Red Nacional de Investigadores en Economía

Not a Sip: Effects of Zero Tolerance Laws on Road Traffic Fatalities

Andrés Ramasco (University of Notre Dame)

DOCUMENTO DE TRABAJO N° 289

Noviembre de 2023

Citar como:
Ramasco, Andrés (2023). Not a Sip: Effects of Zero Tolerance Laws on Road Traffic Fatalities. Documento de trabajo RedNIE N°289.

Not a Sip: Effects of Zero Tolerance Laws on Road Traffic Fatalities.

Andrés Ramasco *

November 9, 2023

Abstract

Curtailing alcohol-related traffic fatalities is especially important for policymakers. I study whether there is an effect on Health Outcomes related to traffic accidents caused by Zero-Tolerance Laws and the mechanism driving these effects. Using Fatalities and Injuries counts at the county level. I exploit time and geographic variation in adopting the laws in a Difference-in-Differences framework. I find no sizeable reductions in various health outcomes, including traffic fatalities. I also test for heterogeneity across age groups, finding no significant differences. I propose and evaluate the persistence of drinking behavior and alcohol-related Hospitalizations as mechanisms explaining the null effects, finding no significant changes in several measures of alcohol consumption.

*University of Notre Dame. Contact: aramasco@nd.edu. I would like to express my sincere gratitude to my classmates, Xinyi Li, Pietro Pellerito, and Virna Vidal-Menezes, for their helpful insights and support throughout the research process. I thank Ethan Lieber, Victoria Barone, and Development Lunch Seminar attendants for their valuable feedback. I am especially grateful to Francesco Angeli for his priceless help with data collection.

1 Introduction

According to estimates from the World Health Organization, traffic fatalities are one the main causes of accidental death worldwide, accounting for more than 1.25 million deaths a year and presenting a growing trend. The burden of injuries related to Road-Traffic Accidents has also been growing in recent decades (Wang et al., 2019). This problem is exacerbated in developing countries by the industrialization phenomenon, which creates mismatches between vehicle fleets and urban and road infrastructure (Iwata et al., 2010,Kopits and Cropper, 2005, Law et al., 2011, Bener et al., 2011).

Particularly, for the country of study, Argentina, Road Traffic fatalities (RTFs) represent the leading cause of accidental death. Despite extensive road safety campaigns and efforts to improve road infrastructure, RTFs in the country have shown a troubling trend of remaining relatively flat or even increasing in recent years. While these campaigns have sought to educate the public about responsible driving behaviors and the importance of adhering to traffic laws, the persistence of high or rising fatality rates underscores the need for more comprehensive and effective measures to address this critical issue. According to the National Road-Traffic Observatory of Argentina, drunk driving accounts for approximately 30 percent of Traffic fatalities. Therefore, during the last decade, media campaigns and policymakers have stressed the necessity for stricter DUI policies. Nevertheless, empirical evidence regarding laws that deter drunk driving is limited for Argentina specifically but also for other countries in Latin America and the Developing World. To address the problem of drunk driving, governments across Latin America have enforced several policies but one particular policy was especially relevant: lowering the maximum Blood Alcohol Content(BAC) allowed for drivers.

Although stricter drink-driving laws are studied in depth for the US and

Western European countries, there is limited empirical work on extreme cases of DUI policies such as ZTL, especially for developing countries. This article aims to fill that gap by evaluating the effect of Zero-Tolerance laws on Traffic fatalities, which reduced the DUI limit from 0.05 to 0. These laws were implemented at the state or county level in Argentina, and from 2014 to 2022, 13 out of 24 provinces [1] implemented this reform. Additionally, three counties located in provinces where this new policy is not implemented passed the law at the local level. Nevertheless, the effect of ZT laws has not been carefully studied at the state level or in other countries that passed similar reforms recently, such as Uruguay and Colombia.

Argentina stands out as a good candidate to study this phenomenon, given that, unlike other countries that implemented these laws in Latin America, Argentina is a federally organized country with state police forces, which are the agencies in charge of enforcing DUI-related laws. At the same time, unlike the cases of Uruguay or Chile, ZT laws in Argentina have been implemented in staggered rollouts across time, which allows for better identification. Lastly, data availability at the county level provides more detailed information to compare treated to non-treated units.

I conduct an empirical analysis relying on several administrative datasets and survey data. I use information on road traffic fatalities and Injuries from the Ministry of Security of Argentina. Second, I complement this data with vital statistics from 2005-2021 provided by the Ministry of Health of Argentina. To establish whether a state or county is treated, I construct a database that relies on official government bulletins and legislative digests from each sub-national unit. Additionally, I rely on the National Risk Factors Survey and data on Hospital Discharges to test plausible mechanisms, using information regarding substance use, related behaviors, and alcohol poisoning hospitalizations.

I rely on the staggered adoption of Zero Tolerance Laws as a source

[1] Henceforth referred to as *states*

of exogenous variation and conduct a differences-in-differences exercise using never-treated units (counties or states) as the comparison group. To control for possible confounders, I include a set of time-varying state-level controls from a nationally representative Household Survey and vehicle registration counts from the National Registry of Automotive Property. I find non-significant effects of the law on fatalities, rejecting a negative effect of a magnitude larger than eight percent, and find a positive and significant effect on Traffic Injuries. When analyzing the Event study specification, I find a short-term increase in both outcomes. All these estimations show similar effects across age groups.

While I cannot identify the specific channel explaining the non-negative results, I analyze two plausible mechanisms: First, I test for behavioral changes in a subset of treated states, where I observe self-assessed measures of alcohol consumption, binge drinking, abusive episodic consumption or drunk driving. While I found reductions in binge drinking, the estimates for the rest of the variables point to a lack of systematic changes regarding alcohol consumption, especially when observing the lack of change in drunk driving. Second, I look at alcohol poisoning hospitalizations, finding no effect on hospital discharges from ZT Laws.

This paper contributes to two strands of the literature. First, it contributes to the branch of the literature that analyzes stricter drink-driving policies. Although extensive literature has contributed to explaining the effects of different related interventions, most of these are focused on Western European countries (Francesconi and James (2021), Norström and Laurell (1997), Lindo et al. (2016), Chang et al. (2020).), and in the US (Carpenter and Dobkin (2009), Benson et al. (1999), Carpenter (2004), Kenkel (1993), Ruhm (1996), Sloan et al. (1995)). However, these policies are not studied in depth for developing countries. Although some countries in the developing world have been implementing ZT laws as an instrument to reduce traffic fatalities (ANSV, 2022), more empirical evidence is needed to assess their effectiveness from a welfare perspective. It complements previous limited evidence for Latin American countries. Otero and Rau (2017) analyze a similar intervention in Chile, although the implementation at the federal does not allow for clear comparison across geographical units differentially affected. Guimarães and da Silva (2019), analyzes a dry law in Brazil, although it lacks a clear causal interpretation since it analyzes the policy from a time-series framework. It also contributes by analyzing the impact a more granular geographical area (counties).

Second, it contributes to the literature by analyzing an extreme version of BAC reduction, such as this Zero-Tolerance Law. Although some papers in the US have studied it in the context of the Minimun Driving Legal Age (MDLA) (Evans et al. (1991), Dee (1999)), there are not many articles studying interventions of this nature for the whole population. In particular, it studies the effect of Zero-Tolerance laws from a more clear causal inference perspective since the only previous contribution studying a similar policy (Davenport et al. (2021), for the case of Uruguay) analyzes it from a synthetic controls framework, constructing a synthetic Uruguay using Chilean counties, which raises concerns about the validity of the assumptions.

2 Background

Argentina is a federally organized country with 23 states and an autonomous city containing the capital. Each state is subdivided into departments or *partidos*, which I will call *counties* henceforth. Although the federal legislative power is responsible for establishing general guidelines for traffic laws, provincial and local legislatures may opt to adhere to the national guidelines or pass their own (more lenient or stringent) legislation. As of 2013, the standard nationwide threshold was 0.05 grams per deciliter [2]. This was the case of the state of Cordoba, which passed a Zero Tolerance

[2] or 0.5 grams per liter

(ZT) law in 2014, being the first sub-national government to do so. Over the following years, thirteen states and five cities passed a ZT law, deviating from the national guidelines.

In most cases, these Zero Tolerance laws modify the existing legal framework by only changing the maximum Blood Alcohol Content (BAC) threshold on Breathalyzers. [3] In most DUI cases, the probability of being imprisoned or facing charges is null unless an accident and fatal victims are involved; this did not change substantially with the new laws, according to legal digests from provincial and local legislatures (SNEEP, 2022).[4] This policy represents a unique intervention since there are not many documented cases of ZT laws for the entire population except for countries that ban drinking and alcohol sales in general (e.g., some Muslim-majority countries).

In 2013, before the passing of the first ZT law in Cordoba, the rate of road-traffic fatalities (RTFs) per 100,000 people at the national level was 13.6, a figure slightly lower than the regional average for the Americas. Nevertheless, unlike the rest of the region, RTFs represent the leading cause of accidental death among individuals between 14 and 49 years old, and according to administrative data, approximately 30 percent of these fatalities are related to impaired drivers. Therefore, these laws could be seen as an effort to curb the count of deaths linked to drunk driving, as advertised by NGOs and government media campaigns. Figure 2 shows which states implemented a ZT law by 2021. We can see substantial variation in treatment status across regions, suggesting better comparability across units, which I will document in the Balance Tables in the next section.

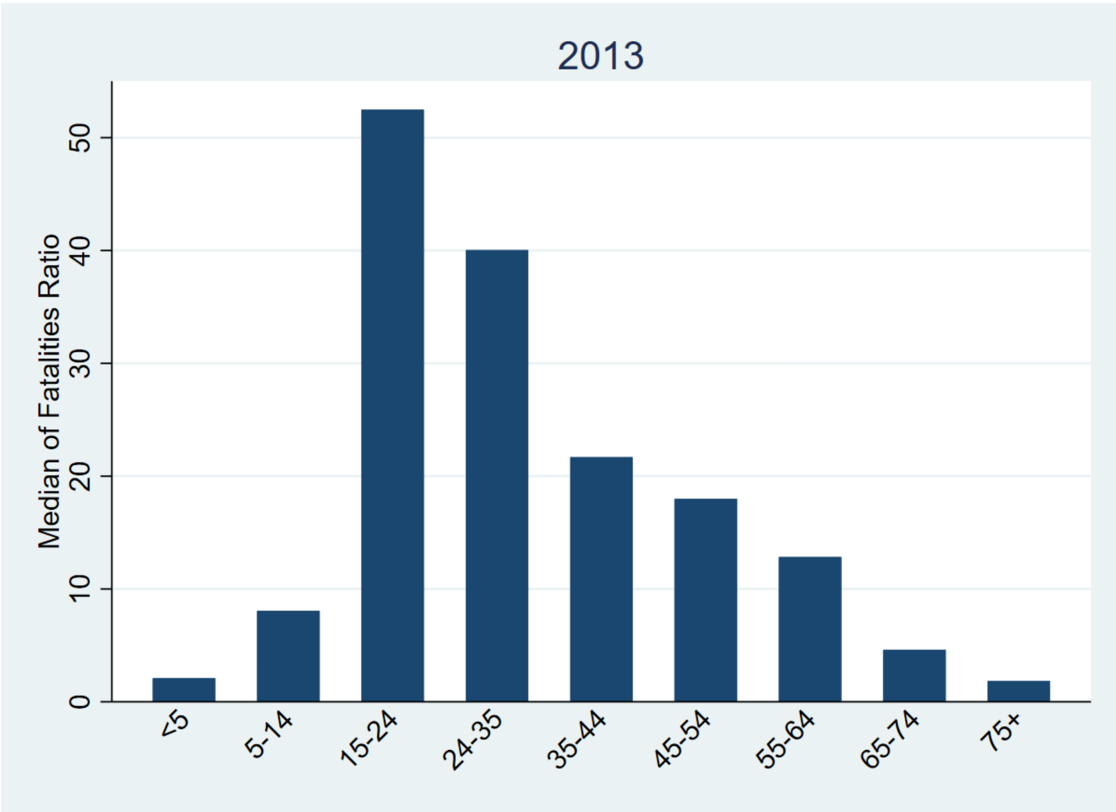
While the enforcement of this particular law varies across districts, the most standard procedures documented by the National Observatory of

[3]Depending on the state, some breathalyzers allow for a maximum BAC of between 0.01 and 0.02 g/dl to minimize false positives due to measurement error

[4]Monetary penalties vary from 150 to 2000 UF(fixed units) for the more severe cases, equivalent to 45 to 600 US dollars.

Road Safety[5] are random sobriety checkpoints where vehicles are stopped, generally at night-time during weekends or holidays. Although the implementation of the ZT Laws was primarily local, since December 2020, the National Observatory of Road Safety created the Federal Breathalyzer Campaign, intending to standardize the policy across territories and obtain comparable statistics. This national campaign involved coordination with local and state police forces. The sobriety test consists of drivers blowing in a Breathalyzer. If a BAC exceeds the DUI limit, the vehicle is towed and returned to its owner upon fee payment.

Figure 1: Distribution of Fatalities by Age



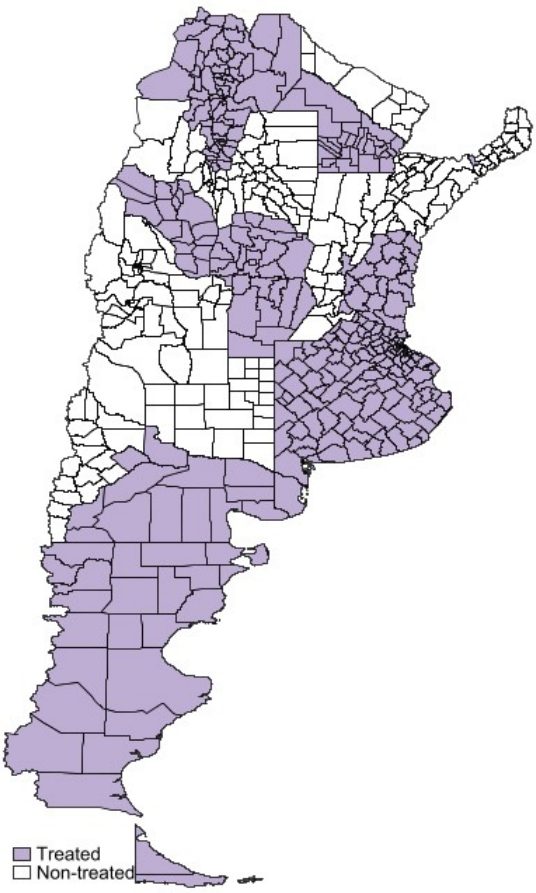
In Argentina's context, Zero-Tolerance laws are passed in an effort to save young people's lives. It is documented that younger individuals, especially those above the minimum driving legal age, are, on average, more prone to be involved in risky driving behavior. This is reflected in rates

[5]See Informe Alcoholemia Federal 2023 from ANSV

of positive breathalyzer tests Huh and Reif (2021) and, consequently, in mortality rates.

An essential aspect of Traffic Fatalities is that the distribution of victims across age groups is not normally distributed. Figure 1 shows that young adults are likelier to die from a traffic crash. Policymakers consider this fact and aim to reduce deaths, specifically among young adults. In line with this, governments and legislative branches are generally advised and encouraged by Civil Organizations created by relatives of Traffic Accident Victims, which tend to be teenagers or young adults.

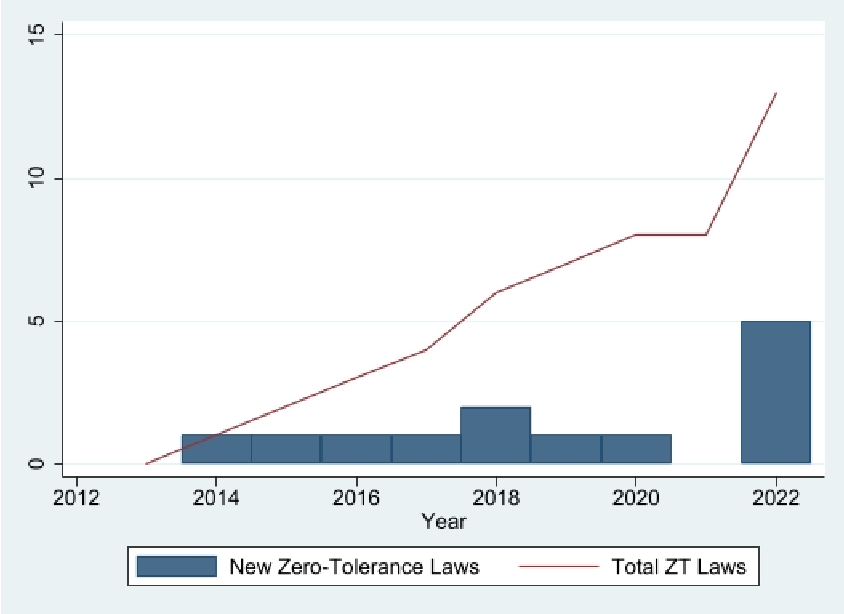
Figure 2: states with a Zero Tolerance law as of 2022



Note: This plot shows (in purple) the counties that passed a law by December 2022.

Figures 2 and **??** show the geographical and time variation of the policy. The map particularly highlights the variation in treatment as of 2022, in which we can observe that the adoption of the laws varies within regions.

Figure 3: states with a Zero Tolerance law as of 2022



Note: This plot shows the staggered adoption of ZT Laws across states

It is also worth noting in Figure 2 that although most laws are passed and enforced at the local level, some counties are differentially treated compared to the state to which they belong. This is the case of Bariloche (an important international touristic destination) in Rio Negro, for example, which does not enforce a ZT law. However, the rest of the state is subject to such regulation. In opposition to this, the capital county of Neuquén enforces a ZT law, unlike the rest of the state of Neuquén, which adheres to the national guidelines. Figure 3 shows the adoption variation across states by year. We can see that states gradually passed these ZT Laws, and except for the absence of new treated units during 2021, we cannot observe abrupt changes.

3 Data

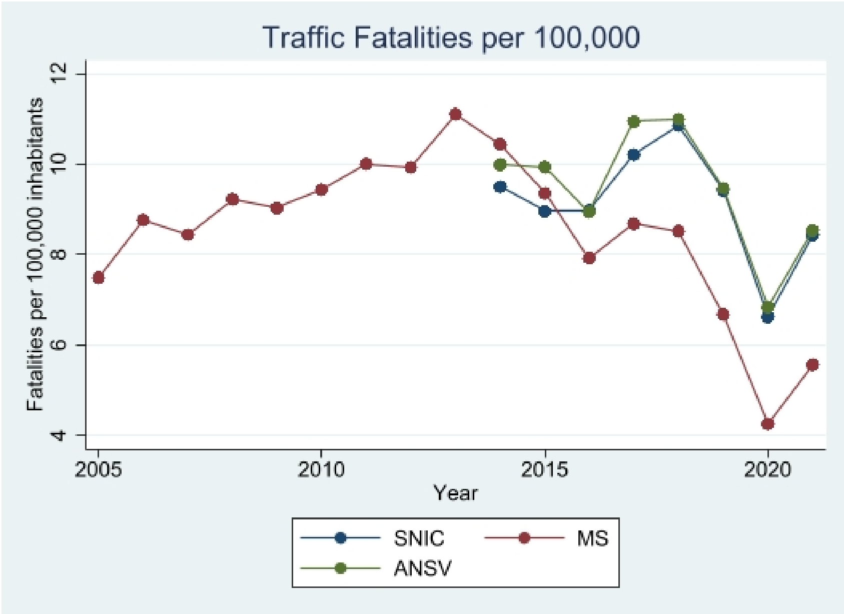
3.1 Health Outcomes

I use four administrative datasets to quantify the effect of ZT laws on health outcomes. First, I use administrative data provided by the National System of Criminal Information (SNIC), which is dependent on the Ministry of Security of Argentina. This data includes information on the number of crimes and victims for ten broadly defined categories, including road traffic accidents. The SNIC is a data collection and consolidation system across law enforcement agencies, including provincial and federal police forces. The information collected stems from the Early Warning System (SAT), a procedure implemented by the Ministry of Security that contains detailed information on four types of crime: Property Crime, Murders, Suicide, and Traffic Fatalities. For this paper, I focus on this latter module of SNIC. I use this database to compare counties because of its finer geographical aggregation. This database reports annual counts of Fatalities and Injuries for the 2014-2022 period.

Second, I use vital statistics from the Ministry of Health of Argentina (MS), which annually provides counts of death by cause following the ICD-10 classification at the state level. Each database register comes from death certificates completed by doctors covering 2005-2021. Third, since NGOs and other government agencies document it (Guerrera, n.d.) that official vital statistics tend to underestimate the actual count of traffic fatalities (as we can observe in Figure 4) due to hospital registry procedures, to provide robustness to the estimates I use data from the National Observatory of Road Safety (ONSV) [6]. This data provides counts of fatal accidents and victims monthly at the state level for 2015-2021. It provides more granularity and statistical power than the data from MS since this dataset provides counts of the outcome variables at a higher frequency (monthly rather

[6] Dependent of the Road Safety National Agency

Figure 4: Road Traffic Fatalities across time



Note: This plot presents the fatalities rate using three different data sources: Reports to the National System of Criminal Information (SNIC), Vital Statistics from the Ministry of Health of Argentina (MS), and Fatalities counts from the National Road Safety Agency (ANSV)

than annually) for almost the same number of periods. The limitation of this data is that since it started in 2015, it is impossible to measure the effect for the state that passed the law in 2014. A common limitation of these datasets is that I cannot distinguish alcohol-related fatalities from non-alcohol-related fatalities.

In Figure 4, we can observe the mean fatalities rate per 100,000. We can note consistent patterns across the three different data sources. It is essential to highlight the under-reporting phenomena in the MS dataset. This might be caused by a lack of information when filling out death certificates.

3.2 Behavioral Outcomes

I use two different surveys to assess the impact of the laws on people's behavior. First, I use the National Survey of Risk Factors. This is a nationally representative household survey, which includes self-assessed information on the use of substances, such as alcohol consumption and impaired driving. It is composed of two cross-sections (2013 and 2018).

Lastly, I use data on Hospital Discharges provided by the Department of Health Information and Statistics (DEIS) depending on the Ministry of Health. This dataset contains annual counts of hospital discharges by gender and cause and is provided at the state level. I filter the observations associated with disorders linked to alcohol use, which in this case is associated with alcohol poisoning.

3.3 Labor Market Outcomes

To control for possible confounding factors, I calculate unemployment and private-sector [7] employment from the Permanent Households Survey, a rotating panel that interviews households every quarter. This survey is representative of around 80 % of the population since it covers most urban areas in the country, which has a considerable urban population (92 percent). In particular, I merged the four quarters for 2013 and used them as a pre-treatment period.

3.4 Treatment Status

To assess the presence of a Zero Tolerance law, I assemble a dataset containing such variables for each county, using information from state and municipality [8] legislative digest and official government bulletins. In cases where I estimate a model at the state level, I assign a state to treatment if more than 60 % of the population is affected by a ZTL. Since the time of passage might differ from implementation, I use the latter to capture the actual timing of the policy change.

[7] Proxy for formality

[8] The equivalent of a county seat

3.5 Descriptive Statistics

Table 1 shows the mean for several pre-treatment characteristics across treatment and control states associated with the outcome variables.[9] We cannot observe statistically significant differences between treatment and control groups for Road Traffic Fatalities or Injuries. Table 1 also shows means and differences of sociodemographic variables across treatment and control groups. No considerable differences are observed across treatment and control variables except for a slightly higher concentration of young adults on treated units, although its magnitude is not considerably large. Altogether, these balance tables support the idea of no significant differences in treated units, addressing the concern of an endogenous treatment. Along the same line, we cannot detect statistically significant differences in outcome variables, such as the rate of traffic injuries or traffic fatalities across the treatment and control samples. Although my main specification is based on county-level variables, sociodemographic data at that geographical level is not available; therefore, I use data at the state level to show balance. Nevertheless, there are no serious concerns about considerable heterogeneity in these variables within states, which could be masking differences across treated and non-treated counties.

For my main specification, in which I use data from SNIC, the observation unit is county per year, encompassing the 2014-2022 period. Since for hospital discharges and heterogeneity analysis, I only have access to state-level data, the unit observation is state-year rather than county-year.

[9] I use 2013 as the pre-treatment year as this is the year before the first province enforced a ZTL

Table 1: Balance table of individual across treated and control states

	Control		Treatment		
	mean	sd	mean	sd	Diff
County-level Variables					
Fatalities Ratio	13.35	10.53	13.17	7.52	0.18
RT Injuries Ratio	189.17	54.13	294.41	224.64	-105.24
Observations	1,704		2,853		
State-level Variables					
Age<18	0.31	0.02	0.30	0.05	0.01
$28 \geq Age \geq 19$	0.19	0.01	0.2	.013	0.00
$66 \geq Age \geq 29$	0.4	0.02	0.39	.019	-0.00
Age ≥ 65	0.087	0.01	0.08	0.03	-0.01
Educ > HS	0.44	0.11	0.52	0.15	-0.08
Income per capita	2905.64	599.15	3245.63	1214.43	-339.99
Unemployed	0.07	0.02	0.05	0.02	0.01
Private Emp.	0.80	0.06	0.80	0.06	0.01
Cars per capita	0.32	0.25	0.25	0.13	0.07
Observations	11		13		

treatment indicates treatment at the state level up until 2021. Rates are expressed as the outcome variable per 100,000 people. Income and employment variables from the Permanent Household Survey(EPH).

4 Empirical strategy

My primary empirical strategy is based on a differences-in-differences estimator, in which I compare treated to non-treated units,[10] before and after the intervention of the following specification.

$$y_{cst} = \alpha_t + \delta_c + \beta \times ZT_c \times I(t > g) + \gamma_1 X_c + \gamma_2 X_s + \epsilon_{cst} \tag{1}$$

Where Y_{cst} is the outcome in period *t*, for county *c*, in state *s*, while α_t and δ_c are time and county fixed effects respectively. X_s and X_c are state and county-level time-varying controls, and ZT_c equals one if the county is in the treatment group and g is the year in which county *c* is treated. Our coefficient of interest is β, the treatment effect of the Zero-Tolerance laws. The parameter β represents the Differences-in-Differences estimator

[10]To avoid strong assumptions regarding anticipation effects, I only use the never-treated unit as a comparison group

of the ZT laws on a given health outcome (Injuries or Deaths). The outcome variables are expressed in terms of prevalence per 100,000 people for comparability. Since the treatment is assigned county-by-county, I cluster standard errors at the county level. Compliance with the assumptions of the DID framework is discussed in the Results section.

Many authors have documented the problems related to the use of Two-Way Fixed-Effects (TWFE) estimators in presence of staggered treatment (De Chaisemartin and d'Haultfoeuille (2022), Sun and Abraham (2021), Callaway and Sant'Anna (2021)). In particular, Goodman-Bacon (2021) highlights the presence of negative weights contaminates the TWFE coefficient, which could cause a change in the sign of the coefficient, for instance, making it negative even when all the average treatment effects have a positive sign. The problem of negative weighting ends up being a problem of what OLS takes as a comparison and what good control groups are. Specifically, a problem these studies address is comparing the treated unit only to never-treated or not-yet-treated units, depending on the anticipation effect assumption.

As defined by Callaway and Sant'Anna (2021), the Average Treatment on the Treated (ATT) for group *g* at time *g+e* is:

$$ATT(g,t) = E[Y_t - Y_{t-1}|G_g = 1] - E[Y_t - Y_{t-1}|C = 1] \tag{2}$$

Where G_g is a binary variable that equals one if the unit belongs to treatment group *g* and zero otherwise. C is a binary variable that equals 1 for never-treated units. This group-time-specific ATT's are estimated in the sample by running a regression with the following specification;

$$y_{ct} = \alpha_t + \delta_c + \sum_{e \geq g-t; e \neq 0}^{t-g} \sum_{g \in \mathcal{G}}^{e-1} A\hat{T}T(g,t) + \gamma X_c + \epsilon_{ct} \tag{3}$$

Where $\mathcal{G}$ is the total number of treatment groups, and X_c is a set of control variables at the county level.

To illustrate the dynamic effects of the policy, I use the estimator proposed by Callaway and Sant'Anna (2021), which is a weighted sum of the previously estimated $ATT(g,t)$'s :

$$\theta_{es}(e) = \sum_{g \in \in \mathcal{G}} \mathbb{1}(g + e \leq t) P(G = g | G + e \leq t) A\hat{T}T(g, g + e). \tag{4}$$

Lastly, I use IPW Regression Adjusted-estimator from Callaway and Sant'Anna (2021) to control for state-varying labor market conditions. I control for the Unemployment rate, the share of formal workers, and the rate of vehicles per capita.

4.1 Parallel Trends

The critical assumption for my identification strategy to be valid is the existence of parallel trends, i.e., in the absence of the treatment, the potential outcomes would follow similar trends. In my case, I invoke the assumption of parallel trends concerning never-treated units. In my setting, violating parallel directions would mean that treated counties face different trends than control counties without the treatment. Although this assumption is not directly testable, there exist testable implications related to it. I will address this concern in three ways. First, I show the baseline characteristics of treated and control units. Second, using the event-study specification developed by Callaway and Sant'Anna (2021), I will test for the existence of pretrends. Third, as a robustness check, I will control for state-level trends in an event-study specification.

Table 2: Diff-in-Diff estimates on Health Outcomes

Panel A: Traffic Fatalities

	(1)	(2)	(3)
ZTL	0.398	0.326	0.777
	(0.575)	(0.549)	(0.713)
N	3,746	3,746	3,306
Mean of Dep. Variable	10.73	13.4	11.05
State Controls	N	Y	Y
Excluding 2020	N	N	Y
County FE	Y	Y	Y
Year FE	Y	Y	Y

Panel B: Traffic Injuries

	(1)	(2)	(3)
ZTL	75.53***	56.14***	61.80***
	(13.36)	(17.95)	(11.22)
N	3,746	3,746	3,306
Mean of Dep. Variable	225.55	246.07	233.32
State Controls	N	Y	Y
Excluding 2020	N	N	Y
County FE	Y	Y	Y
Year FE	Y	Y	Y

Notes: Outcome variables are Traffic Fatalities and Traffic-Related Injuries per 100,000 people. Standard errors clustered at the state level are reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels. Source: Reports from the National System of Criminal Information. County-level controls include the rate of motor vehicles per capita, Unemployment and Private Employment.

5 Results

5.1 Benchmark estimates

Panel A of Table 2 presents the Average Treatment Effect coefficients estimated following Callaway and Sant'Anna (2021) on the fatalities rate for several specifications, together with standard errors clustered at the state level.

Column 1 of Table 2 presents the main specification, showing a positive but insignificant coefficient of 0.39, which implies an increase of 3.8 % with respect to the mean. Column 2 includes state-level controls to control for time-varying potential confounders. This set of state-level covariates consists of the number of vehicles per person, unemployment rates, and private employment rate [11] and as observed, the magnitude and significance of the coefficient are not substantially modified. Column 3 shows the coefficient that results from omitting the year 2020 from estimation, addressing the caveat of whether the pandemic affected the outcome differently across states. The coefficient is slightly larger than in the main specification in Column 1 but still insignificant. Overall, the coefficients in Table 2 show a non-significant impact of the laws on traffic fatalities, and I can rule out a reduction in fatalities of a magnitude larger than six percent, rejecting the presence of considerable reductions in traffic fatalities.

Panel B of Table 2 shows results on the rate of traffic-related Injuries reported to the SNIC by local and provincial police forces. A positive effect is observed for all the different specifications, implying that the law might have the opposite effect of the one policymakers expect. Since the coefficients are positive and statistically significant for all specifications, I can reject the presence of reductions at any level of magnitude.

Some papers in the literature highlight the heterogeneity of the treatment across time, i.e., how the treatment effects evolve at different periods

[11] As a proxy for formal employment

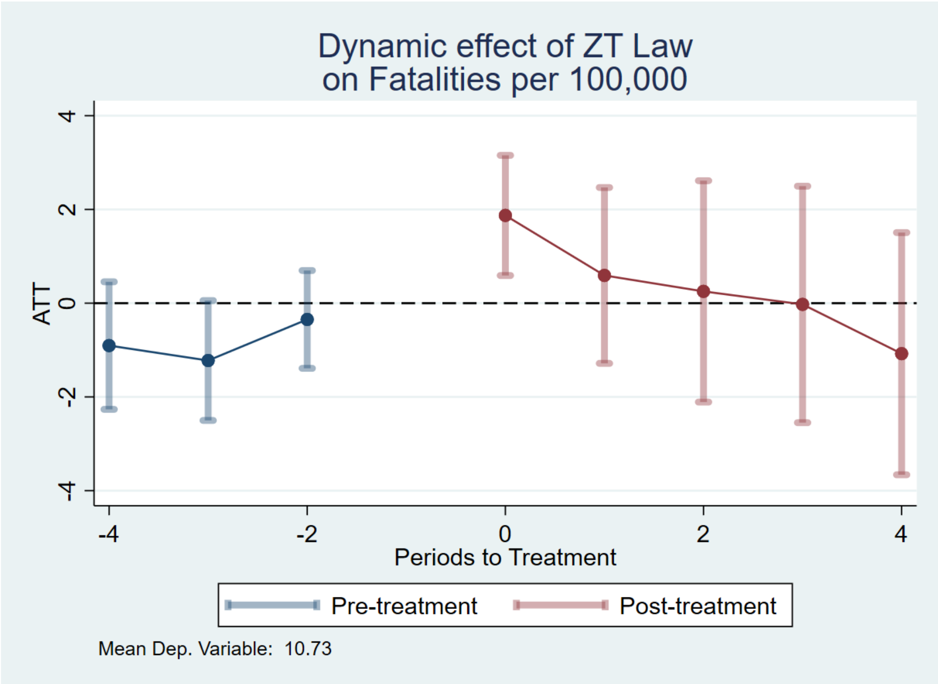
after treatment (Carpenter and Dobkin, 2009). For example, Otero and Rau, 2017 documents a sharp decrease in drunk driving in the months right after implementing a new law, which vanishes in the following periods. To assess the dynamic effects of the laws through time, I run an event-study specification on our outcomes of interest for the county-level data from SNIC. Figure 5 shows the event-study coefficients from Equation 3 for the fatalities rate. As shown in the figure, there are no sizeable effects of the laws on the rate of traffic fatalities per 100,000 people. The coefficient for the first period after treatment is positive and the only one showing statistical significance. I can observe a decreasing trend, although I do not have statistical relevance to rule out effects different than zero. Nevertheless, as noted in Panel A of Table 2, the average treatment is not statistically significant, and I can rule out a reduction of a magnitude more significant than ten percent.

Similarly, in Figure 6, I can observe the dynamic response of traffic-related Injuries per 100,000 at the county level. Unexpectedly, these estimations show positive coefficients, indicating increased traffic Injuries. I observe an abrupt jump in period zero, followed by a decrease in period one, although this coefficient is positive and statistically significant. Overall, I see a clear and steady increase in Injuries. Both figures' coefficients for pre-intervention periods ($e < 0$) suggest the validity of the parallel trends assumption for different treatment groups.

5.2 Heterogeneity

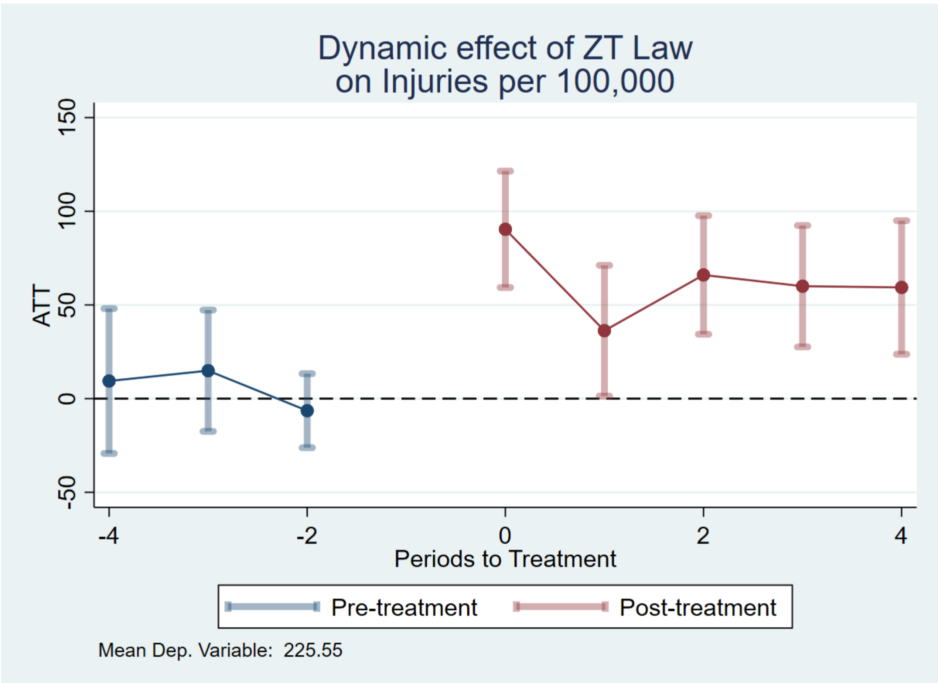
As highlighted in the Background Section, the distribution of deaths related to traffic fatalities is not homogeneous across the age distribution. As modeled by Kenkel (1993), the probability of committing traffic offenses such as drunk driving and, consequently, the likelihood of being involved in a traffic crash is negatively correlated with age since subjective discount

Figure 5: Event Study: Deaths Rate



Note: This figure shows point estimates and confidence intervals of the causal effect of ZT laws on the Fatalities Rate. The base period corresponds to the time when the new policy is passed. Standard errors are clustered at the county level.

Figure 6: Event Study: Traffic Injuries Rate



Note: This figure shows point estimates and confidence intervals of the causal effect of ZT laws on the injury rate. The base period corresponds to the time when the new policy is passed. Standard errors are clustered at the county level.

Table 3: Diff-in-Diff Estimates on Fatalities per 100,000 people

	Age				
	Full Sample	16-25	26-35	36-45	46+
ZTL	-0.61	-1.27	-2.55	0.52	-0.53
(se)	(1.81)	(7.96)	(8.69)	(1.87)	(1.2)
N	2,630	380	382	375	1,493
Mean of Dep. Variable	10.9	18.8	17.7	11.9	6.28
State FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Notes: The Outcome variable is Traffic Fatalities 100,000 people. Wild Bootstrap Standard errors clustered at the state-level are reported in parentheses. A */**/*** indicates significance at the 10/5/1% levels. Source: National Vital Statistics.

rates are higher among younger individuals. Given that the distribution of traffic fatalities across age groups is negatively skewed, i.e., younger adults are more likely to be affected, using data segmented by age groups from the Vital Statistics records, I estimate a separate model for each age group of the following form:

$$y^a_{st} = \alpha_t + \delta_s + \beta \times ZT_s \times I(t > g) + \gamma X_s + \epsilon_{st} \tag{5}$$

where a =1,..,10 represents a 10-year age group. Thus, I perform this exercise for the group from 15 to 24 years old, then for people between 25 and 34, and so on.

Table 3 shows the Average Treatment effect on fatalities from equation 5, showing the whole sample of adults and its disaggregation across age groups. I can observe that the coefficients are not statistically significant across all the age groups. This provides consistent evidence against the presence of significant declines in traffic fatalities. I can also reject heterogeneity of treatment across different age groups that could be offsetting each other and masked behind a null effect for the whole population.

5.3 Robustness

5.3.a Alternative Data Sources

Since the data provided by SNIC comes from subnational authorities (in most cases, state police agencies), it is plausible that compliance with the national guidelines is correlated with political alignment with federal authorities, making underreporting endogenous. To address the possibility across states or counties, I run a model similar to the one in the baseline specification but using state-level data from Vital Statistics provided by the Ministry of Health and the count of fatalities provided by ANSV. I can observe in table 9 in the Appendix that the magnitude and significance of the coefficients do not change substantially, and therefore, the benchmark estimates are robust to the issues mentioned above.

5.3.b Leave-one-out Estimates

To evaluate if a state drives the results from the main specifications, I re-estimate the model by dropping one state at a time and then comparing the estimates to the baseline specification. I analyze both the fatalities and Injuries rate as shown in Figures A1 and A2 in the Appendix, respectively. For the case of Injuries, in only four cases(out of 24), the confidence intervals include zero. This is the case of the provinces of Catamarca, Corrientes, La Rioja and Santa Fe. Nevertheless, the coefficients are not negative and significant for any of the estimations. For the case of fatalities, the estimates mimic the pattern of the baseline estimates in most cases. This evidence suggests that the patterns found in the baseline specification are unlikely to be explained by the inclusion of a given state.

6 Mechanisms

As noted in the previous Section, I can reject sizeable drops in traffic deaths and observe an increase in Traffic-related Injuries, an effect of the opposite sign of the one expected and advertised by policymakers who passed ZT Laws.

Why do I find these *unexpected* results on fatalities and Injuries after the reforms? Is the population modifying their drinking behavior, i.e., is the BAC distribution modified at all? Previous articles in the related literature showed mixed evidence on whether reducing the maximum BAC to zero can generate sizeable decreases in fatalities (Norström and Laurell, 1997), although more recent studies point that since the elasticity of supply of offenses with respect to the probability of conviction on the left tail of the distribution is fairly low, given the relatively low increased relative risk (Compton et al., 2002), the potential for sizeable reductions in Fatal Crashes is small.

To evaluate the impact of the ZT laws on individual compliance, I test its effect on behaviors closely related to traffic crashes and fatalities. Specifically, I evaluate the change in self-reported measures of risky behaviors from the Risk Factor Survey (ENFR): Drinking in the last month, drinking habit, binge drinking, and drunk driving. The variable *Binge Drinking* is a binary variable that equals one if the individual declared having more than five or more drinks during a single occasion in the last 30 days. A person is considered to have a *Drinking Habit* if a person had an average of more than two drinks a day if male and more than one drink a day if female. The population of reference for this variable is those who declared having at least one drink in the last month.

Given the documented heteroskedasticity issues present in Linear Probability Models, I estimate a diff-in-diff equation using the following probit model :

$$P(Y_{ist} = 1|X_{ist}) =$$
$$\Phi(\beta_0 + \beta_1 \times POST_t + \beta_2 \times Treated_s + \beta_{TWFE} \times Treated_s \times Post_t + \beta_3 X_i + \beta_4 X_s + \epsilon_{ist}) \quad (6)$$

Where Y_{ist} correspond to the binary outcome of interest ,$POST_t$ indicates observations in period 2 (2018) , $Treated_s$ equals one for individuals in treated states and X_i and X_s are individual and state-level controls, and Φ represents the Cumulative Distribution Function of the standard normal distribution. [12]

I present in Table 4 the average marginal effect from the probit model in Equation 6. These coefficients can be interpreted as the change in percentage points from the sample average in the outcome of interest. For most variables, the coefficient of interest is not statically significant except for column (3), which shows a reduction of 6.43 pp in Binge Drinking, which implies a reduction of 28% with respect to the mean. Although I observe a decrease in Binge drinking, I do not observe a significant change in the rest of the variables of interest, especially in Drunk driving. In general, I cannot see a clear pattern of decreases over these measures of alcohol consumption and drunk driving.

A possible concern with the estimates on self-assessed measures of alcohol consumption and drunk driving is that the estimates from Table 4 do not analyze a change in observed behavior but in reports from individuals. To address this issue, I use data on Hospital discharges related to Alcohol poisoning (as a proxy for excessive alcohol consumption) and estimate the model in equation (1).

In Table 5, I show the Average Treatment Effect on Hospital Discharges

[12] Since this dataset is composed of only two cross-sections, I do not face the negative weighting issues mentioned in Goodman-Bacon, 2021 and therefore TWFE consistently identifies the parameter of interest

Table 4: Effect of ZT law on Behavioral Outcomes

	(1) Drinking	(2) Abusive Consumption	(3) Binge Drinking	(4) Drunk Driving
ZTL x POST	0.0127	-0.00815	-0.0643**	-0.0139
	(0.0280)	(0.0159)	(0.0255)	(0.0221)
Weighted	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	49,175	30,389	30,644	26,033
Mean of Dep. Variable	0.654	0.153	0.222	0.1373

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors clustered at the state level in parentheses. Dependent variables are binary and take value 1 when the interviewed individuals answers affirmatively to the questions. Coefficients reported are average marginal effects from probit model. Source

related to alcoholism. Column 1 shows the estimates for the whole sample, indicating a positive but statistically insignificant effect. In columns 2-5 of Table 5, I document the impact across different age groups within the adult population, finding similar estimates of positive but insignificant impact on Hospital Discharges. The effect seems to affect the group of individuals older than 44 sensibly more than the average. Nevertheless, the estimates are not statistically distinguishable from zero. Increases seem to be especially higher for the 44+ group and lower for the 25-34 group, while the coefficients for individuals in the 15-24 and 35-44 groups are almost identical to the sample average.

As the literature documents (Compton et al., 2002, Sloan et al., 1995), heavy drinkers are much more likely to be involved in traffic crashes that generate Injuries or fatalities; therefore, these are the individuals more likely to drive a decrease in case they reduce their alcohol intake. However, as noted in the previous analysis, self-reported measures of drinking, abusive consumption, and drunk driving did not fall, while Heavy Drinking, as proxied by hospital discharges, has not decreased either. This pattern of findings points to the hypothesis that the new legislation is not directly targeting individuals in the right tail of the BAC distribution, who, accord-

Table 5: Diff-in-Diff estimates on hospital Discharges related to Alcoholism

	Age				
	Full Sample	15-24	25-34	35-44	44+
Treated	6.09	6.06	4.65	6.04	10.34
	(8.92)	(13.86)	(7.6)	(13.19)	(12.79)
Weighted	Yes	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	392	392	392	392	392
Mean of Dep. Variable	30.10	33.74	21.63	28.49	32.11

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors clustered at the province-gender level in parentheses. The outcome variable is the ratio of Hospital Discharges due to Alcohol Consumption per 100,000 people. DAta from City of Buenos Aires and Santiago del Estero not included.

ing to Compton et al. (2002) are the most likely to be involved in violent Traffic crashes, which involve Injuries and fatalities.

Although I have observed a decrease in binge drinking after the law's passage, this behavioral change does not seem to have beneficial consequences, such as a decrease in other outcomes such as drunk driving and alcohol abuse. Moreover, the decrease in declared binge drinking is not reflected in drops in hospital discharges related to alcohol poisoning, which rationalizes the absence of an effect on Traffic fatalities observed in the previous section.

7 Discussion

Lowering the Maximum Blood Alcohol Content for drivers became a popular policy for governments in recent years across many Latin American countries. In particular, Zero Tolerance Laws have been adopted across different regions, and an example of this is the case of Argentina, where state and local governments have been passing these laws from 2014 to 2022, which eventually concluded with a National Zero Tolerance law passed by the federal Legislature in 2023. Nevertheless, the effect of these policies is yet to be analyzed in depth.

This paper is one of the first to examine the impact of these restrictive laws on several health outcomes relevant to policy in Latin America. Using various data sources at the state and county level and difference-in-differences estimation, I conclude that the Zero Tolerance laws have been ineffective at reducing traffic fatalities and road-related Injuries. I show that there is no heterogeneity across the age distribution.

This pattern of null effects on Health Outcomes is consistent with Compton et al. (2002), which finds relatively low increases in relative risk associated with BAC levels on the margin (0.05). This is also consistent with the exercise performed by Francesconi and James (2021), which shows a very small potential for reductions in violent crashes in the left tail of the BAC distribution.

A uniform pattern found in the analyzed data and the literature related to DUIs and Traffic fatalities is the differential impact on younger individuals. Chao et al. (2009) documents a higher discount rate, i.e., a shorter foresight for individuals in the 20-30 years old bracket. This is compatible with Benson et al. (1999) model of supply of offenses. Therefore, it could be expected that this policy affects differently to several age groups. However, as documented in Section 5, no differential effects by age groups are found in the heterogeneity analysis.

I explore two plausible mechanisms explaining this null effect: self-assessed measures of alcohol consumption and drunk driving obtained from a nationally representative survey of risk factors and Alcohol Poisoning from Hospital discharge data. I find that the laws are ineffective at reducing alcohol consumption in the population. Similarly, no effect is found for abusive consumption and drunk driving measures. This evidence suggests an absence of the reforms' impact on people's behavior concerning alcohol consumption and abuse. Altogether, the results are consistent with Huang et al. (2020), which finds decreases in the probability of DUIs for the general population but no change in previous offenders. This behavioral pattern of individuals on the top quantiles of the BAC distribution is found in Otero and Rau (2017) and derived from a structural model in Grant (2010) and is consistent with habits of alcohol addiction. Likewise, no sizeable changes are detected in the Alcohol poisoning data, implying the lack of efficiency of the policy in curtailing heavy drinking, a phenomenon closely related to traffic crashes and fatalities (García-Echalar and Rau, 2020).

Another aspect of particular notoriety is that the estimates for Road Traffic Injuries are positive and statistically significant, contradicting the policy's expected effect. Further research is needed to explain these unintended effects of the policy. A plausible explanation for this positive effect is that, in the presence of bunching just below the previous cutoff(0.05), a ZTL relaxes this constraint, and some individuals might opt to drink more than previously since the marginal cost of a second or third drink now becomes almost zero, since the probability of conviction becomes one, regardless of the BAC level.

Across the board, the results reject a negative effect of a magnitude larger than eight percent on fatalities and show a statistically positive effect on Injuries. This evidence sheds light on the lack of efficacy of DUI policies that only modify the drink-drive limit and is consistent with estimates of the supply of offenses on the left tail of the distribution of BAC from Francesconi and James (2021) and with relatively low increases of relative risk from BAC on the [0,0.05] domain found by Compton et al. (2002). The article also complements the work from Otero and Rau (2017), which, using high-frequency data, shows a negative but vanishing effect in injuries and no effect on fatalities from a reform reducing the BAC limit in Chile.

8 Conclusion

Zero Tolerance Laws have been adopted throughout the world in the last decades. In particular, these policies became very popular in developing countries, especially Latin America. In this paper, I provide empirical evidence on the usefulness of such policies in the context of Argentina, which changed the existing legal framework by modifying the BAC threshold from 0.05 to zero. I rely on the staggered adoption of this policy across counties, finding that ZTL did not reduce road traffic fatalities (ruling out a negative effect of a magnitude larger than ten percent) and increased Injuries related to traffic accidents.

Taken together, the non-negative results suggest that the welfare improvements fell short of the policymakers' expectations, who rely on this reform as the main tool in reducing the Road-traffic fatalities epidemic in Argentina. Since this paper aims to quantify the impact of the laws on certain outcomes targeted by policymakers, I do not assess welfare effects. Nevertheless, many studies document negative effects on economic efficiency caused by zero-tolerance laws (Grant, 2010, Grant and Lewis, 2014). Thus, the non-negative effect on fatalities should be understood as a lower bound of the welfare costs of the policy. Further research is needed to shed light on the general equilibrium consequences of this type of policy.

Addressing the question of what is an effective policy is cumbersome to reduce the welfare impact of drunk driving. Further studies should focus on optimizing the policy in several dimensions other than only reducing the BAC threshold. For instance, Hansen (2015) and García-Echalar and Rau, 2020 find that increasing jail time reduces recidivism. Another dimension that has been widely studied for developing countries are alcohol sale bans, which, according to Sviatschi (2008) and Barron et al. (2022), are effective in reducing drunk driving in developing countries.

References

ANSV. (2022). *Alcoholemia federal* (tech. rep.). Agencia Nacional de Seguridad Vial.

Barron, K., Parry, C. D., Bradshaw, D., Dorrington, R., Groenewald, P., Laubscher, R., & Matzopoulos, R. (2022). Alcohol, violence and injury-induced mortality: Evidence from a modern-day prohibition. *Review of Economics and Statistics*, 1–45.

Bener, A., Yousif, A., Al-Malki, M., El-Jack, I., & Bener, M. (2011). Is road traffic fatalities affected by economic growth and urbanization development? *Advances in transportation studies*, (23).

Benson, B. L., Rasmussen, D. W., & Mast, B. D. (1999). Deterring drunk driving fatalities: An economics of crime perspective [Publisher: Elsevier]. *International review of law and economics*, *19*(2), 205–225.

Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods [Publisher: Elsevier]. *Journal of econometrics*, *225*(2), 200–230.

Carpenter, C. (2004). How do zero tolerance drunk driving laws work? [Publisher: Elsevier]. *Journal of health economics*, *23*(1), 61–83.

Carpenter, C., & Dobkin, C. (2009). The effect of alcohol consumption on mortality: Regression discontinuity evidence from the minimum drinking age [Publisher: American Economic Association]. *American Economic Journal: Applied Economics*, *1*(1), 164–182.

Chang, H., Chang, K., & Fan, E. (2020). The intended and unintended effects of drunk driving policies [Publisher: Wiley Online Library]. *Oxford bulletin of economics and statistics*, *82*(1), 23–49.

Chao, L.-W., Szrek, H., Pereira, N. S., & Pauly, M. V. (2009). Time preference and its relationship with age, health, and survival probability [Publisher: Cambridge University Press]. *Judgment and decision making*, *4*(1), 1–19.

Compton, R. P., Blomberg, R. D., Moscowitz, H., Burns, M., Peck, R. C., & Fiorentino, D. D. (2002). Crash risk of alcohol impaired driving. *Proceedings international council on alcohol, drugs and traffic safety conference*, *2002*, 39–44.

Davenport, S., Robbins, M., Cerdá, M., Rivera-Aguirre, A., & Kilmer, B. (2021). Assessment of the impact of implementation of a zero blood alcohol concentration law in uruguay on moderate/severe injury and fatal crashes: A quasi-experimental study. *Addiction*, *116*(5), 1054–1062.

De Chaisemartin, C., & d'Haultfoeuille, X. (2022). *Difference-in-differences estimators of intertemporal treatment effects* (tech. rep.). National Bureau of Economic Research.

Dee, T. S. (1999). State alcohol policies, teen drinking and traffic fatalities [Publisher: Elsevier]. *Journal of public Economics*, *72*(2), 289–315.

Evans, W. N., Neville, D., & Graham, J. D. (1991). General deterrence of drunk driving: Evaluation of recent American policies [Publisher: Wiley Online Library]. *Risk Analysis*, *11*(2), 279–289.

Francesconi, M., & James, J. (2021a). None for the road? stricter drink driving laws and road accidents. *Journal of health economics*, *79*, 102487.

Francesconi, M., & James, J. (2021b). None for the road? Stricter drink driving laws and road accidents [Publisher: Elsevier]. *Journal of health economics*, *79*, 102487.

García-Echalar, A., & Rau, T. (2020). The effects of increasing penalties in drunk driving laws—evidence from chile. *International journal of environmental research and public health*, *17*(21), 8103.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics*, *225*(2), 254–277.

Grant, D. (2010). Dead on arrival: Zero tolerance laws don't work. *Economic Inquiry*, *48*(3), 756–770.

Grant, D., & Lewis, J. A. (2014). Zero tolerance. *The Encyclopedia of Theoretical Criminology*, 1–4.

Guerrera, A. (n.d.). AGENCIA NACIONAL DE SEGURIDAD VIAL Dr. Pablo Martínez Carignano.

Guimarães, A. G., & da Silva, A. R. (2019). Impact of regulations to control alcohol consumption by drivers: An assessment of reduction in fatal traffic accident numbers in the Federal District, Brazil [Publisher: Elsevier]. *Accident Analysis & Prevention*, *127*, 110–117.

Hansen, B. (2015). Punishment and deterrence: Evidence from drunk driving. *American Economic Review*, *105*(4), 1581–1617.

Huang, C.-Y., Chou, S.-E., Su, W.-T., Liu, H.-T., Hsieh, T.-M., Hsu, S.-Y., Hsieh, H.-Y., & Hsieh, C.-H. (2020). Effect of lowering the blood alcohol concentration limit to 0.03 among hospitalized trauma patients in southern taiwan: A cross-sectional analysis. *Risk management and healthcare policy*, 571–581.

Huh, J., & Reif, J. (2021). Teenage driving, mortality, and risky behaviors [Publisher: American Economic Association 2014 Broadway, Suite 305, Nashville, TN 37203]. *American Economic Review: Insights*, *3*(4), 523–539.

Iwata, K., et al. (2010). The relationship between traffic accidents and economic growth in china. *Economics Bulletin*, *30*(4), 3306–3314.

Kenkel, D. S. (1993). Drinking, driving, and deterrence: The effectiveness and social costs of alternative policies [Publisher: The University of Chicago Press]. *The Journal of Law and Economics*, *36*(2), 877–913.

Kopits, E., & Cropper, M. (2005). Traffic fatalities and economic growth. *Accident analysis & prevention*, *37*(1), 169–178.

Law, T. H., Noland, R. B., & Evans, A. W. (2011). The sources of the kuznets relationship between road fatalities and economic growth. *Journal of Transport Geography*, *19*(2), 355–365.

Lindo, J. M., Siminski, P., & Yerokhin, O. (2016). Breaking the link between legal access to alcohol and motor vehicle accidents: Evidence from New South Wales [Publisher: Wiley Online Library]. *Health economics*, *25*(7), 908–928.

Norström, T., & Laurell, H. (1997). Effects of the lowering of the legal bac-limit in sweden. *Proceedings International Council on Alcohol, Drugs and Traffic Safety Conference*, *1997*, 87–94.

Otero, S., & Rau, T. (2017). The effects of drinking and driving laws on car crashes, injuries, and deaths: Evidence from Chile [Publisher: Elsevier]. *Accident Analysis & Prevention*, *106*, 262–274.

Ruhm, C. J. (1996). Alcohol policies and highway vehicle fatalities [Publisher: Elsevier]. *Journal of health economics*, *15*(4), 435–454.

Sloan, F. A., Reilly, B. A., & Schenzler, C. (1995). Effects of tort liability and insurance on heavy drinking and drinking and driving [Publisher: The University of Chicago Press]. *The Journal of Law and Economics*, *38*(1), 49–77.

SNEEP. (2022). *Sistema nacional de ejecucion de la pena* (tech. rep.). SNEEP.

Sun, L., & Abraham, S. (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, *225*(2), 175–199.

Sviatschi, M. M. (2008). Dry law for drunk drivers: The impact of alcohol-related laws on car accident mortality rates. *Available at SSRN 1305897*.

Wang, X., Yu, H., Nie, C., Zhou, Y., Wang, H., & Shi, X. (2019). Road traffic injuries in china from 2007 to 2016: The epidemiological characteristics, trends and influencing factors. *PeerJ*, *7*, e7423.

9 Appendix

Table 6: Diff-in-Diff Estimates on Fatalities with ANSV Data

	(1) Fatalities ANSV	(2) Fatalities MS	(3) Fatalities ANSV	(4) Fatalities MS
ZTL	-0.17	-0.526	-0.126	0.597
	(0.10)	(0.72)	(0.09)	(0.654)
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2,179	408	1,944	384
Frequency	Monthly	Annual	Monthly	Annual
Mean of Dep. Variable	0.543	10.16	.557	9.99
Excluding 2020	No	No	Yes	Yes

Standard errors in parentheses. Observations weighted by population.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

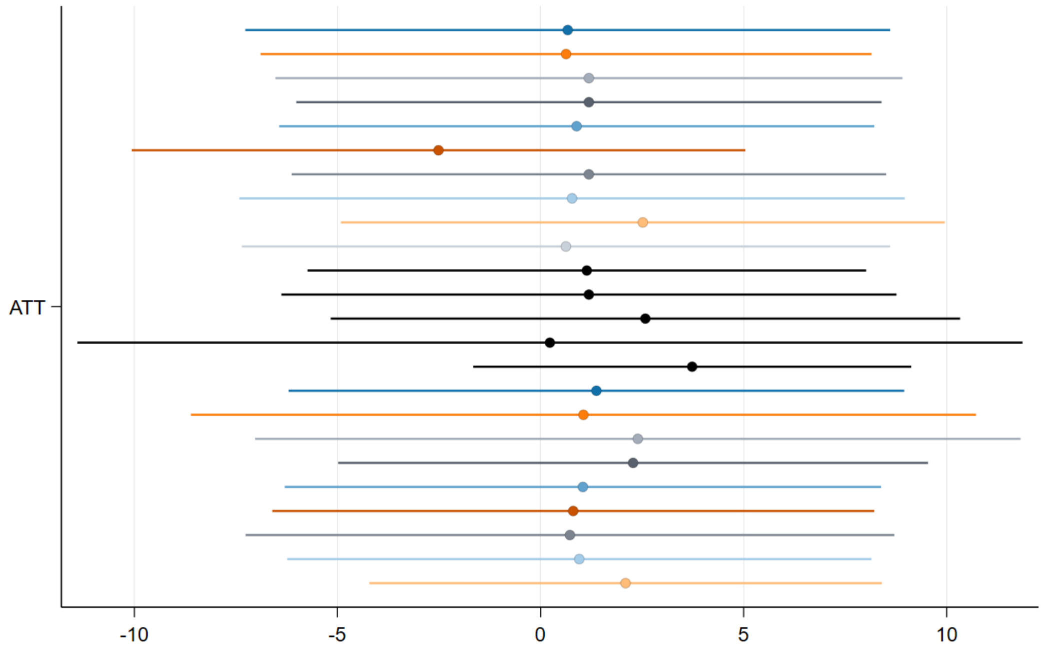
Table 7: Adoption time for states that passed ZT laws

State-level law	
State	Year
Buenos Aires	2022
Chaco	2022
Chubut	2020
Córdoba	2014
Entre Ríos	2018
Jujuy	2019
La Pampa	2022
La Rioja	2022
Río Negro	2017
Salta	2015
Santa Cruz	2018
Tierra del Fuego	2022
Tucumán	2016

Table 8: Adoption time for counties that passed ZT laws

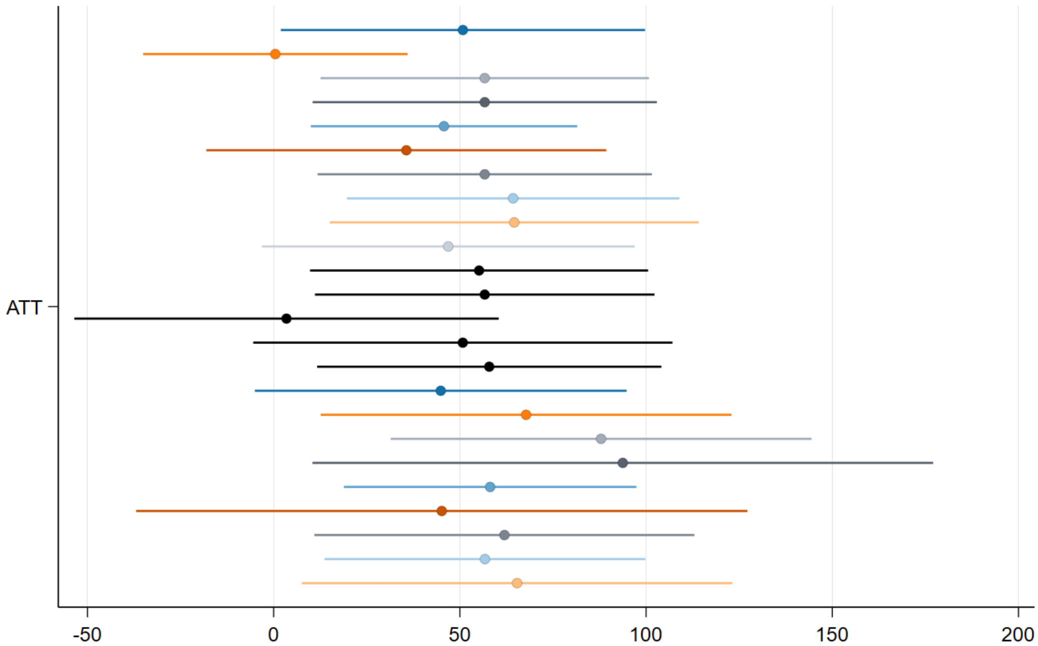
County-level law	
City-County	Year
Mar del plata	sep-18
Ezeiza	jun-21
Tigre	may-21
Moreno	dic-20
Bragado	oct-21
Posadas	abr-16
Neuquen	jun-16
Rosario	abr-21
Santa Fe	feb-20
Ushuaia	sep-18
Rio Grande	may-18
Viedma	ene-20
Moron	oct-22

Figure 7: Leave-one-out Estimates of ATT on Deaths Rate



Note: Each line in this plot represents the estimates and its confidence interval (using clustered standard errors) from the main specification on the Deaths rate excluding one state at a time by alphabetical order.

Figure 8: Leave-one-out Estimates of ATT on Injuries Rate



Note: Each line in this plot represents the estimates and its confidence interval (using clustered standard-errors) from the main specification on the Injuries rate excluding one state at a time by alphabetical order.