption swings associated with approaching and crossing the minimum legal purchase age. The newly eligible (eighteen and

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[25]Becker et al. (1994) focus on the fact that behavior is forward looking in the rational addiction model, though Gruber and Koszegi (2001) note that forward-looking behavior does not distinguish rational addiction from alternative hypotheses. Grossman and Chaloupka (1998) also emphasize the fact that consumption of an addictive good should respond to past consumption and anticipate future consumption.

nineteen year olds) show a substantial increase in consumption immediately after the law change, but this dissipates somewhat as their consumption closes in on consumption of the older group.

5.2 The Effect of a Lower Purchase Age in the Model

The data represented in Figure 7 shows the model's assessment of the steady-state effect of a drinking age of eighteen versus a drinking age of twenty, focusing on the effects on consumption of alcohol. The consequences for hospitalizations h are broadly similar. In addition, we compute several other statistics.

First, we calculate how much alcohol consumption changes when a person crosses the legal threshold as $a_{MPA} - a_{MPA-1}$. We perform this calculation for a high minimum purchase age and a low minimum purchase age. In each case, the initial condition is $s_{16} = 0$: the person has zero accumulated experience upon turning sixteen.

Second, we simulate a change in the drinking age. We consider a population with one individual at each age, each of whom has been behaving as if they face $MPA = MPA^{high} = 20$. Then, at year $y = Y$, we change the drinking age so that $MPA = MPA^{low} = 18$. For those who are already twenty, the change has no effect. Those who are younger than twenty, and even younger than eighteen, alter their behavior. We compute the following, which corresponds to our date-based discontinuities,

$$E[a_{t,y}|MPA^{low} \leq t < MPA^{high}, y \geq Y] - E[a_{t,y}|MPA^{low} \leq t < MPA^{high}, y < Y], \quad (4)$$

limiting the range of y to a narrow window around the date of the law change. It is the change in consumption for the age group that becomes eligible.

Third, given the simulated policy change, we can compute a difference-in-differences estimate. We regress alcohol consumption on age and dummies for whether the observation is before or after the law change, whether the age group is the one that becomes eligible for legal consumption, and

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an interaction of the post and treatment dummies.

We perform these exercises on a particular calibration of the model. The qualitative results we discuss here are mostly insensitive to the model's parameters, with the exception of d, the rate of depreciation of the stock of experience, which we set to 0.3. With a depreciation rate of 0.3, the stock has a half-life of about six months, since each period in our model is intended to correspond to three months. For larger values of β_s the spike in hospitalizations around the age of eligibility is more transitory than the spike in consumption, and the steady-state effect is smaller.

In this model, the steady-state consumption in adulthood depends only on the price of alcohol in adulthood, not on the minimum legal age or the punishment for underage participation. (This rules out the possibility that with a low MPA young people might experiment more with alcohol and maybe acquire bad habits they otherwise would not.) In the steady state of each policy regime, consumption is higher among sixteen and seventeen year olds when the MPA is eighteen instead of twenty, and is substantially higher (50% to 100%) among eighteen and nineteen year olds. Consumption is similar for those over twenty. In the model, the law change leads to substantially higher youth consumption. In our calibration, the age-based discontinuity in consumption is negative, the date-based discontinuity (considering eighteen and nineteen year olds just before and just after the law change) is large and positive, while the difference-in-differences estimate is in between the two RD effects. The true effect is relatively close to the difference-in-differences effect. These results are consistent with our finding that the regression discontinuity method based on the date of the law change often delivers a larger estimated effect than the difference-in-differences method and the age-based regression discontinuity.

Under rational addiction there is a general tendency to consume more alcohol after crossing the MPA, but looking in a neighborhood of the MPA does not reveal the magnitude of this effect. The model's forward looking-agents consume more liquor just before they reach the legal purchase

age because this consumption will add to their future marginal utility when alcohol is more freely available to them. The model suggests that the estimates of the effect of legal eligility on consumption in Carpenter and Dobkin (2009) are smaller than the likely consequences of lowering the minimum legal purchase age in the United States. Conversely, the fact that our DD and age-based RD estimates are close together for males suggests a weakness in the rational addiction model.

The law change based discontinuities, in which the consumption of those in the affected age groups is compared just before and just after the law change, tend to be larger than the true effect of consumption. The reason for this is related to the pre-MPA practicing discussed above. The group suddenly becomes eligible to consume legally but has not been through a practicing period so their consumption jumps up dramatically both because of the increased availability of liquor and the desire to practice for the near future in which alcohol will continue to be more freely available than they had previously anticipated. For these reasons, the law change discontinuity tends to overstate the effect of a change in the drinking age, at least in the model.

The difference-in-differences estimate is the closest to the actual effect of the policy in the model. One reason it is different from the true effect is that the twenty to twenty-three year old age group is affected by the law change – consumption in this age group is lower after the law change. However, the DD estimate is not so influenced by the large changes in consumption right at the time of the law change as in the date-based discontinuity, and does not use only a narrow window around the threshold age that leads the age-based discontinuity astray.

6   Conclusion

In 1999 New Zealand's parliament passed the Sale of Liquor Amendment Act, lowering the minimum legal purchase age from twenty to eighteen years. In light of concerns that the law change led to more alcohol abuse, the current parliament considered a reversal of the earlier reduction in the minimum legal age, before eventually rejecting this proposal.

Using difference-in-differences methods, we estimate a statistically and economically significant increase in alcohol-related hospitalizations among young men as a consequence of the Sale of Liquor Amendment Act. Our point estimates suggest eighteen and nineteen year old males saw a 24.6% increase in hospitalizations (s.e.=5.5%) and females in the same age group had a 22% increase (s.e.=8.1%). These increases are smaller than prior studies have found as we use data from a longer period of time (fourteen years) and larger geographic area (the entire country) in our regressions. In addition, we show that a portion of the increase can be attributed to changing cohort sizes. When we use hospitalizations per person as our outcome, the estimated effects of the law change are one-fifth to one-fourth smaller.

We find that those under eighteen did not experience a significant increase in alcohol-related hospitalizations, again in contrast to some prior work and public concerns about the prevalence of underage drinking, though it is possible that younger drinkers are encountering additional problems but not seeking medical care. Drug-related hospitalizations do not decrease as a consequence of easier access to alcohol as would be expected if substitution is important. We look for, but do not find, evidence that young people benefit from experience with alcohol. We estimate slight adverse effects of experience (more experience increases alcohol-related hospitalizations) around the minimum purchase age, though these are often not statistically significant.

We also estimate the effects of the law change using regression discontinuity methods. Using the discontinuity in legal status for those aged eighteen and nineteen around the time of the law change, estimates for males are generally larger than the DD estimates, though smaller for females. When we use the discontinuity in legal status based on age reaching the minimum purchase age, the estimated effect is generally smaller. This is largely consistent with a rational addiction model in which the true effect of the law is close to the difference-in-difference estimate. As such, we put more weight on the DD estimates in assessing the policy's effect. In the model, discontinuities

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in alcohol consumption at the minimum legal purchase age substantially overstate the effect of a change in the drinking age.

An alternative explanation for the rise in hospitalizations is an increase in the propensity of eighteen and nineteen year olds to go to a hospital conditional on being unwell, rather than the underlying incidence of alcohol-related problems. However, it is not illegal for someone younger than the minimum purchase age to have consumed alcohol, and patient confidentiality means that law enforcement officials would be notified only if a crime had been committed. In addition, if the law change caused more individuals with marginal health problems to check into a hospital this should shorten the average duration of hospital stays and no change in the number of cases with alcohol listed only in the first diagnosis code. In the data, there is no clear evidence of a change in the length of hospital stays and the increase in hospitalizations with alcohol in the first diagnosis code is as large as the overall increase, so self-selection does not seem to be a driving the results.

Alcohol-related hospitalizations in New Zealand have been on an ongoing upward trend, across age groups and for both men and women. Our estimates suggest that only a small part of this trend increase can be attributed to the Sale of Liquor Amendment Act. Understanding the surge in alcohol-related health problems must go beyond the drinking age.

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Figure 1: Alcohol and Drug Hospitalizations by Year and Sex
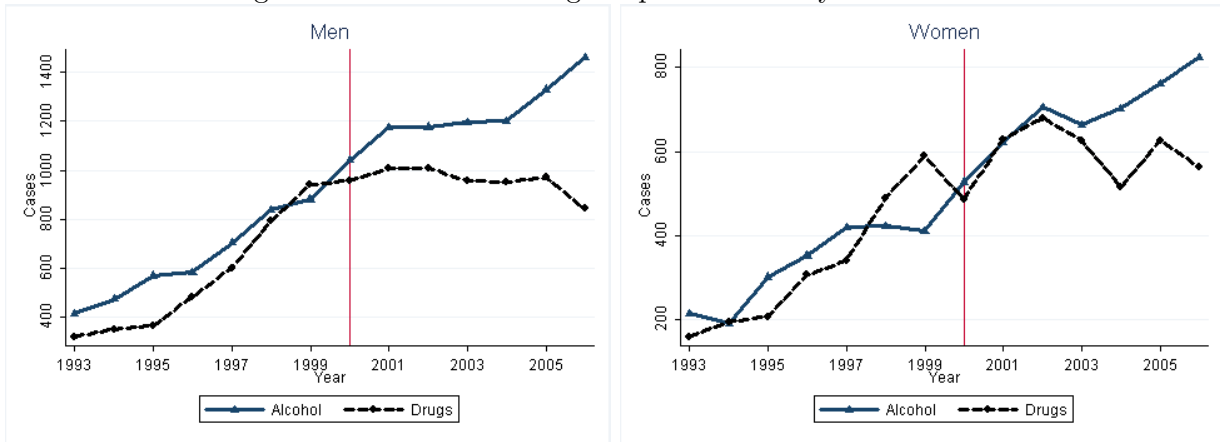
Note: all hospitalizations in public hospitals for alcohol or drug related diagnoses (as coded in Appendix A), for people 16 to 23 years old.

Figure 2: Alcohol and Drug Hospitalizations by Age and Sex
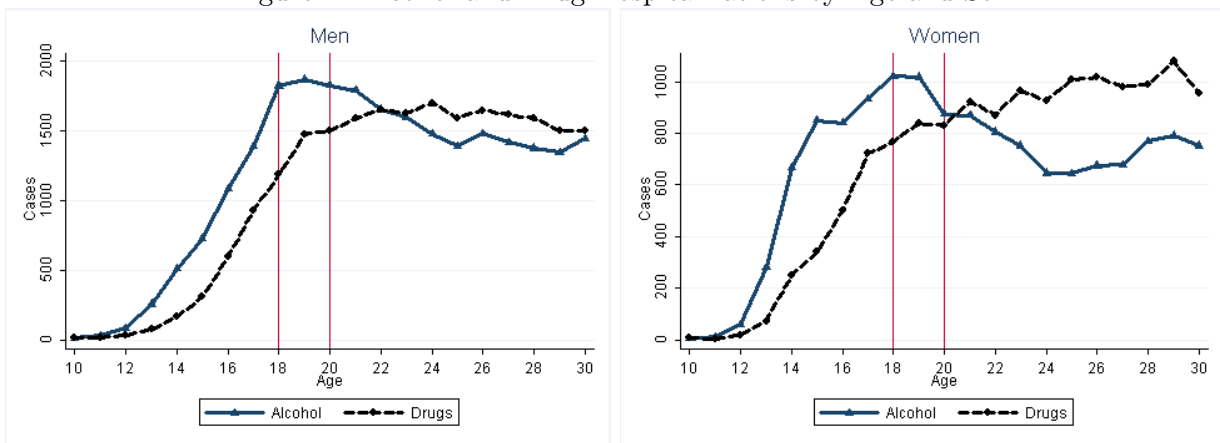
Note: all hospitalizations in public hospitals for alcohol or drug related diagnoses (as coded in Appendix A), between 1993 and 2006.

Figure 3: Alcohol Related Hospitalizations, Men, 1993-2006
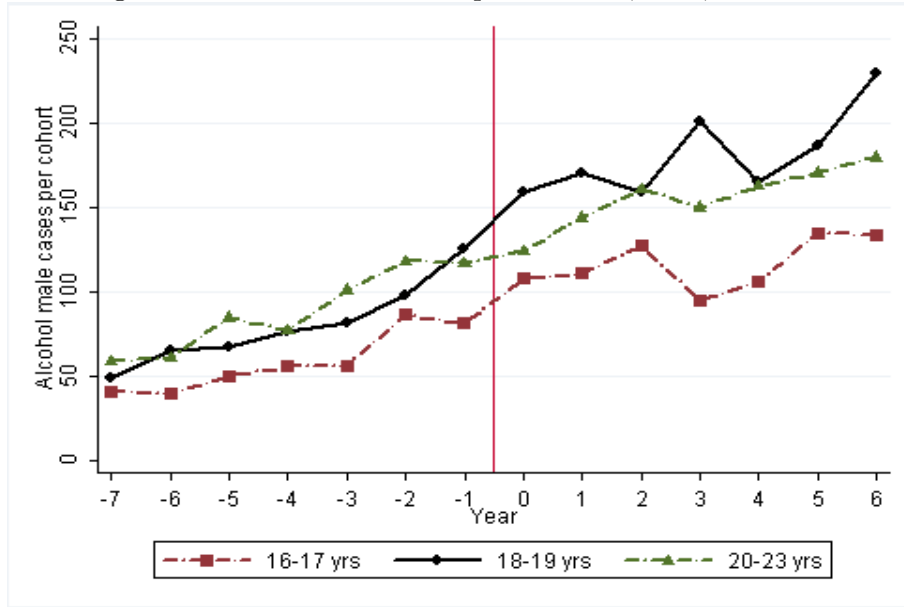
Note: all hospitalizations in public hospitals for alcohol related diagnoses (as coded in Appendix A). The unit of observation is the age group-year, with each line representing the indicated age groups. Corresponding regression results are reported in Table 2, panel A.

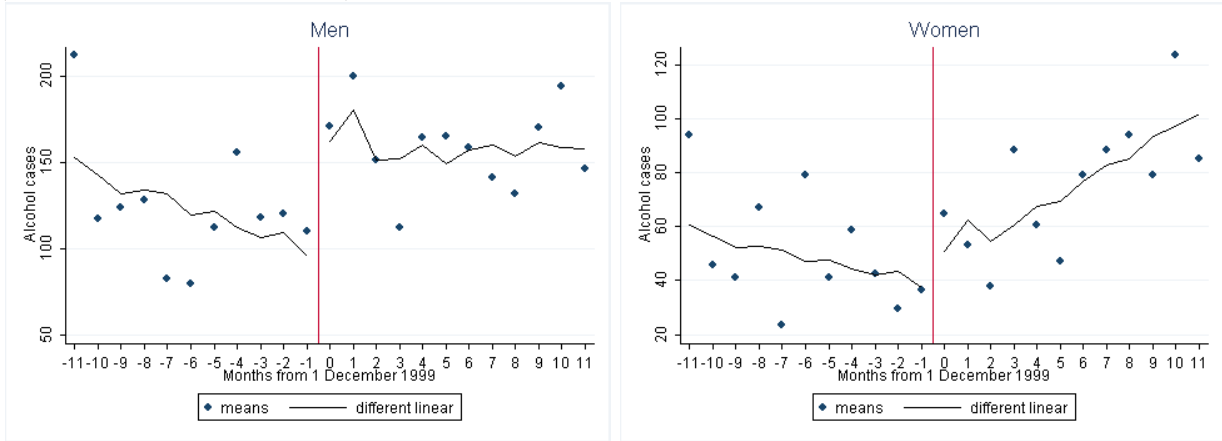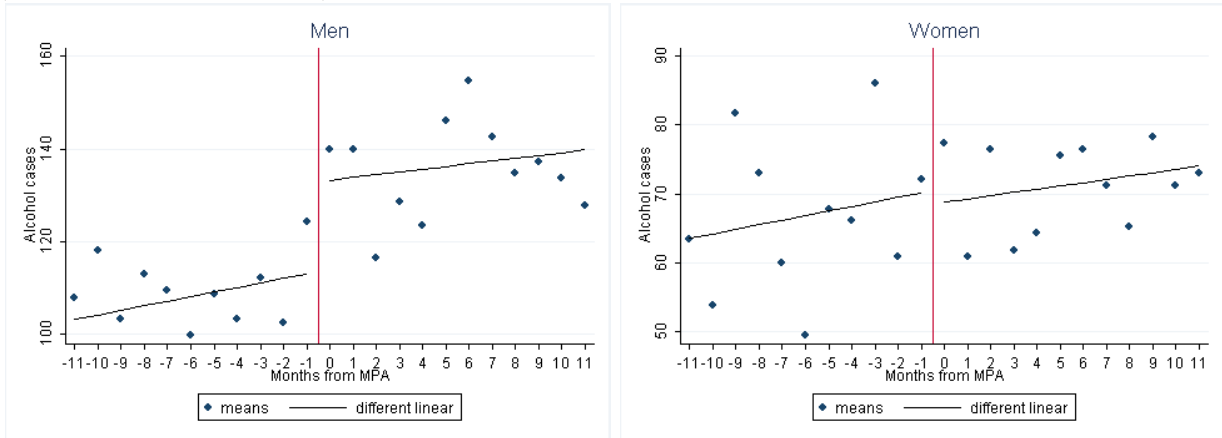Figure 4: Alcohol Related Hospitalizations, Women, 1993-2006
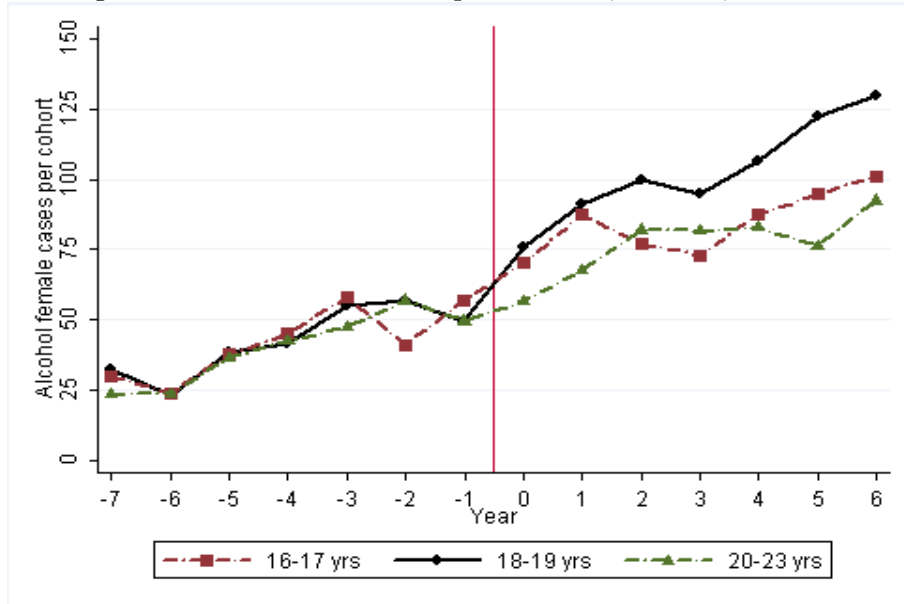
Note: all hospitalizations in public hospitals for alcohol related diagnoses (as coded in Appendix A). The unit of observation is the age group-year, with each line representing the indicated age groups. Corresponding regression results are reported in Table 2, panel A.

Figure 5: Hospitalizations by Month Relative to Sale of Liquor Amendment Act, 18 and 19 year olds,
(Law Change Discontinuity)

Note: December 1, 1999 was when eighteen and nineteen year olds became eligible to legally purchase liquor. Dots indicate the number of alcohol-related hospitalizations each month (per cohort and at an annual rate). The fitted line corresponds to a linear regression using daily data. The regression controls for a function of the running variable that is allowed to be different above and below the threshold, and includes day-of-the-week and New Year's day indicators as control variables. Standard errors are robust and clustered at the running variable level. Corresponding regression results are reported in column (1) (for 18-19 year olds) of Table 5.

Figure 6: Hospitalizations by Month Relative to Minimum Purchase Age (MPA),
(Age-Based Discontinuity)

Note: Dots indicate the number of alcohol-related hospitalizations each month (per cohort and at an annual rate) around the Minimum Purchase Age (20 years prior to the Sale of Liquor Amendment Act, and 18 years after the Sale of Liquor Amendment Act). The fitted line corresponds to a linear regression using daily data. The regression controls for a function of the running variable that is allowed to be different above and below the threshold, and includes year fixed effects as control variables. Standard errors are robust and clustered at the running variable level. Corresponding regression results are reported in column (1) of Table 6.

Figure 7: Age Profile of Alcohol Consumption in the Rational Addiction Model
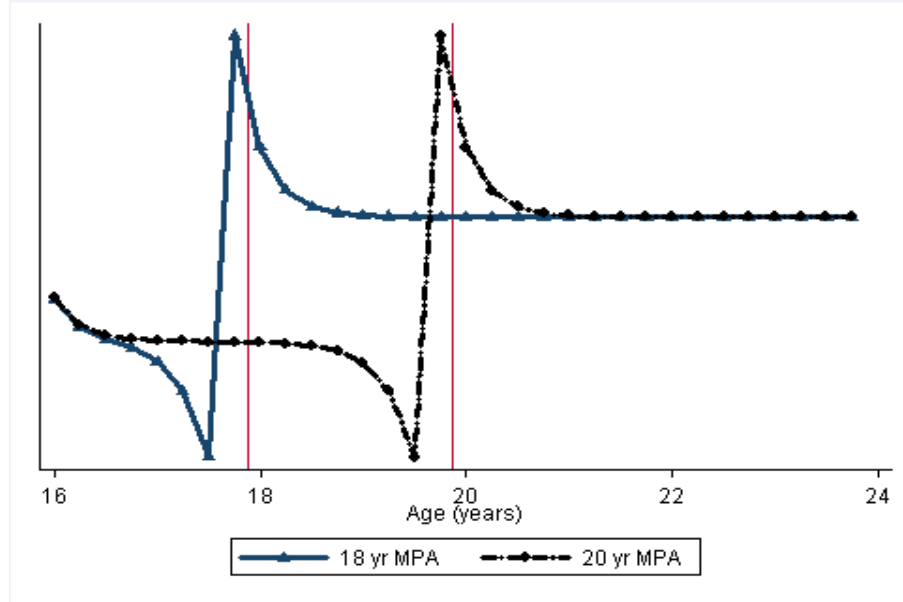
Note: age profile of alcohol consumption computed in the rational addiction model for two separate minimum purchase ages.

Figure 8: Before vs. After Comparisons in the Rational Addiction Model
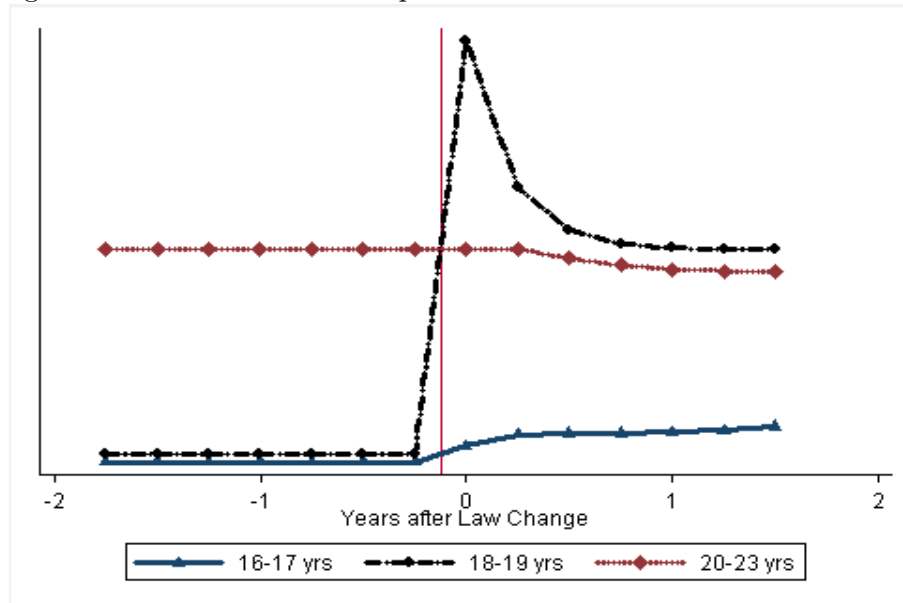
Note: time profile of alcohol consumption by individuals of a given age in the rational addiction model when there is an unanticipated lowering of the minimum purchase age at year zero.

Table 1: Hospitalizations in Public Hospitals for 16 to 23 Year Olds between 1993 and 2006

Panel A: Summary Statistics		
Variable	Percent	Stdev.
Male	0.310	0.462
Age in years	19.858	2.203
European origin*	0.579	0.494
Maori*	0.261	0.439
Pacific Islander*	0.081	0.273
Asian*	0.033	0.178
Other ethnicity*	0.047	0.211
Alcohol incident	0.023	0.150
Drug incident	0.019	0.138
Panel B: Age Distribution		
Age in years	Number of Hospitalizations	
	Female	Male
16	42,192	30,016
17	56,385	32,470
18	69,581	34,002
19	79,059	35,173
20	83,950	35,356
21	86,979	34,958
22	90,155	34,437
23	93,769	33,603
Total	602,070	270,015

Note: all hospitalizations in public hospitals. *Percent calculated among those reported.
Source: New Zealand Health Information Service and authors' calculations.

Table 2: Difference-in-Differences Estimates of the Alcohol-Related Hospitalizations Effect of Lowering the Minimum Purchase Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log linear		Poisson		Norm. Log Linear		Proportion	
	Males	Females	Males	Females	Males	Females	Males	Females

Panel A: Using 20 to 23 Year Olds as the Control Group

Baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
18 or 19 years old * Post	0.246***	0.220***	0.246***	0.227***	0.188***	0.162**	0.008***	0.004***
	(0.055)	(0.081)	(0.050)	(0.078)	(0.056)	(0.081)	(0.003)	(0.001)
16 or 17 years old * Post	0.125**	0.032	0.119**	0.044	0.045	-0.059	-0.007***	0.003**
	(0.051)	(0.078)	(0.049)	(0.071)	(0.051)	(0.080)	(0.002)	(0.001)
Observations	112	112	112	112	112	112	112	112

Collapsed Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
18 or 19 years old * Post	0.246***	0.220*	0.246***	0.227***	0.188***	0.162*	0.008***	0.004**
	(0.040)	(0.090)	(0.028)	(0.053)	(0.034)	(0.069)	(0.001)	(0.001)
16 or 17 years old * Post	0.125**	0.032	0.119***	0.044	0.045	-0.059	-0.007**	0.003**
	(0.038)	(0.089)	(0.024)	(0.056)	(0.034)	(0.074)	(0.002)	(0.001)
Observations	16	16	16	16	16	16	16	16

Panel B: Using 20 to 21 Year Olds as the Control Group

Baseline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
18 or 19 years old * Post	0.206***	0.095	0.198***	0.100	0.176***	0.066	0.007**	0.003**
	(0.058)	(0.099)	(0.052)	(0.091)	(0.059)	(0.099)	(0.003)	(0.001)
16 or 17 years old * Post	0.085	-0.093	0.071	-0.083	0.033	-0.155	-0.008***	0.002
	(0.051)	(0.093)	(0.050)	(0.083)	(0.050)	(0.095)	(0.003)	(0.001)
Observations	84	84	84	84	84	84	84	84

Collapsed Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
18 or 19 years old * Post	0.206***	0.095	0.198***	0.100***	0.176***	0.066	0.007***	0.003*
	(0.031)	(0.048)	(0.022)	(0.024)	(0.019)	(0.036)	(0.000)	(0.001)
16 or 17 years old * Post	0.085*	-0.093	0.071***	-0.083***	0.033	-0.155**	-0.008**	0.002***
	(0.028)	(0.047)	(0.017)	(0.030)	(0.019)	(0.047)	(0.002)	(0.000)
Observations	12	12	12	12	12	12	12	12

Note: Baseline results report robust standard errors in parentheses. Collapsed Data results collapsed the data to one pre and one post observations for each age group following Bertrand et al. (2004). Significant at *** $p < 0.01$, ** $p < 0.05$ or * $p < 0.1$. Dependent variable is alcohol-related hospitalizations as defined in the column headings: *Log linear* is the log of hospitalizations for the group; *Poisson* reports incidence rate ratios; *Normalized Log Linear* reports estimates when the dependent variable is the log of incidents per thousand members of the population cohort; and *Proportion* reports estimates when the dependent variable is alcohol-related hospitalizations as a proportion of all hospitalizations for the relevant age-sex-year group. All regressions control for year and age-in-years fixed effects.

Table 3: Difference-in-Differences Estimates of the Drug-Related Hospitalizations Effect of Lowering the Minimum Purchase Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log linear		Poisson		Norm. Log Linear		Proportion	
	Males	Females	Males	Females	Males	Females	Males	Females
Panel A: Using 20 to 23 Year Olds as the Control Group								
Baseline								
18 or 19 years old * Post	0.069	0.208**	0.059	0.167***	0.011	0.151*	-0.004	0.001
	(0.079)	(0.090)	(0.067)	(0.063)	(0.076)	(0.087)	(0.003)	(0.001)
16 or 17 years old * Post	0.108	0.235**	0.098	0.168**	0.028	0.144	-0.011***	0.001
	(0.082)	(0.096)	(0.068)	(0.076)	(0.079)	(0.092)	(0.003)	(0.001)
Observations	112	112	112	112	112	112	112	112
Collapsed Data								
18 or 19 years old * Post	0.069	0.208**	0.059*	0.167***	0.011	0.151**	-0.004*	0.001*
	(0.075)	(0.070)	(0.031)	(0.032)	(0.070)	(0.056)	(0.002)	(0.000)
16 or 17 years old * Post	0.108**	0.235***	0.098***	0.168***	0.028	0.144**	-0.011**	0.001
	(0.035)	(0.054)	(0.031)	(0.034)	(0.036)	(0.046)	(0.003)	(0.001)
Observations	16	16	16	16	16	16	16	16
Panel B: Using 20 to 21 Year Olds as the Control Group								
Baseline								
18 or 19 years old * Post	0.029	0.159	-0.002	0.124*	-0.000	0.131	-0.005	0.001
	(0.079)	(0.107)	(0.063)	(0.075)	(0.075)	(0.103)	(0.003)	(0.001)
16 or 17 years old * Post	0.068	0.186*	0.037	0.124	0.016	0.124	-0.011***	0.001
	(0.080)	(0.113)	(0.068)	(0.084)	(0.075)	(0.109)	(0.003)	(0.001)
Observations	84	84	84	84	84	84	84	84
Collapsed Data								
18 or 19 years old * Post	0.029	0.159	-0.002	0.124**	-0.000	0.131	-0.005	0.001
	(0.080)	(0.092)	(0.024)	(0.056)	(0.083)	(0.081)	(0.003)	(0.001)
16 or 17 years old * Post	0.068	0.186*	0.037	0.124**	0.016	0.124	-0.011*	0.001
	(0.033)	(0.078)	(0.025)	(0.057)	(0.048)	(0.072)	(0.004)	(0.001)
Observations	12	12	12	12	12	12	12	12

Note: Baseline results report robust standard errors in parentheses. Collapsed Data results collapsed the data to one pre and one post observations for each age group following Bertrand et al. (2004). Significant at *** $p < 0.01$, ** $p < 0.05$ or * $p < 0.1$. Dependent variable is drug-related hospitalizations as defined in the column headings: *Log linear* is the log of hospitalizations for the group; *Poisson* reports incidence rate ratios; *Normalized Log Linear* reports estimates when the dependent variable is the log of incidents per thousand members of the population cohort; and *Proportion* reports estimates when the dependent variable is alcohol-related hospitalizations as a proportion of all hospitalizations for the relevant age-sex-year group. All regressions control for year and age-in-years fixed effects.

Table 4: Estimates of the Benefits of Experience in the Difference-in-Differences Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Log linear		Poisson		Norm. Log Linear		Proportion	
	Males	Females	Males	Females	Males	Females	Males	Females
Panel A: Experience								
Baseline								
Experience	0.029	0.044	0.021	0.023	0.004	0.028	0.002**	0.001
	(0.023)	(0.037)	(0.021)	(0.032)	(0.023)	(0.037)	(0.001)	(0.001)
18 or 19 years old * Post	0.252***	0.229***	0.252***	0.235***	0.189***	0.168**	0.008***	0.004***
	(0.056)	(0.082)	(0.051)	(0.078)	(0.057)	(0.082)	(0.003)	(0.001)
16 or 17 years old * Post	0.158***	0.082	0.145**	0.074	0.049	-0.027	-0.004	0.004**
	(0.059)	(0.085)	(0.057)	(0.080)	(0.061)	(0.087)	(0.003)	(0.001)
Observations	112	112	112	112	112	112	112	112
Panel B: Cumulative Experience								
Baseline								
Cumulative Experience	0.035**	0.032	0.029*	0.036	0.036**	0.041	0.003***	0.001
	(0.018)	(0.041)	(0.015)	(0.033)	(0.017)	(0.040)	(0.001)	(0.001)
18 or 19 years old * Post	0.306***	0.247***	0.296***	0.259***	0.249***	0.198**	0.013***	0.005***
	(0.058)	(0.091)	(0.055)	(0.084)	(0.057)	(0.091)	(0.003)	(0.001)
16 or 17 years old * Post	0.237***	0.085	0.211***	0.107	0.161**	0.011	0.002	0.004**
	(0.069)	(0.109)	(0.065)	(0.095)	(0.067)	(0.109)	(0.003)	(0.002)
Observations	112	112	112	112	112	112	112	112

Note: Baseline results report robust standard errors in parentheses. Significant at *** $p < 0.01$, ** $p < 0.05$ or * $p < 0.1$. Experience is defined as years since attaining the minimum purchase age. Cumulative experience is defined as cumulative number of hospitalizations for a cohort, in hundreds. Dependent variable is drug-related hospitalizations as defined in the column headings: *Log linear* is the log of hospitalizations for the group; *Poisson* reports incidence rate ratios; *Normalized Log Linear* reports estimates when the dependent variable is the log of incidents per thousand members of the population cohort; and *Proportion* reports estimates when the dependent variable is alcohol-related hospitalizations as a proportion of all hospitalizations for the relevant age-sex-year group. All regressions control for year and age-in-years fixed effects.

Table 5: Law Change Regression Discontinuity Estimates of the Minimum Purchase Age

	(1)	(2)	(3)	(4)	(5)
			Panel A: Male		
16-17 years	22.025	-27.419	-23.298	66.187	0.256
	(20.919)	(41.176)	(44.632)	(87.902)	(0.237)
Observations	1,462	518	362		1,462
R-squared	0.099	0.198	0.201		
18-19 years	53.663**	78.024*	77.797	204.551*	0.420**
	(24.605)	(42.695)	(52.536)	(109.275)	(0.189)
Observations	1,462	650	362		1,462
R-squared	0.081	0.145	0.136		
20-23 years	-5.012	-29.042	-35.616	-56.861	-0.042
	(16.843)	(32.186)	(36.222)	(82.839)	(0.147)
Observations	2,924	1,060	724		2,924
R-squared	0.052	0.085	0.077		
			Panel B: Female		
16-17 years	20.839	7.563	-14.966	61.214	0.350
	(15.577)	(25.512)	(30.438)	(60.699)	(0.258)
Observations	1,462	550	362		1,462
R-squared	0.042	0.077	0.077		
18-19 years	4.673	23.809	40.999*	-3.253	0.183
	(14.153)	(19.225)	(24.221)	(46.034)	(0.276)
Observations	1,462	726	362		1,462
R-squared	0.042	0.056	0.061		
20-23 years	-4.875	-16.982	-29.913	-30.972	-0.089
	(10.963)	(17.926)	(21.657)	(51.804)	(0.219)
Observations	2,924	1,108	724		2,924
R-squared	0.015	0.020	0.023		
Bandwidth	365		90		365
Model	Linear	LLR	Linear	I&K	Poisson

Note: Significant at *** $p < 0.01$, ** $p < 0.05$ or * $p < 0.1$. The columns report the change in hospitalizations for different age groups due to the implementation of the Sale of Liquor Amendment Act (SLAA) on 1 December 1999. Daily data is scaled by 365 to make numbers comparable to difference-in-differences results. Linear (columns 1 and 3), LLR (column 2) and Poisson (column 5) regressions are clustered at the day level; report robust standard error in parentheses; include day-of-the-week and New Year holiday indicators as control variables; and control for a function of the running variable that is allowed to be different above and below the threshold. Linear and Poisson regressions in columns (1) and (5) respectively, use data for 365 days before and after the SLAA. Local linear regressions (LLR) in column (2) uses a rule-of-thumb bandwidth following Fan and Gijbels (1996). Column (3) uses data 90 days before and after the SLAA. I & K in column (4) corresponds to results using an optimal bandwidth for local linear regression in an RD setting following Imbens and Kalyanaraman (2012).

Table 6: Age-Based Regression Discontinuity Estimates of the Minimum Purchase Age

	(1)	(2)	(3)	(4)	(5)
			Panel A: Male		
Older than MPA	19.395**	29.095	33.264*	91.479**	0.157**
	(8.452)	(19.352)	(18.753)	(46.124)	(0.068)
Observations	10,262	3,178	2,534		10,262
R-squared	0.043	0.053	0.052		
			Panel B: Female		
Older than MPA	-1.959	8.690	10.046	116.765*	-0.029
	(6.374)	(13.959)	(14.838)	(65.197)	(0.092)
Observations	10,262	3,626	2,534		10,262
R-squared	0.037	0.048	0.051		
Bandwidth	365			90	365
Model	Linear	LLR	Linear	I&K	Poisson

Note : Significant at *** $p < 0.01$, ** $p < 0.05$ or * $p < 0.1$. The columns report the estimated effect of turning 20 on hospitalizations prior to the Sale of Liquor Amendment Act, and of turning 18 on hospitalizations after the Sale of Liquor Amendment Act, captured by the "Age" variable. Daily data is scaled by 365 to make numbers comparable to difference-in-differences results. Linear (columns 1 and 3), LLR (column 2) and Poisson (column 5) regressions are clustered at the running variable level; report robust standard error in parentheses; include year fixed effects as control variables; and control for a function of the running variable that is allowed to be different above and below the threshold. Linear and Poisson regressions in columns (1) and (5) respectively, use data for 365 days before and after the MPA. Local linear regressions (LLR) in column (2) uses a rule-of-thumb bandwidth following Fan and Gijbels (1996). Column (3) uses data 90 days before and after the MPA. I & K in column (4) corresponds to results using an optimal bandwidth for local linear regression in an RD setting following Imbens and Kalyanaraman (2012).

A   Appendix: Alcohol and Drug Diagnoses

Table A1: Alcohol Diagnoses Identified in Sample

Description	Percent of Cases
Alcohol use disorder, unspecified	63.89
Toxic effect of ethyl alcohol	10.06
Other and unspecified alcohol dependence, unspecified	8.28
Personal history of harmful use of alcohol	7.95
Alcoholic gastritis without mention of haemorrhage	1.95
Acute alcoholic intoxication, unspecified	1.54
Alcohol use disorder, episodic	1.15

Note: this table reports alcohol-related diagnoses that occur in more than 1% of all alcohol-related cases. The full set of codes used is in Appendix B .

Table A2: Drug Diagnoses Identified in Sample

Description	Percent of Cases
Other, mixed, or unspecified drug use disorder, unspecified	14.75
Mental and behavioural disorders due to use of cannabinoids, harmful use	10.53
Drug use	9.51
Cannabis dependence, unspecified	7.24
Unspecified drug-induced mental disorder	6.77
Other specified drug-induced mental disorders	6.75
Opioid type dependence, unspecified	4.49
Combinations of drug dependence excluding opioid type drug,unspecified	3.40
Personal history of drug use disorder	3.34
Unspecified drug dependence, unspecified	3.17
Poisoning by psychodysleptics (hallucinogens)	3.06
Poisoning by psychostimulants	2.74
Poisoning by other sedatives and hypnotics	2.13
Other specified drug dependence, unspecified	2.07
Amphetamine or related acting sympathomimetic use disorder,unspecified	1.96
Drug withdrawal syndrome	1.44
Hallucinogen use disorder, unspecified	1.34
Drug induced organic delusional syndrome	1.14

Note: this table reports drug-related diagnoses that occur in more than 1% of all drug-related cases. The full set of codes used is in Appendix B .

B Appendix: ICD codes

We use ICD-9-CMA-II and ICD-10-AM-I for the coding of hospitalizations. ICD-10-AM-I was adopted in July 1999. Most incidents were already back-coded to ICD9 codes. The ICD-9-CMA-II and ICD-10-AM-I lists were provided by New Zealand's Ministry of Health.

B.1 Mention of Alcohol

ICD-9

2910-2915, 2918, 2919, 30300-30303, 30390-30393, 30500-30503, 3575, 4255, 53530, 53531, 5710-5713, 7903, 86000-86019, 86090-86099, 76071, 9773, 9800, V113, V1584, V6141, V791.

ICD-10

E244, F100-F109, G312, G621, G721, I426, K700-K704, K709, K860, R780, T519, Z502, Z714, Z721, K292, X4500-X4504, X4508-X4514, X4518-X4524, X4528-X4534, X4538-X4544, X4548-X4554, X4558-X4564, X4568-X4574, X4578-X4584, X4588-X4594, X4598, X4599, X6500-X6504, X6508-X6514, X6518-X6524, X6528-X6534, X6538-X6544, X6548-X6554, X6558-X6564, X6568-X6574, X6578, X6579, X6580-X6584, X6588-X6594, X6598, X6599, Y1500-Y1504, Y1508-Y1514, Y1518-Y1524, Y1528-Y1534, Y1538, Y1539, Y1540-Y1544, Y1548-Y1554, Y1558-Y1564, Y1568-Y1574, Y1578-Y1584, Y1588-Y1594, Y1598, Y1599, Y900-Y913, Y919, 9200200, 9200300, 9200400, 9200800, 9200900, 9201000, O354, P043, Q860, T510, Z040, Z811, Z8641, Y573.

B.2 Mention of Drugs

ICD-9

2920, 29211, 29212, 2922, 29281-29284, 29289, 2929, 30400-30403, 30410-30413, 30420-30423, 30440-30443, 30450-30453, 30460-30463, 30470-30473, 30480-30483, 30490-30493, 30530-30533, 30540-30543, 30550-30553, 30560-30563, 30570-30573, 30590-30593, 9676, 9678, 9679, 9696, 9697, 30430-30433, 64830-64834, 65550, 65551, 65553, 7795, 9445, 9454, 9466.

ICD-10

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