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

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Abstract

Curtailing alcohol-related traffic fatalities is especially important for policymakers since research indicates that a considerable share of these deaths and their associated consequences are preventable. I exploit time and geographic variation in the adoption of zero-tolerance laws in a difference-in-differences design. Using county-level data, I find no sizeable reductions in fatalities or injury counts after the adoption of such laws. 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 for the null effects, finding no significant changes in several measures of alcohol consumption.

Keywords— traffic fatalities, drunk driving, drinking behavior

JEL classification— I10,I18,R41, K42

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I Introduction

According to the World Health Organization, traffic fatalities are one of the main causes of accidental death worldwide, accounting for more than 1.25 million deaths annually and exhibiting a growing trend. The burden of injuries related to traffic accidents has also increased in recent decades (Xue Wang et al., "Road traffic injuries in China from 2007 to 2016: the epidemiological characteristics, trends and influencing factors," *PeerJ* 7 (2019): e7423). This problem is exacerbated in developing countries by the phenomenon of industrialization, which results in mismatches between vehicle fleets and urban and road infrastructure (Kazuyuki Iwata et al., "The relationship between traffic accidents and economic growth in China," *Economics Bulletin* 30, no. 4 (2010): 3306–3314, Elizabeth Kopits and Maureen Cropper, "Traffic fatalities and economic growth," *Accident analysis & prevention* 37, no. 1 (2005): 169–178, Teik Hua Law, Robert B Noland, and Andrew W Evans, "The sources of the Kuznets relationship between road fatalities and economic growth," *Journal of Transport Geography* 19, no. 2 (2011): 355–365, Abdulbari Bener et al., "Is road traffic fatalities affected by economic growth and urbanization development?," *Advances in transportation studies*, no. 23 (2011)).

This paper focuses on understanding the impact of zero-tolerance laws (ZTLs) on traffic fatalities and injuries in a developing-country context. In Argentina, road traffic fatalities (RTFs) represent the leading cause of accidental death. Troublingly, despite extensive road safety campaigns and efforts to improve road infrastructure, RTFs in the country have shown a relatively flat or even increasing trend in recent years. Drunk driving deaths account for approximately 30 percent of these traffic fatalities, according to the National Road-Traffic Observatory of Argentina. Media campaigns carried out by NGOs and policymakers have stressed the need for stricter driving-under-the-influence (DUI) policies. While these campaigns have sought to educate the public about responsible driving behaviors and the importance of adhering to traffic laws, persistently high or rising fatality rates have induced some Argentinian territories to implement ZTLs, drunk-driving regulations that set the maximum permitted blood alcohol concentration (BAC) for drivers to zero. ZTLs are understudied in the literature partially because of their limited adoption worldwide. In addition, empirical evidence on drunk driving laws is limited for Argentina specifically but also for other countries in Latin America and the developing world.

Argentina's ZTLs were implemented at the state or county level, with 13 out of the country's 24 states implementing this reform from 2014 to 2022. Additionally, three counties in provinces where the new policy was not implemented passed their own laws at the local level. Given this setting, I construct a database of ZTL adoption that relies on official government bulletins and legislative digests from each subnational unit. Based on this information, I conduct an empirical analysis relying on health outcome data from several administrative datasets and surveys. First, I use information on road traffic fatalities and injuries from Argentina's Ministry of Security. Second, I complement these data with vital statistics for 2005–2021 from the Ministry of Health. Additionally, I rely on the National Risk Factors Survey for self-assessed measures of alcohol use and drunk driving and data on hospital discharges linked to alcohol poisoning to test plausible mechanisms.

By relying on the staggered adoption of ZTLs as a source of variation and conducting a differences-in-differences exercise using never-treated units (counties or states) as the comparison group, I estimate the causal effect of ZTLs on fatalities and injuries. 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 nonsignificant effects of the laws on fatalities, rejecting a negative coefficient 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 statistically significant increases in injuries and positive but not statistically significant coefficients for fatalities. These findings remain robust when I use an alternative data source, conduct separate analyses for urban and rural areas, and obtain placebo estimates by excluding one state at a time. Altogether these findings contrast with previous results in the drunk driving literature relying on estimates of short-run effects or of marginal effects for younger adults (Bruce L. Benson, David W. Rasmussen, and Brent D. Mast, "Deterring drunk driving fatalities: an economics of crime perspective," Publisher: Elsevier, *International review of law and economics* 19, no. 2 (1999): 205–225, Sebastián Otero and Tomás Rau, "The effects of drinking and driving laws on car crashes, injuries, and deaths: Evidence from Chile," Publisher: Elsevier, *Accident Analysis & Prevention* 106 (2017): 262–274).

While I cannot identify the specific channel explaining the nonnegative results, I analyze two plausible mechanisms: First, I test for behavioral changes in a subset of treated states for which I observe self-assessed measures of alcohol consumption, binge drinking, abusive episodic consumption, or drunk driving. While I find reductions in binge drinking, the estimates for the rest of the variables point to a lack of systematic changes in alcohol consumption, especially in view with of the lack of change in drunk driving. Second, I estimate the impact of ZTLs on alcohol poisoning hospitalizations as a measure of extremely impaired driving, which is described in the literature as one of the main causes of road fatalities. I find no evidence of a decline in hospital discharges after the implementation of ZTLs.

This paper contributes to two strands of the literature. First, it adds to the branch of the literature that analyzes the effects of stricter drunk-driving policies in developing countries. In particular, this paper complements previous limited evidence for Latin American countries. Although an extensive literature has contributed to explaining the effects of different related interventions, most of these focus on Western European countries (Francesconi and James (2021), Norström and Laurell (1997), Lindo, Siminski, and Yerokhin (2016), Chang, Chang, and Fan (2020)) and the US (Carpenter and Dobkin (2009), Benson, Rasmussen, and Mast (1999), Carpenter (2004), Kenkel (1993), Ruhm (1996), Sloan, Reilly, and Schenzler (1995)), finding mixed evidence regarding the effects on traffic deaths of decreased BAC limits for drivers. Argentina stands out as a good setting in which to study this phenomenon given that, unlike other countries that implemented ZTLs in Latin America, such as Uruguay and Colombia, Argentina is a federally organized country with local or state police forces in charge of enforcing drunk driving and road safety laws.

Although some countries in the developing world have implemented ZTLs as an instrument to reduce traffic fatalities (ANSV, 2022), more empirical evidence is needed to assess their effectiveness. Otero and Rau (2017) analyze a similar intervention in Chile, although its implementation at the federal level raises concerns about its simultaneity with national media campaigns about the policy. Similarly, Guimarães and Silva (2019) analyze a dry law in Brazil, although their results lack a clear causal interpretation since they analyze the policy within a time-series framework. I also contribute to the literature by analyzing the impact within a more granular geographical area (county level) than do previous works. In contrast to those in Uruguay or Chile, the ZTLs in Argentina have been implemented in staggered rollouts across time, which allows better identification of their medium- and long-run effects. Otero and Rau (2017) analyze a similar policy implemented in Chile using administrative data on traffic fatalities and driving behavior. However, the implementation setting and their research design allow them to estimate only the short-run effects of the policy. Similarly, Davenport et al. (2021) analyze the effect of a ZTL in Uruguay; nevertheless, the assumptions required to lend a causal interpretation their synthetic control results do not necessarily hold because of a lack of comparability between their Uruguayan and Chilean municipalities.

Second, this paper contributes to the literature by analyzing an extreme version of a BAC reduction, namely, a ZTL. Such laws are a particular case of BAC limit reductions that set the threshold to zero. Although some papers in the US have studied ZTLs in the context of the minimum driving legal age (MDLA; Evans, Neville, and Graham (1991), Dee (1999)), few articles study the effects of interventions of this nature for a whole population. In particular, I study the effect of ZTLs from a more rigorous causal inference perspective since the only previous contribution studying a similar policy (Davenport et al. (2021), for the case of Uruguay) analyzes it within a synthetic control framework, constructing a synthetic Uruguay using Chilean counties, which raises concerns about the validity of the identification assumptions.

II Background

Argentina is a federally organized country with 23 states and an autonomous capital city. Each state is subdivided into departments or *partidos*, which I refer to as *counties*. Although the federal legislature is responsible for establishing general guidelines for traffic laws, provincial and local legislatures may opt either to adhere to the national guidelines or to pass their own (more lenient or stringent) legislation. As of 2013, the standard nationwide BAC threshold was 0.05 grams of alcohol per deciliter (g/dl). This was the threshold in the state of Córdoba when it passed a ZTL in 2014, becoming the first subnational government to do so. Over the following years, thirteen states, including Córdoba, and five cities passed a ZTL, deviating from the national guidelines.

In most cases, ZTLs modify the existing legal framework by changing only the maximum BAC threshold on breathalyzers.[1] In most DUI cases, the probability of the offender's being imprisoned or facing charges is null unless an accident and fatal victims are in-

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

volved. This tendency did not change substantially with the new laws, according to legal digests from provincial and local legislatures (SNEEP, 2022).[2]

In 2013, before the passing of the first ZTL in Cordoba, the rate of RTFs per 100,000 people at the national level was 13.6, slightly lower than the regional average for the Americas. Nevertheless, unlike in the rest of the region, RTFs in Argentina 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 alcohol-impaired driving. Figure 1 shows which states had implemented a ZTL by 2021. There is substantial variation in treatment status across regions, suggesting appropriate comparability across units, which I document in the balance tables in the next section.

While the enforcement of the particular laws varies across districts, the most standard procedures documented by the National Observatory of Road Safety (ONSV) are random sobriety checkpoints where vehicles are stopped, generally at night-time during weekends or holidays. [3] Although the implementation of ZTLs was primarily local, since December 2020, the ONSV 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 requires drivers to blow into a breathalyzer. If their BAC exceeds the DUI limit, the vehicle is towed and returned to its owner upon payment of a fine.

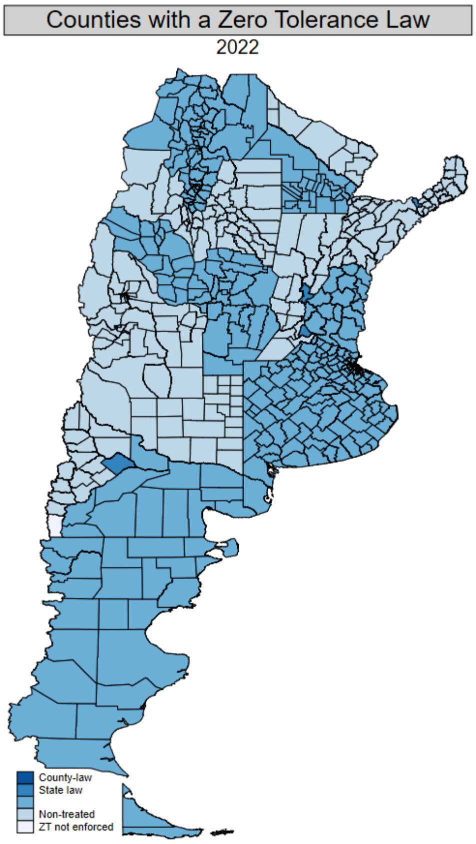
In the Argentinian context, ZTLs are passed in an effort to save young people's lives. It is documented that younger individuals, especially those above the minimum driving legal age, are, on average, more prone to be involved in risky driving behavior. This is reflected in their rates of positive breathalyzer tests Huh and Reif (2021) and, in turn, mortality rates.

An essential aspect of traffic fatalities is that the distribution of victims across age groups is not normally distributed. Figure 2 shows that young adults are likelier to die in 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, who tend to be teenagers or young adults.

2. The monetary penalties associated with DUI offenses vary from 150 to 2000 UF (fixed units), equivalent to 45 to 600 US dollars, for more severe cases.

3. See the Informe Alcoholemia Federal 2023 of the National Road Safety Agency (ANSV).

Figure 1: Counties with ZTLs as of 2022



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

Figure 2: Distribution of Fatality Rates by Age

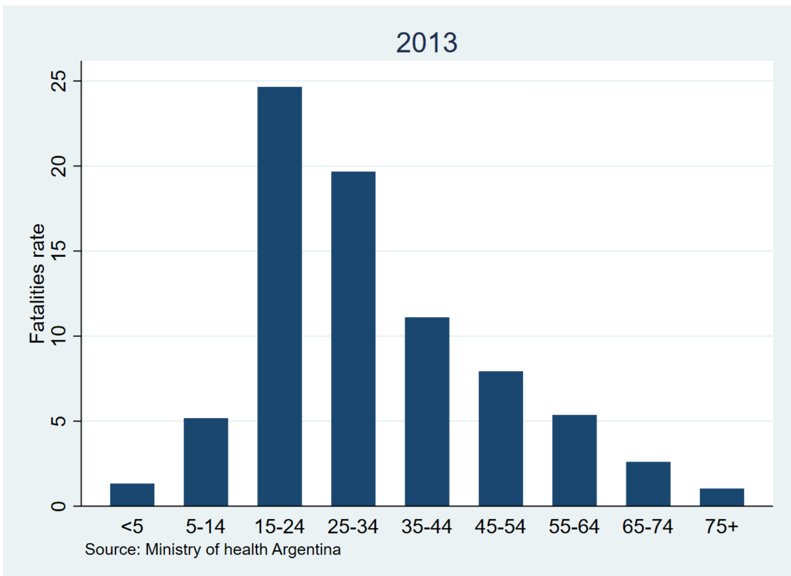
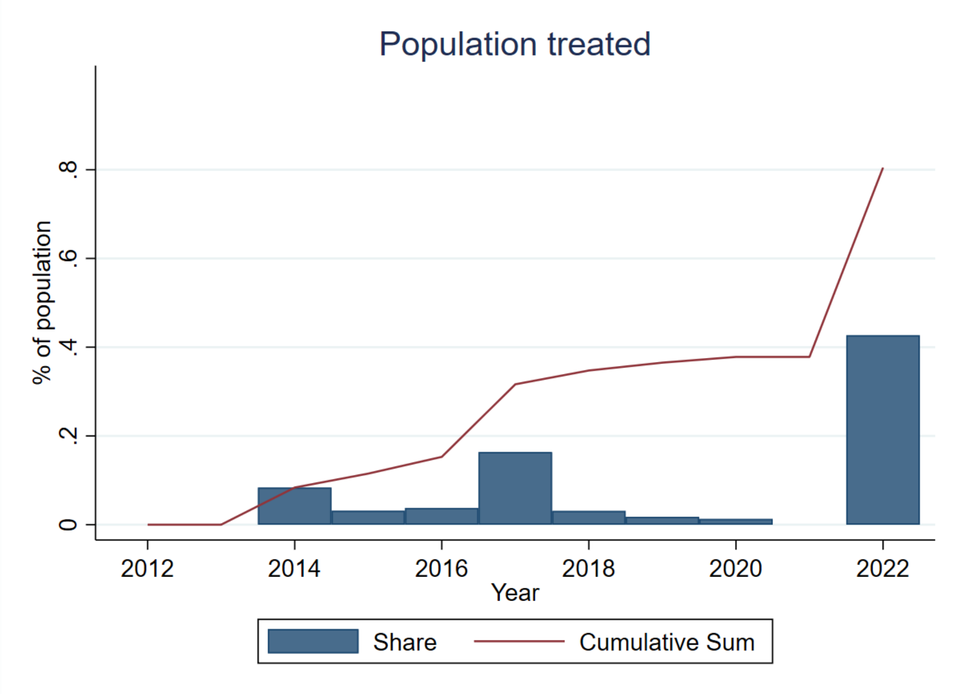


Figure 3: States Adopting ZTLs Across Time



Note: This plot shows the staggered adoption of ZTLs across states.

Figures 1 and 3 show the geographical and time variation of the policy. Specifically, the map highlights the variation in treatment status as of 2022: I can observe that adoption of the laws varies within regions. Note also from Figure 1 that although most laws are passed and enforced at the local level, some counties are differentially treated relative to others in the state to which they belong. This is the case of Bariloche (an important international tourist destination) in Río Negro, for example, which does not enforce a ZTL while the rest of the state is subject to such a regulation. The capital county of Neuquén, in contrast, enforces a ZTL, unlike the rest of the state of Neuquén, which adheres to the national guidelines. Figure 3 shows the variation in adoption across states by year. I can see that states gradually passed these ZTLs, and except for the absence of new treated units during 2021, I observe no abrupt changes in treatment trends.

III Data

A Treatment Status

I assemble a dataset on the implementation of ZTLs for each state and county, using administrative data from state and municipality legislative digests and official government bulletins.[4] In cases where I estimate a model at the state level, I assign a state to treatment if more than 60 percent of its population is affected by a ZTL.

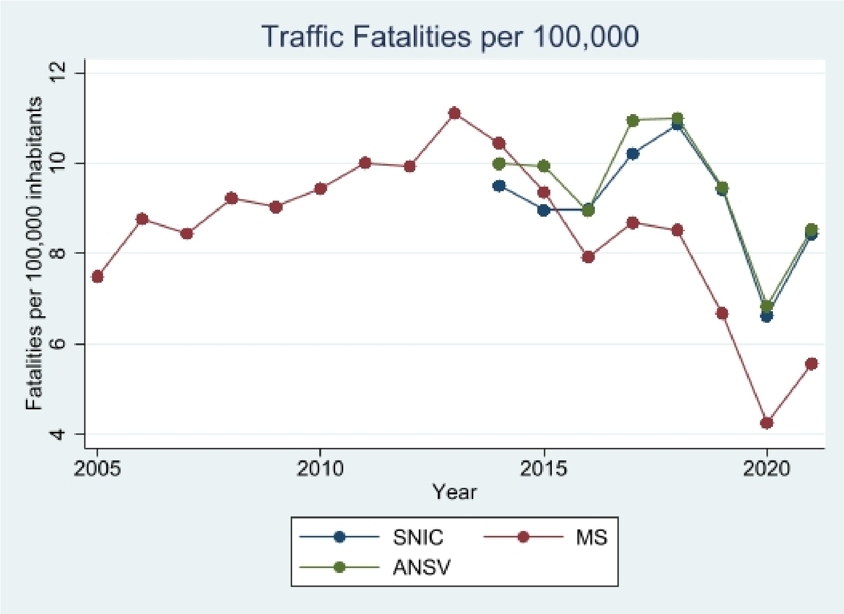
B Health Outcomes

My main outcome variables are road traffic deaths and injuries. I use four administrative datasets to quantify the effect of ZTLs on health outcomes. First, I use administrative data provided by the National System of Criminal Information (SNIC), which is dependent on Argentina's Ministry of Security. These data include information on the number of crimes and victims for ten broadly defined categories, including road traffic accidents. The SNIC is a system to collect and consolidate data 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 to collect detailed information on four types of crime: property crimes, murders, suicides, and traffic fatalities. For this paper, I focus on this latter module of the SNIC. I use this database to compare counties because its data are at finer geographical aggregation. This database reports annual counts of fatalities and injuries for the 2014–2022 period.

Second, I use vital statistics from the Argentinian Ministry of Health (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 nongovernment organizations (NGOs) and other government agencies have documented (Alexis Guerrera, "Informe Agencia Nacional de Seguridad Vial Pablo Martínez Carignano" [in es]) that official vital statistics tend to underestimate the actual count of traffic fatalities (as I can observe in Figure 4) indicated by hospital registry procedures, to check the robustness of my estimates, I use data from the ONSV.[5] These

4. A municipality is the equivalent of a county seat.
5. A dependent body of the ANSV.

Figure 4: Road Traffic Fatalities Across Time



Note: This plot presents the fatalities rate from three different data sources: National System of Criminal Information (SNIC) reports, vital statistics from the Ministry of Health of Argentina (MS), and fatality counts from the National Road Safety Agency (ANSV).

data provide counts of fatal accidents and victims monthly at the state level for 2015–2021. It provides more granularity and statistical power than the MS data since this dataset provides counts for the outcome variables at higher frequency (monthly rather than annually) for almost the same number of periods. The limitation of the data is that since they start in 2015, it is impossible to measure the effect for the state that passed its ZTL in 2014. A common limitation of these datasets is that I cannot distinguish alcohol-related from non-alcohol-related fatalities.

In Figure 4, I can observe the mean fatalities per 100,000. Note the consistent patterns across the three different data sources and the underreporting phenomenon in the MS dataset, which might be caused by missing information when death certificates are completed.

C Behavioral Outcomes

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

Last, I use data on hospital discharges provided by the Department of Health Information and Statistics (DEIS), an organ of the MS. 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—in this case, alcohol poisoning.

D Labor Market Variables

To control for possible confounding factors, I calculate unemployment and private-sector employment from the Permanent Households Survey, a rotating panel that interviews households every quarter.[6] This survey is representative of approximately 80 percent of the population since it covers most urban areas in the country, which has a considerable urban population (92 percent). In particular, I merge the four quarters for 2013 and use them as my pretreatment period.

E Descriptive Statistics

Table 1 shows the means for several pretreatment characteristics associated with the outcome variables across the treatment and control states.[7] There are no statistically significant differences between the treatment and control groups on road traffic fatalities or injuries. Table 1 also shows the means and differences of sociodemographic variables across the treatment and control groups. No differences are observed except the slightly higher concentration of young adults among the treated units, although the magnitude of the difference is not particularly large. Altogether these balance tables provide evidence of no significant differences in the treated units, addressing concerns about treatment endogeneity. In the same vein, I detect no statistically significant differences in the 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 are not available; therefore, I use state-level data to show balance. Nevertheless, there are no serious concerns about within-state heterogeneity in these variables that might mask differences across the treated and nontreated

6. A proxy for formality.

7. I use 2013 as the pretreatment year as this was the year before the first province to implement a ZTLdid so.

counties.

Table 1: Balance Table

	Control		Treatment		
	Mean	SD	Mean	SD	Diff
County-Level Variables					
Fatalities Rate	13.35	10.53	13.17	7.52	0.18
RT Injuries Rate	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	0.20	.01	0.00
$66 \geq Age \geq 29$	0.4	0.02	0.39	.02	-0.00
Age ≥ 65	0.09	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 the frequency per 100,000 people. Income and employment variables are from the Permanent Household Survey.

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

IV Empirical Strategy

I estimate the impact of ZTLs on traffic health outcomes within a differences-in-differences framework, comparing treated to nontreated units before and after ZTL implementation. A recent literature has documented the risks involved when using two-way fixed effects (TWFE) estimators in presence of staggered rollouts (De Chaisemartin and d'Haultfoeuille (2022), Sun and Abraham (2021), Callaway and Sant'Anna (2021), Goodman-Bacon (2021)). Therefore, to avoid negative weights and allow for treatment heterogeneity across treatment groups, I use the estimator developed by Callaway and Sant'Anna (2021). The first step in the method involves estimating the average treatment effect on the treated (ATT) for group g at time t $(ATT(g,t))$:

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

In the equation above, $t = 1, ...\mathcal{T}$, and G is the period when a unit first becomes treated. Accordingly, G_g is a binary variable equal to one if a unit is first treated in period g, and C is a dummy variable that takes value one for never-treated observations.

The second step of the Callaway and Sant'Anna (2021) method involves aggregating the previously estimated ATTs, the (g,t)s, in a weighting scheme to make them comparable to standard difference-in-difference or event-study coefficients. In my main specification, I use the following aggregation scheme:

$$\theta_W^O = \frac{1}{\kappa} \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} \mathbf{1}\{t \geq g\} ATT(g,t) P(G = g \mid G \leq \mathcal{T}) \tag{2}$$

where $\kappa = \sum_{g \in \mathcal{G}} \sum_{t=2}^{\mathcal{T}} 1\{t \geq g\} P(G = g \mid G \leq \mathcal{T})$. This estimator captures what is called the *overall* treatment effect. An advantage of this parameter is that, unlike the TWFE coefficient, it rules out the possibility of negative weights and potential changes in sign of the estimated effects.

Additionally, to illustrate the dynamic effects of the policy, I run the following event-study specification:

$$\theta_{es}(e) = \sum_{g \in \in \mathcal{G}} \mathbb{1}(g + e \leq \mathcal{T}) P(G = g | G + e \leq \mathcal{T} ATT(g, g+e) \tag{3}$$

Last, I use the doubly robust estimator from Callaway and Sant'Anna (2021) to control for state-varying labor market conditions that might act as confounders. These controls include the unemployment rate, the share of formal workers, and the number of vehicles per capita.

A Parallel Trends

Critical for my identification strategy to be valid is the parallel trends assumption—i.e., that in the absence of the treatment, the potential outcomes in both the control and treated groups would have followed similar trends. Here, I invoke the assumption of parallel trends concerning never-treated units. In my setting, a parallel trends violation would mean that the treated counties would have faced different trends from the control counties' without the treatment. Although this assumption is not directly testable, there exist testable implications related to it. I evaluate these implications in three ways. First, I have already shown above the baseline characteristics of the treated and control units, finding no stark differences. Second, I examine the pretreatment coefficients using the event-study specification developed by Callaway and Sant'Anna (2021). Third, as a robustness check, I control for state-level trends in an event-study specification to isolate the effect of ZTLs from effects of confounders.

V Results

A Benchmark Estimates

Panel A of Table 2 presents the coefficients on the fatalities rate from Equation 2 as estimated under alternative specifications, together with standard errors clustered at the state level.[8]

Overall, the coefficients in Table 2 show an insignificant impact of ZTLs on traffic fatalities, and I can rule out a reduction in deaths of a magnitude larger than eight percent, rejecting the presence of considerable reductions in traffic fatalities. 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 percent with respect to the mean. Column 2 includes state-level controls to account for time-varying potential confounders (number of vehicles per person, unemployment rate, and private employment rate), and as observed, the magnitude and significance of the coefficient are not substantially modified.[9] Column 3 shows the coefficient that results from my omitting the year 2020 from the estimation to address the concern that the pandemic might have differentially affected the outcome variables across states. The coefficient is slightly larger than that from the main specification in Column 1 but still insignificant.

8. Henceforth, I refer to Callaway and Sant'Anna (2021) as CS.
9. I use private employment as a proxy for formal employment.

Table 2: Differences-in-Differences Estimates for 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. The analysis period is 2014–2022. Standard errors clustered at the state level are reported in parentheses. */**/*** indicates significance at the 10/5/1% levels. Source: Reports from the National System of Criminal Information. County-level controls include the number of motor vehicles per capita, unemployment and private employment.

Panel B of Table 2 shows the 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 effect of ZTLs might be the opposite of the one that policymakers expect. Since the coefficients are positive and statistically significant for all specifications, I can reject that the laws cause reductions in injuries.

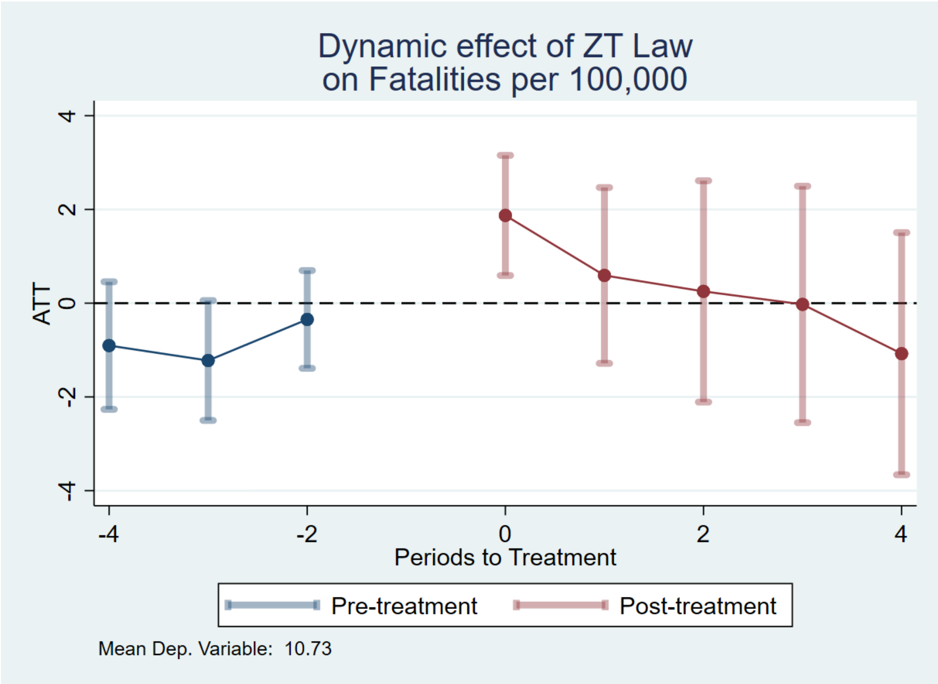
Some papers in the literature highlight heterogeneity in the treatment effects across time, i.e., how the treatment effects evolve at different periods after treatment (Carpenter and Dobkin, "The effect of alcohol consumption on mortality"). For example, Otero and Rau (2017) document a sharp decrease in drunk driving in the months right after implementation of a new law that subsequently vanishes. To assess the dynamic effects of the laws through time, I run an event-study specification on the outcomes of interest in the county-level data from SNIC. Figure 5 shows the event-study coefficients from Equation 3 for the fatalities rate. The laws have no sizeable effect on traffic fatalities per 100,000 people. The coefficient for the first period after treatment is positive and the only one showing statistical significance, denoting an *increase* in fatalities after the implementation of ZTLs, followed by modest but insignificant increases. I can observe a decreasing trend, although I do not have statistical power to rule out effects different from zero. Nevertheless, as noted in Panel A of Table 2, the overall treatment effect is not statistically significant. I can rule out a reduction of a magnitude larger than ten percent.

Similarly, in Figure 6, I 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, implying an abrupt increase in injuries followed by small increases afterward. Overall, I see a clear and steady increase in injuries. The coefficients for the preintervention periods ($e < 0$) in both figures suggest that the parallel trends assumption holds for the different treatment groups.

B Heterogeneity

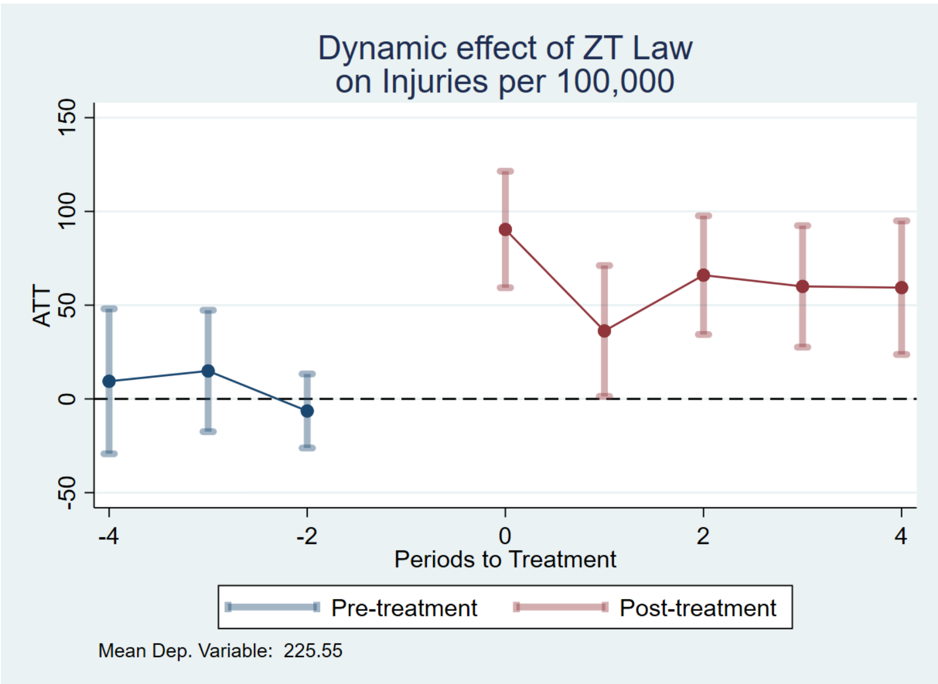
As highlighted in Section 2, the distribution of traffic fatalities is not homogeneous by age. As modeled by Kenkel (1993), the probability of committing traffic offenses such as drunk

Figure 5: Event Study: Deaths



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

Figure 6: Event Study: Traffic Injuries



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

Table 3: Differences-in-Differences Estimates for Fatalities per 100,000 People (by Age Group)

	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: Outcome variable is traffic fatalities per 100,000 people. Wild bootstrap standard errors clustered at state level are reported in parentheses. Each cell in this specification is an age group in a state in a given year. */**/*** indicates significance at the 10/5/1% levels. Source: National vital statistics.

driving and, consequently, of being involved in a traffic crash is negatively correlated with age since subjective discount rates are higher among younger individuals. Given this negative skew in the distribution of traffic fatalities across age groups, I use data segmented by age group from the vital statistics records to estimate a separate CS model for each age group. In this specification, the unit of observation is an age bracket in a state at a given year.

Table 3 shows the overall treatment effect on fatalities from equation 5 for the whole sample of adults and by age group. The coefficients are not statistically significant across all the age groups, consistent with our prior observation of no significant declines in traffic fatalities. I can also reject the possibility that heterogeneity in the treatment effects across different age groups leads the effects to offset each other and yield a null effect for the whole population. One reason why some of the coefficients turn negative here might be that I use a different sample for estimation.

C Robustness

1 Alternative Data Sources

Since the SNIC data come from subnational authorities (in most cases, state police agencies), it is plausible that deferring to the national guidelines could be correlated with political alignment with the federal administration, making underreporting endogenous. To address this concern with respect to states and counties, I run a model similar to the baseline specification but using state-level vital statistics from the MS and the count of fatalities from the ANSV. I observe in Appendix Table 6 that the magnitude and significance of the coefficients do not change substantially. Therefore, the benchmark estimates are robust to my accounting for the issue above.

2 Urban Areas

Given that traffic patterns and driving behavior vary between rural and urban areas, there could be heterogeneity in the treatment effects between these two settings. Therefore, it is informative to separately analyze these subgroups. In Figures 9 and 10, I present the results from the event-study specification from equation 3 on fatalities and injuries, respectively. As observed, there is no significant discrepancy in the dynamic effects for either of the outcome variables, suggesting homogeneous effects between rural and urban areas.

3 5.3.c Leave-One-Out Estimates

To evaluate whether a specific state drives the results of the main specifications, I reestimate the model while dropping one state at a time and then compare the estimates to those from the baseline. I analyze the fatalities and injury rates in Appendix Figures 7 and 8. For injuries, in only four cases out of the 24 (Catamarca, Corrientes, La Rioja, and Santa Fe) do the confidence intervals include zero. However, the coefficients are neither negative nor significant for any of the estimations. Concerning fatalities, the estimates largely mirror the pattern of the baseline estimates in most cases. This evidence suggests that the patterns found in the baseline are unlikely to be attributable to a given state.

VI 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 opposite sign to the one expected and advertised by the policymakers who passed the ZTLs. Why do I find these *unexpected* results on fatalities and injuries after the reforms? Is the population modifying its drinking behavior—i.e., does the BAC distribution change? Previous articles show mixed evidence on whether reducing the maximum BAC to zero can generate sizeable decreases in fatalities (Norström and Laurell (1997)). However, more recent studies point out that since the elasticity of the supply of offenses with respect to the probability of conviction on the left tail of the distribution is reasonably 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 ZTLs on individual compliance, I test their 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. *Binge Drinking* is a binary variable that equals one if the individual declared having five or more drinks on a single occasion in the last 30 days. A person is considered to have a *Drinking Habit* if the person had an average of more than two drinks a day if male and more than one drink a day if female. The reference population for this variable is those who declared having at least one drink in the last month.

Given that I rely on two ENFR cross-sections, I estimate a standard difference-in-differences equation using the following probit model:

$$P(Y_{ist} = 1|X_{ist}) =$$
$$\Phi(\beta_0 + \beta_1 \times POST_t + \beta_2 \times Treated_s + \beta_{TWFE} \times Treated_s \times Post_t + \beta_3 X_i + \beta_4 X_s + \epsilon_{ist}) \tag{4}$$

where Y_{ist} corresponds to the binary outcome of interest, $POST_t$ indicates observations in period 2 (2018), $Treated_s$ equals one for individuals in treated states, X_i and X_s are individual- and state-level controls, and Φ represents the cumulative distribution function of the standard normal distribution. Results from a linear probability model are shown in Appendix Table A4 and imply similar effects.[10]

I present in Table 4 the average marginal effect from the probit model in equation 4. These coefficients can be interpreted as the change in percentage points (pp) from the sample average in the outcome of interest. For most variables, the coefficient of interest is not statically significant, except in column 3, which shows a reduction of 6.43 pp in *Binge*

10. Since this dataset is composed of only two cross-sections, I do not face the negative weighting issues mentioned in Goodman-Bacon, "Difference-in-differences with variation in treatment timing," and therefore, TWFE consistently identifies the parameter of interest.

Drinking, which implies a reduction of 28 percent 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 drunk driving. In general, there is no clear pattern of decreases over these measures of alcohol consumption and drunk driving.

Table 4: Difference-in-Differences Estimates of ZTL Effects 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
State 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 individual answers affirmatively to the questions. Coefficients reported are average marginal effects from a probit model. Column 1 includes observations for the entire sample who responded to the survey. Columns 2 and 3 exclude observations for individuals who do not drink and differ only in their nonresponse rate. Column 4 excludes observations for individuals who do not drive on a regular basis. Source: National Risk Factor Survey.

A valid concern regarding self-reported measures of alcohol consumption and drunk driving is truthful reporting. Therefore, to provide additional evidence on changes in behavior, I use data on hospital discharges related to alcohol poisoning (as a proxy for excessive alcohol consumption) and estimate the model in equation 2.

In Table 5, I show the average treatment effect on hospital discharges 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 a positive but insignificant impact on hospital discharges. Nevertheless, the estimates are not statistically distinguishable from zero. Logically, the increases seem exceptionally 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$_{e}ffects_{1}995), heavydrinkersaremuchmorelike$
$reportedmeasuresofdrinking, abusiveconsumption, anddrunkdrivingdidnotfall. Atthesametime, heavydri$
$al.(2002), arethemostlikelytobeinvolvedintheviolenttrafficcrashesthatgenerateinjuriesandfatalities.$

Table 5: Difference-in-Differences Estimates for 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 state–gender level in parentheses. The outcome variable is hospital discharges due to alcohol consumption per 100,000 people. Data from the City of Buenos Aires and Santiago del Estero are not available.

VII Discussion

Lowering the maximum BAC for drivers has become a widespread policy in recent years among many Latin American governments. In particular, zero-tolerance laws have been adopted across different regions, an example being Argentina, where state and local governments passed a series of such laws from 2014 to 2022, with a national ZTL eventually passed by the federal legislature in 2023. Nevertheless, prior to this research, the effect of these policies had yet to be analyzed in depth. Using various state- and county-level data sources and a difference-in-differences design, I conclude that Argentina's ZTLs have been ineffective in reducing traffic fatalities and road-related injuries. I show that there is no heterogeneity in the effects across the age distribution.

This pattern of null effects on health outcomes is consistent with the finding in Compton et al. (2002) of relatively low increases in relative risk associated with BAC levels on the margin (0.05). It is also consistent with the exercise performed by Francesconi and James (2021), which shows minimal 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) document a higher discount rate, i.e., shorter foresight among individuals in the 20- to 30-years-old bracket. This is compatible with the Benson, Rasmussen, and Mast (1999) model of supply of offenses. Therefore, ZTLs could be expected to affect different age groups differently. However, as documented in Section 5, no differential effects by age group are found in the heterogeneity analysis.

I explore two plausible mechanisms explaining this null effect. First, I analyze self-assessed measures of alcohol consumption and drunk driving obtained from a nationally representative survey of risk factors. Second, I explore the response of alcohol poisoning in hospital discharge data. I find that the ZTLs are ineffective in reducing alcohol consumption in the population. Similarly, no effect is found for alcohol consumption and drunk driving measures, although there is a decrease in binge drinking. This evidence suggests an absence of an impact of the reforms on people's behavior concerning alcohol consumption and abuse. Altogether the results are consistent with the finding from Huang et al. (2020) of decreases in the probability of DUIs for the general population but no change for previous offenders. This behavioral pattern of individuals in 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 also consistent with habits of alcohol addiction. Likewise, no sizeable changes are detected in the alcohol poisoning data, underscoring the lack of efficiency of the policy in curtailing heavy drinking, a behavior closely related to traffic crashes and fatalities (García-Echalar and Rau (2020)).

Another aspect of particular noteworthiness is that the estimates for road traffic injuries are positive and statistically significant, in contrast to the policy's expected effect. Further research is needed to explain these unintended effects of the policy. One 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. Some individuals might opt to drink more than they did previously since the marginal cost of a second or third drink now becomes negligible when the probability of conviction becomes one regardless of 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 impact on injuries. This evidence sheds light on the lack of efficacy of DUI policies that modify only the drunk-driving limit and is consistent with the estimates of the supply of offenses in the left tail of the BAC distribution from Francesconi and James (2021) and with the relatively low increases in relative risk from BAC in the [0,0.05] domain found by Compton et al. (2002). The article also complements the work by Otero and Rau (2017), which, using high-frequency data, shows a negative but vanishing effect on injuries and no effect on fatalities from a reform reducing the BAC limit in Chile.

Taken together, the nonnegative results suggest that the results of ZTLs likely fell short of the expectations of policymakers, who have relied on this reform as the primary tool for reducing the road traffic fatality epidemic in Argentina. Further studies should focus on the optimal design of drunk-driving policies, considering stricter BAC levels and policies proven effective in similar contexts, such as alcohol bans, road safety regulations, and increases in the likelihood and severity of penalties.

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VIII Appendix

Table A1: Difference-in-Differences Estimates on Fatalities with ANSV and MS 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 A2: Adoption Times for States That Passed ZTLs

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 A3: Adoption Times for Counties That Passed ZTLs

County-level law	
City-County	Year
Mar del Plata	2018
Ezeiza	2021
Tigre	2021
Moreno	2020
Bragado	2021
Posadas	2016
Neuquén	2016
Rosario	2021
Santa Fe	2020
Ushuaia	2018
Río Grande	2018
Viedma	2020
Morón	2022

Table A4: LPM Estimates of ZTLs for Behavioral Outcomes

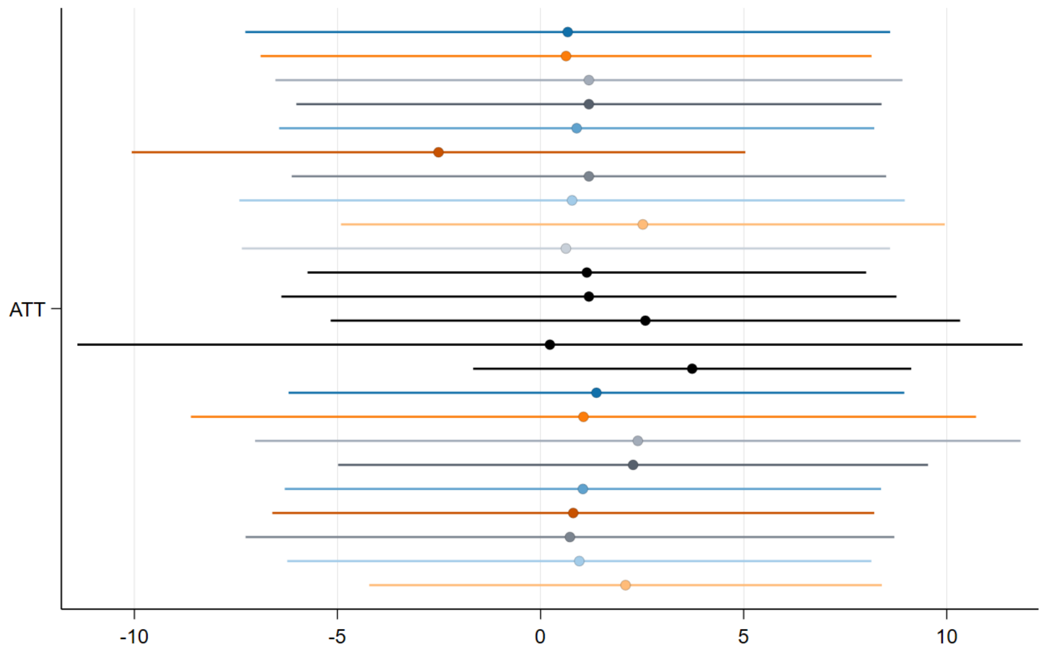
	(1) Drinking	(2) Abusive Consumption	(3) Binge Drinking	(4) Drunk Driving
ZTL x POST	0.0134	0.00622	-0.0000418	-0.0139
	(0.0242)	(0.0187)	(0.0212)	(0.0147)
Weighted	No	No	No	No
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	49175	26033	30389	30644
Mean of Dep. Var.	0.630	0.156	0.147	0.215

Standard errors in parentheses

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

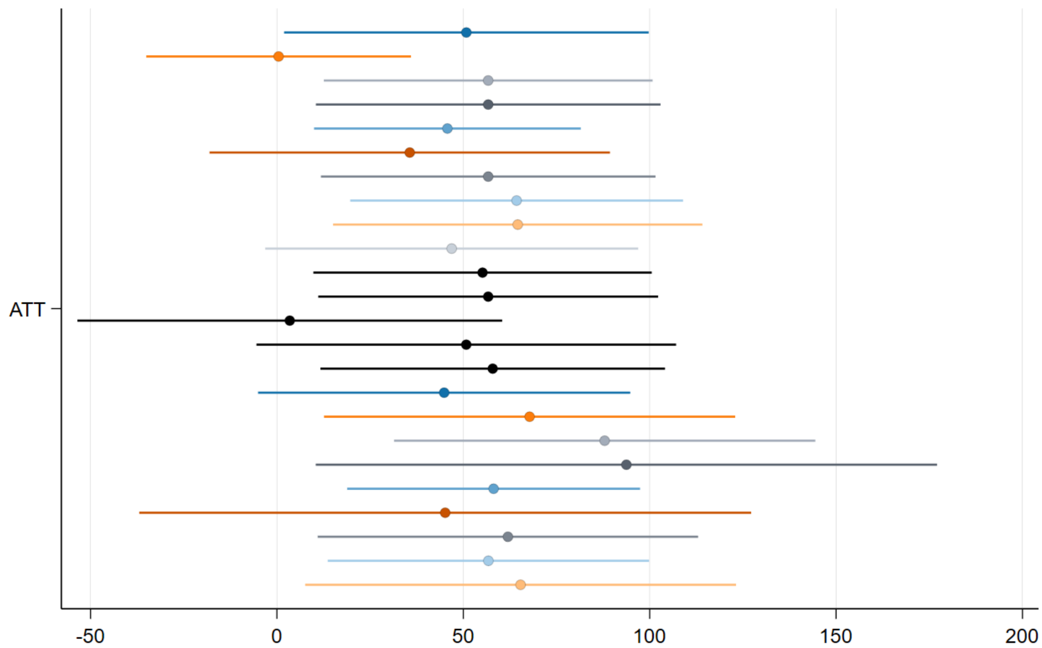
Notes: Standard errors clustered at the state level in parentheses. Dependent variables are binary and take value 1 when the interviewed individual answers affirmatively to the questions. Coefficients reported are average marginal effects from a probit model. Column 1 includes observation for the entire sample who responded to the survey. columns 2 and 3 exclude the observations for individuals who do not drink and differ only in their nonresponse rate. Column 4 excludes the observations for individuals who do not drive in a regular basis. Source: National Risk Factor Survey.

Figure A1: Leave-One-Out Estimates of ATT on Deaths



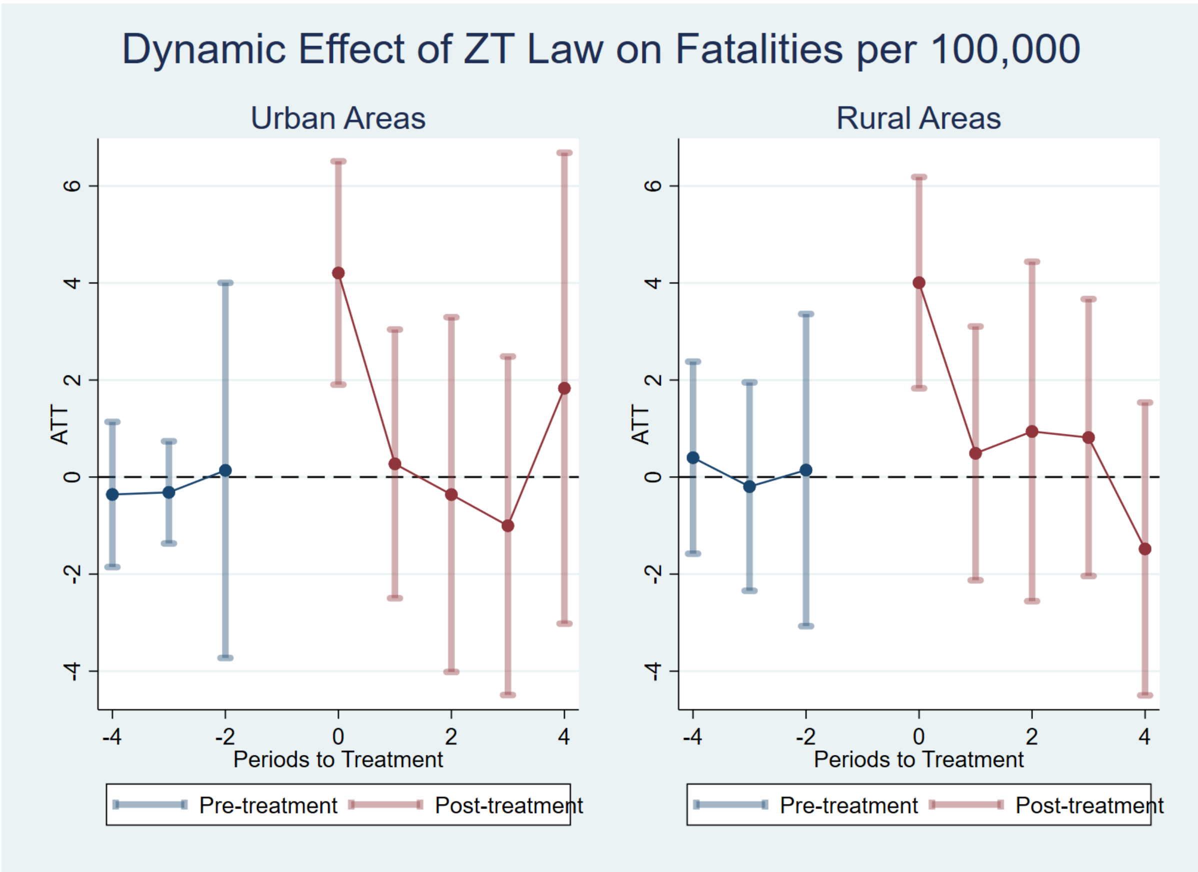
Note: Each line in this plot represents the estimate and its confidence interval (using clustered standard errors) from the main specification on the deaths rate, with one state at a time excluded in alphabetical order.

Figure A2: Leave-One-Out Estimates of ATT on Injuries



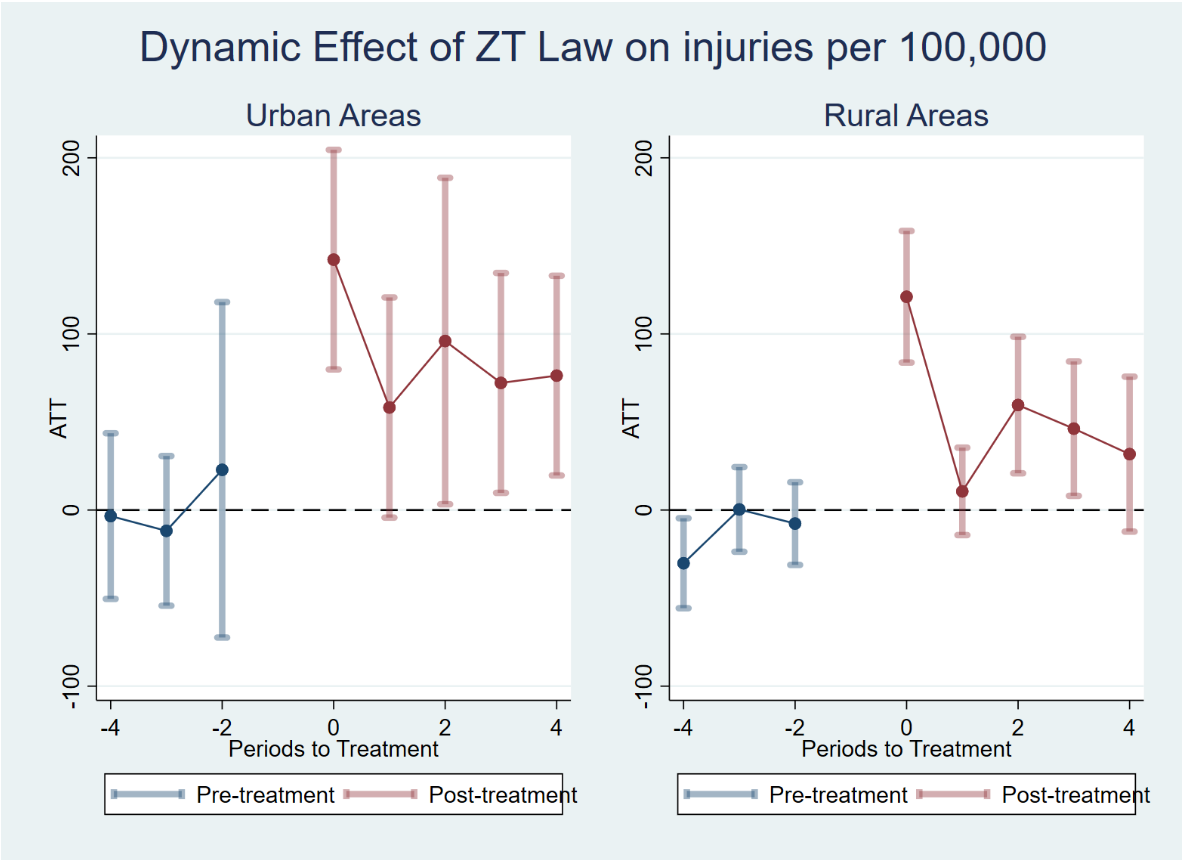
Note: Each line in this plot represents the estimate and its confidence interval (using clustered standard errors) from the main specification on the injuries rate, with one state at a time excluded in alphabetical order.

Figure A3: Event-Study Specification for Fatalities by Urban Status



Note: Each line in this plot represents the estimate and its confidence interval (using clustered standard errors) from the main specification on the injuries rate, with one state at a time excluded in alphabetical order.

Figure A4: Event-Study Specification for Injuries by Urban Status



Note: Each line in this plot represents the estimate and its confidence interval (using clustered standard errors) from the main specification on the injuries rate, with one state at a time excluded in alphabetical order.