New Evidence of the Effects of Zero Tolerance Laws on Drinking and Driving. The Case of Chile*

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PRELIMINARY:
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Abstract

In 2012, the Chilean government enacted a zero tolerance law that lowered the permissible blood alcohol content (BAC) threshold from 0.5 to 0.3, which also instituted severe financial and license revocation penalties for drivers of all ages. Using a rich panel municipality-level administrative record data set, this paper employs a difference-in-differences strategy to estimate the causal impact of the law on car accidents and fatality rates. Results indicate that the law reduced the number of alcohol-related car accidents by 18% to a quarter of all accidents, which is entirely driven by non-fatal incidents. Evidence also indicates that the law works by increasing sober driving, not by reducing drunk drivers' alcohol consumption. Supporting current findings, this paper also shows evidence that the law has no effects on fatality rates. Lastly, it shows a three-month-long anticipation effects that coincide with the bill's announcement, which we attribute to a public confusion of the difference between the announcement and the enactment of the law.

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1 Introduction

Motor vehicle accidents cause 1.24 million deaths and an 20 to 50 million injuries yearly worldwide (WHO, 2013). Traffic accident fatalities are similar in number to malaria's fatalities (Murray et al., 2012) making motor vehicle accidents the eighth leading cause of death in the world and the number one cause of death among young people between the ages of 15 and 29 (WHO, 2013). Expenses associated with traffic accidents are particularly striking for low and middle-income countries with cost estimates varying between 1% to 2% of GDP (Jacobs et al., 2000).

It is well-known that a leading cause of traffic accidents is alcohol-impaired driving. Raw estimates suggest that between 19% and 26% of road crash deaths are due to drunk driving, varying in accordance to the income level of each country (WHO, 2013).[1] This along with the large public health costs associated with drunk driving car accidents have prompted governments to limit driver's legal blood alcohol content (BAC).[2] Assuming that even low levels of BAC impair driving skills, several countries have set the bar quite low and, in some cases, specifically targeted their policies at youth.[3] Such policies are widely known as Zero Tolerance Laws (ZT-Laws).

This paper studies the effects of passing a comprehensive ZT-Law in Chile on the number of car accidents, fatality rates and average blood alcohol content. In 2012, the Chilean government passed a law that decreased the BAC threshold from 0.5 to 0.3 for all drivers (not just young drivers as in most countries) and also implemented severe punishments for offenders.[4] Penalties included the introduction of large monetary fines and doubling to tripling of the time of driver licenses suspension. To the best of our knowledge, this is the first nationally scaled ZT-Law impact evaluation; all others simply target a group of individuals, thus complementing previous literature on the effect of ZT-laws on youth (Grant, 2010).[5]

Even though ZT- Laws have been enacted in different countries, several studies question the effectiveness of these laws in reducing fatality rates. First and foremost, the theoretical impact

[1] In developing countries, these numbers are hard to measure due to lack of official records as consequence of low police enforcement and no available technology to measure BAC levels (Banerjee et al., 2012). As a result, only around 50% of countries have reliable data on alcohol-related road traffic deaths (WHO, 2013). For those countries, the little collected evidence suggests that alcohol is a factor in between 33% and 69% of fatally injured drivers, and between 8% and 29% of non-fatal accidents (Peden et al., 2004) The high presence of alcohol in accidents, is however, not exclusive of poor countries: for example, drunk driving fatalities accounted for 35% of total traffic fatalities in 2004 in the United States, and for 40% of fatalities in Canada for the same year (Road Safety Canada Consulting, 2011)

[2] For a worldwide law review visit http://www.icap.org/Table/BACLimitsWorldwide. There are still 34 countries in the world that either lack any drinking and driving regulation or that only informally assess alcohol intoxication without using BAC measures

[3] Medical literature has found "strong evidence that impairment of some driving-related skills begins with any departure from zero BAC" (Moskowitz and Fiorentino, 2000). This is evidence is supported by economics field studies. For instance Killoran et al. (2010) show that drivers with a BAC between 0.2 and 0.5 have at least a three times greater risk of dying in a vehicle crash; that risk increases to at least six times with a BAC between 0.5 and 0.8, and to eleven times with a BAC between 0.8 and 1. Levitt and Porter (2001) give similar estimates, presenting evidence that drivers who have imbibed alcohol, have at least seven times greater chances of being involved in a fatal crash than sober drivers, and thirteen greater with a BAC higher than 1.

[4] Countries may define BAC differently. We use Chile's legal BAC definition as a ratio of grams of alcohol per liter of blood.

[5] A similar nationwide zero tolerance policy was implemented in Brazil in 2008.

of ZT-laws on motor vehicle accidents is ambiguous. Opponents argue that while ZT-Laws effectively deter mild drinkers (who are not dangerous) from driving, they have have marginal disincentives for heavy drinkers, and thus have a low impact on the overall number of traffic fatalities. Consequently, decreasing the BAC threshold would reduce the welfare of mild drinkers, while having a null or opposite effect on heavy drinkers, those that the policy intends to impact the most (Freeman, 2007; Grant, 2010; Navarro and Nuñez, 2012).

This view is in part supported by an extensive literature with contrasting findings about these laws' effectiveness.[6] Most of these studies however, have important methodological limitations and varying level of accuracy (Eisenberg, 2003). A great number of these papers analyze on uncontrolled before-after comparisons, with small samples and low statistical power. In addition, few of these studies succeed in controlling for simultaneous policies, trends, and sufficient covariates in order to account for omitted variables.[7] In attempting to address these methodological limitations, Dee (2001) and Eisenberg (2003) evaluate multiple 0.8 BAC state laws in the United States both finding significant reductions in fatalities. Nonetheless, they do not control for serial correlation, which as Bertrand et al. (2004) point out, may overstate impact's estimates significance.

Freeman (2007) updates Eisenberg (2003)'s data and by allowing for serial correlation he does not find any measurable evidence that decreasing BAC thresholds reduced traffic fatalities. In addition, Grant (2010) casts additional doubts on the effectiveness of lowering BAC legal thresholds. Implementing a ZT-Law for youth (legal limit of 0.2 BAC) in the United States had no measurable effects on fatalities or on the distribution of the BAC of drivers involved in fatal accidents. For Europe, Albalate (2008) shows that lowering the legal BAC level to 0.5 significantly decreased traffic accident deaths only for men between 18 and 49 years old. After controlling for concurrent interventions such as random alcohol tests and the minimum legal drinking age, the impact for the overall population was not relevant.

Advocating against or in favor of ZT-laws has proven to be a difficult task. A lack of robust evidence can be attributed to four factors. First, ZT-laws are often passed along with other laws. For instance, several states in the United States passed 0.8 BAC laws in close proximity with administrative license revocation laws.[8] Even though Freeman (2007) controls for license revocation laws, there is not any methodology controlling for the interactive effects between laws. From a policy perspective, this caveat is not particularly constraining given that most countries enact BAC laws with administrative license revocations (or with them already in effect) (Dee, 2001).

Second, previous studies typically use aggregated data that does not group accidents by their cause (e.g. alcohol-related, speed, etc.). Therefore, small changes in alcohol-related accidents are undetectable due to a greater variance in total accidents. Third, due to lack of data, previous

[6]See Eisenberg (2003) for a literature review on the evidence of lowering BAC laws.

[7]See for example Rogers (1995); Hingson et al. (1996, 2000); Foss et al. (1998); Apsler et al. (1999); Zwerling and Jones (1999); Shults et al. (2001)

[8]Administrative license revocation implies the automatic suspension of the driving license if BAC exceeds legal limits. For a revision see Dee (2001).

evaluations have not taken into account the effects of the law on non-fatal accidents (Eisenberg, 2003).[9] Fourth, previous studies do not control for endogenous increases in law enforcement as a consequence of the laws, which likely leads to an overstatement of the law's effects.

We have access to a rich monthly panel data set of municipality-level administrative records that allows us to overcome methodological limitations encountered in the literature and determine the causal impact of the law isolated from changes in law enforcement and other confounding components. Administrative records include information on number of car accidents by cause, deaths, injuries, police controls, drunk driving offenders, and pooled BAC data.

Using a difference-in-difference method allowing serial correlation of the errors, we find strong causal evidence that alcohol-related accidents decreased by 18-25% after enacting the ZT-law. Remarkably, after controlling for specific-municipality time trends, there is not any measurable impact of alcohol-related traffic accidents on the number of deaths or serious injuries. We also present differential estimates by accident severity, which are novel to the literature. Similar to Freeman (2007) and Grant (2010), we do not find any robust evidence that the law reduces the fatality rate. However, our data suggests that there is an important reduction in alcohol-related accidents that caused at most minor injuries. Even though ZT-laws may not be effective at reducing humanitarian costs, their impact on reducing economic costs means that these policies may still be effective in reducing traffic accidents' societal burden.

Our findings are also relevant for understanding how these laws work. By exploiting a rich micro-level data set, this paper identifies two channels through which ZT-laws reduce alcohol-related accidents. Previous literature has referred to two mechanisms with mixed results: (1) decrease in alcohol consumption and (2) decrease in the probability of drinking and driving (deterring role). Therefore, this paper contributes to the literature by supporting previously highlighted mechanisms by using administrative records that are more precise than self-reported alcohol use and drunk driving surveys. On the one hand, individuals might respond to ZT-Laws by "abstaining from alcohol use, drinking more moderately, and/or reducing their propensity to drive conditional on having consumed alcohol" (Carpenter, 2004). We present evidence suggesting a reduction in alcohol consumption. In line with Carpenter and Harris (2005)'s findings for the United States we also report heterogeneous effects upon gender.[10] On the other hand, this paper also supports the law's deterring role by documenting an important reduction of drunk driving offenses per unit of enforcement. Previous evidence of the impact of ZT-Laws often shows contrasting findings. For example, Carpenter (2004, 2006) and Carpenter and Harris (2005) do not find any measurable impact of the law on self-declared drinking and driving. Conversely and in line with our results, Wagenaar et al. (2001) and Liang and Huang (2008) document that youth ZT-Laws are efficient in reducing drinking and driving among college students.[11]

[9]This is explained by the United States' National Highway Traffic Safety Administration (NHTSA) -which is the basis of most studies- not reporting all road-crash data, only the fatal ones.

[10]Other studies, as Wagenaar et al. (2001) and Liang and Huang (2008) do not find any impact on alcohol use.

[11]Our results are also in line with studies analyzing the impact of increasing the penalties for drunk drivers. On one hand, Kenkel (1993) and Sloan et al. (1995) for example, report decreased alcohol use in response to tougher criminal sanctions and liabilities. In addition, we support previous evidence indicating that drunk driving can be deterred by stricter punishments (Ross, 1984; Nichols and Ross, 1990; Kenkel, 1993; Sen, 2005; Hansen,

An additional finding from the analysis is the existence of a three-month long anticipatory effect. This suggests that the law impacted individuals' behavior prior to its enactment. We attribute this effect to fuzzy media coverage and general public confusion of the difference between the announcing and enacting of the law. People´s behavioral response prior to the enactment of the law is interesting because it is contrary to many ZT policies in which reactions occur years after implementation (Eisenberg, 2003; Albalate, 2008; Kuo, 2012), emphasizing the role that the media can have on policy implementation.[12] All the discussed literature, its effects, magnitudes, and directions are summarized in Table A.1.

Finally, one relevant caveat to the external validity of this literature's results is the extent to which these laws are enforced across countries. One of the contributions of this paper is that its results have more external validity for middle-income countries than previous studies (the cases of United States and Europe). In contrast to high-income countries, Chile as most of developing countries, is low in the ability of enforcing drinking and driving laws (WHO, 2013)[13]. These similarities turn these results somehow more comparable to that of other developing countries. To our knowledge this is the first BAC law evaluation for a developing country.[14]

The remainder of the paper is organized as follows. Section 2 briefly describes the program and the Chilean regulatory context. Section 3 introduces a conceptual framework. In Section 4 we present the descriptive statistics and data. Section 5 briefly describes the estimation method to be implemented, and Section 6 presents the empirical results. In Section 7 we discuss and present evidence on the mechanisms. Finally, Section 8 is a concluding discussion.

2 The Program

2.1 The Law

Traffic accidents are a particularly sensitive matter in Chile. For the 1995-2009 period, fatal accidents rate increased by 6.5%, while all other OCDE countries, except for Russia, decreased

2013).

[12]A recent study presents evidence in favor of these laws. Kuo (2012) finds endogenous policy lags due to late information acquisition and the cost of adjustment, arguing that difference-in-differences strategies (used by all the prior studies) ignore these lags leading to "severely downwardly biased estimates of policy effects." To surpass this obstacle she combines the multiple structural change methodology and the smooth transition method to estimate the policy effect of two under-age drunk driving laws in California in 1987 and 1994, finding high effectiveness of the policy.

[13]Indeed, the measure of drinking and driving enforcement for Chile is lower then middle income and all south america's countries.

[14]There is only reduced evidence favoring the effectiveness of "dry laws" banning alcohol sales, but nothing on enforcement or penalization for drunk drivers. For Argentina, Sviatschi (2009) uses a progressive implementation of a Dry Law in Argentina that banned nighttime sales at grocery stores and gas stations, finding a 14% decrease in car accident fatality rates. In Brazil, Biderman et al. (2010), using a difference-in-difference approach, find no significant effect in the same direction for the Dry Law implemented in several Sao Paulo municipalities. The Brazilian law consisted of mandatory night closing hours for bars and restaurants in order to restrict the recreational consumption of alcohol when driving. However, their study analyzes overall fatalities instead of those exclusively due to alcohol consumption, and mainly focuses on the impact of murders and not traffic fatality rates.

their rates. Traffic accidents are currently the second major cause of death of young people between 15 and 25 years old and first among children under 15 years old. For the period 2000-2009, close to 1.600 people died per year in road traffic accidents, and more than 20% were related to driver or pedestrian alcohol use.[15]

In this context, in March 15 of 2012, Law 20,580 (from now on ZT-Law) started increasing the penalties for drunk driving, or driving under the influence of alcohol, narcotics, psychotropic drugs. Permitted BAC was lowered from 0.5 to 0.3 grams of alcohol per liter of blood; the equivalent to one can of beer or a glass of wine. In addition the drunk intoxication category was changed from 1.0 to 0.8 BAC. Drivers caught driving under the influence (DUI) must pay a UTA$1 to UTA$5 fine (about US$900 to US$4700 at the time of this writing) and have their license suspended for three to six months. The license suspension increases up to 36 months if driving under the influence caused moderate injuries, and these drivers also face prison time from 20 to 540 days. If deaths or serious injury are involved, suspension and prison times both jump to 5 years.

Drunk driving (above a 0.8 BAC level) leads to the license suspension for 2 years if no injuries are caused and permanent revocation if deaths or serious injuries are involved. Prison times were also increased depending on the severity of injury. In the case of recidivism, the second and third time have suspension periods of 4 and 5 years for DUI offenders, and 5 years and lifetime cancelation for drunk drivers respectively. The penalties before and after the new law are summarized in Table 1.

[Insert Table 1 here]

Furthermore, despite the law's harshness, two other policies were implemented in order to ensure effectiveness. First, a massive awareness campaign was deployed promoting sober driving, showing the fatal risks of drunk driving and reporting the main changes from the ZT-Law. Mass media campaigns have shown to be helpful in preventing alcohol-impaired driving and alcohol related crashes (Elder et al., 2004; Sen, 2005).[16] Second, sober checkpoints were increased before and after the law took effect. As several studies suggest (Peek-Asa, 1999; Shults et al., 2001; Goss et al., 2008) random breathalyzer testing consistently results in decreased traffic related fatalities.

Finally, a relevant feature determining the impact is the timing of the law's passage. On May 8, 2011 the health minister firstly refers to a ZT-Law as response to a connoted case where a child lost both of his legs after being run-over by a drunk driver. The first formal announcement occurred on May 21st during the Chilean president's annual statement.[17] In late December of

[15] The first cause of death in road traffic accidents comes from pedestrian actions, an average of 23% of deaths for the last decade. Accidents related to alcohol are the second major cause.

[16] It was released in two phases starting first on February 23rd and continuing on April 4th the second stage, see the Chilean government's official website.

[17] The annual statement of the President of Chile, also known as the Presidential Message or Speech of May 21st, is a public ceremony in which the President addresses the full Congress on the administrative and political state of Chile.

2011 the launch of the law was announced. In January 11 of 2012 the bill was first approved by the Senate, then was slightly modified by the chamber of deputies and finally passed January 31. In March 15 of 2012 the law took effect. The timing of the events is summarized in Table 2.

[Insert Table 2 here]

3 Conceptual Framework

Drunk driving policies aim at decreasing the overall number of accidents and fatalities. A comprehensive analysis should not only present reduced form estimates, but also shed light on the underlying mechanism through which these laws work. This paper discusses two potential channels mentioned in the literature: deterrence and decreased alcohol consumption. We generalize this framework by proposing a two-stage decision model: first an extensive margin where individuals decide whether to break the law or abide by the regulation, and a later decision in which they optimize their alcohol consumption. The stages are solved backwards. We discuss both of the potential margins, however for simplicity we present a model for the extensive decision only and discuss possible solutions for the intensive one.

3.1 Extensive Margin

We assume that agents are forward looking and thus they first optimize the intensive margin by using backward induction. For simplicity however, we start with the extensive decision (i.e. offending or not), assuming individuals have already decided their alcohol consumptions in the intensive margin. Some considerations of general equilibrium will be discussed later.

We expand the framework of Banerjee et al. (2012) into a two-decision model. There are two types of heterogeneity along which individuals differ, first in their utility from drinking, $v_i \sim [v^L, v^H]$, and second in their driving utility, $d_i \sim [d^L, d^H]$. The joint distribution of these types is defined as $f(v_i, d_i)$. Potential drunk drivers make two discrete choices, whether to get drunk or stay sober and whether to drive or use alternative transportation. We also define two indicator variables T_i, $M_i \in [0, 1]$ taking the value 1 in case the individual decides to drink or drive respectively, and 0 otherwise.[18]

Each decision faces different costs. If the individual decides to drink and drive, he is subject to a cost p_j that represents the expected cost of being caught by the police, reflecting the probability of apprehension multiplied by legal penalties (e.g. fines, time of license suspension, days in prison, etc.). On the other hand, not driving is also costly. This disutility can be broadly construed, for example as discomfort or simply as the price gap between of the transportation

[18]Since both driving and drinking have intrinsic utility for individuals, we assume that every combination of (T_i, M_i) is strongly preferred over $(T_i = 0, M_i = 0)$. This is simply due to the base assumption that every agent is better at driving if not also drinking and vice-versa.

substitute in respect to driving oneself.[19] This cost is expressed as a constant c_j multiplied by the individuals' own not driving disutility. Therefore, the utility derived from the drinking and driving decision is defined as:

$$U(d_i, v_i, T_i, M_i | c_j, p_j) = T_i(v_i - M_i p_j) - (1 - M_i)c_j d_i \tag{3.1}$$

where the first expression is the net utility derived from drinking and driving and the second is the cost of not driving. Given this utility, the type space divides into three decision areas:

1. Drink and drive:

$$d_i > \frac{p_j}{c_j} \quad \cap \quad v_i > p_j \tag{3.2}$$

2. Drink and do not drive:

$$d_i < \frac{p_j}{c_j} \quad \cap \quad d_i < \frac{v_i}{c_j} \tag{3.3}$$

3. Stay sober and drive:

$$d_i > \frac{v_i}{c_j} \quad \cap \quad v_i < p_j \tag{3.4}$$

It is easy to analyze the areas that configure the decision space. The drinking and driving space is set by the integer $\int \int_{p_j}^{v^H} (d^H - p_j/c_j) f(v_i, d_i)\ dv_i dd_i$, while the space where individuals choose to drink and not to drive is given by $\int_{v_L}^{p_j} v_i/c_j\ dv_i + \int_{p_j}^{v_H} (p_j/c_j - d_L)\ dv_i$. The integer $\int_{v_L}^{p_j} v_i/c_j\ dv_i + \int_{v_L}^{p_j} (d^H - p_j/c_j)\ dv_i$ delimits the space where agents prefer to stay sober and drive. For simplicity lets assume that $f(v_i, d_i)$ is a bivariate uniform distribution, thus interpreting all type spaces as areas. The described spaces are therefore, plots A, B and C in Figure 1 respectively.

[Insert Figure 1 here]

The comparative statistics are straightforward. A rise in p_j and c_j are depicted in Figure 1. An increase in p_j (the expected cost of breaking the law) unambiguously decreases the drunk-driving area to the benefit of sober-driving, and drinking-and-not-driving areas. In the left graph of Figure 1, the dashed A rectangle reduces to the red line delimited area. Conversely, the B and C areas increase. The intuition is that since the price of drunk driving is rising, individuals decide to drink less (not-drinking in the model) or to leave the car at home. Consider now an increase in c_j, equivalent to a rise in the price of not driving. Individuals move to substitute

[19]Recently in Chile, many small companies are offering comfortable night transportation. The price of the service is however, around 10 times the cost of driving oneself. See for example http://www.safeback.cl/

driving with not driving and thus the drunk-and sober-driving areas increase as the drinking-and-not-driving space is reduced. The red lines again set the new area in graph (b) of Figure 1. In this scenario, agents substitute alternative transportation (e.g. taxis) for driving.

Interestingly, a rise in p_j and c_j increases sober-driving. In contrast, the sum of the effects predicts ambiguous effects on both drunk driving and on drinking-and-not-driving behavior. Hence, if a ZT-Law indirectly increases the price of substitute transportation, it might be ineffective in deterring drunken individuals driving. An optimal policy design should consider this caveat. In Chile, typical alternatives are public transport (subway and buses) and taxis. These can impact the law's success by increasing or reducing prices, total supply, and waiting time, varying individuals' non-driving cost c_j. Following the model, if as a response to a ZT policy this cost decreases, the deterring should be enhanced. On the contrary, if the cost increases, the effect is ambiguous depending on which of the effects survives.

No substantial change occurred in public transport prices due to the ZT-Law. Moreover, public transport is not likely to change its pricing strategy in response to these policies since most of the substitution happens at night, i.e. low capacity times. Additionally, in respect their total supply and waiting times, it was publicly announced that night services would be significantly increased right after the law enactment.[20] Therefore, from the buses side we should not expect any increase in c_j.

Respect to taxis, they have fixed rates set by the transportation ministry and thus do not vary their fares formally. The supply of taxi permits has been fixed in Chile since 1998, meaning that the number of taxis could not increase. The number of rides they offered is, however, endogenous. One way to indirectly test whether the taxi supply has increased is by looking taxicab permits'(medallions) price variations. Since the per kilometer price is fixed, the change in profits comes exclusively from the number of rides the driver decides to offer. Thus, if the permit price rises, reflecting an increase in the present value of profits stream in an infinite horizon, it means that the ride supply increases as well.[21] There is not any evidence showing a decline or increase in permit prices as result of the ZT-law. However there is some anecdotic evidence of an increase in passengers wait times.[22] If the number of rides and taxis are constant, it implies that more agents looking for taxis would necessarily increase the time needed to get a taxi. This caveat may be hindering greater effects of the law. This is an interesting topic for further research.

If the increase in p_j is not offset by a rise in c_j, the law through an increase in the cost of drunk driving will unambiguously lower the total number of drunk drivers (deterring effect). It should also increase the number of individuals who drink and do not drive and the number of

[20] See for example: Valencia, Manuel."Gobierno reforzará servicios nocturnos de Transantiago tras promulgación de nueva ley" La Tercera. 1 March 2012: 15. Print.

[21] Gallego et al. (2013) do something similar showing the impact of the implementation of a new public transport system "Transantiago" in Chile's capital. They find a 60% increase in taxicab medallion prices, attributing these findings to a substitution behavior from individuals moving from buses to taxis.

[22] Indeed, some applications for mobile phone were created to deal with this problem. See for example Neira, Javier. "SaferTaxi: Aplicación que moderniza la acción de pedir un radiotaxi" Emol. 12 May 2012. Web. 26 July 2013.

sober drivers. Other possible scenarios are presented in Table 3. In the empirical part of the paper we show that the law reduced the number of offenders, supporting the presumption that the costs imposed on drunk driving were greater than those from waiting for taxis.

[Insert Table 3 here]

3.2 Intensive Margin

Regarding the intensive margin, the existing literature has considered alcohol consumption as the main decision variable.[23] The intensity of alcohol use depends however on the drinking and/or driving decision the individual took in the extensive margin. As the previous model showed, if the rise in the drunk driving price is not offset by variations of the alternative transportation's cost, sober-driving and/or drinking but not driving are expected to increase at the benefit of drunk drivers. Two opposite effects regarding their alcohol use are expected. On one hand, agents moving from drunk driving towards just drinking do not face the costs of drunk driving anymore and may increase their alcohol consumption. On the other hand, individuals who move to sober driving are surely reducing their use of alcohol. These opposite effects imply that the impact of the law on overall alcohol consumption is theoretically ambiguous and therefore is an empirical question.

More interesting is what occurs with the consumption of agents who are still driving. This, since the impact on their consumption is not obvious, and these drivers have a direct impact on accidents. Grant (2010) argues that lowering the legal BAC limit decreases the alcohol consumption of newly law-abiding individuals, but increases it for the new offenders. The reasoning is that if individuals differ on their alcohol taste, the law compliers who are in the margin decide to break the law since respecting it is now more costly. His argument is supported by evidence showing no impact of a youth ZT-law on the BAC of drivers involved in fatal accidents in the United States. This reasoning can be proven only if the law does not increase offender penalties; if it does as in the Chilean case, the results are theoretically ambiguous depending on punishment magnitude. If expected penalties are high enough, alcohol consumption should reduce at every level. The decline should probably be lower for heavy drinkers who face greater costs in not consuming due to higher valuation of alcohol use.

Another possible behavioral reaction due to the law besides a reduction of alcohol use is more cautious driving of drunk drivers. The law increases the cost of reckless attitudes since any possible crash is likely to lead to apprehension. This change in individual behavior is driven however by the change in penalties and not by the BAC reduction. Even though it is a relevant and interesting question, we do not have the proper data to test this hypothesis. The next diagram presents the discussed mechanisms

[23] See Carpenter (2006) and the references therein.

3.3 General Equilibrium Considerations

Some general equilibrium considerations should be addressed. As argued, the law increases the price of drunk driving leading to an increase in drunk pedestrians. Since pedestrians recklessness is the greatest cause of accidents in Chile, this substitution could have led to an unintended increase in deaths due to drunk pedestrians. This concern, though valid, is not supported by the data (see Appendix B). We do not find measurable increases or decreases in drunken pedestrians' accident or fatality rates.

Other potential effects of the law arise due to possible spillovers over non-alcohol related accidents. For instance, Rizzi (2012) points out that the massive media announcement could have had communicational dividends impacting drivers behavior even outside of drinking, for example speeding. Nonetheless, this hypothesis is not supported by the data for speed or imprudent driving accidents; we will touch on this again in Section 6.

4 Data and Descriptive Statistics

4.1 Data

The main data used in this paper is administrative records from two departments of Carabineros de Chile. We have a panel of traffic accidents with their cause (59 types as listed in Appendix C), by date (day and hour), number of injured and dead, plus some information about location (municipality and geographic region), as well as climate conditions. This information was provided by the SIAT Office of Carabineros de Chile. The total sample includes 201,824 accidents, 14,494 involving drunken driving.[24] We complete the panel by replacing the missing observations with zeros, since no accident was actually observed. The final panel is total of 340 municipalities between January 2010 and April 2013. The data is particularly rich since it allows us to differentiate accidents by causes. Thus the dependent variable is alcohol-related accidents, narrower than total accidents as used by the existing literature. This is indeed helpful since it decreases the dependent variable's noise and reduces the estimation's standard errors.

We merge these data with a monthly municipality panel of the number of cars investigated by police patrols and the number alcohol-related driving offenders separated by driving under alcohol influence (DUI) and drunk driving. This information was provided by the Criminal

[24]There is not any data for Juan Fernandez Island, Easter Island and the Guaitecas municipalities. We also drop O'Higgins and Tortel municipalities since no data on their motorization rates exist. These, however, represent only 0.06% of total observations and 0,15% of those related to alcohol.

Analysis Office, also from Carabineros de Chile. We also include a monthly panel of regional real prices of gasoline and the unemployment rate, obtained from the transportation ministry and Chilean Central Bank respectively. Population data was taken from the National Institute of Statistics (INE), while total motorized vehicles records for each of the municipalities were obtained from the 2012 Chilean Census. We also have a list of all blood alcohol content exams in the Metropolitan Region for the period 2009-2012 collected by the Chilean Legal Medical Service (SML). Finally we have monthly data of the volume of web searches for the law over time. This was obtained using the "Google trends" tool, and will be used as a proxy of the media impact of the law.[25]

[Insert Figure 2 here]

The media impact trend is depicted in Figure 2 and relates to the time events presented in Table 2. The first solid line indicates the date when the law was passed; the second dashed one is the enactment date. As can be appreciated the impact is flat until the law is announced and then grows steadily to its enactment, then slowly decreasing over time. The next spike is in September, which is the month of national celebrations and with the highest amount of drunk driving offenses.

4.2 Descriptive Statistics

Table 4 shows a summary of the number of total accidents, injuries, and deaths at a national level for the 2010-2012 period. The decrease of accidents related to alcohol is remarkable, 27% between 2011 and 2012, from 5,046 to 3,678 respectively. However, accidents related to other causes slightly increased from 57,788 to 58,113 (0.6%). Overall, the total number of accidents decreased from 62,834 to 61,791 during this period. Deaths and injury analyses have similar results. Deaths and injuries declined both by 28% for drunk driving accidents, but increased by 0.5% and 1% respectively for other accidents.

[Insert Table 4 here]

The break in the trend is even more visible in Figure 3 that shows a longer evolution of accidents, injuries, and deaths for alcohol and non-alcohol related accidents from 2000 to 2012. The graph was constructed using log variables and thus represents the percentage change by time. The series seem to steadily grow with time. However, for alcohol-related accidents, the trend breaks in 2012 when the ZT-Law was enacted. The same is not observed for the non-alcohol-related accidents.

[25] The data can be easily obtained by looking for "ley alcohol" in www.google.com/trends/ and specifying Chile as the interest region and a time window between 2010 and 2012. The data is weekly; we collapsed it to a monthly frequency and then standardized it from 0 to 100. Using other wording as "ley de alcohol," "ley alcoholes," "ley de alcoholes," and "ley tolerancia cero" give similar results.

[Insert Figure 3 here]

In Table 5 we present the summary statistics for all relevant variables used in the estimations. Panel A shows all alcohol related variables, Panel B all the non-alcohol related variables, and Panel C the controls. All presented variables are on the monthly and municipality level, and standard errors are in the parenthesis. The first column shows averages for all months in the sample. Subsequent columns present average levels and differences for the periods before and after the law's enactment and announcement. Differences for accidents, deaths, and injuries are significant for the enactment of the law, and are even greater when the announcement period is captured, suggesting important anticipatory effects. In contrast, no effects are observed on non-alcohol related outcomes. For example, after the law was enacted (announced) the number of alcohol related accidents were significantly reduced by 74 (94) per month in an average municipality. On the contrary, non-alcohol crashes did not have any statistical variation.

[Insert Table 5 here]

5 Identification Strategy

In this section we discuss the identification strategy and the estimation methodology. We discuss the plausibility of implementing a Regression Discontinuity Design exploiting the date of the law's enactment. Given that there are anticipation effects we use a difference-in-differences, controlling for lags and leads to assess these potential effects. In the difference-in-differences approach we use other accidents as a control group.

5.1 Estimates Bounds

Before defining the identification strategy it is important consider that any method exploiting the time as discontinuity will estimate a lower bound of the true impact of the law. This corresponds to a lower bound since before March 15th of 2012 (the law enactment) the level of driving under influence was BAC 0.5-1.0. For the period after, driving under influence was considered with BAC 0.3-0.8, and therefore all accidents with BAC between 0.3-0.49 are considered alcohol related only after the law, but not before its enactment. For a mathematical formalization of the argument, see Appendix D. If we had data matching each alcohol-caused accident to its related BAC, this issue would easily be solved by just using the same level of BAC (0.3 or 0.5) to label accidents before and after the law. However this is not the case and is not possible to merge accidents with BAC because we lack of an accident and BAC matching id.

Although, we cannot find the precise point estimate, we can look for lower and upper bounds and define the region of the true parameter. The upper bound estimate can be easily estimated by using only accidents caused by drunk drivers, e.g. BAC over 0.8, for the post law period. Now the law enactment date sets alcohol related accidents over 0.5 BAC on one side, and over 0.8 on the other which because of the same argument than before is necessarily an upper bound.

5.2 Regression Discontinuity Design

One plausible identification strategy is a sharp Regression Discontinuity Design (RDD) using time as running variable.[26] This approach exploits the discontinuity in the implementation of the ZT policy on the enactment on March 15, 2012. Right before and right after March 15th, all probable confounding covariates are likely to be balanced, thus a comparison of those gives us the immediate effect of the policy. The advantage of this approach is that under minimal assumptions we have a causal effect of the ZT-Law on the outcome variables of interest. The outcome variable y in this application will be the number of traffic accidents related to alcohol and the number of deaths from these accidents. The runny or forcing variable is time t and the treatment variable d_t is a dummy variable equal to 1 when the time trend is on or after March 15, 2012 and 0 otherwise.

However, as pointed out by Lee and Lemieux (2010), this design works only if there is not any anticipation (or lags), in the impact of the treatment. Therefore, using RDD for the evaluation of these policies might be hazardous as an anticipatory or lagged reaction to the law are both possible and arguable. For example, Eisenberg (2003), Albalate (2008) and Kuo (2012) find lags on the impact of laws that reduce permitted BAC due to late information acquisition and the cost of adjustment. For this particular law, the data shows that the law impact was anticipated three months before the enactment, without a clear guess on the true breakpoint date. This concern suggests that using an RDD in this context may not a valid approach. That said, in Figure 4 we show "donut hole" regression discontinuities where we remove observations within the announcement and enactment of the law to avoid anticipatory effects. Next, we graph local linear regressions at the left and right side of the hole.

[Insert Figure 4 here]

5.3 Difference-in-Differences approach

We perform a difference-in-differences analysis to assess the causal effect of the law on accidents related to alcohol. In order to identify the impact, the first challenge is to define a reasonable comparison group to serve as counterfactual for the accidents that would have happened without the law. We group and label all similarly caused accidents generating 15 possible causes, instead of the 59 in the original database. Groups representing the total sample are shown in Table 6. To serve as control group we use all accidents due to car or road technical failures. The 5,166 accidents in this category are presented in Panel B. It seems safe to assume that the law does not affect this kind of accident. Using this as the control group assumes that any idiosyncratic shock affecting driving circumstances (and hence accidents) should have the same effects on all crashes, making it possible to estimate the causal effect of the law on alcohol-related accidents.

[Insert Table 6 here]

[26] See for example Davis (2008)

It is important to note that three different events confounded on March 15, 2012: the law itself, i.e. lowering the legal BAC limit and increased penalties; changes in police enforcement; and an already deployed media campaign. Not controlling for these variables would likely lead to biased estimates of the law's impact. To determine whether there is any evidence of reduced alcohol-related accidents following the ZT-Law, we estimate a standard difference-in-differences framework.[27]

$$y_{it} = \alpha + \beta_1 \cdot zt_t + \beta_2 \cdot enforcement_{it-1} + \gamma \cdot control\ accidents_{it} + X_{it}'\delta + \mu_i + \omega_t + u_{it} \quad (5.1)$$

y_{it} represents the log (total accidents +1) in municipality i at time t. We choose log accidents since this would provide an easy way to interpret the effects of the policies in percentage terms. The date of the law sets the ZT-Law (zt) dummy, and is the parameter of interest. The police control variable is $enforcement$, using the lagged realization to avoid simultaneity issues. The accidents counterfactual is $control\ accidents_{it}$ and is the log (accidents +1) of the car and road failure group. The μ and ω coefficients are municipalities and month-of-the-year fixed effects. Finally, the unemployment rate, gasoline real price, other traffic accidents, and media are used as controls denoted by X. All coefficients are obtained by using weighted least squares using total number of motorized vehicles for each municipality as weights.[28]

We run different specifications to check for robustness by adding or subtracting several controls. All regressions include municipality and month-of-the-year dummies, the latter to control for seasonality. Specific time variant fixed effects are not added since the law enactment was the same date for every municipality without following a staggered implementation.[29] For this same reason, in some specifications we allow for a specific time trend for each municipality. We also add other accidents (besides alcohol-related and road and car failure accidents) in the right hand of the equation in order to control for unknown aspects that would affect all but not random accidents (as our counterfactual). As pointed out by Abouk and Adams (2013), the latter and unique time trends should capture any confounding influences on overall traffic accidents over time.

We also control for regional gasoline's real price to capture the effects of a potential negative elasticity between gasoline prices and accidents. Finally, we add an unemployment variable to

[27] Since in this case $treat = post$, following Kuo (2012) we estimate

$$\begin{aligned} y_{treat_{it}} &= \alpha + \beta \cdot x_{it} + \gamma \cdot post_{it} + \delta \cdot y_{control_{it}} + u_{it} \\ y_{treat_{it}} - y_{control_{it}} &= \alpha + \beta \cdot x_{it} + \gamma \cdot post_{it} + (\delta - 1) \cdot y_{control_{it}} + u_{it} \end{aligned}$$

$$\gamma = \underbrace{\{E(\underbrace{y_{treat_{it}} - y_{control_{it}}}_{diff} | post = 1) - E(\underbrace{y_{treat_{it}} - y_{control_{it}}}_{diff} | post = 0)\}}_{diff}$$

[28] This identification strategy follows Abouk and Adams (2013) who in a similar way evaluate the impact of a traffic policy (texting bans) on traffic accidents. Due to many zero realizations, 1 is added to every observation.

[29] Including a monthly variant fixed effect would confound with the effect from April 2012, the estimated enactment date.

capture any potential relationship between a municipality's economic performance and traffic accidents. The law impact, however, to be driven by this channel should has to be the case of an important economic downturn coinciding with the law enactment. This is unlikely.[30]

Using the same specification we attempt to identify differential impacts on accidents given their severity. We have crash data with the number of dead or injured individuals and the severity of injuries. Chilean law defines that a serious injury occurs when as a consequence of an accident the individual is kept from work by at least 30 days. A less serious (moderate) injury occurs when there are no disabilities, serious or very serious injuries and the person is unable to work for less than 30 days and more than 5. Minor injuries are the residual. We created different types of accidents based on types of injuries, where all other remaining crashes do not have either injuries or deaths. These estimates for differential impacts are novel to the literature and give a better understanding of ZT-Laws' effectiveness. Finally, we also use the sum of deaths and injuries (total and according to their type) to also analyze the economic benefits of the law.

6 Results

The following section presents all regression results. Tables 7 and 8 present the impact of the law on alcohol-related accidents, and robustness checks respectively. Next, Table 9 shows the differential impact on accidents according to their type, and number of deaths and injuries.

6.1 Main Specification Results and Robustness Checks

Columns (1)-(5) in Table 7 show the impact of the law on alcohol-related accidents. Panel A presents the lower bound impact and Panel B the upper bound. The leftmost column reveals an important decline between 16% and 26% of alcohol related accidents. Column (2) shows a slight reduction of the interval to a 15-24% accident decrease as result of controlling by other traffic accidents. Allowing for municipality-specific trends, the estimates increase to 21-29% and then to 18- 25% when non-alcohol related accidents are accounted. The rightmost column uses the same specification as column (4), but includes media as control. Since the law confounds with media, this last estimate is expected to be smaller. Indeed, impact coefficients are reduced at the benefit of an 11-12% decrease of alcohol-related accidents due to media. This result probably understates the law's effectiveness because the law creates the media, and thus adding the media variable is not precise.

[Insert Table 7 here]

Interestingly, although police enforcement has a negative coefficient as expected, it does not have any not measurable effects under any of the specifications in the lower or upper bound

[30]Unemployment data shows a steady economic recuperation post the financial crisis of 2008 until 2012, to then follow a flat path until the end of the sample.

panels. This is somewhat expected given Chile's low enforcement ability on drunk driving as presented in Section 2. Moreover, this result enhances the importance of regulation relative to police control as an effective mean in reducing accidents. At the same time, as also discussed, results suggest there are not any important impacts of unemployment. The same occurs with the gasoline real price. However, other traffic accidents are significantly different from 0 under every specifications highlighting the existence of traffic accident common shocks.

Next, we run estimation robustness checks and present the results in Table 8. In the first row we show the previous table's estimates corresponding to columns (2) and (4) as a benchmark for robustness comparisons. Columns (1) and (2) represent the lower bound, and (3) and (4) the upper one. All columns include municipality and month-of-the-year dummies, while municipality specific trends are only added to even columns.

[Insert Table 8 here]

In Panel A we regress the main specification (equation 5.1) but restricting the sample in order to observe heterogeneous impacts along the population dimension. The Metropolitan Region, where the capital is settled, has most of the population and economic activity and hence most of the traffic accidents. To test whether or not this region accounts for all of the change in accidents, in row (2) we exclude all 52 Metropolitan Region's municipalities. The findings suggest there are not any important effectiveness differences (around 1%) of the policy for both bounds. The results are however enhanced when the municipality population restricts the sample. Rows (3)-(5) present the law impact when excluding 25, 50, and 75% of the less populated municipalities (thresholds of 9,245, 18,455, and 52,252 people respectively). The impact estimates for each of these cases are 15, 19, 20% and 25, 27, 29% for lower and upper bounds respectively. These estimates indicate that the impact is likely to increase for more populated municipalities due to greater availability and usage of cars.

Given that accidents are count data, in Panel B we report estimates drawn from count models. For these models, as shown by Winkelman (2008) under a difference-in-differences framework the parameter accompanying the post dummy has a treatment effect identification. This is however true for small values of beta since this result comes from a linear Taylor approximation. Row (6) reports the estimates using poisson regression, and row (7) the ones from a negative binomial allowing for over-dispersion.[31] Results for both of the models are similar. Following Green et al. (2013), for the ease of interpretation we present incident rate ratios (IRR), so that this context provide the percentage change in alcohol-related accidents. The treatment effect is given by $\beta_1 - 1$. For example for the poisson regression, an IRR of 0.824 implies a 17.6% decrease in the number of alcohol-related accidents as consequence of the policy. When specific trends are not included, results are slightly greater than our lower and upper benchmarks respectively. However, including trends greatly increases the policy impact to a 30.3% or 38.7% decline for the lower and upper bound respectively.

[31] The poisson and negative binomial fixed effects model are nonlinear models where the incidental parameters problem does not apply, thus allowing the estimation. See Cameron and Trivedi (1998), pp. 280-282.

In order to check whether estimates suffer from zero inflation due to the many zero alcohol-related accidents realizations we also run a Zero Inflated Poisson model.[32] Accounting for zero inflation shrinks the lower and upper bound coefficients by 1.5% and 3% respect poisson estimates when specific trends are included and omitted respectively. Nonetheless, the estimated impact still much bigger than our benchmark estimates and it is not worrying since, as previously mentioned, great impact estimates should not be accounted as causal treatment effects.[33]

6.2 Differential Impacts for Accidents

As mentioned, we construct five different categories of accidents, based on their severity. Differential impacts of the law are shown in Panel A of Table 9 and total deaths, injuries and type of injuries, in Panel B. Again, specific time trends are only included in the even columns.

[Insert Table 9 here]

The first row presents the preferred specifications from Table 7. Remaining rows of the first panel show the impact on accidents given their severity. Also, the first column's parentheses exhibit the total number of positive realizations of the variable (the left corresponds to the lower bound accident variable and the right to the upper bound). When specific trends for each municipality are not included, results reveal a significant effectiveness of the law in decreasing all types of accidents for lower and upper bound coefficients. The estimate for fatality rate impact varies between 1.8-2.8%, which is close to an average of the impacts reported in the literature 3.1-7.2% (Dee, 2001; Eisenberg, 2003) and 0% (Freeman, 2007).

All the other results on accidents are novel, and thus do not have possible comparisons. For accidents involving serious injuries, results indicate a decline of 4-7%, which is around 1.5 times the impact on fatality rates. The effect on accidents with moderate injuries is somewhat lower varying between 3-5%. The law is equally efficient in reducing accidents with minor or none injuries, decreasing both by 9% for lower and 14% for upper bound. This analysis strongly suggests a higher effectiveness of the law in decreasing less severe accidents. This is however expectable since it is harder is to decrease accidents with deaths, serious or moderate injuries as they are more rare (see the realizations in each variable's parenthesis) and probably caused by heavier drinkers who are harder to influence legally. Remarkably, if specific trends are added, the impact on fatality rate and accidents involving serious or moderate injuries fades. Accidents with minor injuries maintain similar effects, and the impact on accidents without injuries increases. This specification indicates a null effectiveness of the law in reducing fatal and serious accidents.

The next panel reports the law's effect over total deaths and injuries. Again, the specification without trends gives significant reductions for every type of injuries and even deaths. Regarding

[32] Shankar et al. (1997) points out that zero inflated count models perform better than standard count models for crash counts estimates.

[33] As argued by Abouk and Adams (2013) this result might be explained due to the nonlinearity of the poisson modeling, which does not allow weight observations like the previous linear estimates.

this latter outcome, the results show a 2-3% reduction, greater than the 0.9% forecasted by Rizzi et al. (2011) for Chile.[34] We also present the impact over an injuries breakdown. Interestingly, injury reductions are now greater for serious that moderate injured (5-9% against 4-6%). Coinciding with the accident findings, when trends are included the impact only survives for minor and total injuries, without any impact on deaths or serious injuries. It is worth noting that the lack of measurable impact is not due to a loss of power in response to the degrees of freedom reduction when the trends are included, but due to point estimates close to zero.

6.3 Benefit Analysis

By using these estimates, a rough benefit analysis can be performed. We do not estimate the costs associated to the policy since we do not account the proper data. Given that there were 315 minor injuries a month in municipalities before the law was enacted, previous results suggest that minor injuries were reduced by 35 to 54 (for lower and upper bounds respectively) per month per municipality or roughly 141,372 to 218,484 minor injuries per year nationally.[35] Although Hojman et al. (2005) have calculations for the value of life and severe injury on traffic accidents for Chile, they do not report the value for other type of accidents.[36] CITRA (1996) in contrast has valuations for all accident types, but with estimates understated relative to other international evidence (Hojman et al., 2005).[37] In order to obtain a more precise estimate for the value of a minor injuries, we use the life value given by Hojman et al. (2005), but input CITRA (1996)'s minor injuries relative cost ratio.[38] After correcting for inflation it yields a value of US $14,000 per minor injury.[39] Therefore, just by assessing the direct impact on individuals, the policy benefits vary between US $ 2,000 and 3,000 million. Any cost of implementation under these values is thus supported by a simply economic rationale for legislation.

6.4 Anticipatory Effects

In this subsection we show important anticipatory effects of the law right after its announcement and give plausible reasons for this occurrence.

[34] Their analysis suggests a battery of policies that could reduce traffic fatalities in the short term. They use Chile as case study and propose that a ZT-law implementation should be effective in reducing fatalities.

[35] This is a rough calculation based on the number of minor injury reductions. Specifically 0.11 (lower bound estimate) $\times$ 315 (average municipality's minor injuries) $\times$ 340 (municipalities) $\times$ 12 (months). For the upper bound use 0.17 instead.

[36] They set up a survey in which individuals had to choose between two routes for a trip implicitly revealing their preferences for safety both in terms of reducing the number of fatalities and of severely injured victims.

[37] This is in part explained since they use the human capital approach.

[38] CITRA (1996)'s ratio between cost of deaths and serious injuries, moderate injuries, and minor injuries are of 0.5, 0.13, and 0.03 times respectively. We assume that the cost ratio between deaths and injuries for CITRA (1996) and Hojman et al. (2005) are similar for all injury types. The assumption is met for the ratio between deaths and serious injuries (Hojman et al. (2005)'s estimates for a life and a serious injury are US $ 300,000 and 140,000 giving a similar ratio between deaths and serious injuries).

[39] Updating the UF and exchange rate to current values (22,950 and 510 Chilean pesos respectively) yields a rough estimate of US $14,000. Specifically, US $300,000 (value of life) $\times$ 600 Chilean pesos (exchange rate in 2005) $\div$ 17,318 Chilean pesos (UF in 2005) $\times$ 0,03 (death to minor injuries ratio) $\times$ 22,950 chilean pesos (UF in 2013) $\div$ 510 Chilean pesos (exchange rate in 2013).

The law enactment followed a continuous process already described in Section 2. It was announced and passed three and two months, respectively, before it took effect, thus possibly impacting behavior before its enactment. This was clearly suggested by the descriptive statistics in Section 4. To identify this possible behavior we use the same main regression identification strategy, but include dummies for periods prior and post the enactment date. For this purposes, the parameter of interest is not the post dummy, but the marginal monthly effect captured for before (leads) and after (lags) the law. Estimates of leads different from zero indicate anticipatory effects, while significant lags are interpreted as the persistence of the effect.

$$y_{cit} = \alpha + \beta_1 \cdot zt_t + \sum_{\tau=-t}^{t} \beta_{1,\tau} \cdot zt_\tau + \beta_2 \cdot enforcement_{it-1} \\ + \gamma \cdot control\ accidents_{it} + X'_{it}\delta + \mu_i + \omega_t + u_{cit} \quad (6.1)$$

where the sum of leads and lags captures the anticipatory and lagged effect of the law, measuring whether the effects are just announcement effects or whether they persist over time. The dummy variables $zt_1 - zt_4$ captures the lag effects as $zt_{-4} - zt_{-1}$ measures the lead effect. For example, zt_4 takes the value of 1 if the law was enacted 4 months previously, and 0 otherwise. The same applies for leads, but for months prior to the law enactment. The specification includes municipality and month-of-the-year fixed effects, captured by μ_i and ω_t respectively. The same controls used in equation 5.1 apply. The dummies estimates are presented in Table 10.

[Insert Table 10 here]

Columns present the months difference respect enactment, and rows if it's a month before or after the law enactment. Panel A shows the impact evolution for the variable capturing the lower bound estimate and Panel B, the upper bound. Municipality specific time trends are included only in even columns. As noted by Abouk and Adams (2013), this lead and lags effect are "not to be interpreted relative to no legislation, rather, they are interpreted relative to an average treatment effect." Results indicate a significant effect after the third month prior to enactment, which coincides with the law's announcement and passage, suggesting an important anticipatory effects. In contrast, no effect is observed for the before periods (4 months, i.e. before the law was announced). Noteworthy, the impact increases to 10% in respect average treatment effect one period after the law takes effect. Subsequent periods show a slow but not significant reduction of the effect. The results and a 95% confidence interval are plotted by solid and broken lines respectively in Figure 5. We also add accidents caused by speed to serve as falsification exercise. The plot for alcohol-related accidents shows the same pattern as above. The same trends do not exist for speeding accidents (see Appendix E).

[Insert Figure 5 here]

Several reasons can explain individuals' anticipatory reaction. First, as shown in Figure 2, internets searches of the law steadily rose after its announcement peaking one month prior to the enactment's date. In addition, the bill's passage was announced in mid-January 2012 but was not passed until the end of January. Media coverage started right after this with fuzzy information regarding its starting date.[40] Moreover, semantic confusion of passing or enacting a law also seems to be a reasonable argument. An alternative explanation is that individuals desired to gradually learn on how to behave regarding the new law limits, and thus anticipated due to a self-lead learning process. This last explanation is however not likely. If learning is a gradual process, the anticipation should have been progressive and not drastic as shown in Table 10. The effect increased from 0 to 22% at the announcement, to slowly increase until the bill's enactment.

Next, we perform some falsification exercises using the announcement as the month setting the post dummy in order to capture these anticipatory effects. The results are presented in Panel A of Table 11. First we do not find any measurable impacts of the law on speed or imprudent driving, thus rejecting the traffic accident spillover hypothesis suggested by Rizzi (2012).[41] In row (3) we add one extra dummy capturing the period after law period for the previous year. If the law drives the entire impact, then the new dummy should not have any measurable impact. As expected this variable's point estimate is close to zero and not significant under every specification. Finally, in Panel B we show new estimates of the law's effect including the after-announcement period. If anticipatory behavior was to be observed, impact estimates should increase by capturing this extra impact. Results are displayed in rows (4)-(6), showing significant increases in respect previous estimates. When the announcement sets the treatment period, the law's impact is now a 28-32% reduction in alcohol-related accidents.

[Insert Table 11 here]

7 Mechanisms

The existing literature has used surveys on self-reported alcohol use and drunk driving, suggesting that ZT-Laws work through both reducing drinking and driving when drunk. In this section, using administrative records, we test the existence of these mechanisms. First we test whether the law had or not a deterring role on DUI and drunk offenders. Next, we use BAC of drivers pulled over by the police to assess for the impact on amount drunk.

7.1 Extensive margin mechanism

The offender deterrence mechanism is exploited in the extensive margin framework. The model predicts that an increase in the price of drunk driving that is not offset by an increase in the

[40] There is much anecdotal evidence that makes this point. See for example "Senado aprobó proyecto que aumenta sanciones por conducir en estado de ebriedad" Cooperativa. 31 Jan 2012.Web. 30 July 2013.

[41] The lower bound represents the point estimate coefficient and has not be interpreted as a bound as these accidents do not have the labeling issue that alcohol related accidents do. These regressions control for the same controls as the main regression 5.1, except for the *Other Accidents* variable which includes all non-alcohol-related accidents but speeding and imprudent driving respectively.

alternative transportation cost should:

1. Reduce the number of drunk drivers (offenders)
2. Increase the number of individuals drinking and not driving
3. Increase the number of sober drivers

We directly estimate the causal impact of the law on the number of offenders and indirectly test the remaining predictions.

7.1.1 Offenders deterrence

The Chilean ZT-Law has three potentially deterring features. First, penalties were severely increased, which, according to Becker (1968) and the following crime literature, should deter individuals from committing crime. Specifically, some previous literature has shown that drunk driving can be deterred by increased penalties (Ross, 1984; Nichols and Ross, 1990; Kenkel, 1993; Sen, 2005; Hansen, 2013). Second is an expected variation on police enforcement (i.e. the probability of being apprehended). Increasing this variable should have effects similar to penalties as both increase the expected cost of offending. This variation though, does not seem to be endogenous to the law, and thus controlling for its effect allows us to obtain an estimate of the law's deterring impact not influenced by enforcement variations (see Appendix F). Third, and finally, was a simultaneous legal BAC reduction. Regarding this aspect Grant (2010) has argued it may deter mild drinkers from driving, but increase the number of more dangerous drunk drivers. Due to the obvious confounding, the marginal deterring effect of BAC reductions or punishments increases cannot be disentangled. This caveat makes these results not comparable to the ones from crime models.

Chilean traffic regulation distinguishes among two possible alcohol-related offenders: drunk drivers (drunk) and drivers under alcohol influence (DUI). Graphs representing DUI and drunk driving exhibit decreasing trends right after the law's announcement (first solid line). In contrast, enforcement declines the first month after and then has an upward trend in the following months. Finally, DUI and drunk graphs show an expectable spike in September due to Chilean national celebrations.

[Insert Figure 8 here]

As previously discussed, we need to control for enforcement variations to assess the law's causal effect. Two concerns have to be addressed. First, when enforcement increases two opposites effects arise, *(i)* the expected deterring effect (fewer offenses), and *(ii)* a higher probability of finding offenders due to greater police manpower (more offenders). Which effect survives is an empirical question we do not intend to answer. To surpass this issue we use the rate of apprehended offenders per unit of enforcement as a dependent variable, thus dropping the manpower *(ii)* effect and isolating the deterring impact.

Second, reverse causality exists between the level of accidents and the number of police units; in more dangerous municipalities more there are more polices patrols, and conversely it results in reducing the municipality's accident rate. To address this issue we use the lagged realization of the police control variable thus avoiding any simultaneity concern. We also use fixed effects at the municipality level to account for any omitted variable threats and month-of-the-year dummies to control for seasonality. The estimated regression is

$$r_{it} = \alpha + \beta_1 \cdot zt_t + \beta_2 \cdot enforcement_{t-1} + \beta_3 \cdot enforcement \times zt_t + X'_{it}\delta + \mu_i + \omega_t + u_{i,t} \quad (7.1)$$

r_{it} is the number of offenders apprehended per unit of enforcement $\times$ 100. The remaining notations are the same as before. An interaction is also added to capture differential effects for the post law period. Recall that after the policy was implemented, it is expected that more offenders would be apprehended in response to lower legal limits. The interaction isolates this effect, and is hence expected to be positive. This same reasoning was used in previous sections for using lower and upper bound estimates. Results are presented in Table 12.[42]

[Insert Table 12 here]

Again, only even columns include specific trends. The results indicate that both, the law and enforcement are effective in deterring drunk-drivers, especially drunk offenders. The interpretation is that an increase in 1% of police enforcement (around 17 more police evaluations for the average municipality) results in a significant 1.6 percentage points decline in all offenders.[43] Moreover as expected, the interaction variable is positive and significant and close to a 0.5 percentage points increase. This leads to a net enforcement impact of a 1.1 percentage points decline in offenders per unit of enforcement. The law is 3.5 times more effective in reducing alcohol-related offenses per unit of police enforcement, with a 3.8-4.1 percentage points decline of offenses per unit of enforcement. Interestingly poorer municipalities have fewer offenders than richer ones (measured by unemployment rate).

The interpretations have two implicit assumptions regarding the production function of enforcement. We assume that there are not any complementarities between the law (penalties and BAC reduction) and enforcement, letting us interpret the interaction as a result of the legal BAC limit change, and thus the zt dummy as the unbiased impact of the law. Second we assume constant returns for the enforcement production function such that the cost of increasing the offender rate per enforcement unit is the same for every level of enforcement. This last assumption is however not empirically sustained. De Angelo and Hansen (2013) for instance, find decreasing non-linear returns of enforcement over speed offenses. Thus, the β_2 estimate should not be interpreted as the causal impact of enforcement on offenders rate.

[42] As a consequence of the reported anticipated behavior we also ran the regression using the announcement month. Results are statistically the same.

[43] The results have to be interpreted as a percentage point decline as the dependent variables is a rate $\times$ 100.

7.1.2 Driving substitutions

Following the model, fewer drunk drivers should exist due to sober drivers and/or those who drink but choose not to drive. We already show that the law reduces the number of drunk drivers. We proceed now to test the impact of the law on sober driving and drinking but not driving. Sober drivers should not be reflected in changes in car flow, as they still drive but with reduced alcohol consumption. In contrast, individuals who decided just to drink at the cost of not driving, should reduce the number of drivers and therefore the number of cars, especially at night time.

We indirectly test whether drunk drivers choose not to drive by observing traffic flows in five representative junctures of Santiago. If this hypothesis is true, then the total flow should have decreased due to less drivers.[44] We use hourly car counting data provided by the UOCT (Operative Unit of Traffic Control) in the Transportation Ministry. The analyzed period goes from January 1st 2010 to April 30th 2013. We collapse each counter's data to a daily panel and regress the logarithm of the number of cars against a ZT-Law post dummy including several fixed effects. In addition, specifications (3)-(4) and (7)-(8) include a daily time trend to account for general changes in the traffic flow on time. An estimation using the total sample is in the first four columns. Remaining columns only include nightly data. Night is defined as the hours between 6 pm and 6 am as previously suggested by the literature (NHTSA, 1999; Dee, 2001)

[Insert Table 13 here]

Table 13 presents the results. Only specifications for the nighttime have significant reductions on traffic flow. However, the effect fades once we account for time trends. This suggests that the flow was decreasing before the law took effect, and the decline therefore, does not reflect changes due to the law. The argument is supported by Figure 6 that presents a scatter plot of total traffic flow (the sum of the five junctures) over time[45]. We also plot a locally weighted regression that suggests that the traffic flow started to decline during 2010 before the ZT-Law was even proposed. After the law's announcement (solid line) and enactment (dashed line), a slight rise of the daily flow is observed. This increase is however not significant.

[Insert Figure 6 here]

The results indicate that the law did not reduce the number of drivers, thus suggesting that the deterring impact was mainly driven by sober drivers reducing their alcohol consumption enough to meet the new permitted BAC levels. An alternative explanation for this result could be that the drivers (i.e. agents drinking but not driving) reduction is happening, but is offset by an increase of taxi rides. We have no data to test this hypothesis. However, considering that the permits of taxis is fixed it seems unlikely that the volume of taxis could change to that extent.

[44] The streets are: *i)* Marchant Pereira w/ Matilde Salamanca (east-west), Providencia; *ii)* Asturias w/ Sevilla (west-east), Estación Central; *iii)* Asturias w/ Sevilla (east-west), Estación Central; *iv)* José Miguel De La Barra w/ Purisima (west-east), Santiago; and *v)* José Miguel De La Barra w/ Purisima (east-west), Santiago.

[45] If we analyze each counter separately, similar trends are observed.

7.2 Alcohol Consumption

Prior results suggest that the law deters offenders by reducing their alcohol consumption to new accepted levels. The literature has also discussed whether ZT-Laws work via reducing overall and/or drunk drivers' alcohol consumption. Although the evidence is mixed, there is some belief that these laws reduce alcohol consumption (Carpenter, 2004; Carpenter and Harris, 2005). More robust evidence in this direction is needed, in order to asses who's consumption is reduced.

We have records for every BAC exam requested by courts or offices police local in the Metropolitan Region, which we use to study the law impact on driver's consumption decisions.[46] Although the data might include exams requested due to other crimes besides alcohol-related driving, we expect the results to be unbiased as the sample's noise should the same before and after the law.[47] The estimates would be invalid if the omitted variables affect those other requests for BAC exams before and after the law's enactment period. This is unlikely.

For the period between January 2010 and December 2012 there were a total of 128,020 exams performed in different laboratories of the Metropolitan Region. Of those exams, 25,228 correspond to BAC scores larger than zero. Interestingly, for the post-period the reduction was stronger for men relative to women. After the law, male observations declined by 70% (17,580 to 5,207), while females only did by 22% (1,370 to 1,071). Indeed, before the law 93% (17,580 out of 18,950) of the observations with positive BAC were male, reducing to 82% in the post period. This suggests that the law shocked women's consumption less than men's. These data's descriptive statistics are included in Appendix G. In order to check this intuition, we use pooled OLS including laboratory and month-of-the-year fixed effects, and monthly trends for every specification. Table 14 presents the results.

[Insert Table 14 here]

Panel A shows the law's impact without including gender interactions. Column (1) reveals the impact on drivers' alcohol consumption when all observations of the sample are used, indicating a significant 0.068 BAC reduction due to the law. Remaining columns use subsamples depending on the BAC level: column (1) shows the impact on drivers who have drunk, while columns (3) and (4) present the impact on alcohol-related (namely DUI and drunk), and drunk offenders respectively. The idea of splitting the sample is that the subsample assesses only the impact on offenders, thus capturing the drunk drivers' intensive decision regarding alcohol consumption.

[46] Certainly the law might also indirectly work by reducing overall consumption. In order to test whether the law modifies alcohol use we would need a national level survey of consumption. Chile has a national level survey of those characteristics prepared by SENDA, but it is bi-yearly and distinguishes only for regions and not for provinces or municipalities. This complicates any good identification strategy.

[47] In Chile, the majority of criminal cases requiring BAC tests are driving while intoxicated or under the influence of alcohol, both defined in the law 18.290 (traffic law). These crimes use BAC and breathalyzer test as standard blood alcohol evidence essential to determine whether crime existed or not. In other crimes, however, BAC may eventually become evidence, but it is usually not relevant. Being drunk under the Chilean penal code do not excludes the intent to act. Notwithstanding, there are a few crime occurrences in which BAC exams are important. Therefore, not all, but most of the sample should correspond to driver data.

Interestingly, no effect on alcohol use is observed for drivers who have been drinking (BAC > 0 or greater). Results from previous sections suggest that offender reduction after the law was not due to a substitution towards not driving, but because an increase in sobriety. These results support that finding.

Panel B presents differential effects by gender. Interestingly, the gender breakdown in column (1) reveals that the effect is totally driven by men with a reduction in alcohol consumption of around 0.09 BAC.[48] These results come to support previous gender differential effects found by Carpenter and Harris (2005) for the United States. Meanwhile, women present a slight increase in alcohol use. Regarding the offenders, although columns (3) and (4) indicate that men decreased their alcohol consumption, this reduction is small for offenders with BAC>0.3, and close to zero for drivers with BAC>0.5. Results suggest overall that the law is not working via reducing consumption of individuals who are drunk and driving.

[Insert Figure 7 here]

Finally, we plot the BAC density distributions for men and women for the before and after periods in Figure 7. The (a) and (b) plots presents the BAC density when using only positive BACs and all BACs respectively. The first plot shows the pre-and post-law BAC distribution for men, showing a small but appreciable decline from middle consumption into low consumption. Additionally little bunching is observed at the 0.8 and 0.5 BAC levels for the before and after distribution respectively. This is expected, as both are the pre and post drunk thresholds. The next plot shows the same density but for women. The pre-law distribution is somewhat similar to the men's, also showing some bunching around 0.8. The post-law distribution is different, turning into a bimodal distribution with one mode close to the prior mode and the new one around 0.5.

8 Discussion and Conclusions

This paper is conclusive to show that alcohol-related accidents were strongly reduced following a ZT-Law enacted in Chile. Remarkably, the decline is driven mainly by minor crashes with any measurable impact on accidents involving deaths or serious injuries. we also investigate on the law's underlying mechanisms. The data shows no evidence of driving substitution towards not driving (i.e. alternative transportation). Consistent with results indicating a reduction of drunk offenders, we also find evidence suggesting that these laws work via reducing alcohol use of drivers. This, however, does not occur with consumption of agents who are drunk and driving.

All these results indicate that the law works mainly by inducing certain types of individuals to sober driving. As no impact is observed on serious crashes, this suggests that influenced drivers are likely mild drinkers and not dangerous drivers. These results are in line with some of

[48]This number is obtained by subtracting 0.113-0.024, the law and gender interaction respectively.

the latest evidence regarding BAC reduction's laws indicating that ZT-Laws are not effective in reducing traffic fatalities (Freeman, 2007; Grant, 2010). In this paper however, we also include differential impacts given crashes severity advancing the literature by shedding light on economic gains associated to these policies previously omitted. ZT-laws work, but not as well as we would like them to.

This paper also highlights the role of specific institutions that may differ from broad institutional measures. In spite of an important broad institutional quality, Chilean drunk-driving enforcing ability is modest and even lower than South America's average. Reinforcing the role of specific regulatory institutions, we find no impact of enforcement increases on alcohol-related accidents. The enforcement weakness however, gives this study more external validity respect to other developing countries in relation to the existing literature that analyzes countries (United States and Europe) with high ability on enforcing drunk driving laws.

Finally, regarding policy considerations, we present evidence of important anticipatory effects, likely driven by confusion and media fuzzy information. The leaded effect is however positive as the literature have shown that these laws have delayed effects due to costs in information acquisition, and thus highlights the role of media coverage in the implementation of these laws.

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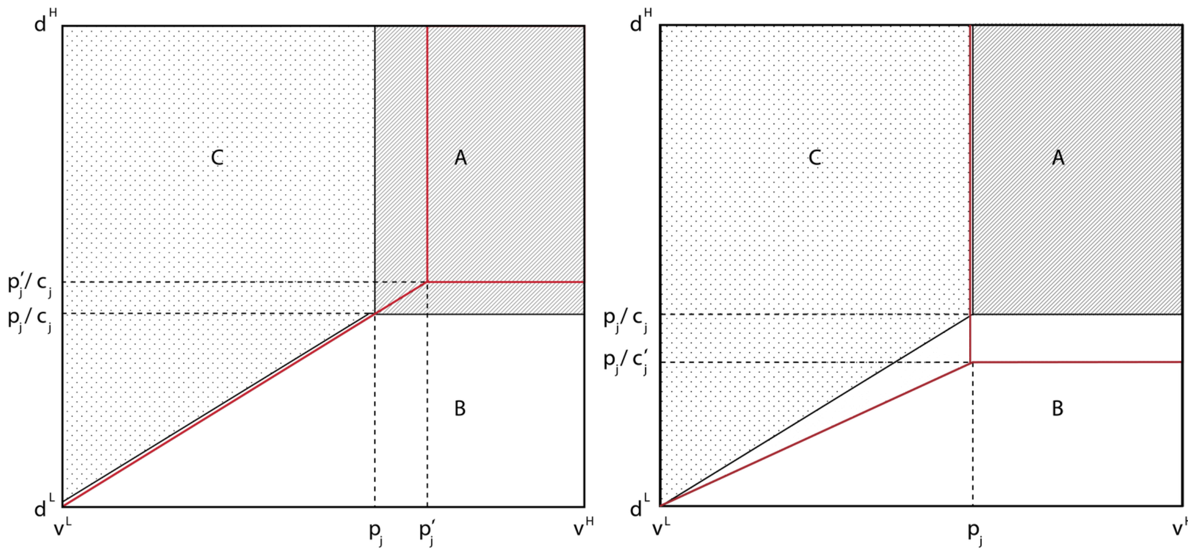
Figure 1: Model type-space and decisions



(a) Change in p_j (b) Change in c_j

Figure 2: Media Impact Index in time

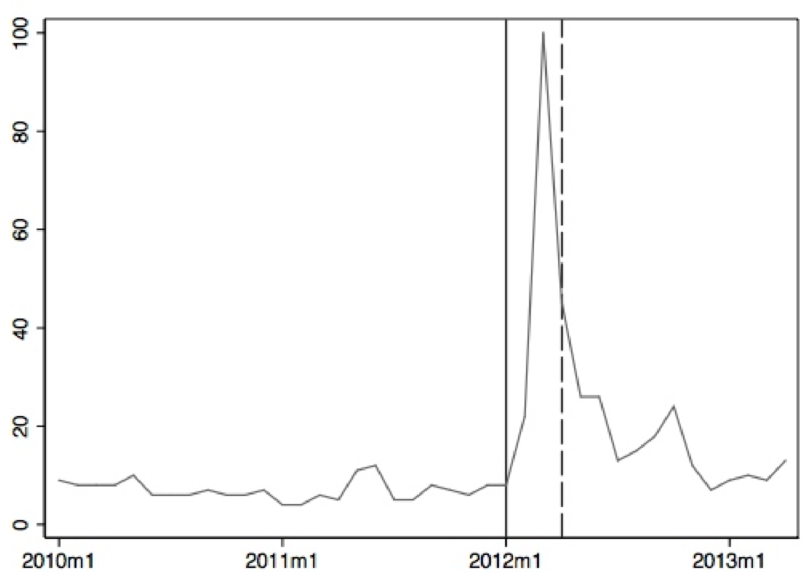
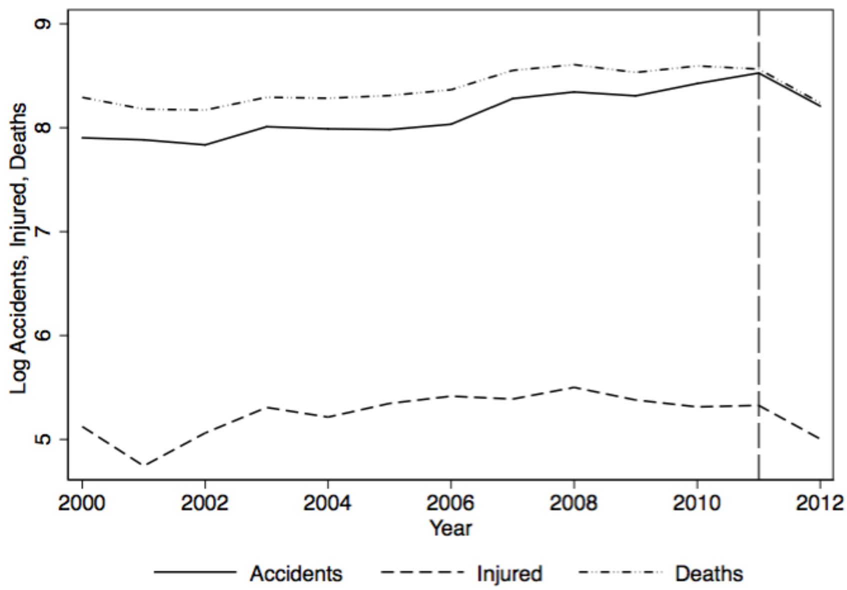
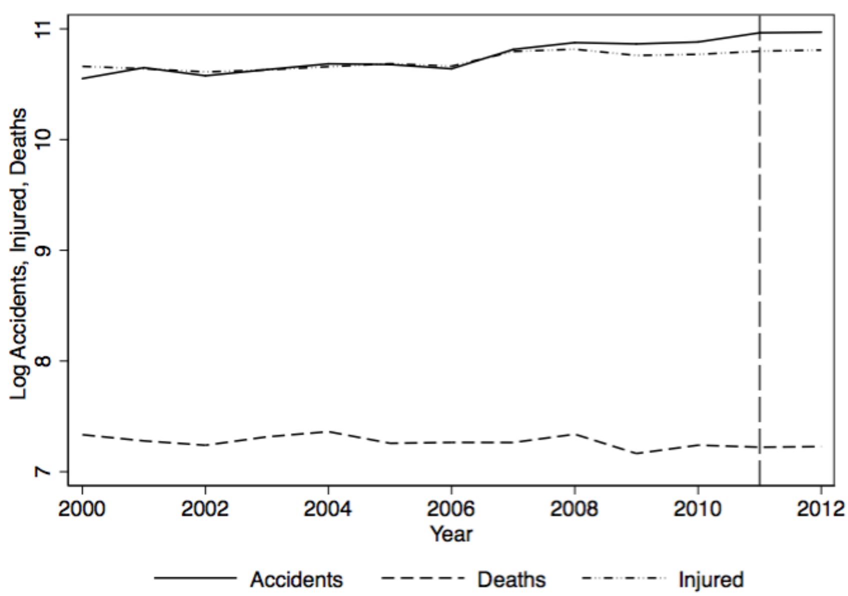


Figure 3: Alcohol-and non-alcohol traffic accidents evolution on time, 2000-2012

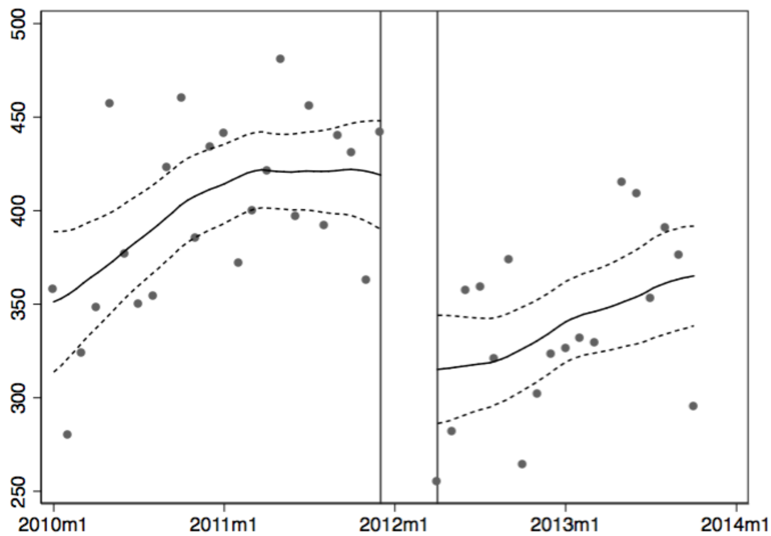


(a) Alcohol-related accidents

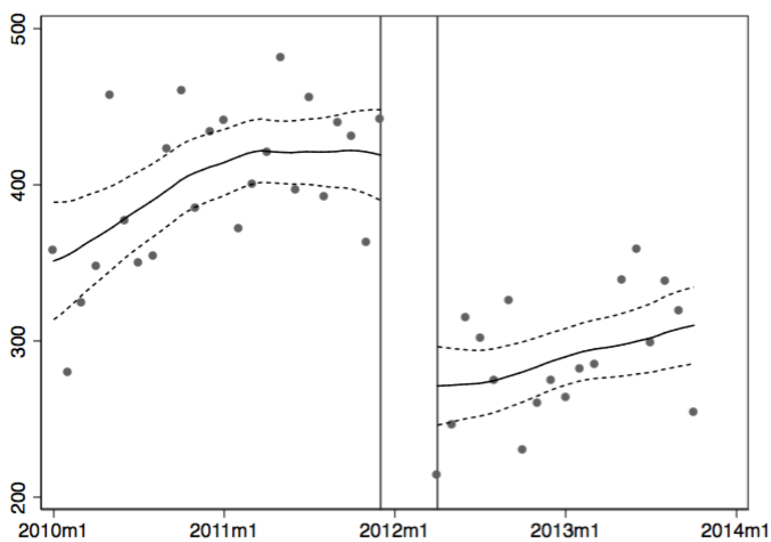


(b) Non-alcohol-related accidents

Figure 4: Donut Hole Regression Discontinuity

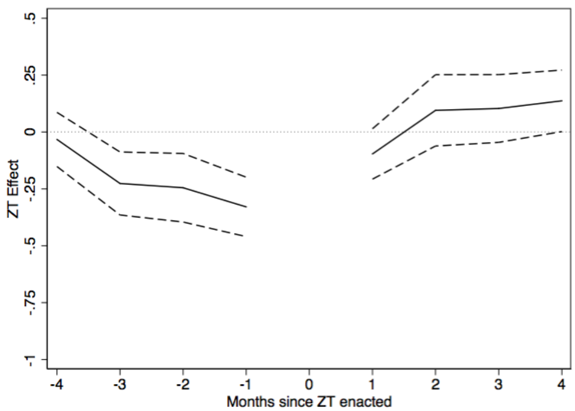


(a) Lower Bound

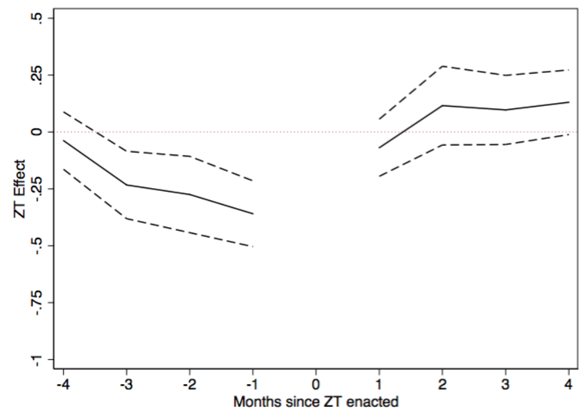


(b) Upper Bound

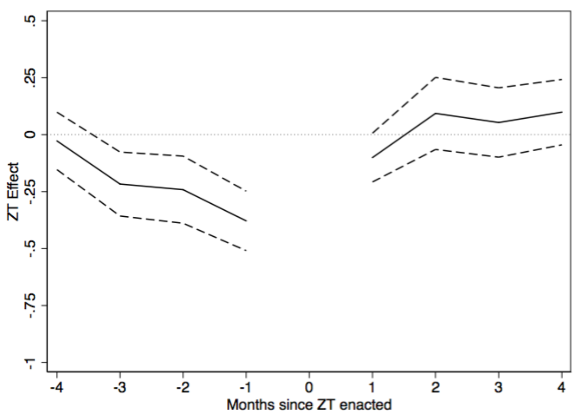
Figure 5: Leads and Lags ZT Effect on alcohol related accidents



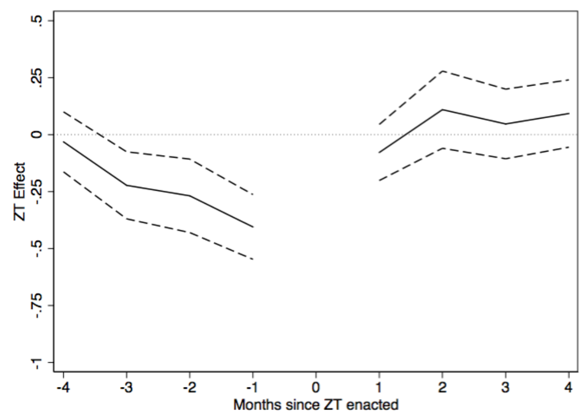
(a) Lower Bound



(b) Lower Bound (w/ specific time trends)



(c) Upper Bound,



(d) Upper Bound (w/ specific time trends)

Figure 6: Traffic Flow

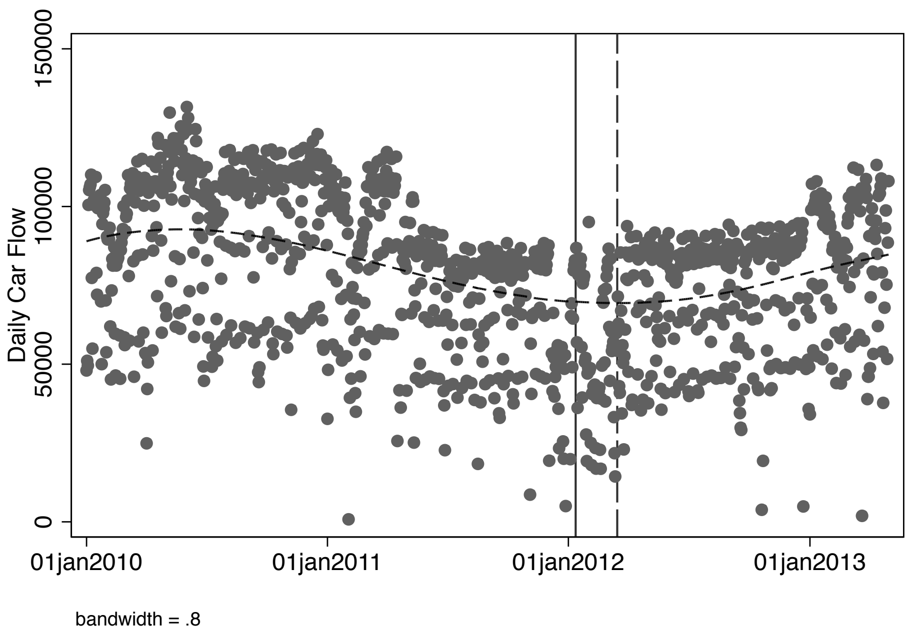
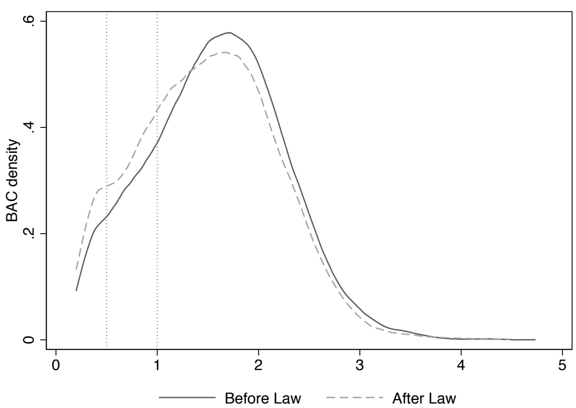
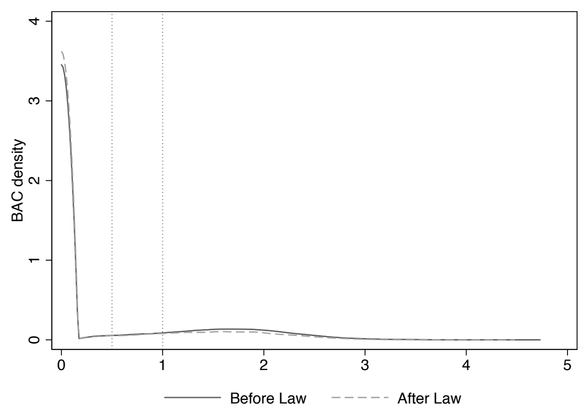


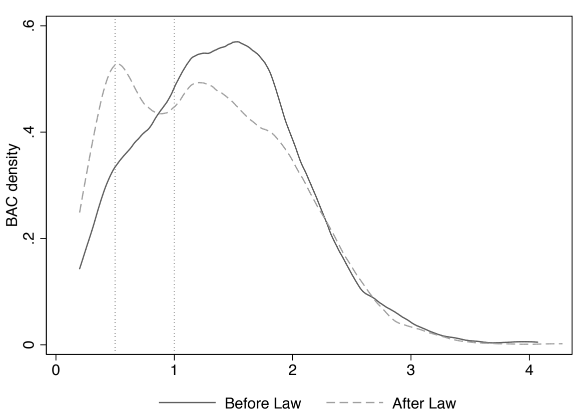
Figure 7: BAC Levels and Percentile Differences Before-After ZT-law



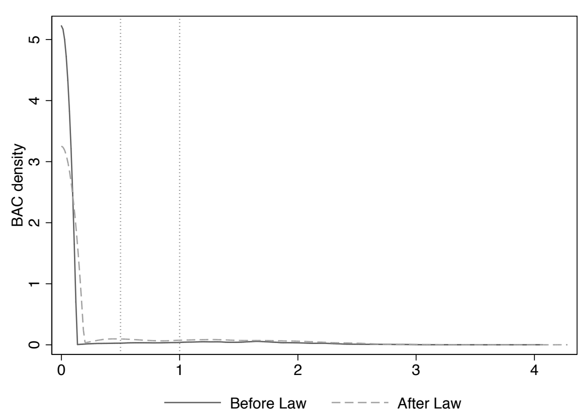
(a) BAC Levels (Only BAC>0, Males)



(b) BAC Levels (All BAC), Males

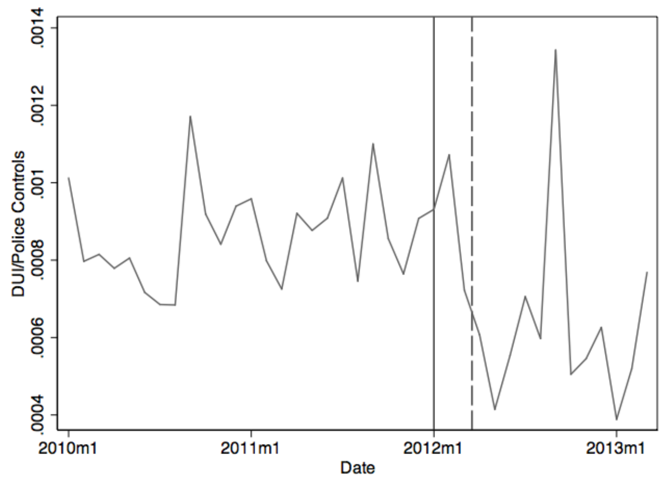


(c) BAC Levels (Only BAC>0, Females)

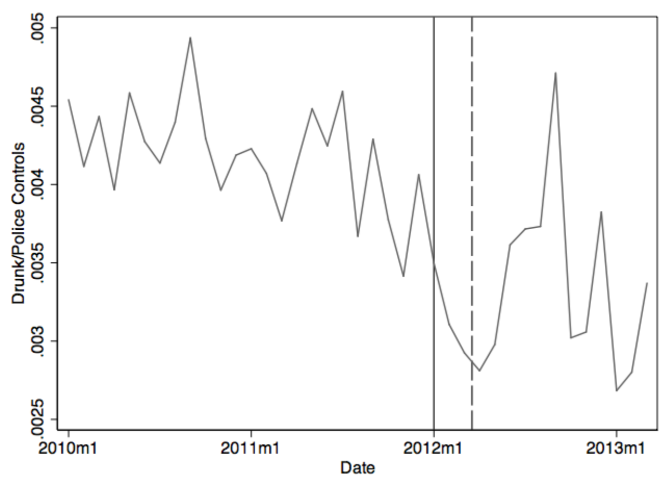


(d) BAC Levels (All BAC, Females)

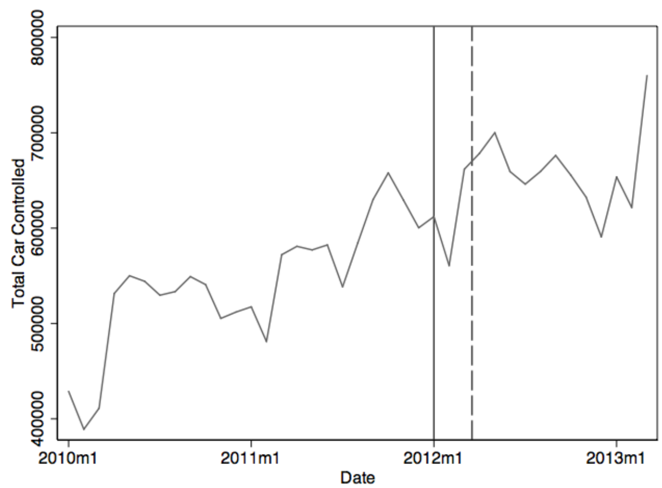
Figure 8: Offenders and Enforcement Evolution in time



(a) DUI



(b) Drunk



(c) Police Enforcement

Table 1: Comparative Law Penalties

BAC	Intoxication	Injuries	Suspension/prison	Fine
Panel A: Actual Law				
0,31-0,79	DUI	None or Minor Injuries	3-6 months / None	1-5 UTM
0,31-0,79	DUI	Moderate Injuries	9-36 months / 20-540 days	11-20 UTM
0,31-0,79	DUI	Serious injuries or dead	36-60 months / 3-5 years	21-30 UTM
0,8 +	Drunk	None or Minor Injuries	2 years / 61-540 days	2-10 UTM
0,8 +	Drunk	Moderate Injuries	3 years/ 541 days - 3years	4-12 UTM
0,8 +	Drunk	Serious injuries or death	Cancelation/3-5 years	8-20 UTM
Panel B: Previous Law				
0,5-0,99	DUI	None or Minor Injuries	1 month	
0,5-0,99	DUI	Serious injuries or death	1-2 years	
1 +	Drunk	No injuries	6-12 months	
1 +	Drunk	Serious Injuries or death	2-4 years	

Notes: In case of recidivism, the license's suspension periods are also increased. For the DUI fault, the second and third time have suspension times of 4 and 5 years respectively, greater than the prior 2 and 4 years. For drunk drivers, the second time carries a 5 years time suspension, and life-time cancelation after the third time.

Table 2: Dates of the Law Announcements

Date	Fact
May 8, 2011	Health minister firstly refers to the zero tolerance law.
May 21, 2011	President Piñera announces the law in his annual account.
Sept. 10, 2011	Senda's pilot "Control Cero Alcohol" program starts.
Dec. 13, 2011	Government's road safety program "Manéjate por la Vida" starts.
Dec. 26, 2011	Authorities announces the launch of the law by January 2012.
Jan. 11, 2012	Senate approves the law.
Jan. 17, 2012	Chamber of deputies ratifies all but two articles of the law.
Jan. 31, 2012	Senate approves mixed commission's report and dispatches the bill to be enacted.
Feb. 23, 2012	Government launches a massive media advertising campaign.
March 15, 2012	The law is enacted and takes effect.

Table 3: Model Predictions

Possible Cases	Marginal Effects $\Delta^+ p_j$	Marginal Effects $\Delta^+ c_j$	Sum of Effects $\Delta^+ p_j > \Delta^+ c_j$	Sum of Effects $\Delta^+ p_j \simeq \Delta^+ c_j$
Drink and Drive (A)	$-$	$+$	$-$	$-/+$
Drink and do not Drive (B)	$+$	$-$	$+$	$-/+$
Sober Driving (C)	$+$	$+$	$+$	$+$

Table 4: All accidents in time

	Accidents	Deaths	Injuries
Year 2010			
DUI	810	78	1,145
Drunk	3,751	124	4,256
All Alcohol Related	4,561	202	5,401
Non Alcohol Related	53,185	1,393	47,563
Total	57,746	1,595	52,964
Year 2011			
DUI	840	88	1,027
Drunk	4,206	117	4,215
All Alcohol Related	5,046	205	5,242
Non Alcohol Related	57,788	1,368	48,970
Total	62,834	1,573	54,212
Year 2012			
DUI	548	30	636
Drunk	3,130	118	3,147
All Alcohol Related	3,678	148	3,783
Non Alcohol Related	58,113	1,375	49,442
Total	61,791	1,523	53,225

Notas: Crashes involving drivers with BAC greater than 0.5 and 1 are labeled as DUI and drunk before March 15th 2012. For the following periods, the threshold changes to 0.3 and 0.8 respectively

Table 5: Averages of Alcohol and Non-Alcohol Related Outcomes and Control Variables

		Enactment			**Announcement**		
	All Months	Pre ZT Months	Post ZT Months	Difference	Pre ZT Months	Post ZT Months	Difference
Panel A: Alcohol Related Variables							
Accidents	351.6	388.6	314.7	-73.9***	399.42	305.44	-93.98***
	(14.5)	(22.8)	(10.7)	(25.1)	(10.15)	(11.18)	(15.43)
Deaths	13.1	13.8	12.5	-1.25	16.96	11.43	-5.53***
	(0.8)	(1.5)	(0.7)	(1.6)	(1.11)	(0.91)	(1.56)
Injuries	359	403.3	314.6	-88.8***	442.58	316.63	-125.96***
	(15.1)	(21.7)	(11.2)	(24.4)	(10.07)	(14.10)	(16.86)
Panel B: Non-Alcohol Related Variables							
Accidents	4,852.9	4,933.8	4,771.9	-161.9	4,621.4	4770.1	148.7
	(54.4)	(56.3)	(89.6)	(105.8)	(71.41)	(81.64)	(110.01)
Deaths	115.4	112.3	118.6	6.3	115	116.19	1.18
	(3.2)	(4.8)	(4.5)	(6.6)	(3.31)	(3.58)	(5.00)
Injuries	4,105.6	4,149,7	4,061.5	-88.2	4,01.8	4,104.9	86.12
	(60.2)	(87.6)	(84.4)	(121,7)	(50.16)	(89.52)	(95.29)
Panel C: Control Variables							
Accidents	129.9	130.8	127.9	-2.9	125.17	134.31	9.15
	(3.2)	(4.1)	(4.7)	(6.9)	(2.86)	(6.90)	(6.61)
Deaths	1.6	1.8	1.2	-0.7	1.75	1.31	-0.44
	(0.3)	(0.4)	(0.4)	(0.6)	(0.37)	(0.31)	(0.53)
Injuries	153	166	140	-26.08	155.67	153.88	-1.79
	(8.9)	(14.3)	(9.9)	(17.4)	(5.37)	(13.02)	(12.41)
Police	631,221	601,303	661,140	59,837***	540,692	610,520	69,829**
	(10,145)	(10,954)	(12,151)	(16,359)	(13,437)	(42,233)	(38,035)
Media	23.84	23.29	24.99	1.69	20.18	29.33	9.15***
	(2.24)	(3.05)	(2.89)	(4.84)	(0.84)	(5.27)	(4.40)
Gasoline	750.1	746.2	753.9	7.6	688.83	754	65.16***
	(4.1)	(3.2)	(7.5)	(8.18)	(10.73)	(5.64)	(13.98)
Months	39	27	12		24	16	
Total Observations	4,080	2,040	2,040		2,040	2,040	

Notes: Crashes involving drivers with BAC grater than 0.5 and 1 are labeled as DUI and drunk before March 15th 2012. For the following periods, the thresholds change to 0.3 and 0.8 respectively. Standard errors are in parentheses.

Table 6: All accidents grouped by cause

Group	**Percent**	**Total**
Panel A: All Causes		
Alcohol, DUI	1.9	2,398
Alcohol, Drunk	6.0	12,097
Alcohol, Pedestrian	0.7	1,552
Alcohol, Passenger	0.0	55
Backward Driving	2.3	4,681
Deficient Driving	0.9	1,813
Disobedience	9.8	19,723
Imprudence, Driver	19.6	39,656
Imprudence, Passenger	0.6	1,244
Imprudence, Pedestrian	5.2	10,465
Load	0.0	416
Loss of Control	7.2	14,609
No Attention	21.1	42,763
Others	7.6	15,473
Overtake	3.0	6,064
Random	**2.5**	**5,166**
Speed	2.5	5,158
Undetermined reasons	9.1	18,490
Total	**100**	**201,824**
Panel B: Random Causes		
Chassis	1.9	100
Steering	7.7	393
Electric	0.9	47
Brakes	29.8	1,545
Engine	1.9	96
Tires	23.9	1,235
Suspension	0.7	37
Animals on the road	29.2	1,509
Traffic light in disrepair	2.6	135
Improper or defective signaling	1.3	69
Total	**100**	**5,166**

Table 7: ZT-law Impact on Alcohol Related Accidents

	Dep Variable: Alcohol Related Accidents				
	(1)	(2)	(3)	(4)	(5)
Panel A: Lower Bound Estimates					
Zero Tolerance	-0.164***	-0.148***	-0.210***	-0.177**	-0.143*
	(0.054)	(0.049)	(0.077)	(0.076)	(0.074)
Enforcement_{t-1}	-0.034	-0.033	-0.010	-0.009	-0.010
	(0.030)	(0.028)	(0.025)	(0.023)	(0.024)
Control Accidents	0.120***	0.072***	0.073***	0.049**	0.052**
	(0.031)	(0.027)	(0.027)	(0.025)	(0.025)
Unemployment	-0.029	0.003	-0.137	-0.128	-0.152
	(0.096)	(0.091)	(0.098)	(0.093)	(0.093)
Gasoline real price	-0.066	-0.127	-0.424	-0.344	-0.124
	(0.289)	(0.280)	(0.404)	(0.401)	(0.386)
Other Accidents	–	0.239***	–	0.191***	0.193***
	–	(0.038)	–	(0.036)	(0.037)
Media	–	–	–	–	-0.111***
	–	–	–	–	(0.023)
R^2	0.642	0.659	0.692	0.700	0.703
Panel B: Upper Bound Estimates					
Zero Tolerance	-0.258***	-0.243***	-0.285***	-0.254***	-0.216***
	(0.052)	(0.048)	(0.073)	(0.073)	(0.071)
Enforcement_{t-1}	-0.031	-0.030	-0.007	-0.006	-0.006
	(0.029)	(0.027)	(0.022)	(0.021)	(0.021)
Control Accidents	0.115***	0.071***	0.068***	0.046*	0.048**
	(0.031)	(0.027)	(0.026)	(0.023)	(0.023)
Unemployment	-0.024	0.006	-0.152	-0.143	-0.170*
	(0.103)	(0.097)	(0.102)	(0.097)	(0.097)
Gasoline real price	-0.069	-0.127	-0.291	-0.215	0.029
	(0.292)	(0.284)	(0.392)	(0.388)	(0.374)
Other Accidents	–	0.225***	–	0.181***	0.183***
	–	(0.037)	–	(0.036)	(0.036)
Media	–	–	–	–	-0.123***
	–	–	–	–	(0.023)
R^2	0.645	0.660	0.698	0.706	0.710
Observations	12,920	12,920	12,920	12,920	12,920
Municipality-specific trends	No	No	Yes	Yes	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the natural logarithm of the number of alcohol related accidents + 1. Each specification includes municipality and month-of-the-year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality.

Table 8: Robustness Checks

	Lower Bound		Upper Bound	
	(1)	(2)	(3)	(4)
(1) Table 7 estimates	-0.148***	-0.177***	-0.243***	-0.254***
	(0.049)	(0.050)	(0.048)	(0.049)
Panel A: Restricted Sample				
(2) Excluding Metropolitan Region	-0.159***	-0.159***	-0.237***	-0.238***
(exclude 52 municipalities)	(0.044)	(0.045)	(0.044)	(0.046)
(3) Excluding 25% less populated	-0.154***	-0.148***	-0.253***	-0.246***
municipalities (population<9,245)	(0.051)	(0.051)	(0.049)	(0.051)
(4) Excluding 50% less populated	-0.160***	-0.188***	-0.264***	-0.273***
municipalities (population<18,455)	(0.054)	(0.085)	(0.053)	(0.081)
(5) Excluding 75% less populated	-0.182***	-0.198***	-0.296***	-0.291***
municipalities (population<52,252)	(0.062)	(0.099)	(0.060)	(0.094)
Panel B: Alternative Modelling				
(6) Poisson	0.824***	0.697***	0.706***	0.613***
	(0.038)	(0.058)	(0.034)	(0.054)
(7) Negative Binomial	0.838***	0.684***	0.710***	0.590***
	(0.040)	(0.086)	(0.035)	(0.053)
(8) Zero Inflated Poisson	0.853***	0.713***	0.734***	0.627***
	(0.036)	(0.056)	(0.339)	(0.524)
Municipality-specific trends	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Panel A reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the natural logarithm of the number of alcohol related accidents + 1. Panel B reported are coefficients from least squares and poisson regressions for 340 municipalities over 39 months. The dependent variable is the number of alcohol related accidents + 1. Each specification, in Panel A and B, includes municipality and month-of-the-year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality.

Table 9: Accidents Breakdown and Total Outcomes Estimates

	Lower Bound		Upper Bound	
	(1)	(2)	(3)	(4)
Panel A: Dependent Variable: Alcohol-related accidents breakdown				
Total Accidents	-0.148***	-0.177***	-0.243***	-0.254***
(N=14,202, 13,642)	(0.049)	(0.050)	(0.048)	(0.049)
Accidents with deaths	-0.018***	-0.001	-0.028***	-0.008
(N=520, 492)	(0.008)	(0.03)	(0.009)	(0.026)
Accidents with serious injuries	-0.037***	-0.027	-0.066***	0.053*
(N=1,891, 1,795)	(0.018)	(0.029)	(0.018)	(0.031)
Accidents with moderate injuries	-0.032***	0.006	-0.047***	-0.015
(N=1,072, 1,028)	(0.120)	(0.030)	(0.120)	(0.030)
Accidents with minor injuries	-0.088***	-0.098***	-0.137***	-0.132***
(N=5,518, 5,317)	(0.031)	(0.045)	(0.030)	(0.045)
Accidents with none	-0.089***	-0.175***	-0.144***	-0.217***
(N=5,201, 5,010)	(0.043)	(0.076)	(0.042)	(0.072)
Panel B: Dependent Variable: Alcohol-related total outcomes				
Deaths	-0.0215***	0.001	-0.032***	-0.006
(N=586, 553)	(0.009)	(0.027)	(0.010)	(0.028)
Total Injuries	-0.163***	-0.109*	-0.247***	-0.176***
(N=15,399, 14,786)	(0.041)	(0.066)	(0.040)	(0.069)
Serious Injuries	-0.047***	0.016	-0.085***	-0.048
(N=2,546, 2,416)	(0.021)	(0.035)	(0.022)	(0.039)
Moderate Injuries	-0.040***	0.011	-0.056***	-0.015
(N=1,568, 1,508)	(0.014)	(0.034)	(0.014)	(0.033)
Minor Injuries	-0.147***	-0.114*	-0.213***	-0.166***
(N=11,285, 10,862)	(0.039)	(0.059)	(0.038)	(0.062)
Observations	12,920	12,920	12,920	12,920
Municipality-specific trends	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the natural logarithm of the number of alcohol related accidents (outcome) + 1. Each specification includes municipality and month of the year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality.

Table 10: ZT-law Leads and Lags Impact

	Dep. variable: Alcohol Related Accidents							
	1 Month		2 Months		3 Months		4 Months	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Alcohol-related accidents Lower Bound								
Leads (−)	-0.329***	-0.359***	-0.245***	-0.275***	-0.223***	-0.233***	-0.033	-0.037
	(0.066)	(0.73)	(0.076)	(0.084)	(0.070)	(0.074)	(0.061)	(0.063)
Lags (+)	-0.097*	-0.069	0.095	0.115	0.103	0.098	0.137**	0.132*
	(0.056)	(0.063)	(0.079)	(0.087)	(0.076)	(0.0766)	(0.069)	(0.071)
Panel B: Alcohol-related accidents Upper Bound								
Leads (−)	-0.379***	-0.405***	-0.242***	-0.269***	-0.217***	-0.223***	-0.028	-0.032
	(0.066)	(0.072)	(0.075)	(0.082)	(0.071)	(0.075)	(0.064)	(0.067)
Lags (+)	-0.101**	-0.078	0.093	0.109	0.053	0.047	0.099	0.092
	(0.054)	(0.063)	(0.804)	(0.086)	(0.077)	(0.078)	(0.073)	(0.075)
Observations	12,920	12,920	12,920	12,920	12,920	12,920	12,920	12,920
Municipality-specific trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the natural logarithm of the number of alcohol-related accidents + 1. Each specification includes municipality and month of the year fixed effects, media, police control and gasoline price as controls. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality.

Table 11: ZT-law Impact using Leads and Falsification

	Lower Bound		Upper Bound	
	(1)	(2)	(3)	(4)
(1) Table 7 estimates	-0.148***	-0.177***	-0.243***	-0.254***
	(0.049)	(0.050)	(0.048)	(0.049)
Panel A: Alternative dependent variables and falsification (using announcement as post)				
(1) Speed accidents as dependent variable	-0.123	-0.021	–	–
	(0.083)	(0.080)	–	–
(2) Imprudent driving accidents as dependent variable	-0.065	-0.036	–	–
	(0.048)	(0.065)	–	–
(3) Estimating a new ZT dummy for one year before (April 2011)	-0.016	-0.055	-0.016	-0.019
	(0.038)	(0.047)	(0.038)	(0.045)
Panel B: Alternative leads and lags estimates				
(4) Using three months leads (Announcement)	-0.219***	-0.284***	-0.310***	-0.321***
	(0.053)	(0.063)	(0.053)	(0.060)
(5) Using two months leads	-0.200***	-0.284***	-0.295***	-0.337***
	(0.050)	(0.064)	(0.050)	(0.062)
(6) Using one month lead	-0.177***	-0.251***	-0.275***	-0.327***
	(0.049)	(0.064)	(0.049)	(0.062)
Municipality-specific trends	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the natural logarithm of the number of alcohol related accidents + 1. Each specification includes municipality and month of the year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality.

Table 12: ZT-law impact on DUI and Drunk offenders

	Dep. Variable: Alcohol-related Offenses					
	DUI		Drunk		All	
	(1)	(2)	(3)	(4)	(5)	(6)
Zero Tolerance	-0.347**	-0.225	-3.501*	-3.902**	-3.847**	-4.126**
	(0.154)	(0.199)	(1.843)	(1.724)	(1.920)	(1.837)
Enforcement$_{t-1}$	-0.133***	-0.134***	-1.413***	-1.523***	-1.546***	-1.657***
	(0.032)	(0.042)	(0.425)	(0.509)	(0.447)	(0.537)
Zero Tolerance	0.040**	0.025	0.429*	0.496**	0.468**	0.521**
$\times$ Enforcement$_{t-1}$	(0.020)	(0.025)	(0.228)	(0.229)	(0.238)	(0.243)
Unemployment	-0.049	-0.056	-0.687*	-1.078*	-0.737*	-1.134*
	(0.036)	(0.049)	(0.385)	(0.597)	(0.399)	(0.619)
Gasoline real price	0.051	0.098	0.116	1.084	0.166	1.182
	(0.130)	(0.244)	(0.785)	(2.214)	(0.849)	(2.326)
Observations	12,920	12,920	12,920	12,920	12,920	12,920
R-squared	0.210	0.237	0.182	0.208	0.202	0.228
Municipality specific trends	No	Yes	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the (rate of alcohol-related offenses per police control) $\times$ 100. Each specification includes municipality and month of the year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality

Table 13: ZT-Law impact on Traffic Flow

	Dep. Variable: Ln(Traffic Flow)							
	All Day				Night Only			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ZT-Law	-0.046	-0.059	0.050	0.046	-0.078*	-0.086**	0.027	0.048
	(0.044)	(0.039)	(0.079)	(0.071)	(0.040)	(0.037)	(0.070)	(0.066)
Observations	5,718	5,718	5,718	5,718	5,706	5,706	5,706	5,706
R^2	0.528	0.637	0.528	0.637	0.522	0.609	0.522	0.609
Trend	No	No	Yes	Yes	No	No	Yes	Yes
Day of Week F.E.	No	Yes	No	Yes	No	Yes	No	Yes
Month of Year F.E.	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from pooled OLS regressions. The dependent variable is the car flow level. Each specification includes specific car counter's fixed effects. Robust standard errors are in parentheses.

Table 14: ZT-law Impact on drivers' BAC levels

	Dep Variable: BAC level			
	All	BAC>0	BAC>0.3	BAC>0.5
	(1)	(2)	(3)	(4)
Panel A: Without Law and Sex interaction				
Zero Tolerance	-0.068***	-0.021	-0.016	0.005
	(0.008)	(0.023)	(0.023)	(0.026)
Sex ($1 = Male$)	0.182***	0.095***	0.091***	0.078***
	(0.011)	(0.018)	(0.019)	(0.019)
Observations	128,012	25,228	24,713	23,426
R^2	0.043	0.045	0.046	0.039
Panel B: With Law and Sex interaction				
Zero Tolerance	0.024**	0.056*	0.056*	0.101***
	(0.012)	(0.034)	(0.030)	(0.033)
Sex ($1 = Male$)	0.214***	0.129***	0.122***	0.119***
	(0.013)	(0.018)	(0.019)	(0.019)
Zero Tolerance	-0.113***	-0.089***	-0.082***	-0.110***
$\times$ Sex	(0.011)	(0.032)	(0.030)	(0.031)
Observations	128,012	25,228	24,713	23,426
R^2	0.044	0.046	0.046	0.039

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from pooled OLS regressions, for observations between January 2010 and December 2013. The dependent variable is the BAC level. Each specification includes monthly trends, laboratory and month of the year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same laboratory.

Table A.1: Summary of the Literature

	Traffic Fatality Rate Impact	Magnitudes Of the Impact	Laws for Youth only	Identification Issues	Clustered Standard Errors
Panel A: Traffic fatality rate as dependent variable					
Dee (2001)	Yes	7.2%	No	No	No
Eisenberg (2003)	Yes	3.1%	No	No	No
Freeman (2007)	No	–	No	No	Yes
Albalate (2008)	Partial	5.7-21%	No	No	Yes
Grant (2010)	No	–	Yes	No	Yes
Kuo (2012)	Lagged	16-36%	Yes	No	–

	Self-reported Drinking Impact	Alcohol-involved Driving impact	Laws for Youth only	Identification Issues	Clustered Standard Errors
Panel B: Survey's outcomes as dependent variables					
Wagenaar et al. (2001)	No	Yes	No	Yes	No
Carpenter (2004)	Yes	No	Yes	No	Yes
Carpenter and Harris (2005)	Yes	No	No	No	Yes
Carpenter (2006)	No	No	Yes	No	–
Liang and Huang (2008)	No	Yes	Yes	No	Yes

Notes: Kuo (2012)'s identification strategy does not need for clustered standard errors. She studies the impact of two different policies implemented in California that reduced traffic fatality rate in 36% and 16% after 3 and 2 years respectively. Albalate (2008)'s results show heterogeneous effects for different groups of drivers. Specifically the reductions where 5.7% for males, 11.5% for young road users, increasing to 9.2% and 21% in urban zones respectively. Carpenter (2006) does not account for any clustering, however it is likely to be done since his previous research with similar difference-in-differences strategies do so.

B ZT-law impact on pedestrians

In this appendix we show that the law has not had any unintended effect in pedestrians outcomes. First we use drunk pedestrians as dependent variable to check whether the law had any impact on individuals substituting drunk-driving for drunk-walking (or other transportation means substitutions). Since pedestrians and alcohol related accidents concentrates the first and second traffic accidents deaths leading causes, it has been suggested that the law would carry perverse effects through this margin. None of this is validated by the data. Results in table B.1, show in contrast, a low an non-significant decline in drunk pedestrians accidents. Using fatality rate result in even smaller estimates.

Table B.1: ZT-law impact on pedestrians accidents and fatality rate

	Accidents				Fatality Rate			
	Alcohol		No Alcohol		Alcohol		No Alcohol	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zero Tolerance	-0.021	-0.039	-0.043	-0.024	0.000	-0.002	-0.003	-0.017
	(0.014)	(0.035)	(0.026)	(0.053)	(0.005)	(0.014)	(0.011)	(0.029)
Enforcement$_{t-1}$	0.004	0.002	0.016	0.007	0.001	0.001	0.009	0.008
	(0.012)	(0.013)	(0.019)	(0.015)	(0.003)	(0.004)	(0.008)	(0.009)
Observations	12,862	12,862	12,862	12,862	12,862	12,862	12,862	12,862
R-squared	0.261	0.284	0.634	0.660	0.053	0.082	0.147	0.166
Municipality-specific trends	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 36 months. The dependent variable is the natural logarithm of the number of accidents (all or fatal) + 1. Each specification includes municipality and month of the year fixed effects. Gasoline real price, and other than pedestrian accidents as controls. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality.

C List of accidents causes

Table C.1: Accidents Causes

Cause	Number	Percent
Overtaking on bends crossing, slope, tunnel, etc	699	0
Overtaking without making corresponding signal	451	0
Overtaking without enough time and space	4,223	2
Forward by the berm	103	0
Forward surpassing continuous line	588	0
Animals on the road	1.509	1
Change of traffic lane unexpectedly	6,960	3
Loads slips on driveway	205	0
Greater than the permitted load to vehicle	36	0
Load obstructs driver visual	23	0
Load vehicle structure protrudes	152	0
Chassis	100	0
Unidentified motives	18,490	9
Driving under influence of drugs or narcotics	50	0
Driving under the influence of alcohol	2.398	1
Driving poor physical conditions (fatigue, sleep or others)	1.763	1
Driving against the traffic direction	1.116	1
Driving while intoxicated	12,097	6
Not attentive to driving traffic conditions	42,763	21
Driving on the left axis of the road	399	0
Driving without reasonable or prudent distance	17,651	9
Backwards driving	4,681	2
Disobey policeman indication	53	0
Disobey traffic light flashing	185	0
Disobey traffic red light	5,956	3
Disobey other signage	214	0
Disobey yield sign	5,563	3
Disobey stop sign	7.966	4
Steering	393	0
Drunk passenger	55	0
Drunk pedestrian	1,552	1
Electric	47	0
Brakes	1.545	1
Escape by criminal act	448	0
Recklessness passenger	342	0
Recklessness pedestrian	1,987	1

Table C.1: Accidents causes (continued)

Motor	96	0
Tires	1.235	1
No respect pedestrian passage	4,542	2
No respect vehicle passage	5,316	3
Other causes	14,684	7
Passenger goes up or down while the vehicle is moving	747	0
Passenger traveling in the vehicle sill	155	0
Pedestrian crosses pedestrians step out	1,280	1
Pedestrian crossing road or highway with no precautions	702	0
Pedestrian careless crossing	5.719	3
Pedestrian remains in the driveway	777	0
Loss control of the vehicle	14,609	7
Traffic light in disrepair	136	0
Improper or defective signaling	69	0
Suicide	21	0
Suspension	37	0
Vehicle detention without signaling or deficient	99	0
Greater than the permitted speed	261	0
Lower than minimum speed	7	0
Not reasonable or prudent speed	3.707	2
Not reduce speed in intersection, road, etc	1.132	1
Excess of speed in restricted zones	58	0
Improper turning	3,672	2
Total	201.824	100

Notes: This table includes all traffic accidents recorded by Carabineros de Chile for the period between January 2010 to April 2013

D Bounds formalization

In this appendix we formalize the upper and lower bound intuition presented above. Let's define y_{it} as a count variable of the number of accidents, and δ_{it} as a count function such that:

$$y_{it} = \sum \delta_{it}, \qquad \text{where } \delta_{it} = \begin{cases} 1 & \text{if BAC}_{it} \geq 0.5 \\ 0 & \text{otherwise} \end{cases}$$

$$\tilde{y}_{it} = \sum \tilde{\delta}_{it}, \qquad \text{where } \tilde{\delta_{it}} = \begin{cases} 1 & \text{if BAC}_{it} \geq 0.3 \\ 0 & \text{otherwise} \end{cases}$$

Hence, by construction $\tilde{y}_{it} \geq y_{it}$. Redifining:

$$y_{it} \equiv \tilde{y}_{it} - \nu_{it}, \qquad \text{where } \nu_{it} \geq 0$$

Defining d_t as a before after dummy, we can express the accidents dependent variable as:

$$y^*_{it} = \tilde{y}_{it} d_t + y_{it}(1 - d_t) \qquad \text{where } d_t = \begin{cases} 1 & \text{if } t \geq \text{March 2012} \\ 0 & \text{otherwise} \end{cases}$$

We would like to regress: $y_{it} = x_t\beta + \varepsilon$, where x_t is the zt_t (or post) dummy. However, we are able only to estimate: $y^*_{it} = x_t\beta + \varepsilon$. Therefore, the estimates can be expressed as:

$$\begin{aligned}
\hat{\beta}^* &= \frac{\sum x_t y^*_{it}}{\sum x_t^2} = \frac{\sum x_t(y_{it}(1 - d_t) + \tilde{y}_{it} d_t)}{\sum x_t^2} \\
\hat{\beta}^* &= \frac{\sum x_t(\overbrace{y_{it}(1 - d_t) + y_{it} d_t}^{y_{it}} + \nu_{it} d_t)}{\sum x_t^2} \\
\hat{\beta}^* &= \frac{\sum x_t y_{it}}{\sum x_t^2} + \frac{\sum x_t \nu_{it} d_t}{\sum x_t^2}
\end{aligned}$$

Recall that $x_t = d_t \in \{0, 1\}$ since both are post dummies. Therefore,

$$\begin{aligned}
\hat{\beta}^* &= \hat{\beta} + \frac{\sum x_t \nu_{it}}{\sum x_t^2} \\
\hat{\beta}^* &= \hat{\beta} + \frac{plim \frac{1}{n} \sum x_t \nu_{it}}{plim \frac{1}{n} \sum x_t^2}
\end{aligned}$$

Since $\nu_{it} \geq 0 \ \forall \ it$, it can be proved that

$$\begin{aligned}
plim \frac{1}{n} \sum x_t \nu_{it} = A &\geq 0 \\
plim \frac{1}{n} \sum x_t^2 = B &\geq 0
\end{aligned}$$

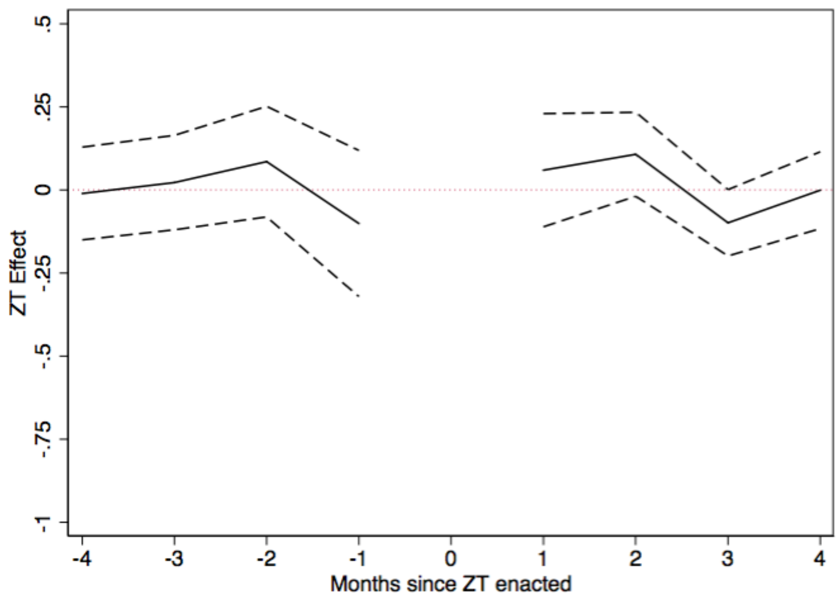
Then:

$$\hat{\beta}^* = \beta + C, \qquad \text{where } C = \frac{A}{B} \geq 0$$

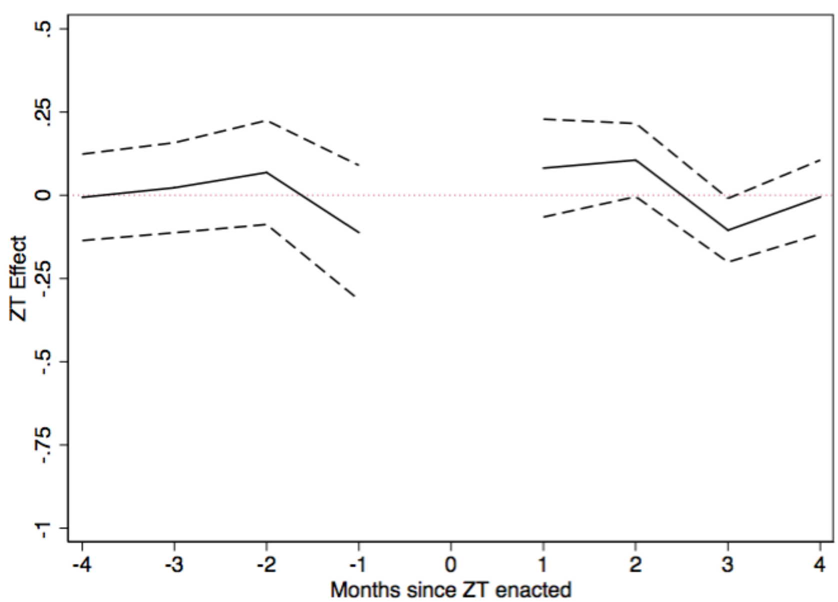
Since $\beta \leq 0$ and $C > 0 \implies \hat{\beta}^*$ is a lower bound. For the upper bound the same logic applies, but using 0.8 instead of the 0.3 BAC threshold. The results are obviously the other way around.

E Leads and lags ZT-Law falsification

Figure E.1: Leads and Lags ZT Effect on speed accidents



(a) Lower Bound, includes municipality fixed effects



(b) Lower Bound, includes municipality fixed effects and specific time trends

F ZT-law impact on police enforcement

Table F.1: ZT-law impact on police enforcement

	Dep. Variable: Police Enforcement							
	Enactment				**Announcement**			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Zero Tolerance	-0.088	-0.084	-0.088	-0.084	–	–	–	–
	(0.078)	(0.077)	(0.071)	(0.078)	–	–	–	–
Zero Tolerance	–	–	–	–	-0.072	-0.017	0.091	-0.018
(Announcement)	–	–	–	–	(0.084)	(0.075)	(0.068)	(0.075)
Unemployment	-0.297**	-0.282**	-0.234***	-0.292**	-0.271**	-0.304**	-0.239***	-0.314**
	(0.130)	(0.124)	(0.088)	(0.127)	(0.126)	(0.131)	(0.089)	(0.134)
Gasoline real price	0.967***	1.086**	0.066	1.092**	1.024***	1.493***	0.818**	1.496***
	(0.362)	(0.471)	(0.337)	(0.475)	(0.374)	(0.554)	(0.385)	(0.559)
Police related	-0.035	-0.041**	-0.019	-0.042**	-0.032	-0.039**	-0.016	-0.040**
Accidents$_{t-1}$	(0.031)	(0.018)	(0.018)	(0.018)	(0.031)	(0.018)	(0.018)	(0.018)
Observations	8,186	8,186	6,396	7,275	8,186	8,186	6,396	7,275
R^2	0.706	0.786	0.891	0.780	0.705	0.786	0.891	0.780
Exclude if pop<9,245	No	No	No	Yes	No	No	No	Yes
Exclude if RM	No	No	Yes	No	No	No	Yes	No
Municipality-specific trends	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Significance level *** p<0.01, ** p<0.05, * p<0.1. Reported are coefficients from weighted least squares regressions, weighted by municipalities' number of motorized vehicles for 340 municipalities over 39 months. The dependent variable is the natural logarithm of the police enforcement + 1 Each specification includes municipality and month of the year fixed effects. Standard errors are in parentheses and are clustered to allow for non-independence of observations from the same municipality

G BAC's descriptive statistics

Table G.1: BAC sample before and after ZT-law

	Before ZT-law			After ZT-law			
	Men	Women	All	Men	Women	All	Total
Panel A: All observations							
Observations	77,534	16,019	93.553	27,984	6,483	34,467	128,020
Mean	0.356	0.119	0.315	0.277	0.212	0.265	0.302
Panel B: Only observations with $BAC > 0$							
Observations	17,580	1,370	18,950	5,207	1,071	6,278	25,228
Mean	1,568	1.388	1.555	1.492	1.284	1.456	1.531

Notes: Only observations with gender's information are included. The remaining 1,926 observations are dropped and not included in the sample. Including these however, does not change any regression result.