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# The effects of drinking and driving laws on car crashes, injuries, and deaths: Evidence from Chile[☆]

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ABSTRACT

This paper analyzes the effects of lowering the legal blood alcohol content limit for drivers from 0.05 to 0.03 grams of alcohol per deciliter of blood (g/dL) and increasing license suspension periods for offenders. We take advantage of a rich data set of administrative records that allow us to identify direct measures of accidents involving alcohol including fatalities and injuries. Results show a significant decrease of 32% in alcohol-related car accidents right after the law was approved but the effects moderate over time (15% after three years). There is also a significant reduction in injuries (31% right after the approval and 11% after three years) but no statistically significant effects on deaths. Complementary analysis of blood samples shows that the law had an effect on blood alcohol content (BAC) of male drivers up to the 90th percentile of the BAC distribution.

## 1. Introduction

Motor vehicle accidents cause over 1.24 million deaths and 20–50 million injuries yearly worldwide. Traffic accident fatalities are the leading cause of death among people between the ages of 15 and 29, and these accidents are expected to rise from the ninth to the fifth leading cause of death among all individuals by 2030 (WHO, 2013). With the aim of reducing car crashes, injuries, and fatalities, many countries have lowered the blood alcohol limit for driving, some even setting it to zero.[1]

The empirical evidence on the effects of *per se* blood alcohol limits for driving on car accidents, injuries, and deaths show mixed results (Mann et al., 2001; Beirness and Simpson, 2002; Bernat et al., 2004; Fell and Voas, 2006; Elvik et al., 2009; Assum, 2010). Most of the studies, if not all, focus on the developed world and some lack information on accidents due to alcohol consumption and use proxy measures to circumvent this data issue.

In this paper, we analyze a 2012 Chilean law that reduced the legal blood alcohol limit from 0.05 to 0.03 g/dL for the overall population and increased license suspension periods for offenders. We have access to data on car accidents and their causes, which allows us to analyze the direct effects of the law on alcohol-related crashes. This also includes data on injuries and deaths associated with each accident. We implement regression discontinuity, generalized Poisson, and linear logit regressions to assess the effects of the law on three main outcome variables: accidents, injuries, and deaths.

The related literature is extensive, and there have been some efforts to summarize the empirical evidence. For example, Mann et al. (2001) examine the evidence for five developed countries that introduced or lowered the legal per se blood alcohol limit for driving (the United Kingdom, Canada, Japan, the Netherlands, and the United States) concluding that "some beneficial effect on traffic safety measures has been reported." However, the effects are in some cases small and temporary. In an extensive review for different jurisdictions of the United States, Canada, Sweden, Australia, and Norway, Beirness and Simpson (2002) conclude that the evidence (even those from methodologically rigorous studies) is inconsistent, showing beneficial effects in some cases, but mixed results, or even no positive effects in others. Other authors conclude that lowering legal BAC limits are associated with lower car crash fatalities and injuries (Wagenaar et al., 2007; Fell and Voas, 2006; Bernat et al., 2004). This evidence focuses on laws that reduced the limits from BAC 0.1 to 0.08 or from 0.08 to 0.05 g/dL.

This paper contributes to the literature on blood alcohol limits in four ways. First, it provides new evidence on lowering legal blood

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[1] Medical literature has found "strong evidence that impairment of some driving-related skills begins with any departure from zero blood alcohol content" (Moskowitz and Fiorentino, 2000).

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alcohol limits to a small amount: from 0.05 to 0.03 g/dL. This type of intervention has been less studied because few countries have implemented it.[2] Second, we can analyze alcohol-related crashes directly. Thus, we do not have to rely on proxy measures such as single-vehicle nighttime accidents as many articles do. Third, we analyze various outcome variables such as accidents, injuries by degree of severity, and fatalities. We also have access to blood tests from the period examined that allows us to assess the effects of the law on drinking and driving. Lastly, this is one of the first papers analyzing this type of intervention for a less developed country.

Two similar experiences to that of Chile have been studied. (Assum, 2010) analyzed the experience in Norway of reducing the legal BAC limit from 0.05 to 0.02 g/dL in 2001. However, he does not have access to data on alcohol-related crashes. Instead, he uses single-vehicle nighttime accidents, weekend personal-injury, and fatal crashes as proxy measures. He does not find any reduction in these proxies when comparing six years before with six years after the decrease of the legal limit. Ross and Klette (1995) studied the Swedish law that lowered the BAC legal limit from 0.05 to 0.02 g/dL in 1990. They perform an interrupted time series analysis finding a 15% reduction in overall traffic fatalities (they cannot distinguish if alcohol was involved in these accidents) but when accidents were split in daytime and nighttime incidents, the overall result is lead by daytime accidents. There were not any reductions in nighttime accidents, those most likely to be related to alcohol consumption. Lastly, Nörstrom and Laurell (1997) also analyze the Swedish case, finding that there was a 9.7% decrease in overall fatal crashes and an 11% decrease in single-vehicle crashes, i.e. those more likely to be related to alcohol. Our paper complements the limited international literature.

## 2. Institutional background

Traffic accidents are an important matter in Chile. They are currently the second leading cause of death of young people between 15 and 25 years old and first among children under 15 years old. For the period 2000–2012, nearly 1600 people died yearly in road traffic accidents, and more than 20% were related to driver or pedestrian alcohol use (INE, 2010).[3]

In this context, on January 31st, 2012 the Chilean Senate approved Law 20,580, which increased license revocation time for driving under the influence (DUI) and drunk driving, and lowered the legal driving BAC level. Permitted BAC was reduced from 0.05 to 0.03 g/dL, and driving under the influence (DUI) was set between 0.03 and 0.079 BAC (instead of 0.05–0.099 BAC range). The starting threshold for drunk driving was reduced from 0.1 to 0.08 BAC. The law was announced on the day it was approved but started being enforced on March 15, 2012.

Two important features of the penalty structure in Chile are the distinction between DUI and drunk driving, and that license suspension time depends on the type of injuries caused by the car crash. Thus, DUI drivers would have their license suspended for three months to five years depending on the injuries involved. Meanwhile drunk drivers (above a 0.08 BAC level) would have a license suspension for two years if no injuries were caused and permanent revocation if deaths or serious injuries were involved. In the case of recidivism, the second and third times would have suspension periods of four and five years for DUI offenders, and five years and lifetime cancelation for drunk drivers respectively.[4]

Lastly, two key background elements may affect the effectiveness of the law: enforcement and information. Regarding the first, even though Chile classifies as a high-income economy (World Bank, 2016), the enforcement of drinking and driving regulations is lower than that of developed countries, and it is similar to that of other Latin American countries (WHO, 2013).[5] Also, enforcement, as measured by police stops, did not change with the law (see Section 3.2 for a description of the data). Regarding the announcement of the law, an awareness campaign was deployed in late February 2012 to decrease traffic fatalities by educating the public about the fatal risks of drunk driving and the main legal changes (Table 1).

## 3. Data and descriptive statistics

### 3.1. Data

The primary data used in this paper are administrative records from the Chilean national police force, the *Carabineros de Chile*. We have data on all registered traffic accidents in the country with their causes (listed in Appendix A) by date including the number of injuries and deaths for the 2009–2014 period. This information comes from the Highway Traffic Agency of the Carabineros de Chile (OS2). The total sample includes 377,531 accidents, 26,979 of them involving DUI and drunk driving. The total number of drivers in all accidents is 604,801 and 40,813 in those related to alcohol.[6]

It is important to mention that the OS2 uses only one cause to classify an accident such as distracted driving, DUI, or drunk driving (see Appendix A). Certainly, a car crash may have more than one cause, but they are classified according to the "root cause," as determined by a police officer at the accident scene.[7] An implicit assumption in our analysis is that the law did not change the way police labeled the root causes of accidents. We discuss this in Section 5.1 and provide evidence against the hypothesis of a change in police officers reporting.

We merge these data with the number of cars stopped by police patrols and the number of alcohol-related driving offenses (DUI and drunk driving). This information was provided by the Criminal Analysis Office (CAO), also part of the Carabineros de Chile, and allows us to control for enforcement in our regressions. To control for variables affecting the number of rides in a given month, we include gasoline sales, obtained from the Transportation Ministry.

Lastly, we have access to all blood alcohol tests in the Metropolitan Region for the period 2009–2014 collected by the Chilean Legal Medical Service (SML).[8] We use this data to study the channels through which the law works. Although the data might include tests requested

[2] For example, Russia, Sweden, and Norway decreased from 0.05 to 0.02 g/dL, Poland, and Japan from 0.05 to 0.03 g/dL (Beirness and Simpson, 2002; Desapriya et al., 2007).

[3] The primary cause of death in road traffic accidents comes from pedestrian actions, such as pedestrians in driveways and careless crossing, among others, representing 23% of deaths from car accidents for the last decade. Accidents related to alcohol are the second leading cause (INE, 2010).

[4] In Chile, drivers must be 18 years or older to drive alone. There is a special permit for teens at 17 that allows them to drive when accompanied by an adult driver. Additionally, buying alcohol is legal for those 18 years old or older.

[5] WHO (2013) performed surveys in more than 100 countries about the effectiveness of enforcement of several aspects of road safety legislation, the enforcement of drink-driving laws being one of them. The responses are on a scale from 0 to 10 where 0 is "not effective," and 10 is "highly effective." Chile gets a score of 5.0. Latin American countries get a 4.8 and high income countries a 6.6.

[6] All drivers involved in a car accident are not necessarily tested for alcohol. If there are injuries or deaths all drivers are blood tested. If there are neither injuries nor deaths, the police officer may or may not perform a breath test. In these cases, if the breath test exceeds the 0.03 g/dL, a blood test is given as well.

[7] According to Rizzi and Fariña (2013), this may be due to the need to establish legal responsibilities of drivers involved in a car crash. They also argue that alcohol-related accidents may be underreported since those with drivers in legal BAC limits (between 0 and 0.03) are not classified as alcohol-related.

[8] The Metropolitan Region is one of the 15 regions of the country and the most populated one. While the total population is 17.8 million, the Metropolitan Region has 7.3 million, 41% of Chile's population (INE, 2014b). Regarding the comparability of the samples, the Metropolitan Region has a 19% higher per capita income than the national average (INE, 2012) but it is a representative sample of the country regarding motorized vehicles and car accidents. The share of motorized vehicles is 41%, and the proportion of accidents in respect to the total is 36%, which are similar to the share of its population (INE, 2014a). Blood alcohol content tests for other regions were not available for the period analyzed.

**Table 1**
Comparative law penalties.

| | BAC (g/dL) | | Injuries | Suspension | |
|---|---|---|---|---|---|
| | Pre | Post | | Pre | Post |
| DUI | 0.05–0.099 | 0.03–0.079 | None | 1 month | 3–6 months |
| DUI | 0.05–0.099 | 0.03–0.079 | Minor | 2–4 months | 9 months |
| DUI | 0.05–0.099 | 0.03–0.079 | Moderate | 4–8 months | 18–36 months |
| DUI | 0.05–0.099 | 0.03–0.079 | Severe or dead | 1–2 years | 3–5 years |
| Drunk | 0.1 + | 0.08 + | None | 6–12 months | 24 months |
| Drunk | 0.1 + | 0.08 + | Minor or Moderate | 1–2 years | 3–5 years |
| Drunk | 0.1 + | 0.08 + | Severe or dead | 2–4 years | Cancelation |

due to other crimes besides alcohol-related driving, the vast majority of criminal cases requiring blood alcohol tests are driving while intoxicated or under the influence of alcohol as defined in the law 18,290 (traffic law).[9]

### 3.2. Descriptive statistics

Traffic accidents in Chile have been increasing over the past decade as a consequence of a substantially increased number of cars.[10] Table 2 shows a summary of the number of total accidents, injuries, and deaths at the national level for the 2009–2014 period.

As can be observed, the total number of car accidents increased before the law from 56,011 in 2010 to 60,845 in 2011 but then slightly decreased to 59,465 in 2012. However, there was a considerable decrease in the number of alcohol-caused accidents from 5014 to 3613 when comparing one year before, 2011, with the year of enactment of the law, 2012. However the number rebounded in 2013, and figures seemed to return to their previous levels in 2014. Interestingly, the number of accidents not related to driver drinking increased monotonically in the entire period. The number of deaths due to alcohol showed a decreasing pattern with an important reduction right after the law from 204 to 147.

Table 2 also illustrates the evolution of injuries by their degree of severity: minor, moderate, and severe.[11] As can be observed, the evolution of injuries follows a similar pattern to that of accidents with an important reduction in 2012 and a rebound thereafter.

In Table 3 we show the number of police stops, column (1), and the number of drunk/DUI offenses, column (3), between 2009 and 2014. Columns (2) and (4) show the percentage change between years and column (5) shows the ratio of offenses over police stops. As can be observed, the total number of police stops increased between 2009 and 2013 with increases of 15.4% and 11.2% in 2011 and 2012 respectively. The number of offenses decreased by 6.9% between 2011 and 2012 but increased between 2012 and 2013. Lastly, the number of offenses relative to police stops decreased between 2011 and 2012 from about 5 offenses per 1000 stops to about 4 stabilizing in 3.7 offenses per 1000 stops in 2014.

Regarding blood alcohol samples, there were 244,484 tests given in the Metropolitan Region between 2009 and 2014. Of those tests, 44,580 had BAC scores greater than zero. As explained in Section 3.1, the data include all blood samples taken in the Metropolitan Region rather than from drivers in accidents. In Table 4 we present values for percentiles of the BAC distribution (g/dL) before and after the law. As can be seen, there is a BAC reduction after the law with a more pronounced effect for the 85th quantile. The level decreases by approximately 0.04 g/dL (from 0.09 to 0.05 d/dL), which corresponds to a reduction of 49%. However, the higher the quantile, the less pronounced the decline. For example, for the 90th percentile, there is a decrease from 0.15 to 0.12 g/dL (17 %), and for the 95th percentile the reduction is only from 0.24 to 0.23 g/dL (7%).

## 4. Methods

The before and after comparison suggests potential beneficial effects of the law on different outcomes. Now, we implement several empirical approaches to reduce the influence of confounding covariates and to analyze the time pattern of the effects. To assess the impact of the law on accidents, injuries, and fatalities, we first propose a regression discontinuity (RD) design using time as the forcing variable, exploiting the discontinuous switch regime due to the law's approval date. This approach gives us an immediate effect right before and right after the policy announcement. For the medium/long run effects we estimate a generalized Poisson regression and linear logit models and examine the time pattern of the effects. Lastly, to evaluate the channel through which the effects of the law manifest, we estimate the impact of the law on blood alcohol samples performing generalized linear models (GLM) and quantile regressions that allows us to measure the effects on different quantiles of the BAC distribution.

### 4.1. Regression discontinuity analysis

One plausible strategy to estimate the law's impact is a sharp regression discontinuity (RD) design using time as the running variable. This approach is the nonparametric version of an interrupted time-series analysis that focuses only on the immediate effect. The unexpected policy approval date permits us to identify a short-run effect, comparing measures right after and right before the intervention. The advantage of this approach is that, under minimal assumptions, we can estimate the causal effect of the law on the outcome variables of interest, controlling by time-varying unobservables (Burger et al., 2014).[12] The use of RD designs using time as a running variable is commonly seen in health and transportation policy evaluation. See for example Conover and Scrimgeour (2013), Davis (2008), and Gallego et al. (2013). In particular, Burger et al. (2014) evaluate the effect of California hand-held cell phone ban on accidents using this approach. The model proposed is the following

$$y_t = \alpha + \beta \text{Police}_{t-1} + \gamma \text{Gas}_t + \delta \text{Post}_t + g(t) + \epsilon_t \quad (1)$$

where $y_t$ is the number of accidents at time $t$, $Police_{t-1}$ is the number of police stops in time $t-1$ (to avoid simultaneity issues), and $Gas_t$ is gasoline sales. The dummy variable $Post_t$ is equal to one if the period $t$ is after the law's approval and equal to zero otherwise. Lastly, $g(t)$ is a smooth function of $t$ which is not necessarily a linear trend. It can be

[9] In other crimes, such as domestic violence, blood alcohol tests may be given, but this does not affect general trends in BAC testing. Notwithstanding, there are a few crimes for which BAC tests are important outside of drunk driving. Therefore, most but not all of the sample should be applicable to our question. The estimates would be invalid if the omitted variables differently affect other requests for BAC tests before and after the law's enactment period. We believe this is unlikely to occur.

[10] The number of cars have almost doubled, increasing from 2,164,540 motorized vehicles in 2002 to 3,884,581 in 2012.

[11] Chilean law defines a severe injury as when, as a consequence of an accident, the individual cannot work for at least 30 days after the crash. A moderate injury takes place when there are no disabilities, and the person is unable to work for more than five but less than 30 days. Minor injuries are all others.

[12] The assumptions are that there is a discontinuous assignment to treatment probability in a variable known as the running or forcing variable. In our case, the running variable is time, and the treatment status is the post-law period. It is required that the forcing variable (time) is not manipulated by agents to sort into treatment status and that drivers do not change their behavior before the law was approved (January 31st, 2012). In our opinion it is unlikely that drivers started complying with the law before it was approved in the Senate (January 31st, 2012) since it was not announced before the approval date. The last assumption is a continuity of the conditional expectations of the outcome on the forcing variable.

**Table 2**
All accidents in Chile 2009–2014.

| | Accidents | Drivers | Injuries | | | | Deaths |
|---|---|---|---|---|---|---|---|
| | | | Total | Moderate | Minor | Severe | |
| *Panel A: 2009* | | | | | | | |
| All alcohol-caused | 4031 | 6132 | 5061 | 866 | 541 | 3654 | 215 |
| Non-alcohol-caused | 50,664 | 80,813 | 45,879 | 5659 | 3707 | 36,513 | 1198 |
| Total | 54,695 | 86,945 | 50,940 | 6525 | 4248 | 40,167 | 1413 |
| *Panel B: 2010* | | | | | | | |
| All alcohol-caused | 4524 | 6844 | 5370 | 875 | 544 | 3951 | 200 |
| Non-alcohol-caused | 51,487 | 82,328 | 46,329 | 5788 | 3672 | 36,869 | 1294 |
| Total | 56,011 | 89,172 | 51,699 | 6663 | 4216 | 40,820 | 1494 |
| *Panel C: 2011* | | | | | | | |
| All alcohol-caused | 5014 | 7602 | 5223 | 840 | 559 | 3824 | 204 |
| Non-alcohol-caused | 55,831 | 89,029 | 47,638 | 5665 | 3777 | 38,196 | 1261 |
| Total | 60,845 | 96,631 | 52,861 | 6505 | 4336 | 42,020 | 1465 |
| *Panel D: 2012* | | | | | | | |
| All alcohol-caused | 3613 | 5340 | 3767 | 630 | 364 | 2773 | 147 |
| Non-alcohol-caused | 55,852 | 89,043 | 47,898 | 5673 | 3408 | 38,817 | 1291 |
| Total | 59,465 | 94,383 | 51,665 | 6303 | 3772 | 41,590 | 1438 |
| *Panel E: 2013* | | | | | | | |
| All alcohol-caused | 4643 | 6982 | 4651 | 738 | 470 | 3443 | 148 |
| Non-alcohol-caused | 66,509 | 108,210 | 53,420 | 6452 | 3824 | 43,144 | 1447 |
| Total | 71,152 | 115,192 | 58,071 | 7190 | 4294 | 46,587 | 1595 |
| *Panel F: 2014* | | | | | | | |
| All alcohol-caused | 5154 | 7913 | 4722 | 788 | 401 | 3533 | 142 |
| Non-alcohol-caused | 70,209 | 114,565 | 51,411 | 6376 | 3460 | 41,575 | 1472 |
| Total | 75,363 | 122,478 | 56,133 | 7164 | 3861 | 45,108 | 1614 |

*Note*: All alcohol-caused refers to accidents, injuries, and deaths caused by drivers classified as DUI or drunk driving. Panels A–C show information from pre-law periods.

**Table 3**
Police stops and drunk/DUI offenses.

| Year | Police stops (1) | $\Delta_t$% (2) | Drunk/DUI (3) | $\Delta_t$% (4) | $\frac{\text{Drunk/DUI}}{\text{Police stops}}$ (5) |
|---|---|---|---|---|---|
| 2009 | 5,703,811 | – | 29,376 | – | 0.0052 |
| 2010 | 6,027,135 | 5.7% | 31,144 | 6.0% | 0.0052 |
| 2011 | 6,954,283 | 15.4% | 34,289 | 10.1% | 0.0049 |
| 2012 | 7,736,123 | 11.2% | 31,938 | −6.9% | 0.0041 |
| 2013 | 8,823,367 | 14.1% | 32,845 | 2.8% | 0.0037 |
| 2014 | 8,493,560 | −3.7% | 31,232 | −4.9% | 0.0037 |

*Note*: Columns (2) and (4) show the percentage change between years and column (5) the ratio of drunk/DUI offenses over the number of police stops.

**Table 4**
Percentiles for BAC (g/dL) before and after the law.

| Quantile | Before (1) | After (2) | Δ% (3) |
|---|---|---|---|
| 80th | 0.00 | 0.00 | – |
| 85th | 0.09 | 0.05 | −49 |
| 90th | 0.15 | 0.12 | −17 |
| 95th | 0.19 | 0.18 | −7 |
| 99th | 0.24 | 0.23 | −3 |
| N | 126,500 | 120,437 | – |

*Note*: Percentiles for BAC (g/dL) from blood samples taken in the Metropolitan Region from January 2009 to December 2014. The average number of observations per percentile is 1265 and 1204 before and after the law respectively. The percentage change is defined as (Post-Pre/Pre).

estimated by polynomial or nonparametric methods such as local linear regressions. See Imbens and Lemieux (2008) and Lee and Lemieux (2010) for more details on RD analysis.

Using weekly data on alcohol-caused car crashes, injuries, and deaths, we estimate local linear regressions (LLR) with a triangular kernel at both sides of the discontinuity induced by the announcement of the law. We use weekly data since a higher frequency improves the consistency of LLR estimates and helps to control for time-varying unobservables (Burger et al., 2014). We implement the robust optimal bandwidth of Calonico et al. (2014), but results are robust to other bandwidth choices such as the optimal bandwidth proposed by Imbens and Kalyanaraman (2012), and the leave-one-out cross validation bandwidth used by Ludwig and Miller (2007).[13]

The interpretation of the results following these approaches is the short-run effect (if any) on drivers by the law. Since the medium and long-run effects are the most relevant parameters for policy, we implement generalized Poisson regression and linear logit models that allows us to assess the time pattern of the effects.

### 4.2. Generalized Poisson regression and linear logit

As discussed by Fridstrøm (2015), accident counts follows a (generalized) Poisson distribution. We estimate the following gamma-Poisson model, also known as negative binomial

$$E[y_{i,t}|x_{i,t}, \text{Post}_t] = \omega_{i,t} = \exp[\alpha_i + \beta\text{Police}_{i,t-1} + \gamma\text{Gas}_{i,t} + \delta\text{Post}_t + \eta\text{trend}_t] \quad (2)$$

where $x_{i,t}$ is a row vector that includes all the covariates but $Post_t$, the index $i$ refers to the geographical region (there are 15 regions) and $t$, the time period from January 2009 to December 2015, i.e. 72 months. Thus, $\alpha_i$ is a geographical region fixed effect and $trend_t$ is a linear trend. We exploit the panel nature of the data (time and region) since it allows us to control for region and month fixed effects. In this model $Var(y_{i,t}|x_{i,t}) = \omega_{i,t}(1 + \theta\omega_{i,t})$ and $\theta > 0$ is the overdispersion parameter.

The policy relevant parameter in this model is $\delta$. Since $Post_t$ is a binary variable, the effects of the law on accidents (injuries or deaths)

[13] The bandwidth is a parameter that defines the range of observations around the cutoff used in the estimation. See Lee and Lemieux (2010) and Calonico et al. (2014) for details.

are simply $E[y_{i,t}|x_{i,t}, Post_t = 1] - E[y_{i,t}|x_{i,t}, Post_t = 0] = \exp(x_{i,t}\Lambda)(e^{\delta} - 1)$, where $\Lambda = (\alpha_i, \beta, \gamma, \eta)'$. Since $E[y_{i,t}|x_{i,t}, Post_t = 0] = \exp(x_{it}\Lambda)$, we have that $(e^{\delta} - 1)$ can be interpreted as the percentage change in the expected count variable from a change in $Post_t$ from 0 (before the law) to 1 (after the law).

To assess the effect's dynamics we add the interaction term $\lambda Post_t \times trend_t$ to Eq. (2). This makes the model more flexible allowing the law's effects to increase or decrease after approval. We centered the trend such that $trend_t = t - t_0$ where $t_0$ is when the law was approved. Hence, the percentage change of accidents $t'$ months after the law with respect to the approval date ($t_0$) is equal to $(e^{\delta+\lambda t'} - 1)$.[14]

A second approach to assess the law's effects is a linear logit analysis to the share of accidents where alcohol is involved. The advantage of this approach is that it allows us to control for non-alcohol related accidents by construction. Thus, we propose the following regression

$$\ln\left(\frac{s_{i,t}}{1 - s_{i,t}}\right) = \alpha_i + \beta \mathrm{Police}_{i,t-1} + \gamma \mathrm{Gas}_{i,t} + \delta \mathrm{Post}_t + \eta \mathrm{trend}_t + \lambda \mathrm{Post}_t \times \mathrm{trend}_t + \phi \mathrm{DUI}_{i,t-1} + \epsilon_{i,t} \quad (3)$$

where $s_{i,t}$ is the share of alcohol-related accidents (injuries or deaths) over the total number of accidents in region $i$ and period $t$. We include as covariates the number of police stops, gasoline sales, the month fixed effects and the interaction term $Post_t \times trend_t$. In addition to police stops, we also include the incidence of DUI/drunk drivers apprehended by police officers the previous month as an alternative measure of deterrence.

The interpretation of this model's parameters is different from that of Poisson regressions. Here $\delta$ is the effect of the law on the log ratio of accidents due to alcohol to those due to other causes (odd-ratio). Put differently, $(e^{\delta} - 1)$ is the percentage change of the odd-ratio from a change in $Post_t$ from 0 (before the law) to 1 (after the law).

### 4.3. Generalized linear models and quantile regressions

We also measure the law's effect on alcohol blood samples to test if alcohol is one plausible channel through which the effects of the law manifest. We estimate linear models and generalized linear models such as

$$E[\mathrm{BAC}_{i,t}|x_{i,t}] = g^{-1}(\alpha + \beta \mathrm{Post}_t + \gamma \mathrm{Male}_i + \delta \mathrm{Post}_t \times \mathrm{Male}_i + \mu_t) \quad (4)$$

where $g(\cdot)$ is the *link* function, $Post_t$ is a dummy variable equal to 1 after the reform and 0 otherwise, $Male_i$ is a dummy equal to 1 if the blood sample $i$ in period $t$ is from a male driver and 0 otherwise, and $\mu_t$ is a month-year fixed effect. We consider the interaction between the $Post_t$ and the $Male_i$ dummy to allow for a heterogeneous response as found by Albalate (2008) for Europe.[15]

We estimate two versions of Eq. (4). First, we use simple linear models, which are equivalent to an identity link function and Gaussian residuals. Then, we estimate a specification with a log link function and Gaussian residuals to address the non-negative nature of the BAC data.

Finally, we estimate censored quantile regressions to assess if there are different responses at different quantiles of the BAC distribution (Koenker and Bassett, 1978; Powell, 1986). Thus, we model the $\tau$th quantile of the BAC distribution as follows:

$$Q_{\mathrm{BAC}_{i,t}}(\tau|x_{it}) = \alpha(\tau) + \beta(\tau)\mathrm{Post}_t + \gamma(\tau)\mathrm{Male}_i + \delta(\tau)\mathrm{Post}_t \times \mathrm{Male}_i + \mu_t \quad (5)$$

We allow for left-censoring at 0 to address the nonnegative nature of the BAC data.

## 5. Results

### 5.1. Regression discontinuity

For graphical purposes, we implement the optimal data-driven regression discontinuity plots proposed by Calonico et al. (2015) which use parametric models for $g(t)$, but when estimating we use semi/nonparametric methods (LLR).

Fig. 1 shows RD plots for car accidents and deaths using a cubic polynomial specification for $g(t)$. Figures on the left side refer to alcohol-related outcomes while the ones on the right represent variables not related to alcohol serving as placebo checks. As can be seen, there is a significant decrease in the weekly number of alcohol-caused accidents. In contrast, we do not find significant immediate effect on deaths. Lastly, non-alcohol-related car crashes show a small but significant reduction of accidents by 6% (significant at the 5% level) but there is not any effects on fatalities (at the 10% level). This reduction may suggest a small spillover effect of the law on accidents due to other causes but we cannot rule out other reasons such as a general increase in traffic law enforcement, smaller traffic volumes, or any other relevant factor. However, this allows us to rule out the hypothesis of changes based solely on police recording different accident causes.[16]

Fig. 2 shows RD plots for the total number of alcohol-related injuries and injuries by degree of severity: minor, moderate, and severe. As can be observed, there is a statistically significant reduction (at the 1% level) in the total number of alcohol-related injuries. By degree of severity, we see a statistically significant drops in all three – minor, moderate, and severe injuries – at the 1% level.

Table 5 shows the RD point estimates using local linear regressions with the optimal bandwidth parameter proposed by Calonico et al. (2014). It also includes the standard errors (s.e.), the bandwidth, and the pre-law levels. The latter were computed as the estimated conditional expectation at approval date (from LLR) using a triangular kernel and the optimal bandwidth with pre-law data. As can be seen, the results are statistically significant (at the 1% level) for alcohol-related accidents with a reduction of 26 accidents per week right after the law's approval, which is about 32%. For injuries from alcohol-related crashes, we find a similar result to that of car accidents with about 38 fewer injured per week after the law (36%). The effects are significant at the 1% level for all degrees of severity. However, there is not any significant effect of the law on deaths (at the 10% level).

In Appendix B we provide a robustness check and tests of the validity of the RD assumptions. In particular, we estimate the effect of the law on Emergency Room (ER) admissions in public and private hospitals due to car accidents in the 2010–2014 period. This data is an independent source of information and not affected by any potential change in police officers reporting. We find a statistically significant reduction of ER admissions (at the 5% level). We also show that neither gasoline sales nor police stops suffered from any discontinuities when the law was approved and, hence the estimated effects can be attributed to the law.

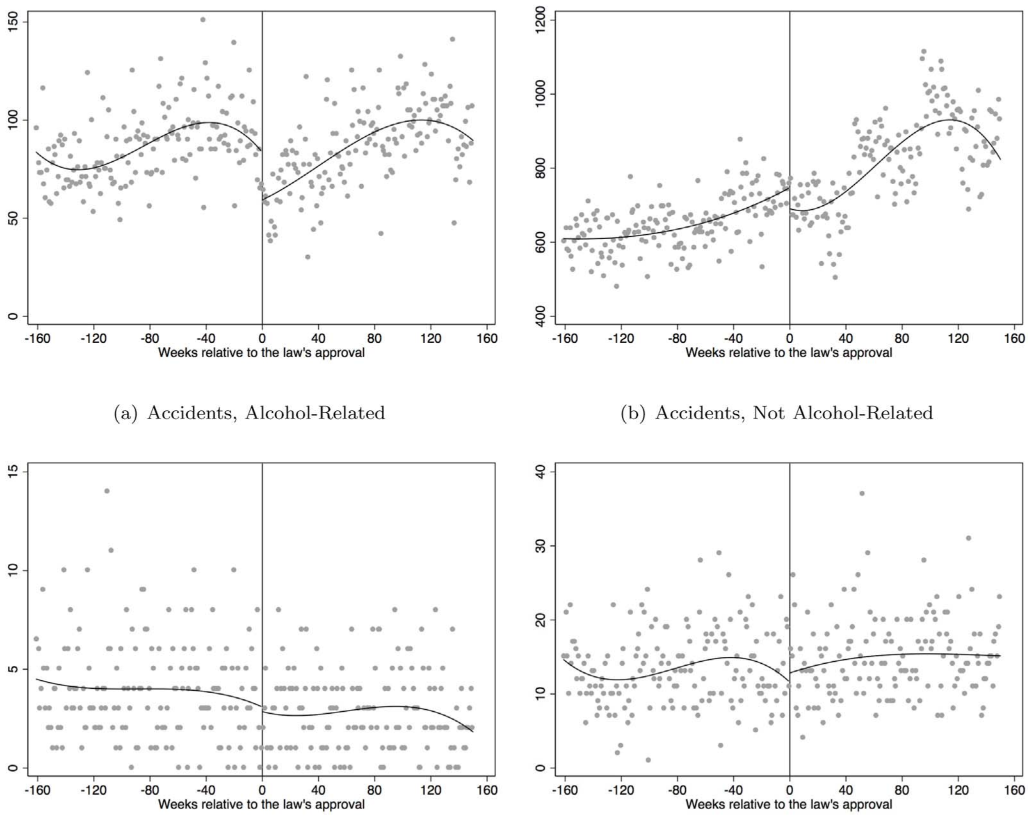
### 5.2. Generalized Poisson regression

To implement generalized Poisson and linear logit regressions we

[14] It follows from $E[y_{i,t}|x_{i,t}, \mathrm{Post}_t = 1, t = t_0 + t'] - E[y_{i,t}|x_{i,t}, \mathrm{Post}_t = 0, t = t_0] = \exp(x_{i,t}\Lambda)(e^{\delta+\lambda t'} - 1)$.

[15] Albalate (2008) shows that lowering the legal BAC level from 0.08 to 0.05 g/dL significantly decreased accidental traffic deaths only for men between 18 and 49 years old in 15 European countries. After controlling for concurrent interventions such as random alcohol tests and the minimum legal drinking age, the impact for the overall population was negligible. Also, Abdel-Aty and Abdelwahab (2000) find that men are more likely to be involved in alcohol/drugs related crashes.

[16] If there was a change in the way officers register the accidents' causes after the law, such as recording fewer accidents due to alcohol and more due to other causes, we should observe an increase in accidents not related to alcohol after the law and we observed the opposite.



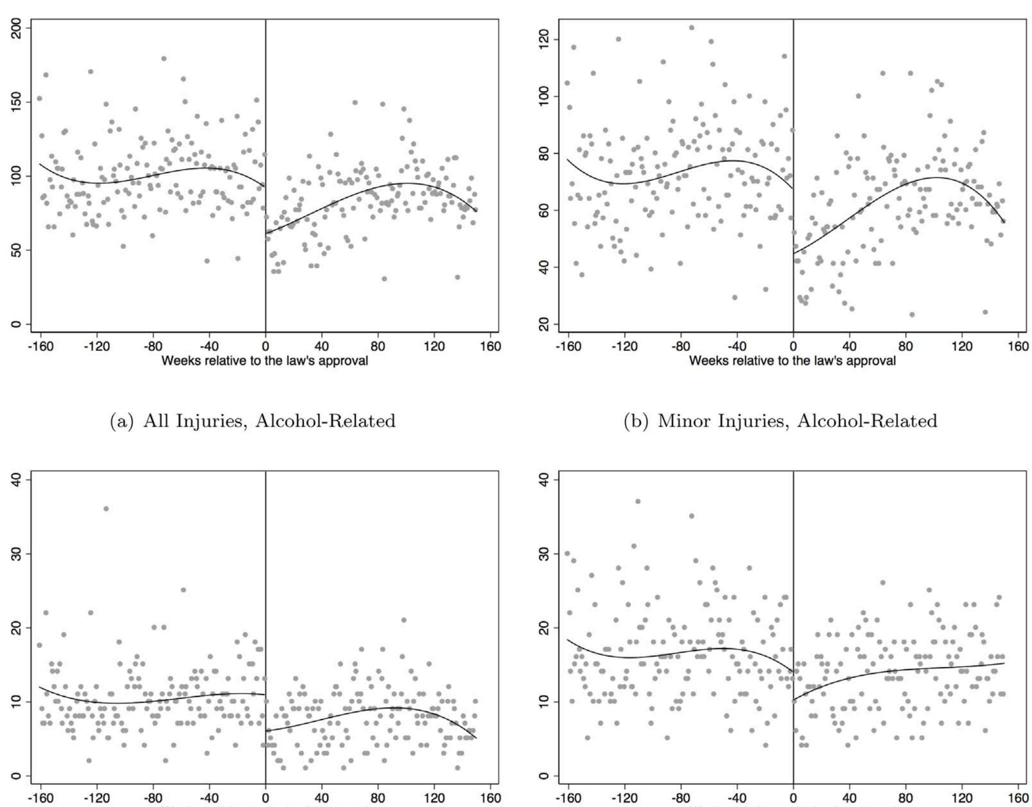

(a) Accidents, Alcohol-Related

(b) Accidents, Not Alcohol-Related

(c) Deaths, Alcohol-Related

(d) Deaths, Not Alcohol-Related

**Fig. 1.** Regression discontinuity plots for road accidents, with and without alcohol, before and after lowering the legal blood-alcohol limit. *Notes*: These figures show regression discontinuity plots for weekly data using a cubic model and the law's approval as the cutoff date. Each dot represents the number of car accidents in a given week.




(a) All Injuries, Alcohol-Related

(b) Minor Injuries, Alcohol-Related

(c) Moderate Injuries, Alcohol-Related

(d) Severe Injuries, Alcohol-Related

**Fig. 2.** Regression discontinuity plots for Injuries by degree of severity before and after lowering the legal blood-alcohol limit. *Notes*: These figures show regression discontinuity plots for weekly data using a cubic model with the law's approval as the cutoff date. Each dot represents the number injured due to alcohol-related accidents in a given week.

**Table 5**
Regression discontinuity estimates.

| | Accidents | Injuries | | | | Deaths |
|---|---|---|---|---|---|---|
| | | All | Minor | Moderate | Severe | |
| RD estimate | −26.06*** | −37.92*** | −25.78*** | −5.41*** | −7.08*** | −0.47 |
| Standard error | 5.55 | 10.50 | 7.83 | 1.77 | 2.26 | 1.21 |
| Bandwidth | 38.27 | 40.95 | 40.01 | 47.62 | 40.13 | 39.14 |
| Pre-law level | 82.23 | 106.77 | 78.48 | 12.03 | 16.46 | 3.00 |

*Note*: RD estimates for accidents, injuries, and deaths due to DUI and drunk driving using the bandwidth proposed by Calonico et al. (2014). Pre-law levels computed as the estimated conditional expectation at approval date (from LLR) using a triangular kernel, the optimal bandwidth, and pre-law data.
*Significance level $p < 0.1$.
**Significance level $p < 0.05$.
*** Significance level $p < 0.01$.

**Table 6**
Generalized Poisson regressions: alcohol-related accidents, injuries and deaths 2009–2014.

| | Accidents | Injuries | | | | Deaths |
|---|---|---|---|---|---|---|
| | | All | Minor | Moderate | Severe | |
| Post | −0.3894*** | −0.3649*** | −0.3852*** | −0.3525*** | −0.2819*** | −0.2333 |
| | (0.0454) | (0.0614) | (0.0618) | (0.1061) | (0.0826) | (0.1599) |
| Trend | 0.0074*** | 0.0016 | 0.0017 | 0.0030 | −0.0013 | −0.0046 |
| | (0.0014) | (0.0018) | (0.0019) | (0.0030) | (0.0024) | (0.0047) |
| Post × trend | 0.0064*** | 0.0073*** | 0.0087*** | 0.0015 | 0.0098*** | 0.0047 |
| | (0.0022) | (0.0028) | (0.0029) | (0.0049) | (0.0037) | (0.0075) |
| Gas sales | 0.0100*** | 0.0077** | 0.0081** | 0.0052 | 0.0102*** | 0.0132** |
| | (0.0031) | (0.0033) | (0.0038) | (0.0060) | (0.0039) | (0.0059) |
| Police stops | −0.0011* | −0.0020** | −0.0015 | −0.0057*** | −0.0028** | −0.0013 |
| | (0.0007) | (0.0008) | (0.0010) | (0.0017) | (0.0013) | (0.0024) |
| Constant | 1.7167*** | 1.9575*** | 1.5821*** | −0.0359 | 0.2217 | −1.8334*** |
| | (0.1094) | (0.1425) | (0.1581) | (0.2367) | (0.1910) | (0.3867) |
| $\theta$ | 0.0641*** | 0.1547*** | 0.1566*** | 0.1727*** | 0.0957*** | 0.2306*** |
| | (0.0079) | (0.0147) | (0.0153) | (0.0280) | (0.0164) | (0.0610) |
| $N$ | 1065 | 1065 | 1065 | 1065 | 1065 | 1065 |
| Pseudo $R^2$ | 0.2075 | 0.1501 | 0.1541 | 0.1386 | 0.1862 | 0.1637 |

*Note*: Robust standard errors in parenthesis. All models include monthly fixed effects and region fixed effects. $\theta$ corresponds to the overdispersion parameter.
* Significance level $p < 0.1$.
** Significance level $p < 0.05$.
*** Significance level $p < 0.01$.

construct a monthly panel data of the 15 geographical regions of the country and 72 months (from January 2009 to December 2014) implying a sample size of 1065 (15 × 71, due to the lagged value of police stops). We work with monthly data to avoid excess zeros in some regions when higher frequency data (e.g. weekly) is used.

In Table 6 we present the estimation results for the parameters of the generalized Poisson regression in Eq. (2). There is a negative and statistical significant effect (pvalue < 0.001) for the post-law dummy alcohol-related accidents, and injuries from those accidents (−0.39 and −0.36 respectively). In terms of percentage reduction it is equivalent to 32% and 31% respectively ($e^{\delta} - 1$). The effect is observed in minor, moderate, and severe accidents. We find a negative but statistically insignificant ($p$-value > 0.1) effect on deaths. The overdispersion parameter $\theta$ is positive and statistically significant for all outcomes ($p$-value < 0.01). Lastly, since there are about 50% of observations (region-month) without any deaths from alcohol-related accidents, in Appendix B we estimate a zero-inflated negative binomial regression, finding similar results as those in Table 6.

As discussed in Section 4.2, the interaction of $post_t$ and $trend_t$ allows us to assess if there is a rebound after the initial decrease in accidents. The results in Table 6 show that the initial effect decreases each month by 0.0064 in absolute value. Hence, while after two years the effect on the hazard rate of accidents goes from −0.39 to −0.24 (s.e. of 0.06), in three years it is −0.16 (s.e. of 0.08). The effects after two and three years are statistically significant at the 1% and 10% level, respectively. In percentage terms, the effects are −21% and −15%, after two an three years are respectively. For injuries the effect is −0.19 (s.e. 0.08) after two years and −0.10 (s.e. 0.10) after three years. The effect after two years remains statistically significant at the 1% level but looses significance after three years. In percentage terms the effects are −17% and −11% respectively.[17]

Eq. (2) assumes that the magnitude of the changes are constant, i.e. each month the effect on accidents decreases by 0.0064 in absolute value. In Appendix B we provide a more flexible modeling for the time pattern finding similar results.

### 5.3. Linear logit regressions

Table 7 shows the results of the linear logit model described in Eq. (3) for three outcomes: alcohol-related accidents, injuries, and deaths. As can be observed, there are statistically significant reductions in alcohol-related accidents and injuries after the law (at 1% level). For deaths there is a negative effect, which is statistically insignificant at the 10% level.[18] The incidence of the number of DUI/drunk drivers apprehended in the previous month is negative and significant at the

[17] The percentage change of the count variable $t'$ months after the law with respect to the law's approval date ($t_0$) is ($e^{\delta+\lambda t'} - 1$) as mentioned in Section 4.2. The standard errors are computed by the delta method.

[18] The lack of statistical power for deaths may be affected by a reduced number of observations (469) due to cases where there are no fatalities for a given region in a given month, making the log of the shares ratio undefined.

**Table 7**
Linear logit models.

| | Accidents | Injuries | Deaths |
|---|---|---|---|
| Post | −0.2657*** | −0.3189*** | −0.1198 |
| | (0.0715) | (0.0821) | (0.1475) |
| Trend | 0.0011 | −0.0004 | −0.0023 |
| | (0.0023) | (0.0026) | (0.0049) |
| Post × trend | −0.0020 | 0.0045 | −0.0050 |
| | (0.0035) | (0.0039) | (0.0068) |
| DUI/drunk | −0.0006* | −0.0007* | −0.0003 |
| | (0.0003) | (0.0004) | (0.0008) |
| Gas sales | 0.0080** | 0.0034 | 0.0125* |
| | (0.0036) | (0.0039) | (0.0071) |
| Police stops | −0.0022** | −0.0035** | 0.0038 |
| | (0.0010) | (0.0012) | (0.0024) |
| Constant | −2.7973*** | −2.7005*** | −0.9155** |
| | (0.1327) | (0.1366) | (0.3314) |
| $N$ | 1050 | 1041 | 469 |
| $R^2$ | 0.4287 | 0.3026 | 0.3305 |

*Note*: Robust standard errors in parenthesis. All models include month and region fixed effects.
* Significance level $p < 0.1$.
** Significance level $p < 0.05$.
*** Significance level $p < 0.01$.

10% level for accidents and injuries. These results are consistent with those found in generalized Poisson regressions.

### 5.4. Blood alcohol content

The literature has discussed whether drinking and driving laws work via reducing overall drunken driving or drunk drivers' alcohol consumption. Although the evidence is mixed, there is some research finding that these laws reduce alcohol consumption (Carpenter, 2004; Carpenter and Harris, 2005). We provide evidence along these lines. We estimate linear models for BAC including a post dummy, a dummy for males and its interaction with the post dummy variable, month-of-the-year fixed effects, and a linear trend for every specification. Generalized linear models (GLM) with Gaussian family and log link function are also estimated to address the censored nature of the dependent variable.[19]

Table 8 presents the results. As can be observed in column (1), there is a significant reduction in BAC after the announcement of the law to the overall population. Column (2) shows that this effect is entirely driven by men with a significant decrease in alcohol consumption (interaction of the post and the male dummy variable). Marginal effects of GLMs are very close to those for linear models (columns (3) and (4)). Thus, the results are robust to different specifications. These findings agree with descriptive statistics from Table 4 and support previous gender differential effects found by Carpenter and Harris (2005) for the United States.[20]

Thus far, the mean reduction in BAC does not tell us what type of drinkers (mild or heavy) is reducing their alcohol consumption. Thus, we perform a conditional quantile analysis by estimating quantile regressions to confirm the findings showed in Table 4. We implement censored quantile regressions to measure the effect of the law on the 85th, 90th, 95th and 99th quantiles of the BAC distribution instead of its mean, as in Table 8. Due to the significant fraction of zeroes in the sample (about 80%), we cannot estimate the effects on lower quantiles. The covariates included are the same as those in the OLS and GLM regressions: a post dummy, a male dummy, an interaction between the two of them (male × post), month fixed effects and a linear trend. Table 9 shows the results for the interaction of the male dummy

[19] We estimate TOBIT models, finding very similar results for marginal effects of censored outcomes.
[20] There is also evidence of differential effects of the minimum legal drinking age (MLDA) on later alcohol use in life by gender. See Kaestner and Yarnoff (2011).

**Table 8**
Impact of the law on BAC.

| | OLS | | GLM | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Post | −0.0074*** | −0.0004 | −0.0087*** | 0.0021* |
| | (0.0005) | (0.0006) | (0.0006) | (0.0011) |
| Male | 0.0192*** | 0.0235*** | 0.0274*** | 0.0326*** |
| | (0.0007) | (0.0004) | (0.0005) | (0.0008) |
| Post × Male | | −0.0084*** | | −0.0109*** |
| | | (0.00052) | | (0.0010) |
| $N$ | 239,047 | 239,047 | 239,047 | 239,047 |
| $R^2$ | 0.016 | 0.016 | – | – |

*Note*: Robust standard errors in parenthesis. All models includes month fixed effects and a linear trend. Marginal effects reported. GLM uses a log link function and a Gaussian family.
* Significance level $p < 0.1$.
**Significance level $p < 0.05$.
*** Significance level $p < 0.01$.

**Table 9**
Impact of the law on BAC, quantile regressions.

| | Quantile | | | |
|---|---|---|---|---|
| | 85th (1) | 90th (2) | 95th (3) | 99th (4) |
| Post × Male | −0.036** | −0.040** | −0.015 | −0.013 |
| 95% C.I. lower bound | −0.04 | −0.052 | −0.042 | −0.022 |
| 95% C.I. upper bound | −0.032 | −0.017 | 0.021 | 0.009 |
| $N$ | 239,047 | 239,047 | 239,047 | 239,047 |

*Note*: All models include post, male and post × male dummies plus month fixed effects and a linear trend.
*Significance level $p < 0.1$.
** Significance level $p < 0.05$.
***Significance level $p < 0.01$.

variable with the post-law dummy variable. We present the point estimate and a 95% confidence interval using the STATA library CQIV (Chernozhukov et al., 2012). The effects on BAC are significant for the 85th and the 90th quantile but nonsignificant for the 95th and 99th ones. These results agree with those from Table 4; there are important reductions for blood samples between BAC 0.09 and 0.15 g/dL when comparing before and after the law for male drivers.

## 6. Discussion and conclusions

The significant decrease in alcohol-related accidents found in this paper was accompanied by a significant decline in all injuries. We found that the magnitude of the effects decreases with time, which has been already reported by other authors (Ross, 1973; Vingilis et al., 1988). This has been linked to relatively low levels of enforcement of a law after it has been introduced or changed.

Our results also show that there were not any statistically significant effects for traffic fatalities after enactment. The lack of statistical power may be due to a small number of fatalities per region/month and the null effect on deaths have to be taken with caution. Another hypothesis that might reconcile these findings is that drivers who were more likely to be affected by the law were also more likely to be involved in non-fatal crashes. According to the *Carabineros de Chile* data, between 2012 and 2014, 76% of traffic fatalities in alcohol-related crashes were due to drunk driving (i.e., 0.08 BAC and above) and the rest to DUI (0.03–0.079 BAC). The analysis of blood alcohol samples in the Metropolitan Area shows a significant reduction of BAC after the law particularly for men, similar to the findings of Albalate (2008) in Europe. Interestingly, the quantile regressions results show that the reductions occurred in the 85th and 90th quantiles and not in the upper quantiles. This result suggests that drivers who drink heavily (above the 90th quantile) are less likely to decrease their alcohol intake compared to drivers who drink less.

The hypothesis of heterogeneous effects of drinking and driving laws has been discussed in Grant (2010, 2016). According to Grant (2010) this type of laws “encourage mild drinkers to reduce their drinking, without providing further incentives for heavy drinkers to reduce their drinking.” Certainly if along with lowered legal BAC, the law incorporates an increase in penalties for offenders, the incentives could be modified. The Chilean experience shows success in reducing blood alcohol content for a large subpopulation of drivers who drink. However, those in the upper quantiles (95th and up) of the alcohol distribution seemed not to be affected. While hypotheses such as measurement error bias cannot be ruled out, the lack of effects on higher quantiles may be due to a weak incentive scheme for heavy drinkers but also to a lack of enforcement of the new law. As discussed in Section 2, license revocation periods did increase in respect to that of previous legislation. However, the enforcement of the new regulation, as measured by police stops, did not change from its pre-law trend. The interaction of incentives and enforcement is an interesting channel to understand how this type of laws work, and further research is needed to shed more light on this matter.

In conclusion, this paper provides new evidence on the effectiveness of drinking and driving laws and how they work using a comprehensive set of Chilean administrative records that allow us to identify its effects on car accidents, injuries, and deaths due to drivers’ alcohol consumption. We find that the law was effective in reducing alcohol-caused car accidents and injuries. We also find that these laws led to reduced blood alcohol content abet inconsistently. Our results suggest that the law works by inducing mild and moderate drinkers to reduce alcohol consumption, but has a smaller effect on those in the upper quantiles of the BAC distribution.

## Appendix A. List of accidents causes

**Table A.1**
Accidents causes.

| Accident cause | *N* | Percent |
|---|---|---|
| Alcohol, DUI | | |
| Driving under the influence of alcohol | 4357 | 1.15 |
| Alcohol, drunk | | |
| Driving while intoxicated | 22,622 | 5.99 |
| Alcohol, passenger | | |
| Drunk passenger | 102 | 0.03 |
| Alcohol, pedestrian | | |
| Drunk pedestrian | 2384 | 0.63 |
| Reverse driving | | |
| Reverse driving | 8596 | 2.28 |
| Deficient driving | | |
| Driving under poor physical conditions (fatigue, sleep, etc) | 3091 | 0.82 |
| Disobedience | | |
| Disobey policeman indication | 116 | 0.03 |
| Disobey traffic light flashing | 402 | 0.11 |
| Disobey traffic red light | 11,406 | 3,02 |
| Disobey other signage | 386 | 0.10 |
| Disobey yield sign | 10,631 | 2.82 |
| Disobey stop sign | 15,499 | 4.10 |
| Distracted driving | | |
| Inattentive Driving to the transit conditions | 86,720 | 22.97 |
| Imprudence, driver | | |
| Change of traffic lane unexpectedly | 13,359 | 3.54 |
| Driving against the traffic direction | 2085 | 0.55 |
| Driving on the left axis of the road | 722 | 0.19 |
| Driving without reasonable or prudent distance | 32,294 | 8.55 |
| No respect pedestrian passage | 6.958 | 1.84 |
| No respect vehicle passage | 10.241 | 2.71 |
| Improper turning | 6926 | 1.83 |
| Imprudence, pedestrian | | |
| Recklessness pedestrian | 3590 | 0.95 |
| Pedestrian crosses pedestrians step out | 2162 | 0.57 |
| Pedestrian crossing road or highway with no precautions | 1168 | 0.31 |
| Pedestrian careless crossing | 9865 | 2.61 |
| Pedestrian remains in the driveway | 1112 | 0.29 |
| Imprudence, passenger | | |
| Passenger entering while the vehicle is moving | 1272 | 0.34 |
| Recklessness passenger | 622 | 0.16 |
| Passenger traveling in the vehicle's sill | 266 | 0.07 |
| Load | | |
| Load slips on driveway | 400 | 0.11 |
| Greater than the permitted load to vehicle | 62 | 0.02 |
| Load obstructs driver vision | 45 | 0.01 |
| Load vehicle structure protrudes | 262 | 0.07 |
| Lose control | | |
| Lose control of the vehicle | 27,833 | 7.37 |
| Other causes | | |

(continued on next page)

**Table A.1** *(continued)*

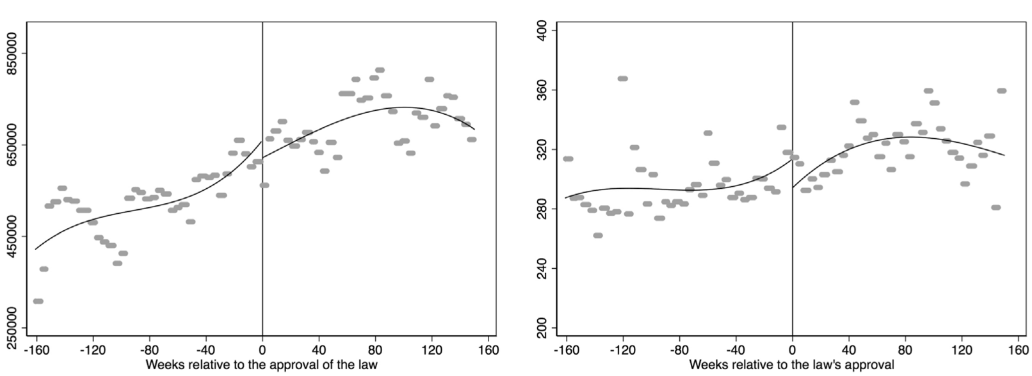
| Accident cause | *N* | Percent |
|---|---|---|
| Other causes | 26,232 | 6.95 |
| Escape by criminal act | 797 | 0.21 |
| Suicide | 37 | 0.01 |
| Vehicle detention without signaling or deficient | 173 | 0.05 |
| Lower than minimum speed | 19 | 0.01 |
| Overtake | | |
| Overtaking on bends, crossing, slope, tunnel, etc | 1377 | 0.36 |
| Overtaking without making corresponding signal | 819 | 0.22 |
| Overtaking without enough time and space | 8019 | 2.12 |
| Forward by the berm | 189 | 0.05 |
| Forward surpassing continuous line | 1166 | 0,31 |
| Road or car failure | | |
| Animals on the road | 3116 | 0.83 |
| Brakes | 2.838 | 0.75 |
| Chassis | 183 | 0.05 |
| Electric | 88 | 0.02 |
| Improper or defective signaling | 131 | 0.03 |
| Motor | 206 | 0.05 |
| Suspension | 66 | 0.02 |
| Steering | 727 | 0.19 |
| Tires | 2.185 | 0.58 |
| Traffic light in disrepair | 235 | 0.06 |
| Speed | | |
| Greater than the permitted speed | 490 | 0.13 |
| Not reasonable or prudent speed | 5780 | 1.53 |
| Not reducing speed in intersection, road, etc | 1809 | 0.48 |
| Excess of speed in restricted zones | 138 | 0.04 |
| Undetermined | | |
| Unidentified cause | 33,155 | 8.78 |
| Total | 377,531 | 100 |

*Note*: This table includes all traffic accidents recorded by Carabineros de Chile for the period between January 1st 2009 and December 31st 2014.

## Appendix B. Testing the validity of the identification assumption and robustness checks

### *B.1 Testing the validity of the identification assumption*

The estimated effects can be attributed to the law if there are no other discontinuous changes in control covariates related to accidents such as gasoline sales and police stops. Fig. B.1 displays RD plots for those variables, showing smooth conditional expectations around the cutoff date.




(a) Police stops

(b) Gasoline sales (in thousand $m^3$)

**Fig. B.1.** Regression discontinuity plots – police stops and gasoline sales. *Notes*: These figures show RD plots for monthly data using cubic polynomials and the law announcement date as a cutoff. Each dot represents the number of police stops and gasoline sales in a given month.

### *B.2 Robustness checks*

Another concern with our approach is with potential changes in reporting of alcohol-related accidents by police officers after the law. As a robustness check, we use data from Emergency Room (ER) admissions in Chilean public and private hospitals due to car accidents for the 2010–2014 period. This data has the advantage of being an independent source of information, free of changes in reporting by police officers. Fig. B.2 depicts the evolution of weekly ER admissions due to car accidents. As in previous figures, the vertical line is the cutoff date. As can be seen, there is a discontinuity in the series of ER admissions due to car accidents at the approval's date. The point estimate of the jump at the cutoff is -158 admissions per week significant at the 2% level.

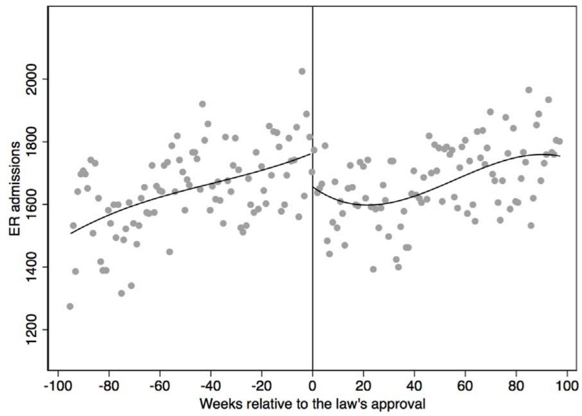



**Fig. B.2.** Emergency Room admissions due to car accidents. *Notes*: This figure shows the number of emergency room admissions in Chile due to car accidents. The vertical line represents the week of the law's approval.

### *B.3 Zero-inflated negative binomial (ZINB) model for deaths*

We estimate a ZINB for deaths in alcohol-related accidents. In this implementation we use the logit link to for $y_{i,t} = 0$ and negative binomial (NB) for $deaths > 0$. Column (1) of Table B.1 shows the results for the NB part and column (2) for the logit part modeling the probability of observing $y_{i,t} = 0$ in a given region-month. AS can be seen, the results for $\delta$ are the same as that of Table 6.

**Table B.1**
Zero-inflated negative binomial regression for deaths 2009–2014.

| | Negative binomial<br>(1) | Logit (inflate)<br>(2) |
|---|---|---|
| Post | −0.2200 | −1.0396 |
| | (0.213) | (1.490) |
| Trend | −0.0035 | 0.0278 |
| | (0.005) | (0.027) |
| Post × trend | −0.0016* | −0.0388 |
| | (0.010) | (0.046) |
| Gas sales | 0.0123** | 0.0117 |
| | (0.006) | (0.031) |
| Police stops | 0.0002 | 0.0181 |
| | (0.003) | (0.031) |
| Accidents alcohol | – | −0.1133** |
| | | (0.058) |
| Constant | −1.3783** | −0.7469 |
| | (0.585) | (2.4511) |
| $\theta$ | 0.0979*** | – |
| | (0.062) | |
| Nonzero obs. | 486 | |
| Zero obs. | 579 | |

*Note*: Robust standard errors in parenthesis. Zero-inflated negative binomial regression for deaths in alcohol-related accidents. Negative binomial specification is the same as in Table 6 and logistic regression includes also month fixed effects and the number of accidents due to alcohol. $\theta$ corresponds to the overdispersion parameter.
* Significance level $p < 0.1$.
** Significance level $p < 0.05$.
*** Significance level $p < 0.01$.

### *B.4 Alternative modeling for the effects' time patterns*

An alternative approach to evaluating the time pattern of the effects implies incorporating dummy variables with leads and lags of the approval date instead of the $Post_t$ and $Post_t \times trend_t$ (Abouk and Adams, 2013; Borusyak and Jaravel, 2016). Let $T_L$ and $T_H$ be the first pre-period event time and last post-period event time respectively. Let also $K_t = t - t_0$ where $t_0$ is the time event. Thus,

$$E[y_t|x_{i,t}] = \exp\left[\alpha_i + \beta \text{Police}_{i,t-1} + \gamma \text{Gas}_{i,t} + \sum_{k=-T_L}^{T_H} \delta_k 1(K_t = k)\right] \tag{B.1}$$

where $1(K_t = k) = 1$ is an indicator function that is equal to 1 when the argument is true and 0 otherwise, and $\delta_k$ are relative event time fixed effects. Hence, we can observe if the effects persist or decay over time simply by examining the estimated parameters, $\delta_k$. We estimate Eq. (B.1) including 15 quarter dummies from the first quarter of 2011 to the third quarter of 2014. We use quarterly dummies instead of monthly dummies to reduce the

**Table B.2**
Poisson regressions 2009–2014: effect's time patterns.

| Parameters | Accidents | Injuries | Parameters | Accidents | Injuries |
|---|---|---|---|---|---|
| $\delta_{-4}$ | 0.085 | 0.055 | $\delta_4$ | $-0.265^{**}$ | $-0.260$ |
| | (0.071) | (0.094) | | (0.132) | (0.168) |
| $\delta_{-3}$ | $-0.001$ | $-0.122$ | $\delta_5$ | $-0.315^{**}$ | $-0.232$ |
| | (0.074) | (0.094) | | (0.138) | (0.178) |
| $\delta_{-2}$ | 0.015 | $-0.090$ | $\delta_6$ | $-0.283^{**}$ | $-0.283$ |
| | (0.072) | (0.096) | | (0.136) | (0.177) |
| $\delta_{-1}$ | 0.006 | 0.015 | $\delta_7$ | $-0.315^{*}$ | $-0.245$ |
| | (0.094) | (0.114) | | (0.162) | (0.200) |
| $\delta_0$ | $-0.412^{***}$ | $-0.324^{**}$ | $\delta_8$ | $-0.181$ | $-0.145$ |
| | (0.110) | (0.148) | | (0.166) | (0.212) |
| $\delta_1$ | $-0.509^{***}$ | $-0.550^{***}$ | $\delta_9$ | $-0.284^{*}$ | $-0.304$ |
| | (0.107) | (0.139) | | (0.170) | (0.218) |
| $\delta_2$ | $-0.314^{***}$ | $-0.350^{**}$ | $\delta_{10}$ | $-0.250$ | $-0.298$ |
| | (0.102) | (0.136) | | (0.173) | (0.222) |
| $\delta_3$ | $-0.463^{***}$ | $-0.447^{***}$ | | | |
| | (0.129) | (0.163) | | | |
| $N$ | 1065 | 1065 | | | |

*Note*: Robust standard errors in parenthesis. $\delta_k$, $k \in \{-4, -3, \ldots, 0, 1, \ldots, 10\}$ corresponds to the parameter associated to quarter $k$ in Eq. (B.1) with quarter 0 being the quarter the law was approved and enacted. All models include gas sales, police stops, quarter and region fixed effects, and a linear trend.

* Significance level $p < 0.1$.
** Significance level $p < 0.05$.
*** Significance level $p < 0.01$.

number of parameters to estimate and for an easier interpretation of the results. Table B.2 shows the dynamics of the law's effects on accidents.

As can be seen, the estimated parameters $\hat{\delta}_k$ for $k \in \{-4, -3, \ldots, -1\}$, which are those before the law, are statistically nonsignificant (at the 10% level). On the other hand, $\hat{\delta}_0 = -0.41$ is statistically significant (at the 1% level) implying a 34% reduction in alcohol-related accidents in the quarter the law was passed and enacted. The effects decreases with time to about a half, with $\hat{\delta}_{10} = -0.25$ (22%) being not statistically significant (at the 10% level). For injuries the effects follow a similar pattern with $\hat{\delta}_0 = -0.32$ (27%) statistically significant at the 5% level and $\hat{\delta}_{10} = -0.30$ (26%) being not statistically significant (at the 10% level). It is important to note that standard errors almost increase more than 50% for $\hat{\delta}_{10}$ in comparison to $\hat{\delta}_0$.

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