# Do Stand-Your-Ground Laws Increase or Decrease Traffic Fatalities?*

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Nov 7, 2021

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Abstract

Historically, one has a duty to retreat before applying lethal force in self-defense. From 1994 to 2021, 31 states implemented stand-your-ground (SYG) laws to remove the retreat requirement in one's vehicle or any place one is legally present. The laws could potentially impact driving behaviors and traffic fatalities by increasing gun prevalence in the traffic fleet and altering the expected cost of aggressive driving actions. This paper is the first to evaluate the impact of SYG laws on traffic fatalities. Using state-level traffic fatality data and an event study approach, I find that the implementation of SYG laws is associated with a 3% increase in total traffic fatalities. The legalization raises the aggressive driving fatalities by 12% in the first year of implementation. In addition, the laws are associated with increases in both gun ownership and road rage crimes. These findings are robust to alternative estimation methods addressing staggered policy implementation and heterogeneous treatment effects. The evidence in this paper suggests broader impacts of SYG laws on public health than initially considered.

**Keywords**: stand-your-ground laws; traffic safety; gun
**JEL Classification**: J18, K14, R41

* I am incredibly thankful for the advice and feedback from my dissertation committee: Donna Ginther, David Slusky, Dietrich Earnhart, Tsvetan Tsvetanov, and Tim Pleskac. I appreciate the insightful comments from Jisang Yu, Amanda Gaulke, Yuhan Xue, the participants of the Midwest Economics Association (MEA) 2021 Conference, the University of Kansas Economics Department 2021 Seminar, and the Kansas State University Agricultural Economics Department 2021 Seminar. All opinions and errors are my own.
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## 1. Introduction

The prevalence of aggressive driving and road rage is a big concern in the U.S. As stated in a survey conducted by the AAA Foundation for Traffic Safety, over 78% of U.S. drivers disclosed having taken part in aggressive driving behaviors that specifically targeted other vehicles in 2013 (AAA Foundation for Traffic Safety, 2016).[3] Upon confronting an assailant in the traffic fleet, one might consider "meeting force with force" instead of running away. Historically, amid facing a confrontation, one is required by the state self-defense laws to retreat before applying deadly force if there is still room to do so.[4] However, from 1994 to 2021, 31 states have revised their laws to remove any duty to retreat in one's vehicle or any place one is legally present. These laws, which are referred to as stand-your-ground (SYG) laws in the current study, may alter drivers' driving behavior and related traffic safety outcomes. The purpose of this study is to examine how SYG laws could affect traffic fatality, an extremely pertinent but largely ignored public health outcome for gun policy literature. [5]

It is still uncertain whether SYG laws would impact traffic fatalities. Several possible mechanisms may exist to support both traffic fatalities increasing and traffic fatalities decreasing. First, SYG laws may increase the presence of guns in the traffic fleet and subsequently alter driving behaviors and traffic fatalities. The increase of gun ownership in vehicles could be because SYG laws provide more legal protections for self-defense in one's vehicle. Also, it is prevalent for

[3] These driving behaviors include "tailgating another vehicle, yelling at another driver, honking to show annoyance or anger, making an angry gesture, trying to block from changing lanes, cutting off another vehicle on purpose, exiting vehicle to confront another driver, and bumping another vehicle on purpose." (AAA Foundation for Traffic Safety, 2016)

[4] The only exception is that when the crime happens in a person's home, the person has no duty to retreat before using lethal force. This type of self-defense law is often referred as "castle doctrine law."

[5] No SYG laws have provisions specifically mention "firearm" was allowed for self-defense. Majority of SYG states use a more general term, like "deadly force", instead of "firearm" (Gius, 2016; Cherney et al., 2018). However, considering the prevalence of gun uses in fatal confrontations in the U.S., the SYG laws were largely regarded as gun laws in the literature (Weaver, 2008; Donohue and Ribeiro, 2012; Crifasi et al, 2018; Smart et al, 2020).

American people to possess a gun for self-protection (Newton and Zimring, 1969; Smith and Uchida, 1988; Kleck et al., 2011). Survey evidence shows that a motorist armed with a gun may drive more offensively or even more likely to commit road rage crimes than an unarmed motorist (Miller et al., 2002; Hemenway et al., 2006; Bushman et al., 2017). Thus, traffic fatalities may increase after the passage of the SYG laws. It could also be possible that a person with a gun in the vehicle may feel safer and thus drive more frequently or drive in an unsafe area or at an unsafe time. Consequently, traffic fatalities would increase as well. Additionally, law-abiding citizens who drive with a deadly weapon may drive more responsively and thus lower the traffic fatality rate.

Second, the legalization could decrease the expected cost of applying deadly force behind the wheel. A threatened driver's willingness to use deadly force, such as one's vehicle, against crime would be higher. For instance, when targeted by an assailant on the road, instead of escaping by changing lanes or driving to the nearest police station, an SYG-law-protected driver may respond by hitting the offender's vehicle. Therefore, after enacting the SYG laws, crash risk and traffic fatalities could rise for both attackers and victims in a road rage incident.[6]

Third, according to Cheng and Hoekstra (2013), "the laws increase the expected cost of committing violent crimes." The perceived risk of committing road rage crime would increase for potential assailants as the SYG laws protect immediate self-defense using a deadly weapon. On the one hand, the increased expected cost could deter crime (Becker, 1968; Polinsky and Shavell, 1979; İmrohoroğlu et al., 2004; McCrary and Lee, 2009, as cited in DeAngelo and Hansen, 2014). Hence, there would be fewer road rage confrontations and related casualties in the traffic fleet. On

[6] Gun deaths could also increase in a road rage incident if the victim's willingness to use a gun upon facing an assailant increases. However, traffic fatality only includes death from traffic collisions. Thus, gun use in a road rage case is not closely related to the topic of the current study.

the other hand, the increased expected cost could escalate potential aggressive driving behavior into a deadly conflict.[7] For example, bearing in mind the targeted person may carry a gun in the vehicle, an offender, driving with or without a firearm, may hit the targeted vehicle harshly instead of cutting off the line to stop the vehicle for an argument. Consequently, traffic fatalities might increase.

As explained above, the effect SYG laws have on traffic fatalities is still unclear. However, this study will examine the relationship more thoroughly. To the author's knowledge, no study has been done on the relationship between gun laws and traffic fatalities. The current study uses state-level traffic fatality data from the Fatality Analysis Reporting System (FARS) for 1989-2018. During this period, twenty-nine states relieved the duty-to-retreat in one's vehicle or any other places that such person has a legal right to be. An event study analysis confirms that the passage of SYG laws is associated with a 3% to 6% increase in traffic fatalities.

Furthermore, the study investigates SYG laws' heterogeneous impact on numerous measures of traffic fatality outcomes. The study finds salient heterogeneity by related contributing factors. After the implementation of SYG laws, aggressive-driving-related fatalities increase by around 6%, speeding-related fatalities increase by around 10%, non-alcohol-related fatalities rose by around 5%. Additionally, the study identifies law heterogeneity effects by location, timing, gender, and age. Under SYG laws, the traffic fatalities in urban areas experience a 13% increase, nighttime traffic fatalities increase by around 4%, weekday traffic fatalities rise by 3%, and male traffic fatalities increase by 4%. Traffic fatalities for ages 15-19 and 20-29 are mostly affected by the laws; the outcomes increase by 7% and 4%, respectively.

[7] Similar assumptions about how permissive gun laws initiate harsher crime due to the increased risk of committing "minor crime" could be found in Donohue et al. (2019). Donohue et al. (2019) notes that the criminals would "arm themselves more frequently, attack more harshly, and shoot more quickly when citizens are more likely to be armed."

The results are robust to alternative models such as the event study model proposed by Sun & Abraham (2020) and specifications with different functional forms or definitions of outcome variables. Furthermore, the results are not sensitive to controlling for potential spatial spillover effects, applying the alternative definitions of SYG law, and adding state-specific time trends.

After running the reduced form regressions, attention is paid to the mechanism. The study explores the impact of SYG laws on gun prevalence in traffic fleet and road rage incidences. Due to the limited availability of gun data and road rage data, the current study examines the impact of SYG laws on their corresponding proxies. Specifically, the current study uses gun ownership data from Rand Cooperation and firearm suicide data from the Centers for Disease Control and Prevention (CDC) as proxies for gun prevalence in the traffic fleet. This study proposes a novel method to approximate the road rage cases by using the subgroup crime data from the Federal Bureau of Investigation (FBI) 's Uniform Crime Reporting Program (UCR) and National Incident-Based Reporting System (NIBRS). Difference-in-differences results imply that gun ownership increases by 3% and the approximated road rage incidences increase by around 25% after implementing the laws. A back-of-envelop analysis indicates road rage crimes increase by around 450 cases per year after implementing SYG laws.

The remainder of the paper proceeds as follows: Section 2 presents background information for SYG laws. Section 3 describes the data. Section 4 explains the empirical methods. Section 5 estimates the SYG laws' effect on several measures of traffic fatalities outcomes. Section 6 provides robustness checks and sensitivity checks for the main findings. Section 7 uncovers the mechanisms about how SYG laws could influence traffic fatalities, and Section 7 concludes.

## 2. Background

### 2.1 A Brief History of Stand-Your-Ground Laws

Since the 1800s, upon facing an intruder in one's home, the occupants could legally apply lethal force even if there is still room for retreat. The related self-defense laws are called castle doctrine laws, given the notion that "a man's home is his castle" (Catalfamo, 2006). Conversely, for attacks in places other than one's home, states laws impose a duty to retreat for the victims if withdrawal from the scenes is possible. However, since the 1970s, 35 states have expanded the castle doctrine laws to places outside one's own home and lifted the duty-to-retreat requirement. For ease of expression, these laws are referred to as expanded castle doctrine (ECD) laws in the current study.

From 1971 to 1984, nine states have relaxed the "castle doctrine" to include one's workplace.[8] In 1994, Utah was the first state that expanded the castle doctrine beyond home and workplace. No ECD law was implemented until the 2000s. In 2005, an ECD law like Utah's was enacted by Florida and "became the basis for a model law adopted by the American Legislative Exchange Council (Smart et al., 2020)." Since then, 29 more states have followed the legislative efforts and removed the duty to retreat in places outside one's home.[9][10] The "duty to retreat" requirement has been replaced with the "no duty to retreat" principle to some extent.

The current study only focuses on a sub-group of ECD laws. The laws, referred to as SYG laws in the current study, extended castle doctrine laws to one's vehicle or any place one has a

[8] According to Cherney et al. (2018), Connecticut, Nebraska, Hawaii, Delaware, Pennsylvania, Arkansas, North Dakota, Louisiana, Iowa, and Rhode Island extended the castle doctrine to one's place of work in 1971, 1972, 1973, 1973, 1973, 1975, 1977, 1978, and 1984, respectively. The current study does not consider these states having implemented SYG laws.

[9] Arkansas and North Dakota passed SYG laws in 2021 and are considered as control states in the sample of the study.

[10] Studies suggested the elevated self-protection needs from major events like September 11 attack and Hurricane Katrina looting may partly contributed to the widespread implementation of SYG laws (Fisher and Eggen, 2012; Jansen and Nugent-Borakove, 2016).

legal right to be.[11] Table 1 shows the detailed effective dates for SYG laws for 48 contagious states during 1989-2018. And Figure 1 demonstrates the share of SYG-laws state out of the 48 states in the sample.[12] During the time period, 28 states in the sample implemented SYG laws. Utah was the first state that extended the castle doctrine law to one's vehicle and even any place one has a legal right to be. The share of SYG-laws states began to rise dramatically in 2006, in which 11 states adopted the laws. The second wave of legislative movement was in 2011, in which five states adopted the laws.

## 2.2 Studies on ECD laws

No literature has investigated the impact of ECD laws on traffic safety outcomes. And the ECD studies mainly focus on the laws' impact on crime. Cheng and Hoekstra (2013) is the first empirical study that comprehensively examines the impact of expanded castle doctrine on crimes (McClellan and Tekin, 2017). Specifically, using state-level yearly data from the Uniform Crime Reporting Program (UCR) between 2000 and 2010, and a difference-in-differences method, Cheng and Hoekstra (2013) finds the ECD laws do not reduce burglary, robbery, or aggravated assault, but increase the total of murder and nonnegligent manslaughter by 8%. Using data from UCR and specifications of difference-in-differences and instrumental variables, Gius (2016) identifies no evidence indicating any crime deterrence effect from ECD laws. Additionally, Gius (2016) indicates ECD laws may increase sub-categories of crimes, and whether one sub-category is affected depends on the related specification. McClellan and Tekin (2017) investigates ECD laws'

[11] The exact definition of SYG laws varies in the literature. Specifically, Cheng and Hoekstra (2013), Gius (2016), Humphrey et al., (2017), Munasib et al., (2018), and Crifasi et al., (2018) define SYG laws as laws that extend castle doctrine to some places outside one's home. Carlson (2013), Butz et al., (2015), Everytown for Gun Safety Support Fund (2013), and McClellan and Tekin (2017) only consider SYG laws as laws extend castle doctrine to any place one has a legal right to be.

[12] Figure A1 in appendix A visualizes the adoption of SYG laws in a geographic way.

impact on firearm-related homicides and injuries by using state-level monthly homicides data from the U.S. Vital Statistics, 2000-2010, and difference-in-differences specifications. The findings of McClellan and Tekin (2017) suggests that ECD laws increase the total firearm-related homicide rate by 7.5%. Munasib et al. (2018) studies the impact of ECD laws on non-suicide-related gun death with mortality data from 1999 to 2013 and a difference-in-difference design. Though the effect of ECD laws on overall gun death is inconclusive in Munasib et al. (2018), location heterogeneity analysis shows that ECD laws increase gun deaths by 7-9% and 6-7% in central city areas and suburban areas, respectively.

Smart et al. (2020) systematically reviews the ECD laws' empirical literature on public health outcomes concerning suicide, crime, defensive gun use, and the gun industry, published from 1995 to 2019. With a complete evaluation of the methodological strength in the ECD laws literature, Smart et al. (2020) recognizes moderate evidence that ECD laws may increase total homicide and supportive evidence that ECD laws may increase firearm homicides. However, the directions of the ECD laws' impact on suicide, mass shooting, defensive gun use, and firearm ownership and purchases are identified as inconclusive.[13]

### 2.3 The Relationship between Guns and Aggressive Driving Behaviors

Studies based on survey data or laboratory experiments suggest that gun carrying in one's vehicle could be associated with aggressive driving behavior. Notably, Miller et al. (2002)

[13] Smart et al. (2020) develops five scales to describe the strength of available evidence: no studies, inconclusive evidence, limited evidence, moderate evidence, and supportive evidence. The supportive evidence is identified "when at least three studies not compromised by serious methodological weaknesses found suggestive or significant effects in the same direction using at least two independent data sets. (Smart et al., 2020)" The moderated evidence is recognized "when two or more studies-at least one of which was not compromised by serious methodological weaknesses-found significant effects in the same direction, and contradictory evidence was not found in other studies with equivalent or stronger methods. (Smart et al., 2020)" And the inconclusive evidence is designated "when studies with comparable methodological rigor identified inconsistent evidence for the policy's effect on an outcome or when a single study found only uncertain or suggestive effects. (Smart et al., 2020)"

analyzes the data from a 1999 Arizona cross-sectional telephone survey and finds that carrying a gun in the vehicle is related to aggressive driving behaviors such as making rude gestures, cursing or shouting fiercely at other drivers, intentionally blocking other's way or following too closely. Additionally, using data from a 2004 national survey of over 2400 licensed drivers, Hemenway et al. (2006) finds that making rude gestures and aggressively following other drivers are related to driving with a firearm in the vehicle. Bushman et al. (2017) conducts a laboratory experiment by randomly designating 60 university students to drive with a gun or a tennis racket, aiming to identify the "weapon effect."[14] The result shows the mere sight of a gun in the vehicle may elicit more aggressive driving behaviors.

## 3. Data

### 3.1 Data on Traffic Fatalities

State-level traffic fatalities are from National Highway Traffic Safety Administration (NHTSA) 's Fatality Analysis Reporting System (FARS) for 1989-2018. FARS is the most comprehensive database for fatal accidents in the United States (Saffer, 1997). The data documented detailed information for each person and each vehicle involved in a deadly traffic accident and the circumstance of the fatal accident. The data have been used to evaluate traffic safety related policies, such as Minimum legal drinking age laws (Ruhm, 1996), primary and secondary seat belt laws (Cohen & Einav, 2003), Blood Alcohol Content (BAC) .08 laws (Freeman, 2007), zero-tolerance laws (Darren Grant, 2010), social host law (Dills, 2010), texting ban and hands-free laws (Rocco & Sampaio, 2016), or to evaluate alcohol or drug policy that could potentially affect traffic safety, such as beer tax (Ruhm, 1996), medical marijuana laws (Anderson

[14] "Weapon effect" was first illustrated in a seminal work from Berkowitz and LePage (1967). The study conducts a laboratory experiment by allowing angered university students to "punish" others in scenarios with or without the presence of a gun and the finding suggest that the presence of guns could incite more intensive aggressive behavior.

et al., 2013), and drug per se laws (Anderson & Rees, 2015). The current study is the first attempt to use FARS data to evaluate gun policy's effect on public health outcomes.

The current study constructs state-year total traffic fatalities as well as a series of sub-group traffic fatality measures. To investigate the impact of SYG laws on aggressive driving fatalities, I aggregate the traffic fatalities involved in specific behaviors classified by Stuster (2004) as potentially aggressive driving related. The aggressive driving related behaviors in the analysis include speeding, following improperly, improper or erratic lane changing, failure to yield right of way, and failure to obey traffic signs, traffic control devices, or traffic officers, failure to observe safety zone traffic laws.[15] In addition, this study breaks down the total traffic fatalities by alcohol involvement, degree of urbanization, time, gender, and age group to examine the heterogeneity effect of SYG laws.

Table 2 shows the descriptive statistics of dependent variables to be used in the following analysis. On average, the total fatality rate, defined as fatalities per 100,000 state population, is 13.65. Aggressive driving related fatality rate and speeding-related traffic rate are 6.49 and 4.2, respectively. The non-alcohol-related fatality rate is about 5.5, whereas the alcohol-related fatality rate is 3.7. [16] On average, the rural traffic fatality rate is higher than the urban traffic fatality by 1.82. Daytime and nighttime traffic fatality rates are very similar. The male traffic fatality rate is

[15] Stuster (2004) lists twelve potential aggressive driving related factors. Those factors are "following improperly; improper or erratic lane changing; illegal driving on road shoulder, in ditch, or on sidewalk or median; passing where prohibited by posted signs, pavement markings, hill or curve, or school bus displaying warning not to pass, passing on wrong side, passing with insufficient distance or inadequate visibility or failing to yield to overtaking vehicle; operating the vehicle in an erratic, reckless, careless, or negligent manner or suddenly changing speeds; failure to yield right of way; failure to obey traffic signs, traffic control devices, or traffic officers, failure to observe safety zone traffic laws; failure to observe warning or instructions on vehicle displaying them; failure to signal; driving too fast for conditions or in excess of posted speed limit; racing; making an improper turn;" However, only five aggressive driving related factors are continuously reported in FARS during the studies time period, 1989-2018. The traffic fatalities related to the five selected factors make up 51.6% of total traffic fatalities from 2003 to 2007. While the traffic fatalities related to all factors mentioned in Stuster (2004) sumake up 55.7% of total traffic fatalities from 2003 to 2007. Thus, the current study's selection of related factors is representative.

[16] The rest of traffic fatalities could not be identified either as alcohol or non-alcohol related.

more than twice the female traffic fatality rate. For the traffic fatality rate broken down by age group, most traffic fatalities happened to the age group 15-39 years old. This is also the age group where people were more often engaged in risky driving practices (Gross, 2016). Table 2 also displays the descriptive statistics of outcome variables stratified by the SYG law status. Overall, the traffic fatality rates for SYG states are higher than those for states that have not implemented SYG laws. These differences necessitate the inclusion of fixed effects in the models.

### 3.2 Data on Gun Ownership

There is no panel data available for gun ownership in the traffic fleet.[17] Thus, the current study examines the impact of SYG laws on "vehicular gun ownership" by using corresponding proxies. This study focuses on two proxies, namely, state-level estimates of household firearm ownership data from Rand Cooperation (Schell et al., 2020) and firearm suicide data from CDC, 1989-2016. Schell et al. (2020) attempts to address the concerns that current survey data suffers from enormous missing values and household gun ownership proxies may not capture the long-run time trends. Using multi-level regression with poststratification and a structural equation model, Schell et al. (2020) pools four national representative survey data, several related proxies and laws indicators, and demographic data together to construct state-year level household gun ownership data. Household gun ownership is defined as "the annual proportion of adults living in a household with a firearm for each state. (Schell et al., 2020)" Before the release of Schell et al. (2020), the gun literature mainly relied on proxies such as the firearm suicide rate to measure the household gun ownership (Kleck, 1997; Cook and Ludwig, 2002; Moody and Marvell, 2003; Cook and Ludwig, 2006). The firearm suicide rate is defined as the percentage of suicides that are

[17] Based on a nationally representative telephone survey with 2,568 respondents, Cook and Ludwig (1997) estimates the U.S. private gun ownership in 1994. The estimates suggest 14 million adults have carried firearms in vehicles in the past year, and about 3 million people routinely carry firearm in vehicles.

committed by guns. Table 3 presents the summary statistics for the gun ownership proxies. Estimates from Schell et al. (2020) suggests 36.23% of adults live in a household with a firearm.

In contrast, estimates based on firearm suicide rate implies a higher number of 52.72%. On average, states with SYG laws likely have higher gun ownership than those without SYG laws, based on both proxies. The Rand estimates include various sources, including firearm suicide rate by gender, and address the potential pitfall of existing data by using advanced and sophisticated methods. Thus, the current study considers Rand estimates to be superior to the firearm suicide rate for measurement of household gun ownership as well as an approximation of vehicular gun ownership.

### 3.3 Data on Crime

Following the literature investigating the ECD laws' impact on violent crimes, the current study uses Uniform Crime Reporting Program (UCR) data, 1989-2018, to examine SYG laws' impact on aggravated assault rate and murder rate. UCR data contains monthly crime and arrest information that is voluntarily reported by the individual law enforcement agencies. Table 3 reports the average murder rate and aggravated assault rate are 6.61 and 1286 per 100,000 population, respectively.

### 3.4 Data on Road Rage Crime

Survey data reveals that in 2019, 82% of people admitted to committing an act of road rage in the past year. And a total of 12,610 injuries and 218 murders have been attributed to road rage over seven years in the United States (Covington, 2021). However, there is no administrative data with a concentration on road rage crime. The current study proposes a novel method to approximate the road rage cases using subgroup crime data from the National Incident-Based

Reporting System (NIBRS). NIBRS data provides detailed information on every crime reported to the police, including victim and offender demographics, whether the assailant was arrested, when the crime happened, the relationship between victims and assailants, and the crime location. Specifically, this study aggregates the crime incidence that occurs on highway/road/alley, contains aggravated assault and murder, contains circumstances as argument and other circumstances, and contains circumstances that all victims are individuals, and all victims are strangers to the offenders. In terms of the weapon used in the crime incident, the crimes are grouped into "road rage using firearm," "road rage using vehicle," and "road rage using other weapons." Table 3 presents the estimated road rage cases involving weapons that are firearm and vehicle are 2.29 and 1.69 per 100,000 population, respectively.[18]

### 3.5 Data on Vehicle Miles Traveled

It could also be possible that a person with a gun in the vehicle may feel safer and thus drive more frequently or drive in an unsafe area or time. Consequently, traffic fatalities would increase as well. Though no study supports this claim, I use vehicle miles traveled data from Federal Highway Administration to study the impact of SYG laws on vehicle miles driven (VMT). The last panel in Table 3 presents the vehicle miles traveled per 100,000 licensed drivers, stratified by degree of urbanization. Though the total VMT and urban VMT are very similar across SYG states and other states, the rural vehicle miles traveled in SYG states are about twice those in other states.

[18] These estimates are likely underestimate the number of road rage incidences in the U.S. According to Ponomarova (2019), road rage crimes may involve assault with a deadly weapon, assault, battery, hit and run, reckless driving, criminal threats, and vandalism. The current study only focuses on aggravated assaults and murder because NIBRS only provides circumstance information for those two types of crimes.

## 4. Empirical Method

To estimate the effects of SYG laws, this study uses a difference-in-differences model and an event-study analysis. The baseline difference-in-differences model is estimated for the following equation:

$$Ln(Y_{st}) = \beta_1 SYG_{st} + X_{st}\beta_2 + v_s + w_t + \varepsilon_{st}, \quad (1)$$

Here, $Y_{it}$ represents the various measures of traffic fatality rate in Table 2 for state $s$ at year $t$, $w_t$ represent the full set of year fixed effects, $v_s$ represent the full set of state fixed effects, $SYG_{st}$ is a dummy variable, indicating when the state has the SYG law. The dummy variable is set equal to the fraction of the effective year and one for all the following years. The interested coefficient, $\beta_1$, represents the relationship between SYG laws and traffic safety outcomes. $X_{st}$ represent a vector of control variables listed in Table C1[19]. $\varepsilon_{st}$ is an error term. Robust standard errors are clustered at the state level.

In addition to equation (1), the following two event study specifications are estimated to examine the dynamic impact of SYG laws.

$$Ln(Y_{st}) = \sum_{b=0}^{4} \delta_b SYG_{s,t+b} + X_{st}\beta_2 + v_s + w_t + \varepsilon_{st} \quad (2)$$

$$Ln(Y_{st}) = \sum_{b=-5;b\neq-1}^{4} \delta_b SYG_{s,t+b} + X_{st}\beta_2 + v_s + w_t + \varepsilon_{st} \quad (3)$$

Both equation (2) and equation (3) replace the dummy variable $SYG_{st}$ in equation (1) with a series of indicator variables that take the value one if the SYG law has been in effect for b periods, and zero otherwise. One exception is for $SYG_{st}$ , which is set equal to the fraction of the effective year, and equal to zero for all the other years.

[19] The list of data source could be found in Appendix Table D1

# 5. Results

## 5.1 The Relationship between SYG Laws and Traffic Fatalities

Table 4 shows the estimates of the impact of SYG laws on total traffic fatalities. The number of fatalities in states with lower populations is more variable, and the regressions are weighted by the corresponding state population in year $t$.[20] The standard errors are clustered at the state level (Bertrand, Duflo, and Mullainathan 2004). The baseline analysis in column 1 shows that the passage of SYG laws increases the fatality rate by 3%.[21] The event study estimates in column 2 and column 3 show that the effect is quite persistent across years. For column 2, the traffic fatality rate increases by 8% in the first year of legalization. The magnitudes of the rise are steady at 4% until the fourth year of implementation. For four years beyond the legalization, the impacts are positive and statistically insignificant at 3%. Figure 2 provides a visualization of the dynamic effect for the specification of column 3. The first year of implementation leads to a 6% increase in traffic fatalities. The estimate for the second year of implementation is positive but statistically insignificant. The estimates for the third and fourth years indicate 3% increases in total traffic fatalities. The magnitude of effect diminishes to an insignificant 2% rise in total traffic fatalities after four years.

In Table 5, the current study replaces total traffic fatalities with fatalities involving aggressive driving factors. The aggressive driving related factors in the analysis include speeding, following improperly, improper or erratic lane changing, failure to yield right of way, and failure

[20] Weighted least square are common method that has been used in traffic safety literature, including Chaloupka et al. (1993), Ruhm (1996), Saffer (1997), Dee (1999), Mast & Rasmussen (1999), Young & Likens (2000), Grant & Rutner (2004), Young & Biuelinska-Kwapisz (2006), Miron & Tetelbaum (2009), Dills (2010), Anderson (2008), Cotti & Walker (2010), Kaestner & Yarnoff (2011), Voas et al. (2003), Grant (2010), Cotti & Teffe (2011), Anderson et al. (2013), Abouk & Adams (2013), Anderson & Rees (2015), Santaella-Tenorio et al. (2017), French & Gumus (2018), Sabia & Argys (2018), Ruhm (2000), and French & Gumus (2014),

[21] The passage of SYG laws is by no means aiming to affect traffic safety. Adding the control variables should not have a huge impact on the estimates. In analysis not showed here, the model which exclude all control variables except for state and year fixed effect produce a similar estimate, a 6.4% percentage increase.

to obey traffic signs, traffic control devices, or traffic officers, failure to observe safety zone traffic laws. Columns 1-3 present the estimates for the relationship between SYG laws and traffic fatalities related to any aggressive driving factors. Columns 4-6 show the estimates for the impact of SYG laws on speeding fatalities. Though the simple difference-in-differences estimation gives positive insignificant estimates, the results in event studies might suggest SYG laws are positively associated with aggressive driving fatalities. Figure 3 visualizes the estimates for column 3 and column 6. The left panel shows the legalization is associated with a 12% increase in aggressive driving fatalities in the first year of implementation. The impact shrinks to a 6% increase in the second year of implementation. Then the impacts become statistically insignificant and reduce from 5% to -1% for the subsequent years. The right panel displays that the implementation increases speeding-related fatalities by 17% in the first year of implementation. Then the impact wanes to an 8% increase in the third year of implementation. The effects turn into statistically insignificant for the following years.

Yu et al. (2004) surveyed 431 people from 50 alcoholism and substance abuse treatment facilities across New York state and found that alcohol problems were associated with aggressive driving, but not road rage behavior. In Table 6, the study explores SYG laws' impact on alcohol-related and non-alcohol-related traffic fatalities. The difference-in-differences estimate from column 1 shows that SYG laws are associated with a 4% increase in traffic fatalities without alcohol involvement. The event study estimates from column 2 and column 3 present the law increase the non-alcohol traffic fatalities by 3-9%. The Estimates for columns 4 -12 do not identify any treatment effect heterogeneity for alcohol-related fatal injuries. Figure 4 shows a visual representation of event study results for traffic fatalities stratified by alcohol involvement. The

upper-left panel shows the non-alcohol traffic fatality rate rises by 7% in the first year of implementation. For the following years of the law implementation, the increases are around 3%.

According to Smart et al. (2003), people residing in an urban locale experienced greater victimization of road rage (48.2% vs. 35.2%) and are more involved in offending (33.7% vs. 25.2%) than rural residents. To explore the treatment effect heterogeneity across regions, this study focuses on the fatalities in urban and rural areas separately.[22] Table 7 shows the estimates of the relationship between SYG laws and traffic fatalities by location. The difference-in-differences estimates from column 1 and column 4 show the legalization is associated with an insignificant 2% decrease in rural traffic fatalities and a significant 13% increase in urban traffic fatalities. Columns 2 and 5 show that SYG laws do not significantly affect rural traffic fatalities but significantly increase urban traffic fatalities by 9-22%. Figure 5 visualizes the estimates from column 3 and column 6. The left panel shows that SYG laws decrease traffic fatalities by around 5-10%. However, the estimate for five years before the implementation is negative and statistically insignificant, which may indicate a potential violation of the parallel trend assumption. The right panel presents that the legalization is associated with a 22% increase in urban traffic fatalities in the first year. The estimate for the second year shows a 9% increase. The estimates for the third and fourth years are positive but insignificant at 7-10%. After four years, the legalization is associated with a 16% rise in traffic fatalities.

Auto Insurance Center (2016) analyzed more than 65,000 Instagram posts with the hashtag "#RoadRage" to determine when aggressive driving behavior is at its worst. The result shows aggressive driving behaviors peak in the late afternoon during the rush back home, with 6 p.m

[22] State highway departments use the rural and urban boundaries defined by the Census Bureau. According to Census (2010), urban areas encompass densely developed territory such as residential, commercial, and other non-residential urban land uses. Rural is defined as areas not identified as urban areas.

being the worst hour. Additionally, Friday is the worst day of the week for aggressive driving. Monday and Saturday aggressive driving behaviors were less common, and Sunday was the best. Table 8 represents the relationship between SYG laws and traffic fatalities by day and time of the day. Estimates of columns 1-3 do not identify any significant impacts of SYG laws on daytime traffic fatalities. Difference-in-differences estimates from column 4, column 7, and column 10 show that nighttime traffic fatalities, weekday traffic fatalities, and weekend traffic fatalities increase by around 3%. Figure 6 visualizes the event study estimates from column 3, column 6, column 9, and column 12. The impacts on daytime traffic fatalities and weekend traffic fatalities are insignificant. SYG laws are associated with an initial 7% increase in nighttime traffic fatalities and weekday traffic fatalities. The estimates for the following years for both outcomes are around 3-4%.

The findings concerning gender differences in road rage show that male drivers are more likely to be involved in road rage than females (Evans, 1991; Miller et al., 2002; Sansone and Sansone, 2010; AAA Foundation for Traffic Safety, 2020). Table 9 illustrates the relationship between SYG laws and traffic fatalities by gender. Simple difference-in-differences analysis shows that male traffic fatalities increase by 4%, while the effect of the SYG law on female traffic fatalities is insignificant. The event studies in column 3 and column 6, visualized in Figure 7, suggest that the male traffic fatalities increase by 4-8% following the policy change and the female traffic fatalities experience a 3% increase after the law implementation.

According to AAA Foundation for Traffic Safety (2020), male drivers between the ages of 19 and 39 are more likely to become aggressive behind the wheel. It could be possible the impact of SYG laws on traffic fatalities for ages between 19 to 39 are more saliant than other age groups. Table 10 shows the estimated impact of SYG laws on traffic fatalities by age group. Difference-

in-differences estimates for age groups of 15-19, 20-29, 30-39, 40-49 are statistically significant at conventional levels. The event study estimates in Figure 8 show that the traffic fatalities for ages 15-19 increase by around 9-15%, traffic fatalities for ages 20-29 rise by about 7%, and traffic fatalities for ages 50-64 increase by around 4-8%. The estimated coefficients for the rest outcomes are statistically insignificant.

## 6. Robustness Checks and Sensitivity Checks

Recent literature raises the concern that the two-way fixed effect model could potentially be biased for the settings with staggered policy implementation and heterogeneous treatment effect (Borusyak, Jaravel and Spiess, 2021; Goodman-Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2020). The canonical difference-in-differences setting contains a pair of treatment group and control group with pre- and post-treatment time periods. According to Goodman-Bacon (2021), for settings with staggered policy implementation, the two-way fixed effect estimator is a weighted average of the estimators from all possible canonical difference-in-differences settings within the data. The bias potentially arises when the average treatment effects vary across time within groups and the canonical difference-in-differences setting contains both an earlier-treated group and a later-treated group. Following Goodman-Bacon (2021), I decompose the difference-in-differences estimate for the total traffic fatalities analysis (0.034) into three types of canonical settings: the earlier treatment group as the control group vs. the later treatment group as the treatment group, the earlier treatment group as the treatment group vs. the later treatment group as the control group, and the treatment group vs. the never treated control group.[23] The average estimate for the models comparing treatment groups and never treatment

[23] Unlike previous models with the dummy variable takes on fraction value for partial year implementation, the model estimated by Goodman-Bacon (2021) assigns the policy variable with a value equal to zero or one. This is because Goodman-Bacon (2021) only discuss the setting with pure dummy variables.

groups is 0.031, with a weight of 0.78. The potential bias raised with heterogeneous treatment effects may not be a major concern.

To formally address the concern of heterogeneous treatment effects, I estimate the effect of SYG laws by using the event study method introduced by Sun and Abraham (2020). Sun and Abraham (2020) provides a decomposition of estimators for lead and lags in the traditional event study models to discuss the potential bias and proposes "interaction-weighted" (IW) estimators to perform event studies. The method follows three steps. First, it estimates the two-way fixed effect model controlling for relative time dummies interacted with cohort dummies. The cohort dummies indicate the group of the initial treatment timing. The control group consists of never treated states. Second, it estimates each cohort share in the sample for the relative time period(s). Third, it calculates the weighted average of the estimates from step one with weights of the estimated shares in step 2. Figure 9 through Figure 15 present the corresponding event study estimates based on the new approach. The results are similar to the two-way fixed effect model estimates. Specifically, the legalization increases total traffic fatalities by around 3-4%. The effect on aggressive driving fatalities is positive but insignificant, while speeding fatalities increase by about 8%. In addition, SYG laws are associated with an 8% increase in non-alcohol-related traffic fatalities, 7-10% increases in urban traffic fatalities, and 3-4% increases in male traffic fatalities. The two-way fixed effects results are robust to Sun and Abraham (2020)'s method.

Table 11 shows additional robustness checks for the total traffic fatalities analysis. Column 1 repeats the baseline event-study model for total traffic fatalities. In columns 2 and 3, this study checks the robustness of the results using alternative treatment indicators. Specifically, Column 2 runs the baseline regression for the sample excluding states that extended castle doctrine to vehicle. Column 3 runs the baseline regression for the sample excluding states that extended castle doctrine

to any places one has a legal right to be. The robustness check confirms that SYG laws are associated with an increase in total traffic fatalities. The magnitudes of coefficients on two years after and three years after are quite similar.

In Table 12, Column 1 repeats the baseline regression result. Column 2 through Column 4 run regressions with the different definitions of the natural log of the traffic fatalities outcome variable. Specifically, column 2 defines the outcome variable as traffic fatalities count. Column 3 defines the outcome variables as traffic fatality count divided by 100,000 licensed drivers. Column 4 defines the outcome variables as traffic fatality count divided by the total vehicle miles traveled (VMT). Column 2 also controls for the natural log of the state population. Among these definitions of the dependent variable, log(count/population) is the most widely used in traffic safety literature. The results across the four specifications are very similar. All results show the SYG law is associated with a 6% increase in traffic fatalities for the first year of implementation.

In Table 13, the current study performs sensitive checks by checking alternative covariates. Specifically, the study progressively adds covariates to the basic regression that only includes state fixed effects and year fixed effects. The magnitudes of the coefficients are quite alike. The result in Column 1 is the preferred specification. Columns 6 and 7 display the result for baseline regression, including linear state-specific time trends and quadratic state-specific time trends. The results are robust for the coeffects on the first year of law implementation.[24] People who live in

[24] The current study does not include state-specific trend in the main models, following Justin Wolfer's (2006) criticism of adding state trends since the trends could confound state-specific time varying unobserved factors and effects of the policy itself. Controlling for state time trends is still controversial in gun policy research and other labor economics research. As Kyriazis(2019) points out, "include state-specific trends is the most debatable issue in right-to-carry literature." As for labor economics literature, Goodman Bacon (2021) observes unit-specific trend over control for time-varying treatment effects. This is consistent with Lee and Solon(2011), Meer and West (2016), Neumark, Salas, and Wascher (2014). Neumark, Salas, and Wascher (2014) heavily criticized the use of state-specific trend when estimate the impact of minimum wages on employment of low-skilled workers. "..if recessions led to cross-state deviations between teen employment rates and aggregate labor market conditions, then the estimated longer-term trends in teen employment could be biased."

the SYG States may drive to non-SYG states, which could have a potential spillover effect and invalidate the difference-in-differences assumption. To test the potential spillover effect, following Kyiazi (2019), the current study constructs the index below,

$$Spillover_{st} = \frac{\#ofNeigboringStateunderSYG_{st}}{\#\ ofNeigboringStates_{st}}$$

Therefore, $Spillover_{st}$ is the ratio of the neighboring states under SYG laws. Column 8 of Table 13 show the regression result for baseline model controls for the spillover index. Adding the spillover index has a minimum impact on the event-study coefficients. And this result further supports the validity of the model. Table 14 through Table 16 show the robustness checks and sensitivity checks for urban traffic fatalities analysis, and the results are robust across different specifications.

Following Anderson et al. (2013), the current study runs a series of placebo regressions. Each panel in Figure 16 shows the histogram of placebo estimates from 1,000 trials of the simple difference-in-differences model of Equation (1). For each trial, I randomly assigned 29 states that legalized SYG laws in random years because 29 states legalized SYG during 1989-2018. The vertical solid lines represent the actual difference-in-differences estimates, and the dashed lines are 5$^{th}$ and 95$^{th}$ percentile of the placebo estimates. The permutation tests show all coefficients are significant at the 10% level, and thus, it is unlikely that the estimates are obtained due to chance.

## 7. Mechanism

Table 17 shows the regression result by regressing SYG law on two types of gun ownership proxies for 1989-2016. The simple difference-in-differences estimates for both proxies show the SYG laws are associated with a 3% increase in gun ownership. Figure 17 visualizes the event study estimates from column 3 and column 6. The estimates from the regression of Rand estimated gun ownership is more salient than the estimates from firearm suicide regression. The SYG laws

increase Rand estimated gun ownership by 11% in the first year of legalization, while the laws increase firearm suicides by 3% in the first year.

Next, attention is paid to crime. Chen and Hoeksta (2013) studies extended castle doctrine to places outside the home and finds ECD laws increase murder by 8%. McClellan and Tekin (2017) studied the impact of extended castle doctrine to any place one has a legal right to be and finds ECD laws increase total homicide by 7.5% and increase homicides among white total and white males by 12.5% and 15.3%. But McClellan and Tekin (2017) finds an insignificant impact of ECD laws on African-Americans or blacks. The current study contributes to the literature by studying the effect of extending castle doctrine to one's vehicle and any place one has a legal right to be on crime. Specifically, Table 18 summarizes the regression results that analyze SYG laws' impact on aggravated assault and murder. The regression is very similar to the baseline regression but replaces the natural log of traffic fatality rate with the log of the crime rate. The simple difference-in-differences estimate from column (4) shows SYG laws are associated with a 13% increase in the murder rate. The estimate from the current paper is higher than estimates from Cheng and Hoekstra (2013) (7.5%) and McClellan and Tekin (2017) (8%). A back-of-envelope analysis suggests the legalization is associated with 431 additional homicides each year for the SYG laws states.[25] This is smaller than the finding from McClellan and Tekin (2017), which indicates SYG laws cause 600 additional murders each year for the SYG laws states. Both Cheng and Hoekstra (2013) and McClellan and Tekin (2017) study the time period of 2000-2010, but the current study covers 1989-2018. Ten states implement SYG laws after 2010. In addition, the

[25] The difference-in-differences estimate shows the murder increase by 13%. For SYG states, the average of murder one year before the implementation was 4.76 cases per 100,000 agency-reported population. This implies 0.62 additional cases per 100,000 agency-reported population. The total SYG laws state population in the year prior to the legalization was 69,656,773. Multiplying the total SYG state population and 0.62 cases per 100,000 agency-reported population gives 431 additional murders per year among the SYG laws states

definition of SYG laws varies. Figure 18 represents the event-study results from column 3 and column 6 in graphs. The estimates from the left panel are statistically insignificant. The right panel presents that the legalization increases the murder rate by14% in the first year of implementation and the impacts are consistent over the years at around 12-18% increases.

Table 19 displays the impacts of SYG laws on road rage crime proxies from murder and aggravated assault. Given the nature of the data, which contains many zeros, the study applies Poisson fixed effect model. The simple difference-in-differences show that the road rage crime involving firearms and vehicles significantly increase by 23% and 27%, respectively[26]. Figure 19 visualizes the event study estimates. SYG laws are associated with initial increases of 43% for both road rage crime proxies. Back-of-envelope calculations indicate the road rage crimes associated with firearms and the one associated with vehicles increase by 516 cases and 439 cases each year after the SYG law was implemented.[27] By contrast, similar analysis shows SYG laws may cause additional 893 traffic casualties each year.[28] This indicates other channels possibly exist between SYG laws and traffic fatalities. The current study only focuses on road rage crimes involving murder and aggravated assault. According to Ponomarova (2019), road rage crimes may involve other types of crimes such as hit and run, reckless driving, criminal threats, and vandalism. Due to the data limitation, the current study does not address these potential road rage crimes.

[26] The percentage changes are calculated as $(e^{\beta} - 1) \times 100\%$.

[27] The difference-in-differences estimate shows the road rage crime involved vehicle increase by 27%. For SYG states, the mean of road rage crime involved vehicle one year before the implementation was 2.32 cases per 100,000 agency-reported population. This implies 0.63 additional cases per 100,000 agency-reported population. The total SYG laws state population in the year prior to the legalization was 69,656,773. Multiplying the total SYG state population and 0.63 cases per 100,000 agency-reported population gives 439 additional road rage cases per year among the SYG laws states.

[28] The difference-in-differences estimates shows SYG laws is associated with a 3% increase in total traffic fatalities. The average traffic fatalities one year prior to the legalization is 18.06 fatalities per 100,000 population. The 3% increase indicate an increase of 0.54 casualties per 100,000 population. Multiplying the increased share by the SYG states population yields 893 additional deaths per year.

To address the concern that SYG laws could potentially make people feel safer and drive more frequently. Table 20 presents the results for the impact of SYG laws on VMT and Figure 20 visualizes the event study estimates. No significant impact was detected for the VMT outcomes, and the results do not identify any evidence to support this anecdotal evidence.

## 8. Conclusion

Historically, upon facing a confrontation beyond one's home, one is required by state self-defense laws to retreat before applying deadly force if there is still room to do so. However, from 1994 to 2021, 31 states have implemented the SYG laws to remove any duty to retreat in one's vehicle or any place one is legally present. The expansion of laws, which offered more protection for self-defense in the traffic fleet, could potentially alter people's driving behavior and affect public health outcomes such as traffic fatalities. However, no empirical study has examined the relationship between SYG policies and traffic safety.

The current study uses data from the Federal Highway Administration and National Highway Traffic Safety Administration to investigate the impact of SYG laws on traffic fatalities. The reduced form regressions show that the SYG laws are associated with a 3% increase in traffic fatalities. In other words, the law could cause around 900 traffic fatalities each year in the SYG law states. This is much higher than the number of homicides (431 cases) the laws likely could increase. Furthermore, this study finds important heterogeneity by driving related factors, alcohol involvement, urbanization, and time. To explore the mechanism through which SYG laws could affect traffic fatalities, I examine SYG laws' impact on household firearm ownership, road rage crime, and vehicle miles driven. The results show that firearm ownership increased by 11%, and the road rage crime increased by 23%-27% after passing laws. In other words, the SYG laws induce

around 450 additional road rage cases per year. No significant impact is identified for vehicle miles driven.

Although comparable numbers of people die in car crashes and by firearm suicides and homicides, research on gun policy has lagged behind the research on traffic fatalities (Bui & Sanger-katz, 2018). This is partially due to the constrained financial incentives imposed by the Dickey amendment and limited gun and crime data quality. Another possible reason for this lagging behind is that most gun policies only slightly affect the flow of gun ownership but hardly have a massive impact on the overall stock of guns in the U.S. (Smart et al., 2020). In addition to being the first study to examine the effect of SYG laws on traffic safety outcomes, the current research overcomes the issue of data quality and addresses the concern of "flow vs. stock." Specifically, to bypass the data quality issue in the gun policy research, I use the well-documented traffic fatality data and other related highway statistics data. Also, I take advantage of the novel gun ownership data and find that the SYG laws increase gun ownership. For mitigating the concern of "flow vs. stock," the SYG laws likely increase the awareness of self-defense and increase the gun stock in the traffic fleet even if the overall gun stock remains the same. Furthermore, the significant positive effects on traffic fatalities suggest the additional negative externalities of SYG laws extend beyond violent crime and suicide.

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# Tables

**Table 1 Stand-Your-Ground Laws, 1989-2018**

| State | Effective Date | State | Effective Date |
|---|---|---|---|
| Alabama | 6/1/06 | Nevada | 10/1/11 |
| Arizona | 4/24/06 | New Hampshire | 11/11/11 |
| Florida | 10/1/05 | North Carolina | 12/1/11 |
| Georgia | 7/1/06 | Ohio | 9/9/08 |
| Idaho | 7/1/18 | Oklahoma | 11/1/06 |
| Indiana | 7/1/06 | Pennsylvania | 8/29/11 |
| Iowa | 7/1/17 | South Carolina | 6/9/06 |
| Kansas | 5/25/06 | South Dakota | 7/1/06 |
| Kentucky | 7/12/06 | Tennessee | 5/22/07 |
| Louisiana | 8/15/06 | Texas | 9/1/07 |
| Michigan | 10/1/06 | Utah | 3/2/94 |
| Mississippi | 7/1/06 | West Virginia | 2/28/08 |
| Missouri | 8/28/07 | Wisconsin | 12/21/11 |
| Montana | 4/27/09 | Wyoming | 7/1/18 |

Notes. This table lists the states which implemented SYG laws in 1989-2018 and their corresponding effective dates. Alaska adopted the SYG law in 2013. Arkansas and North Dakota implemented SYG laws in 2021.

**Table 2 Outcomes for Reduced Form Regressions**

| Outcome Variables | Mean All | SYG States | Other States | Observations |
|---|---|---|---|---|
| Total Fatalities | 13.65 (5.04) | 16.11 (4.52) | 10.58 (3.81) | 1440 |
| **Traffic Fatalities by Related Factors** | | | | |
| Aggressive Driving | 6.49 (2.77) | 7.67 (2.65) | 5.02 (2.13) | 1440 |
| Speeding | 4.2 (2.11) | 4.86 (2.23) | 3.37 (1.62) | 1440 |
| No Alcohol Involved | 9.25 (3.4) | 11.01 (3.15) | 7.05 (2.23) | 1440 |
| Alcohol Involved (BAC > 0) | 3.7 (1.72) | 4.26 (1.61) | 2.99 (1.59) | 1440 |
| Alcohol Involved (BAC ≥ .08) | 3.01 (1.46) | 3.51 (1.37) | 2.38 (1.31) | 1440 |
| Alcohol Involved (BAC ≥ .1) | 2.9 (1.41) | 3.39 (1.33) | 2.29 (1.27) | 1440 |
| Rural | 7.71 (4.72) | 9.78 (4.51) | 5.11 (3.55) | 1440 |
| Urban | 5.89 (2.08) | 6.25 (2.24) | 5.44 (1.75) | 1440 |
| Daytime | 6.58 (2.55) | 7.79 (2.33) | 5.07 (1.93) | 1440 |
| Nighttime | 6.97 (2.62) | 8.21 (2.41) | 5.41 (1.96) | 1440 |
| Weekday | 7.83 (2.86) | 9.24 (2.55) | 6.06 (2.15) | 1440 |
| Weekend | 5.8 (2.24) | 6.84 (2.07) | 4.5 (1.71) | 1440 |
| Male | 19.23 (7.03) | 22.68 (6.36) | 14.91 (5.21) | 1440 |
| Female | 8.28 (3.27) | 9.79 (2.98) | 6.39 (2.54) | 1440 |
| Ages 15-19 | 20.88 (10.37) | 24.7 (10.16) | 16.11 (8.48) | 1440 |
| Ages 20-29 | 21.8 (8.32) | 25.62 (7.56) | 17.02 (6.56) | 1440 |
| Ages 30-39 | 14.66 (6.33) | 17.79 (5.73) | 10.73 (4.63) | 1440 |
| Ages 40-49 | 13.38 (5.5) | 16.17 (4.93) | 9.88 (3.98) | 1440 |
| Ages 50-64 | 12.97 (4.59) | 15.25 (4.07) | 10.11 (3.47) | 1440 |
| Ages 65+ | 17.31 (5.71) | 19.47 (5.6) | 14.6 (4.59) | 1440 |

Notes. This table shows the summary statistics for traffic fatality rates from National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS), 1989-2018. The second, third, and fourth columns show the weighted means with standard deviations in parentheses for all states, states that implemented SYG laws, and states that have not yet implemented SYG laws in the sample, respectively. The last columns display the number of observations for all states in the sample. Traffic fatalities by gender and age group are corresponding fatality counts divided by 100,000 relevant gender or age group population. For the rest of the traffic fatality rates, the statistics are the related fatality counts divided by 100,000 state population. All means are weighted by the state-by-year population.

**Table 3 Outcomes for Regressions Testing Mechanisms**

| Outcome Variables | Mean All | SYG States | Other States | Observations |
|---|---|---|---|---|
| **Gun Ownership Proxies** | | | | |
| Rand Estimates | 36.23 (12.95) | 43.06 (8.56) | 27.71 (12.44) | 1344 |
| Firearm Suicide Rate | 52.72 (12.31) | 59.74 (7.89) | 43.96 (11.17) | 1344 |
| **Crime** | | | | |
| Murder | 6.61 (3.57) | 6.8 (2.92) | 6.38 (4.23) | 1440 |
| Aggravated Assault | 1286 (480) | 1454 (445) | 1076 (437) | 1440 |
| **Road Rage Crime Proxies** | | | | |
| Firearm Involved | 2.29 (1.62) | 2.6 (1.6) | 1.54 (1.42) | 374 |
| Vehicle Involved | 1.69 (1.15) | 2 (1.16) | 0.93 (0.68) | 374 |
| Other Weapon Involved | 3.91 (2.42) | 3.56 (2.01) | 4.81 (3.06) | 374 |
| **Vehicle Miles Traveled** | | | | |
| Total Vehicle Miles Traveled | 1412 (207) | 1490 (201) | 1316 (170) | 1440 |
| Urban Vehicle Miles Traveled | 914 (187) | 891 (196) | 942 (173) | 1440 |
| Rural Vehicle Miles Traveled | 498 (254) | 599 (225) | 372.61 (231) | 1440 |

Notes. This table shows the summary statistics for gun ownership proxies, road rage crime proxies, and vehicle miles traveled in the sample. Rand estimates of gun ownership, defined as the percentage of adults living in a household with a firearm, are from Schell et al. (2020), 1989-2016. The firearm suicide rate, defined as the percentage of suicide committed by a firearm, is from the Centers for Disease Control and Prevention Compressed Mortality File, 1989-2016. Murder and aggravated assault, defined as the actual crime divided by the agency reported population, are from the Federal Bureau of Investigation's Uniform Crime Reporting Program, 1989-2018. Road rage proxies are from the Federal Bureau of Investigation's National Incident-Based Reporting System, 2001-2018. Vehicle miles traveled related variables, defined as corresponding vehicle miles traveled divided by 100,000 licensed drivers, are obtained from Federal Highway Administration's Highway Statistics, 1989-2018. The second, third, and fourth columns show the weighted means with standard deviations in parentheses for all states, states which implemented SYG laws, and states which not yet implemented SYG laws in the sample, respectively. The last columns display the number of observations for all states in the sample. The means are weighted by the state-by-year population.

**Table 4 The Effect of SYG Laws on Traffic Fatalities**

| | Total Traffic Fatalities | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| SYG | 0.03* | | |
| | (0.02) | | |
| 5+ years before | | | -0.02 |
| | | | (0.02) |
| 4 years before | | | -0.00 |
| | | | (0.02) |
| 3 years before | | | -0.00 |
| | | | (0.02) |
| 2 years before | | | -0.01 |
| | | | (0.01) |
| Year 0 for SYG | | 0.08* | 0.06* |
| | | (0.04) | (0.03) |
| 1 years after | | 0.03* | 0.02 |
| | | (0.02) | (0.01) |
| 2 years after | | 0.04** | 0.03** |
| | | (0.02) | (0.01) |
| 3 years after | | 0.04** | 0.03** |
| | | (0.02) | (0.02) |
| 4+ years after | | 0.03 | 0.02 |
| | | (0.02) | (0.02) |
| Observations | 1,440 | 1,440 | 1,440 |
| R-squared | 0.89 | 0.89 | 0.89 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 5 The Effect of SYG Laws on Traffic Fatalities Related to Aggressive Driving**

| | All Related Factors | | | Factor Related to Speeding | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SYG | 0.02 | | | 0.02 | | |
| | (0.03) | | | (0.05) | | |
| 5+ years before | | | -0.00 | | | -0.01 |
| | | | (0.04) | | | (0.06) |
| 4 years before | | | 0.01 | | | 0.01 |
| | | | (0.03) | | | (0.05) |
| 3 years before | | | 0.00 | | | -0.01 |
| | | | (0.03) | | | (0.04) |
| 2 years before | | | 0.01 | | | -0.01 |
| | | | (0.02) | | | (0.03) |
| Year 0 for SYG | | 0.11 | 0.12* | | 0.19* | 0.17** |
| | | (0.08) | (0.07) | | (0.10) | (0.08) |
| 1 years after | | 0.05 | 0.06* | | 0.10** | 0.09** |
| | | (0.03) | (0.03) | | (0.05) | (0.04) |
| 2 years after | | 0.05 | 0.05 | | 0.09* | 0.08* |
| | | (0.04) | (0.03) | | (0.05) | (0.05) |
| 3 years after | | 0.03 | 0.04 | | 0.05 | 0.04 |
| | | (0.03) | (0.03) | | (0.05) | (0.05) |
| 4+ years after | | -0.01 | -0.01 | | -0.05 | -0.05 |
| | | (0.05) | (0.04) | | (0.06) | (0.06) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.78 | 0.78 | 0.78 | 0.66 | 0.66 | 0.66 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 6 The Effect of SYG Laws on Traffic Fatalities Split Per Level of Alcohol Involvement**

| | No Alcohol | | | BAC > 0 | | | BAC ≥ .08 | | | BAC ≥ .1 | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| SYG | 0.04** | | | 0.01 | | | 0.02 | | | 0.01 | | |
| | (0.02) | | | (0.06) | | | (0.05) | | | (0.05) | | |
| 5+ years before | | | -0.01 | | | -0.03 | | | -0.02 | | | -0.02 |
| | | | (0.02) | | | (0.08) | | | (0.08) | | | (0.08) |
| 4 years before | | | -0.01 | | | 0.01 | | | 0.01 | | | 0.01 |
| | | | (0.02) | | | (0.08) | | | (0.07) | | | (0.07) |
| 3 years before | | | -0.01 | | | 0.03 | | | 0.02 | | | 0.03 |
| | | | (0.02) | | | (0.07) | | | (0.07) | | | (0.07) |
| 2 years before | | | -0.01 | | | -0.04 | | | -0.05 | | | -0.06 |
| | | | (0.02) | | | (0.06) | | | (0.06) | | | (0.07) |
| Year 0 for SYG | | 0.09* | 0.07** | | 0.04 | 0.00 | | 0.04 | 0.02 | | 0.03 | 0.01 |
| | | (0.04) | (0.03) | | (0.13) | (0.12) | | (0.11) | (0.11) | | (0.11) | (0.11) |
| 1 years after | | 0.03 | 0.02 | | 0.04 | 0.03 | | 0.05 | 0.04 | | 0.04 | 0.04 |
| | | (0.02) | (0.01) | | (0.04) | (0.06) | | (0.04) | (0.06) | | (0.04) | (0.06) |
| 2 years after | | 0.04* | 0.03** | | 0.06 | 0.05 | | 0.06 | 0.05 | | 0.06 | 0.05 |
| | | (0.02) | (0.01) | | (0.06) | (0.07) | | (0.06) | (0.07) | | (0.06) | (0.07) |
| 3 years after | | 0.05** | 0.04** | | 0.03 | 0.02 | | 0.03 | 0.02 | | 0.02 | 0.01 |
| | | (0.02) | (0.02) | | (0.07) | (0.08) | | (0.06) | (0.08) | | (0.06) | (0.08) |
| 4+ years after | | 0.04* | 0.04* | | -0.03 | -0.04 | | -0.01 | -0.02 | | -0.02 | -0.03 |
| | | (0.02) | (0.02) | | (0.07) | (0.08) | | (0.07) | (0.08) | | (0.07) | (0.08) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.81 | 0.81 | 0.81 | 0.68 | 0.68 | 0.68 | 0.68 | 0.68 | 0.68 | 0.67 | 0.67 | 0.68 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 7 The Effect of SYG Laws on Traffic Fatalities Split Per Location Type**

| | Rural | | | Urban | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SYG | -0.02 | | | 0.13** | | |
| | (0.04) | | | (0.06) | | |
| 5+ years before | | | -0.10** | | | 0.02 |
| | | | (0.04) | | | (0.05) |
| 4 years before | | | -0.06 | | | -0.06 |
| | | | (0.04) | | | (0.09) |
| 3 years before | | | -0.04 | | | 0.01 |
| | | | (0.03) | | | (0.05) |
| 2 years before | | | -0.02 | | | -0.05 |
| | | | (0.02) | | | (0.04) |
| Year 0 for SYG | | 0.05 | -0.07 | | 0.22** | 0.22*** |
| | | (0.06) | (0.06) | | (0.09) | (0.08) |
| 1 years after | | 0.01 | -0.05* | | 0.09** | 0.09** |
| | | (0.03) | (0.03) | | (0.04) | (0.04) |
| 2 years after | | 0.01 | -0.04 | | 0.08 | 0.07 |
| | | (0.04) | (0.04) | | (0.06) | (0.05) |
| 3 years after | | -0.01 | -0.06 | | 0.10 | 0.10 |
| | | (0.05) | (0.04) | | (0.08) | (0.08) |
| 4+ years after | | -0.05 | -0.10** | | 0.16** | 0.16** |
| | | (0.04) | (0.04) | | (0.07) | (0.06) |
| Observations | 1,440 | 1,440 | 1,440 | 1,438 | 1,438 | 1,438 |
| R-squared | 0.81 | 0.81 | 0.81 | 0.37 | 0.37 | 0.37 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 8 The Effect of SYG Laws on Traffic Fatalities Split Per Time**

| | Daytime | | | Nighttime | | | Weekday | | | Weekend | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| SYG | 0.02 | | | 0.04* | | | 0.03* | | | 0.03* | | |
| | (0.01) | | | (0.02) | | | (0.02) | | | (0.02) | | |
| 5+ years before | | | -0.02 | | | -0.01 | | | -0.02 | | | -0.02 |
| | | | (0.02) | | | (0.03) | | | (0.02) | | | (0.03) |
| 4 years before | | | 0.00 | | | -0.01 | | | -0.00 | | | -0.01 |
| | | | (0.02) | | | (0.03) | | | (0.02) | | | (0.03) |
| 3 years before | | | -0.01 | | | 0.00 | | | 0.01 | | | -0.02 |
| | | | (0.02) | | | (0.02) | | | (0.02) | | | (0.02) |
| 2 years before | | | -0.00 | | | -0.02 | | | -0.01 | | | -0.02 |
| | | | (0.01) | | | (0.01) | | | (0.01) | | | (0.02) |
| Year 0 for SYG | | 0.07 | 0.05 | | 0.09* | 0.07* | | 0.09** | 0.07** | | 0.07 | 0.05 |
| | | (0.05) | (0.04) | | (0.05) | (0.04) | | (0.04) | (0.03) | | (0.06) | (0.04) |
| 1 years after | | 0.03 | 0.02 | | 0.03* | 0.02 | | 0.03 | 0.02 | | 0.03* | 0.02 |
| | | (0.02) | (0.01) | | (0.02) | (0.02) | | (0.02) | (0.01) | | (0.02) | (0.02) |
| 2 years after | | 0.03 | 0.02 | | 0.04 | 0.03 | | 0.04** | 0.03** | | 0.04 | 0.03 |
| | | (0.02) | (0.02) | | (0.03) | (0.02) | | (0.02) | (0.01) | | (0.03) | (0.02) |
| 3 years after | | 0.03 | 0.02 | | 0.06** | 0.05* | | 0.05** | 0.04** | | 0.04* | 0.02 |
| | | (0.02) | (0.01) | | (0.03) | (0.03) | | (0.02) | (0.02) | | (0.02) | (0.02) |
| 4+ years after | | 0.02 | 0.01 | | 0.04 | 0.03 | | 0.03 | 0.03 | | 0.03 | 0.02 |
| | | (0.02) | (0.02) | | (0.03) | (0.03) | | (0.02) | (0.02) | | (0.02) | (0.03) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.87 | 0.87 | 0.87 | 0.85 | 0.85 | 0.85 | 0.86 | 0.86 | 0.86 | 0.86 | 0.86 | 0.86 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 9 The Effect of SYG Laws on Traffic Fatalities Split Per Gender**

| | Male | | | Female | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SYG | 0.04** | | | 0.01 | | |
| | (0.02) | | | (0.02) | | |
| 5+ years before | | | -0.02 | | | -0.00 |
| | | | (0.02) | | | (0.03) |
| 4 years before | | | -0.00 | | | -0.01 |
| | | | (0.02) | | | (0.03) |
| 3 years before | | | -0.01 | | | 0.01 |
| | | | (0.02) | | | (0.02) |
| 2 years before | | | -0.02* | | | 0.01 |
| | | | (0.01) | | | (0.02) |
| Year 0 for SYG | | 0.11** | 0.08** | | 0.02 | 0.02 |
| | | (0.05) | (0.04) | | (0.04) | (0.04) |
| 1 years after | | 0.04** | 0.03 | | 0.00 | 0.00 |
| | | (0.02) | (0.02) | | (0.02) | (0.02) |
| 2 years after | | 0.04** | 0.03* | | 0.03* | 0.03* |
| | | (0.02) | (0.02) | | (0.02) | (0.02) |
| 3 years after | | 0.05** | 0.04** | | 0.02 | 0.03 |
| | | (0.02) | (0.02) | | (0.02) | (0.03) |
| 4+ years after | | 0.04* | 0.03 | | 0.01 | 0.01 |
| | | (0.02) | (0.02) | | (0.02) | (0.03) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.87 | 0.87 | 0.87 | 0.87 | 0.87 | 0.87 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 10 The Effect of SYG Laws on Traffic Fatalities Split Per Age**

| | Ages 15-19 | | | Ages 20-29 | | | Ages 30-39 | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| SYG | 0.07** | | | 0.04* | | | 0.05* | | |
| | (0.03) | | | (0.02) | | | (0.02) | | |
| 5+ years before | | | -0.01 | | | -0.01 | | | -0.02 |
| | | | (0.04) | | | (0.03) | | | (0.03) |
| 4 years before | | | 0.01 | | | -0.01 | | | -0.01 |
| | | | (0.04) | | | (0.03) | | | (0.04) |
| 3 years before | | | -0.02 | | | -0.00 | | | -0.01 |
| | | | (0.02) | | | (0.03) | | | (0.04) |
| 2 years before | | | 0.01 | | | -0.02 | | | -0.04 |
| | | | (0.03) | | | (0.03) | | | (0.03) |
| Year 0 for SYG | | 0.17* | 0.15** | | 0.07 | 0.05 | | 0.07 | 0.04 |
| | | (0.08) | (0.07) | | (0.07) | (0.07) | | (0.06) | (0.06) |
| 1 years after | | 0.09*** | 0.09** | | 0.01 | -0.00 | | 0.04 | 0.03 |
| | | (0.02) | (0.03) | | (0.03) | (0.04) | | (0.03) | (0.03) |
| 2 years after | | 0.03 | 0.02 | | 0.08** | 0.07** | | 0.06* | 0.04 |
| | | (0.04) | (0.05) | | (0.03) | (0.03) | | (0.03) | (0.03) |
| 3 years after | | 0.08 | 0.07 | | 0.07* | 0.06* | | 0.07** | 0.06 |
| | | (0.05) | (0.05) | | (0.04) | (0.04) | | (0.03) | (0.04) |
| 4+ years after | | 0.07* | 0.07 | | 0.04 | 0.03 | | 0.04 | 0.03 |
| | | (0.04) | (0.05) | | (0.03) | (0.03) | | (0.03) | (0.03) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.89 | 0.89 | 0.89 | 0.79 | 0.79 | 0.79 | 0.69 | 0.69 | 0.69 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 10 (Continued)**

| | Ages 40-49 | | | Ages 50-64 | | | Ages 65+ | | |
|---|---|---|---|---|---|---|---|---|---|
| | (10) | (11) | (12) | (13) | (14) | (15) | (16) | (17) | (18) |
| SYG | 0.04* | | | 0.04 | | | 0.00 | | |
| | (0.02) | | | (0.02) | | | (0.02) | | |
| 5+ years before | | | -0.06** | | | -0.03 | | | 0.00 |
| | | | (0.03) | | | (0.02) | | | (0.03) |
| 4 years before | | | -0.06** | | | 0.00 | | | 0.02 |
| | | | (0.03) | | | (0.04) | | | (0.03) |
| 3 years before | | | -0.01 | | | 0.01 | | | 0.01 |
| | | | (0.02) | | | (0.03) | | | (0.03) |
| 2 years before | | | -0.05** | | | 0.00 | | | 0.03 |
| | | | (0.02) | | | (0.02) | | | (0.02) |
| Year 0 for SYG | | 0.10* | 0.00 | | 0.10** | 0.08* | | 0.03 | 0.05 |
| | | (0.05) | (0.04) | | (0.04) | (0.04) | | (0.06) | (0.05) |
| 1 years after | | 0.05** | 0.01 | | 0.04 | 0.03 | | 0.01 | 0.02 |
| | | (0.03) | (0.02) | | (0.02) | (0.02) | | (0.02) | (0.02) |
| 2 years after | | 0.05** | 0.01 | | 0.05** | 0.04* | | -0.02 | -0.02 |
| | | (0.02) | (0.02) | | (0.03) | (0.02) | | (0.02) | (0.02) |
| 3 years after | | 0.04 | 0.00 | | 0.03 | 0.02 | | 0.01 | 0.02 |
| | | (0.03) | (0.03) | | (0.03) | (0.03) | | (0.03) | (0.03) |
| 4+ years after | | 0.04 | 0.00 | | 0.04 | 0.03 | | 0.00 | 0.01 |
| | | (0.03) | (0.03) | | (0.03) | (0.02) | | (0.03) | (0.02) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.59 | 0.59 | 0.59 | 0.49 | 0.49 | 0.49 | 0.81 | 0.81 | 0.81 |

Notes. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 11 Robustness Check for Total Traffic Fatalities Analysis**

| | Baseline | All-Places provisions | Vehicle Provisions |
|---|---|---|---|
| | (1) | (2) | (3) |
| 5+ years before | -0.02 | -0.03 | -0.02 |
| | (0.02) | (0.03) | (0.03) |
| 4 years before | -0.00 | -0.01 | -0.01 |
| | (0.02) | (0.04) | (0.03) |
| 3 years before | -0.00 | -0.03 | 0.00 |
| | (0.02) | (0.03) | (0.03) |
| 2 years before | -0.01 | -0.01 | -0.01 |
| | (0.01) | (0.03) | (0.01) |
| Year 0 for SYG | 0.06* | 0.05 | 0.05 |
| | (0.03) | (0.05) | (0.04) |
| 1 years after | 0.02 | 0.02 | 0.02 |
| | (0.01) | (0.02) | (0.02) |
| 2 years after | 0.03** | 0.03* | 0.03 |
| | (0.01) | (0.02) | (0.02) |
| 3 years after | 0.03** | 0.03 | 0.04* |
| | (0.02) | (0.03) | (0.02) |
| 4+ years after | 0.02 | 0.04 | 0.02 |
| | (0.02) | (0.03) | (0.03) |
| Observations | 1,440 | 810 | 1,230 |
| R-squared | 0.89 | 0.91 | 0.89 |

Notes. Column (1) repeats the baseline regression result from Column (3) of Table 4. Column (2) runs the baseline regression for sample excluding states implemented SYG laws which extend castle doctrine to any places one has a legal right to be. Column (3) runs the baseline regression for sample excluding states implemented SGY laws which extend castle doctrine to vehicle. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. All regressions include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions have controls for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 12 Sensitivity Check for Total Traffic Fatalities Analysis - Alternative Functional Form**

| | log (count/population) | log (count) | log (count/driver) | log (count/VMT) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 5+ years before | -0.02 | -0.02 | -0.02 | -0.02 |
| | (0.02) | (0.02) | (0.03) | (0.03) |
| 4 years before | -0.00 | -0.00 | -0.01 | -0.01 |
| | (0.02) | (0.02) | (0.03) | (0.03) |
| 3 years before | -0.00 | -0.00 | -0.02 | -0.01 |
| | (0.02) | (0.02) | (0.02) | (0.02) |
| 2 years before | -0.01 | -0.01 | -0.02 | -0.01 |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Year 0 for SYG | 0.06* | 0.06* | 0.06* | 0.06* |
| | (0.03) | (0.03) | (0.03) | (0.03) |
| 1 years after | 0.02 | 0.02 | 0.02 | 0.02 |
| | (0.01) | (0.01) | (0.02) | (0.02) |
| 2 years after | 0.03** | 0.03** | 0.02 | 0.03 |
| | (0.01) | (0.01) | (0.02) | (0.02) |
| 3 years after | 0.03** | 0.03** | 0.03 | 0.03 |
| | (0.02) | (0.02) | (0.03) | (0.02) |
| 4+ years after | 0.02 | 0.03 | 0.04 | 0.02 |
| | (0.02) | (0.02) | (0.03) | (0.03) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.89 | 0.80 | 0.87 | 0.91 |

Notes. Column (1) repeats the baseline regression result from Column (3) of Table 4. Column (2) through Column (4) run regressions with the different definitions of the natural log of traffic fatalities outcome variable. Column (2) defines the outcome variable as traffic fatalities count. Column (3) defines the outcome variables as traffic fatality count divided by 100,000 licensed drivers. Column (4) defines the outcome variables as traffic fatality count divided by the total vehicle miles traveled (VMT). All regressions include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions have controls for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Column (2) also controls for the natural log of the state population. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 13 Sensitivity Check for Total Traffic Fatalities Analysis - Alternative Covariates**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| 5+ years before | -0.02 | -0.05* | -0.04 | -0.05** | -0.03 | 0.04 | 0.03 | -0.02 |
| | (0.02) | (0.03) | (0.03) | (0.02) | (0.02) | (0.03) | (0.03) | (0.02) |
| 4 years before | -0.00 | -0.02 | -0.02 | -0.02 | -0.01 | 0.01 | 0.01 | -0.00 |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.03) | (0.02) | (0.02) |
| 3 years before | -0.00 | -0.02 | -0.01 | -0.01 | -0.01 | 0.00 | 0.00 | -0.00 |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| 2 years before | -0.01 | -0.02 | -0.01 | -0.02 | -0.01 | -0.00 | -0.00 | -0.01 |
| | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) | (0.01) |
| Year 0 for SYG | 0.06* | 0.07** | 0.07* | 0.05 | 0.05* | 0.05** | 0.05** | 0.05* |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.02) | (0.03) |
| 1 years after | 0.02 | 0.03 | 0.02 | 0.02 | 0.02 | 0.00 | -0.00 | 0.01 |
| | (0.01) | (0.02) | (0.02) | (0.01) | (0.01) | (0.01) | (0.01) | (0.02) |
| 2 years after | 0.03** | 0.03* | 0.03 | 0.04*** | 0.03** | -0.00 | -0.00 | 0.02 |
| | (0.01) | (0.02) | (0.02) | (0.01) | (0.01) | (0.02) | (0.02) | (0.02) |
| 3 years after | 0.03** | 0.03 | 0.02 | 0.05*** | 0.04** | -0.01 | -0.01 | 0.03* |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) |
| 4+ years after | 0.02 | 0.02 | 0.02 | 0.05** | 0.04 | -0.04 | -0.04 | 0.02 |
| | (0.02) | (0.03) | (0.03) | (0.02) | (0.02) | (0.03) | (0.03) | (0.02) |
| State fixed effects | - | - | - | - | - | - | - | - |
| Year fixed effects | - | - | - | - | - | - | - | - |
| Right-to-carry laws | - | - | - | - | - | - | - | - |
| Policing variables | - | | - | - | - | - | - | - |
| Economics variables | - | | | - | - | - | - | - |
| Traffic regulations | - | | | | - | - | - | - |
| Demographics | - | | | | | - | - | - |
| Linear time trends | | | | | | - | | |
| Quadratic time trends | | | | | | | - | |
| Spillover effect | | | | | | | | - |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.89 | 0.85 | 0.85 | 0.88 | 0.89 | 0.92 | 0.92 | 0.90 |

Notes. Column (1) repeats the baseline regression result from Column (3) of Table 4. Column (2) through Column (8) presents the regression results of progressively adding covariates to the basic model with state fixed effects and year fixed effects. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. All regressions include leads and lags for SYG laws, omitting the dummy for one year prior. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 14 Robustness Check for Urban Traffic Fatalities Analysis**

| | Baseline | All-Places provisions | Vehicle Provisions |
|---|---|---|---|
| | (1) | (2) | (3) |
| 5+ years before | 0.02 | -0.10 | 0.02 |
| | (0.05) | (0.12) | (0.05) |
| 4 years before | -0.06 | -0.22 | -0.02 |
| | (0.09) | (0.28) | (0.07) |
| 3 years before | 0.01 | 0.04 | -0.00 |
| | (0.05) | (0.09) | (0.06) |
| 2 years before | -0.05 | -0.11** | -0.03 |
| | (0.04) | (0.05) | (0.05) |
| Year 0 for SYG | 0.22*** | 0.25 | 0.20** |
| | (0.08) | (0.28) | (0.08) |
| 1 years after | 0.09** | 0.13* | 0.08 |
| | (0.04) | (0.06) | (0.05) |
| 2 years after | 0.07 | 0.11 | 0.08 |
| | (0.05) | (0.07) | (0.06) |
| 3 years after | 0.10 | 0.14 | 0.10 |
| | (0.08) | (0.11) | (0.10) |
| 4+ years after | 0.16** | 0.19 | 0.16** |
| | (0.06) | (0.11) | (0.07) |
| Observations | 1,438 | 808 | 1,229 |
| R-squared | 0.37 | 0.45 | 0.48 |

Notes. Column (1) repeats the baseline regression result from Column (6) of Table 6. Column (2) runs the baseline regression for sample excluding states implemented SYG laws which extend castle doctrine to any places one has a legal right to be. Column (3) runs the baseline regression for sample excluding states implemented SGY laws which extend castle doctrine to vehicle. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. All regressions include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions have controls for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.

Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 15 Sensitivity Check for Urban Traffic Fatalities Analysis - Alternative Functional Form**

| | log (count/population) | log (count) | log (count/driver) | log (count/VMT) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 5+ years before | 0.02 | 0.02 | 0.02 | 0.01 |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| 4 years before | -0.06 | -0.06 | -0.06 | -0.08 |
| | (0.09) | (0.09) | (0.09) | (0.09) |
| 3 years before | 0.01 | 0.02 | 0.00 | 0.01 |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| 2 years before | -0.05 | -0.05 | -0.06 | -0.04 |
| | (0.04) | (0.04) | (0.05) | (0.04) |
| Year 0 for SYG | 0.22*** | 0.22*** | 0.22*** | 0.21** |
| | (0.08) | (0.08) | (0.07) | (0.08) |
| 1 years after | 0.09** | 0.09** | 0.09** | 0.09** |
| | (0.04) | (0.04) | (0.04) | (0.04) |
| 2 years after | 0.07 | 0.07 | 0.07 | 0.07 |
| | (0.05) | (0.05) | (0.05) | (0.05) |
| 3 years after | 0.10 | 0.10 | 0.10 | 0.09 |
| | (0.08) | (0.08) | (0.09) | (0.08) |
| 4+ years after | 0.16** | 0.15** | 0.17** | 0.15** |
| | (0.06) | (0.06) | (0.07) | (0.06) |
| Observations | 1,438 | 1,438 | 1,438 | 1,438 |
| R-squared | 0.37 | 0.32 | 0.37 | 0.39 |

Notes. Column (1) repeats the baseline regression result from Column (6) of Table 7. Column (2) through Column (4) run regressions with the different definitions of the natural log of traffic fatalities outcome variable. Column (2) defines the outcome variable as traffic fatalities count. Column (3) defines the outcome variables as traffic fatality count divided by 100,000 licensed drivers. Column (4) defines the outcome variables as traffic fatality count divided by the total vehicle miles traveled (VMT). All regressions include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions have controls for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Column (2) also controls for the natural log of the state population. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 16 Sensitivity Check for Urban Traffic Fatalities Analysis - Alternative Covariates**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| 5+ years before | 0.02 | -0.05 | -0.02 | -0.01 | 0.02 | 0.06 | 0.07 | 0.02 |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.08) | (0.08) | (0.05) |
| 4 years before | -0.06 | -0.10 | -0.10 | -0.08 | -0.06 | -0.04 | -0.03 | -0.05 |
| | (0.09) | (0.08) | (0.08) | (0.09) | (0.09) | (0.09) | (0.10) | (0.09) |
| 3 years before | 0.01 | -0.02 | -0.01 | 0.00 | 0.01 | 0.02 | 0.03 | 0.02 |
| | (0.05) | (0.04) | (0.04) | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) |
| 2 years before | -0.05 | -0.06 | -0.06 | -0.05 | -0.05 | -0.03 | -0.02 | -0.05 |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.05) | (0.05) | (0.05) |
| Year 0 for SYG | 0.22*** | 0.24*** | 0.24*** | 0.22** | 0.22*** | 0.24*** | 0.26*** | 0.21*** |
| | (0.08) | (0.09) | (0.08) | (0.08) | (0.08) | (0.07) | (0.07) | (0.08) |
| 1 years after | 0.09** | 0.10** | 0.09** | 0.10** | 0.09** | 0.08** | 0.07* | 0.08** |
| | (0.04) | (0.04) | (0.04) | (0.04) | (0.04) | (0.03) | (0.04) | (0.04) |
| 2 years after | 0.07 | 0.08* | 0.08 | 0.09* | 0.08 | 0.04 | 0.02 | 0.06 |
| | (0.05) | (0.05) | (0.05) | (0.05) | (0.05) | (0.06) | (0.06) | (0.05) |
| 3 years after | 0.10 | 0.09 | 0.07 | 0.11 | 0.10 | 0.04 | 0.01 | 0.09 |
| | (0.08) | (0.08) | (0.08) | (0.08) | (0.08) | (0.09) | (0.10) | (0.08) |
| 4+ years after | 0.16** | 0.18*** | 0.16*** | 0.19*** | 0.16** | 0.07 | 0.01 | 0.15** |
| | (0.06) | (0.06) | (0.06) | (0.05) | (0.06) | (0.09) | (0.10) | (0.06) |
| State fixed effects | - | - | - | - | - | - | - | - |
| Year fixed effects | - | - | - | - | - | - | - | - |
| Right-to-carry laws | - | - | - | - | - | - | - | - |
| Policing variables | - | | - | - | - | - | - | - |
| Economics variables | - | | | - | - | - | - | - |
| Traffic regulations | - | | | | - | - | - | - |
| Demographics | - | | | | | - | - | - |
| Linear time trends | | | | | | - | | |
| Quadratic time trends | | | | | | | - | |
| Spillover effect | | | | | | | | - |
| Observations | 1,438 | 1,438 | 1,438 | 1,438 | 1,438 | 1,438 | 1,438 | 1,438 |
| R-squared | 0.37 | 0.32 | 0.33 | 0.36 | 0.37 | 0.45 | 0.45 | 0.37 |

Notes. Column (1) repeats the baseline regression result from Column (6) of Table 7. Column (2) through Column (8) presents the regression results of progressively adding covariates to the basic model with state fixed effects and year fixed effects. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. All regressions include leads and lags for SYG laws, omitting the dummy for one year prior. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 17 The Effect of SYG Laws on Gun Ownership Proxies**

| | Rand Estimates | | | Firearm Suicide Rate | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SYG | 0.03* | | | 0.03** | | |
| | (0.02) | | | (0.01) | | |
| 5+ years before | | | 0.02 | | | -0.02 |
| | | | (0.03) | | | (0.02) |
| 4 years before | | | 0.03 | | | -0.01 |
| | | | (0.02) | | | (0.01) |
| 3 years before | | | 0.03 | | | -0.01 |
| | | | (0.02) | | | (0.01) |
| 2 years before | | | 0.02 | | | 0.00 |
| | | | (0.02) | | | (0.01) |
| Year 0 for SYG | | 0.07 | 0.11** | | 0.05** | 0.03* |
| | | (0.05) | (0.05) | | (0.02) | (0.02) |
| 1 years after | | 0.03** | 0.05** | | 0.02 | 0.01 |
| | | (0.02) | (0.02) | | (0.01) | (0.01) |
| 2 years after | | 0.02 | 0.03 | | 0.02** | 0.01 |
| | | (0.02) | (0.03) | | (0.01) | (0.01) |
| 3 years after | | 0.02 | 0.04 | | 0.03** | 0.02 |
| | | (0.03) | (0.04) | | (0.01) | (0.01) |
| 4+ years after | | 0.05* | 0.06** | | 0.04*** | 0.03** |
| | | (0.03) | (0.03) | | (0.01) | (0.02) |
| Observations | 1,344 | 1,344 | 1,344 | 1,344 | 1,344 | 1,344 |
| R-squared | 0.82 | 0.82 | 0.82 | 0.78 | 0.78 | 0.78 |

Notes. The dependent variable in columns (1) is the natural log of the percentage of adults living in a household with a gun. The dependent variable in Column (2) is the natural log of the percentage of suicide committed by a gun. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average agency-reported population across years. Standard errors in parenthesis are clustered at the state level. Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 18 The Effect of SYG Laws on Crime**

| | Aggravated Assault | | | Murder | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| SYG | 0.04 | | | 0.13* | | |
| | (0.11) | | | (0.07) | | |
| 5+ years before | | | -0.16 | | | 0.01 |
| | | | (0.13) | | | (0.06) |
| 4 years before | | | -0.03 | | | 0.03 |
| | | | (0.05) | | | (0.05) |
| 3 years before | | | -0.01 | | | 0.04 |
| | | | (0.05) | | | (0.04) |
| 2 years before | | | -0.02 | | | 0.04 |
| | | | (0.03) | | | (0.03) |
| Year 0 for SYG | | 0.18 | 0.01 | | 0.10 | 0.14** |
| | | (0.15) | (0.06) | | (0.10) | (0.06) |
| 1 years after | | 0.07 | -0.01 | | 0.10** | 0.12*** |
| | | (0.08) | (0.02) | | (0.05) | (0.03) |
| 2 years after | | 0.06 | -0.01 | | 0.12* | 0.14*** |
| | | (0.09) | (0.03) | | (0.06) | (0.04) |
| 3 years after | | 0.03 | -0.04 | | 0.16** | 0.18*** |
| | | (0.10) | (0.04) | | (0.07) | (0.05) |
| 4+ years after | | 0.03 | -0.03 | | 0.15 | 0.17** |
| | | (0.14) | (0.09) | | (0.10) | (0.08) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.41 | 0.41 | 0.43 | 0.68 | 0.68 | 0.68 |

Notes. The dependent variable in Column (1) through Column (3) is the natural log of aggravated assault counts divided by 100,000 agency-reported population. The dependent variable in Column (4) through Column (6) is the natural log of murder counts divided by 100,000 agency-reported population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average population across years. Standard errors in parenthesis are clustered at the state level.

Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 19 The Effect of SYG Laws on Road Rage Proxies**

| | Road Rage - Firearm | | | Road Rage - Vehicle | | | Road Rage - Other | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| SYG | 0.21** | | | 0.24** | | | 0.09 | | |
| | (0.08) | | | (0.10) | | | (0.07) | | |
| 5+ years before | | | -0.16 | | | 0.03 | | | -0.38*** |
| | | | (0.23) | | | (0.12) | | | (0.10) |
| 4 years before | | | -0.25 | | | 0.00 | | | -0.40*** |
| | | | (0.19) | | | (0.08) | | | (0.07) |
| 3 years before | | | -0.19 | | | 0.03 | | | -0.26*** |
| | | | (0.14) | | | (0.07) | | | (0.08) |
| 2 years before | | | -0.31** | | | -0.05 | | | -0.26*** |
| | | | (0.15) | | | (0.07) | | | (0.08) |
| Year 0 for SYG | | 0.27** | 0.06 | | 0.15 | 0.13 | | 0.33*** | 0.03 |
| | | (0.12) | (0.16) | | (0.16) | (0.13) | | (0.11) | (0.09) |
| 1 years after | | 0.46*** | 0.36*** | | 0.37*** | 0.36*** | | 0.19*** | 0.05 |
| | | (0.12) | (0.11) | | (0.10) | (0.08) | | (0.07) | (0.06) |
| 2 years after | | 0.30** | 0.18 | | 0.25** | 0.24** | | 0.08 | -0.09 |
| | | (0.12) | (0.15) | | (0.12) | (0.12) | | (0.09) | (0.08) |
| 3 years after | | 0.01 | -0.12 | | 0.12 | 0.12 | | -0.12* | -0.29*** |
| | | (0.11) | (0.13) | | (0.12) | (0.11) | | (0.06) | (0.08) |
| 4+ years after | | 0.05 | -0.09 | | 0.13 | 0.12 | | 0.04 | -0.14*** |
| | | (0.11) | (0.08) | | (0.09) | (0.07) | | (0.06) | (0.05) |
| Observations | 360 | 360 | 360 | 360 | 360 | 360 | 360 | 360 | 360 |

Notes. Coefficients from the Poisson fixed effect model are reported. The three dependent variables are the crime counts for the proxy road rage cases involving firearms, vehicles, and other weapons. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, natural log of agency-reported population, state fixed effects, and year fixed effects. Regressions are weighted by the agency-reported population. Standard errors in parenthesis are clustered at the state level.

Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

**Table 20 The Effect of SYG Laws on Vehicle Miles Traveled (VMT) per Drivers**

| | Total VMT | | | Rural VMT | | | Urban VMT | | |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| SYG | 0.02 | | | 0.02 | | | 0.03 | | |
| | (0.01) | | | (0.03) | | | (0.02) | | |
| 5+ years before | | | -0.00 | | | -0.05 | | | -0.00 |
| | | | (0.02) | | | (0.04) | | | (0.04) |
| 4 years before | | | -0.00 | | | -0.05 | | | 0.00 |
| | | | (0.02) | | | (0.03) | | | (0.03) |
| 3 years before | | | -0.01 | | | -0.02 | | | -0.01 |
| | | | (0.01) | | | (0.02) | | | (0.02) |
| 2 years before | | | -0.01 | | | 0.02 | | | -0.02 |
| | | | (0.01) | | | (0.03) | | | (0.02) |
| Year 0 for SYG | | 0.01 | 0.00 | | 0.06 | -0.00 | | 0.01 | 0.00 |
| | | (0.03) | (0.02) | | (0.04) | (0.04) | | (0.05) | (0.03) |
| 1 years after | | 0.01 | 0.00 | | 0.00 | -0.03 | | 0.02 | 0.02 |
| | | (0.01) | (0.01) | | (0.03) | (0.03) | | (0.02) | (0.01) |
| 2 years after | | -0.00 | -0.00 | | -0.00 | -0.03 | | 0.01 | 0.01 |
| | | (0.02) | (0.01) | | (0.04) | (0.04) | | (0.03) | (0.02) |
| 3 years after | | 0.01 | 0.01 | | 0.02 | -0.01 | | 0.02 | 0.01 |
| | | (0.02) | (0.01) | | (0.04) | (0.04) | | (0.03) | (0.02) |
| 4+ years after | | 0.03 | 0.02 | | 0.03 | 0.01 | | 0.04 | 0.03 |
| | | (0.02) | (0.01) | | (0.03) | (0.03) | | (0.03) | (0.02) |
| Observations | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 | 1,440 |
| R-squared | 0.52 | 0.52 | 0.52 | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 | 0.70 |

Notes. The three dependent variables from Column (1) through Column (9) are the natural log of total VMT, rural VMT, and urban VMT divided by 100,000 driver population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYG laws, omitting the dummy for one year prior. All regressions control for state-level covariates listed in Table C1, state fixed effects and year fixed effects. Regressions are weighted by the state average driver population across years. Standard errors in parenthesis are clustered at the state level.
Asterisks denote: *** p<0.01, ** p<0.05, * p<0.1

## Figures

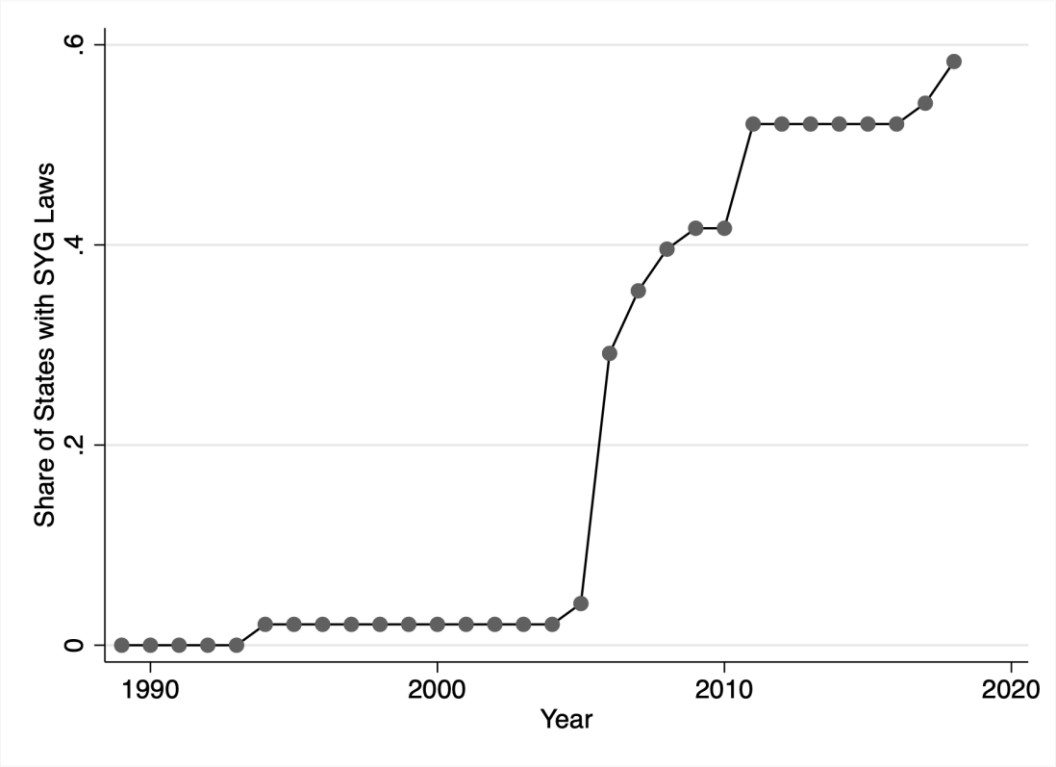


**Figure 1. Share of States with SYG Laws, 1989-2018**

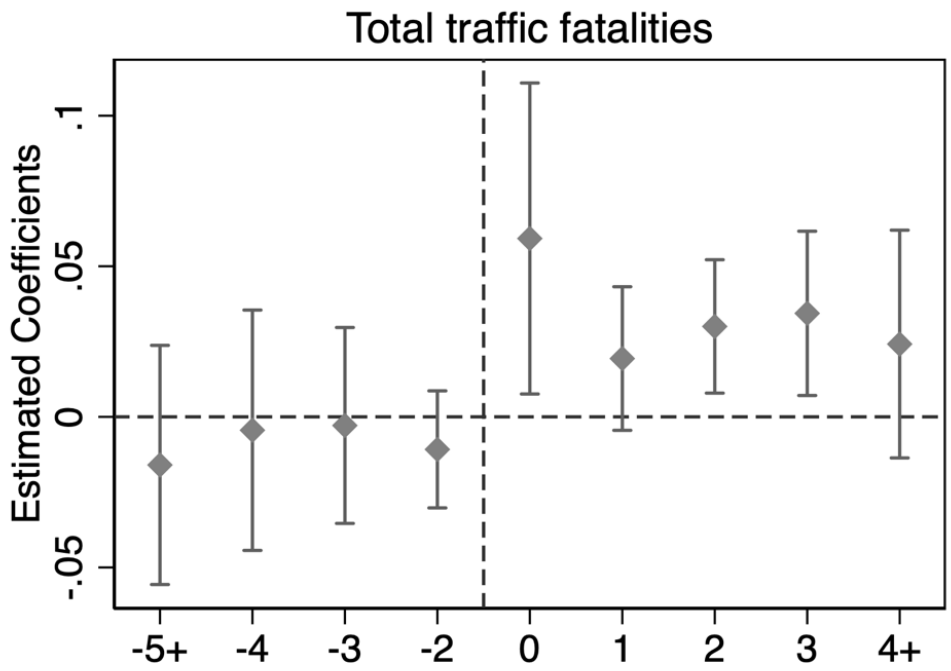


**Figure 2. SYG Laws and Total Traffic Fatalities**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The model is estimated by OLS and controls for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

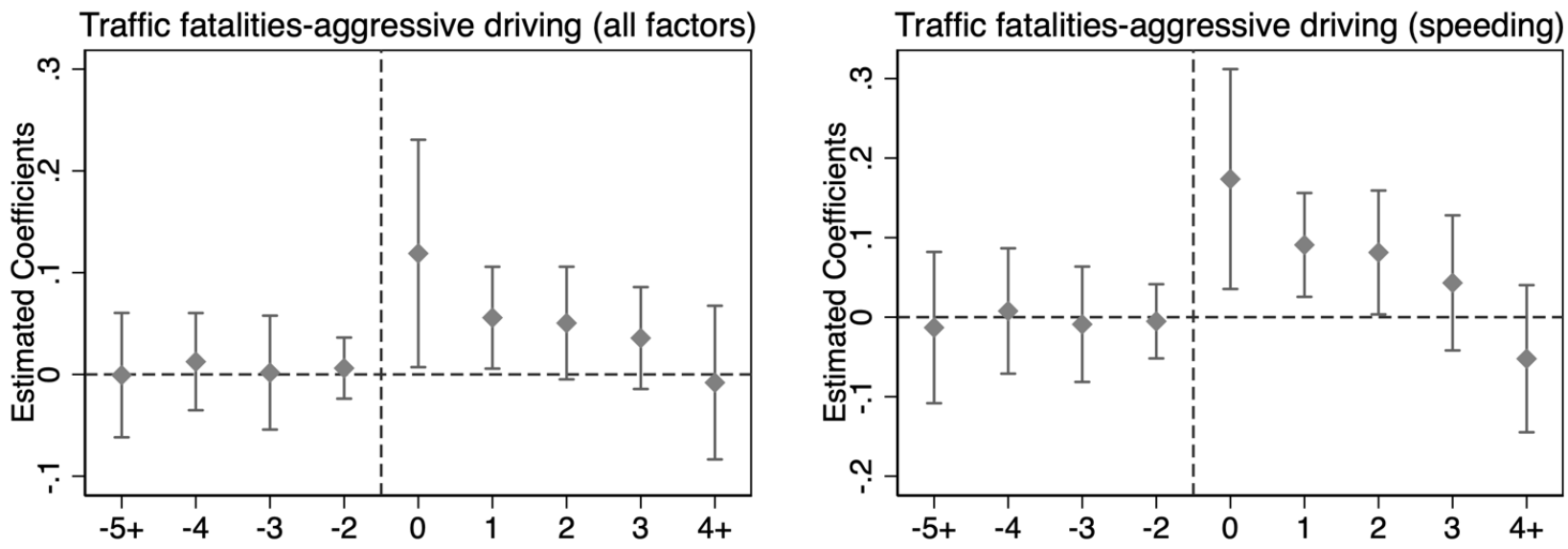


**Figure 3. SYG Laws and Traffic Fatalities Related to Aggressive Driving**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. Both models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

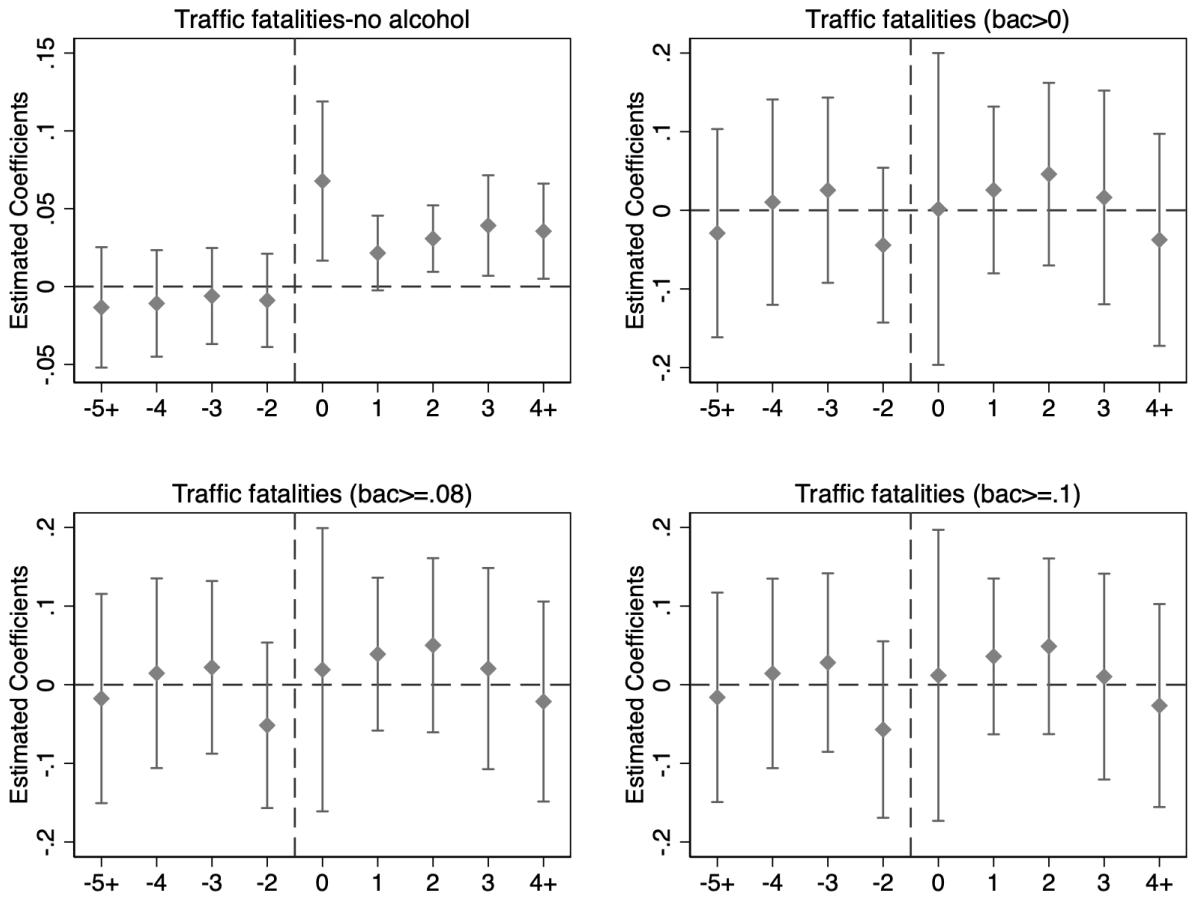


**Figure 4. SYG Laws and Traffic Fatalities Related to Alcohol Involvement**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. All models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

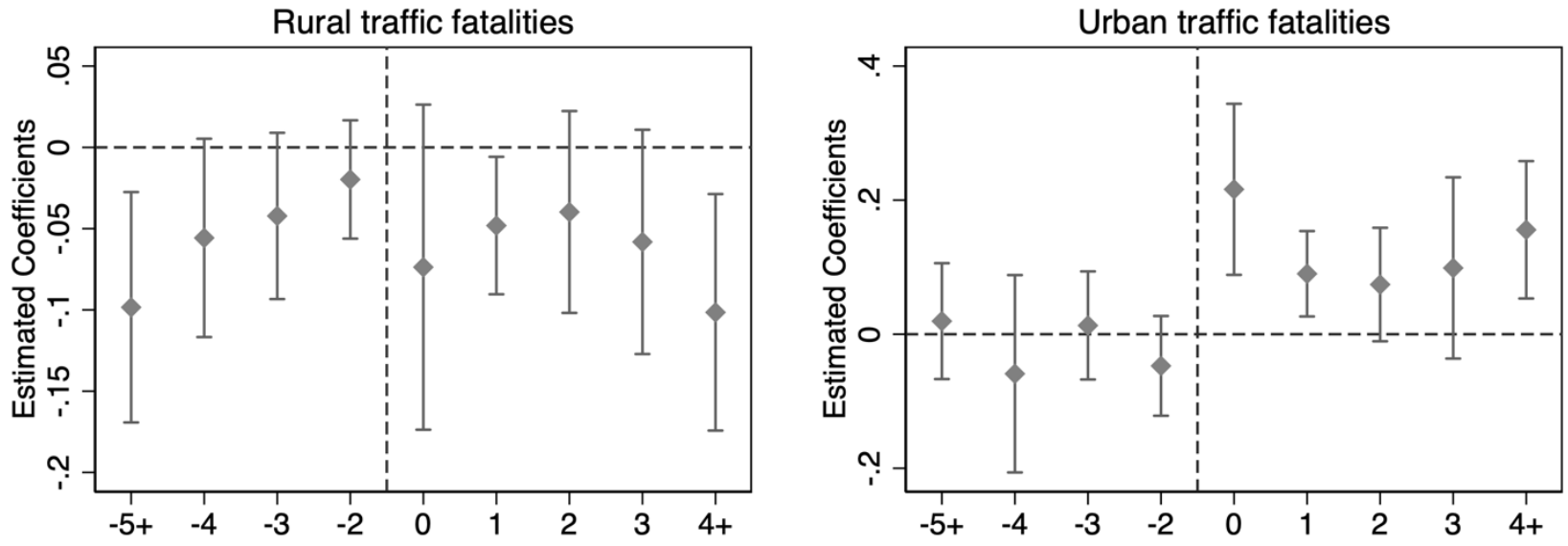


**Figure 5. SYG Laws and Traffic Fatalities Related to Location**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. Both models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

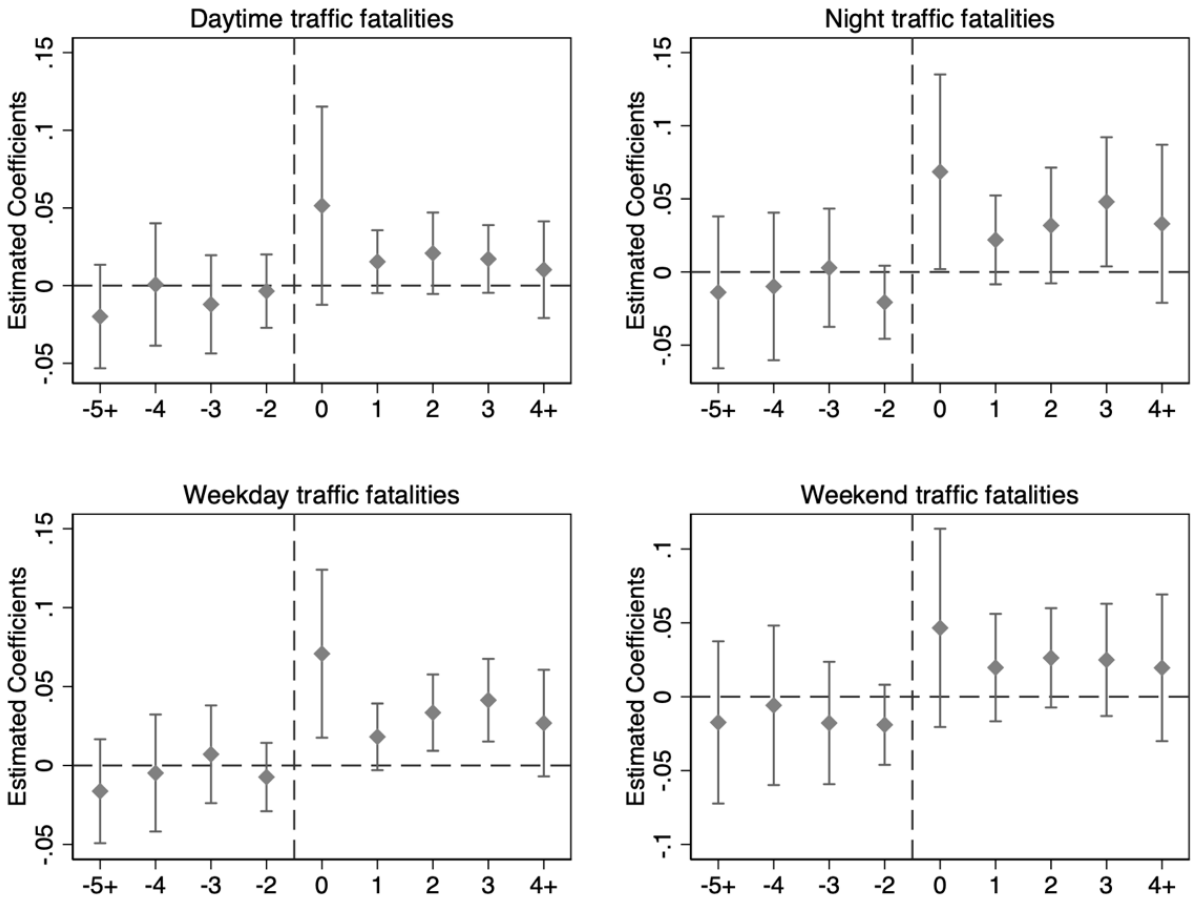


**Figure 6. SYG Laws and Traffic Fatalities by Time**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. All models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

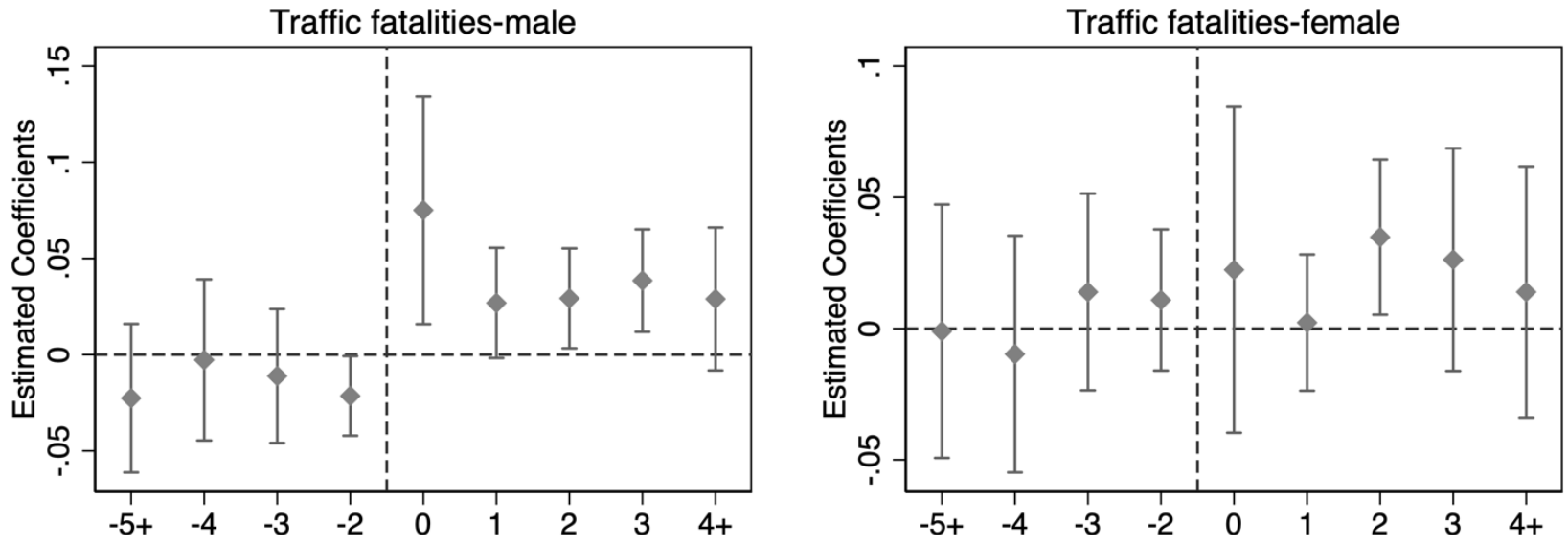


**Figure 7. SYG Laws and Traffic Fatalities by Gender**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. Both models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

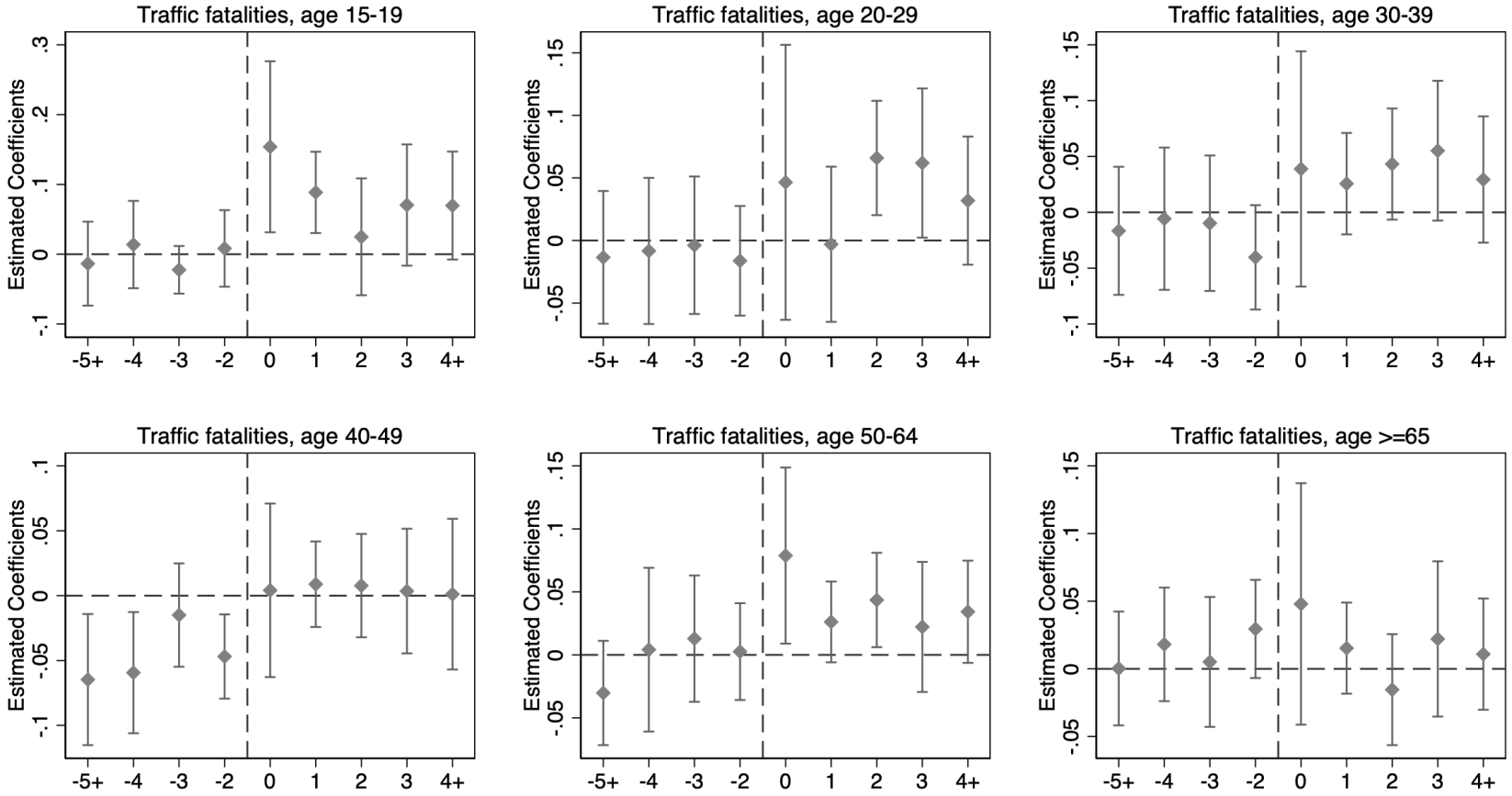


**Figure 8. SYG Laws and Traffic Fatalities by Age**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. All models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

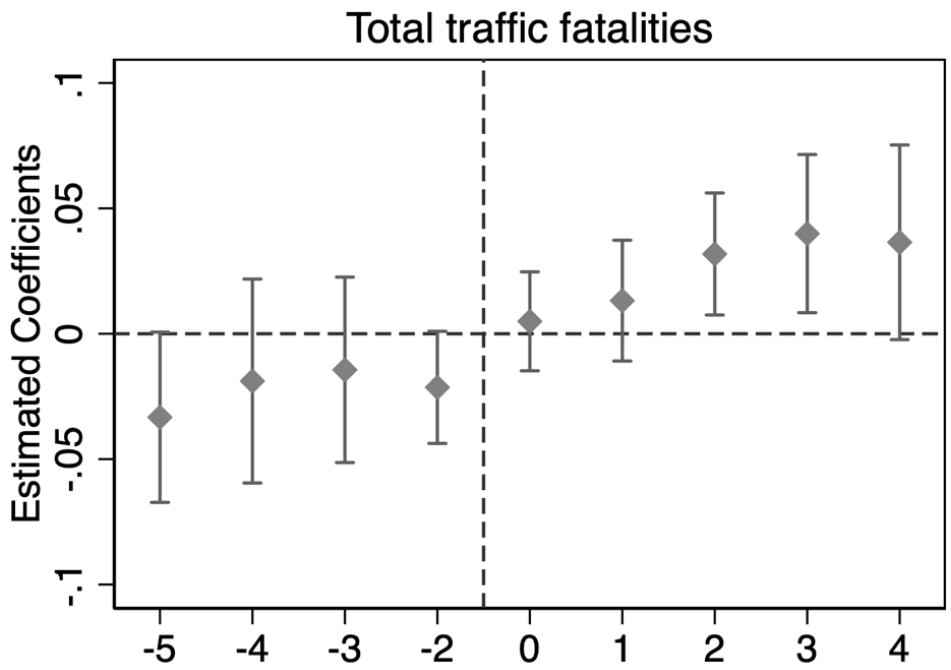


**Figure 9. SYG Laws and Total Traffic Fatalities – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

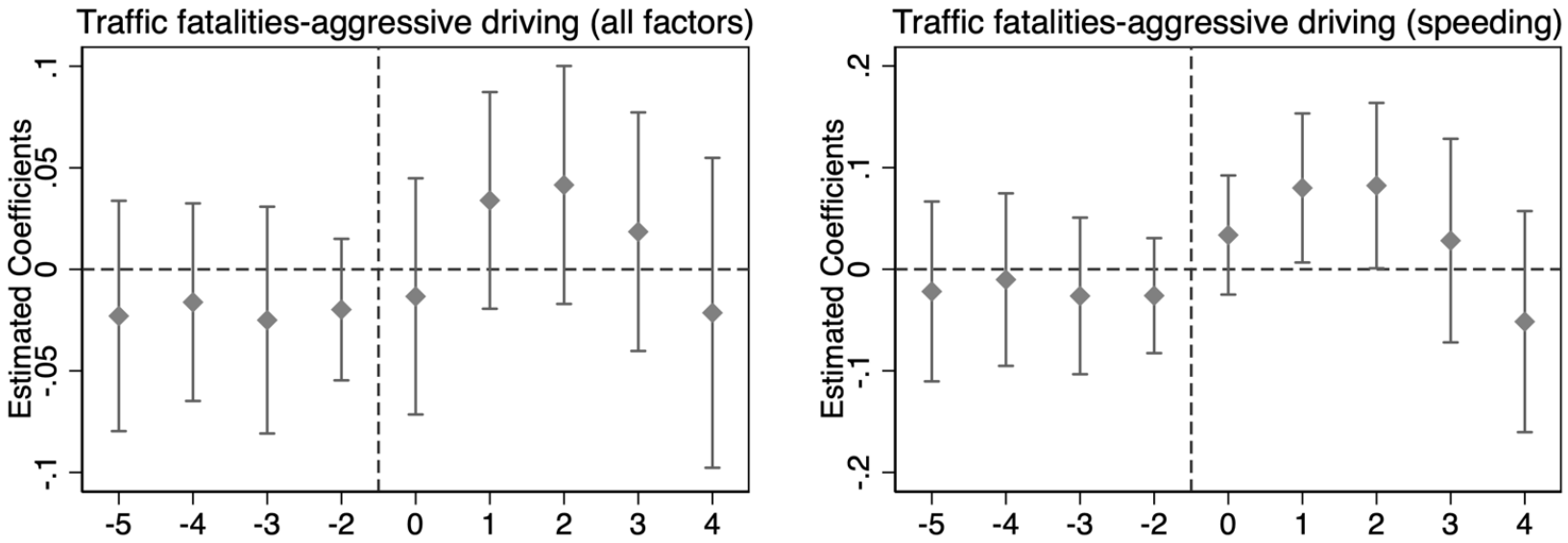


**Figure 10. SYG Laws and Traffic Fatalities Related to Aggressive Driving – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

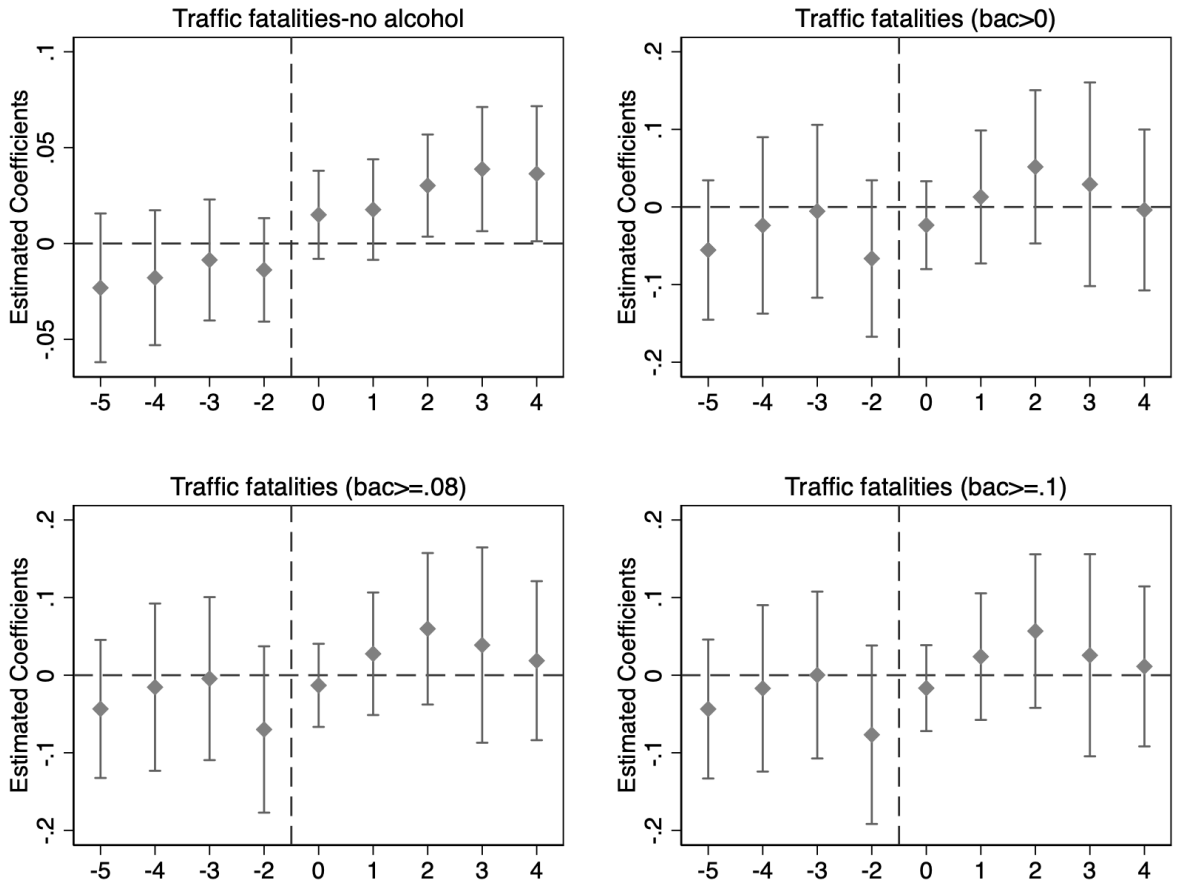


**Figure 11. SYG Laws and Traffic Fatalities Related to Alcohol Involvement – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

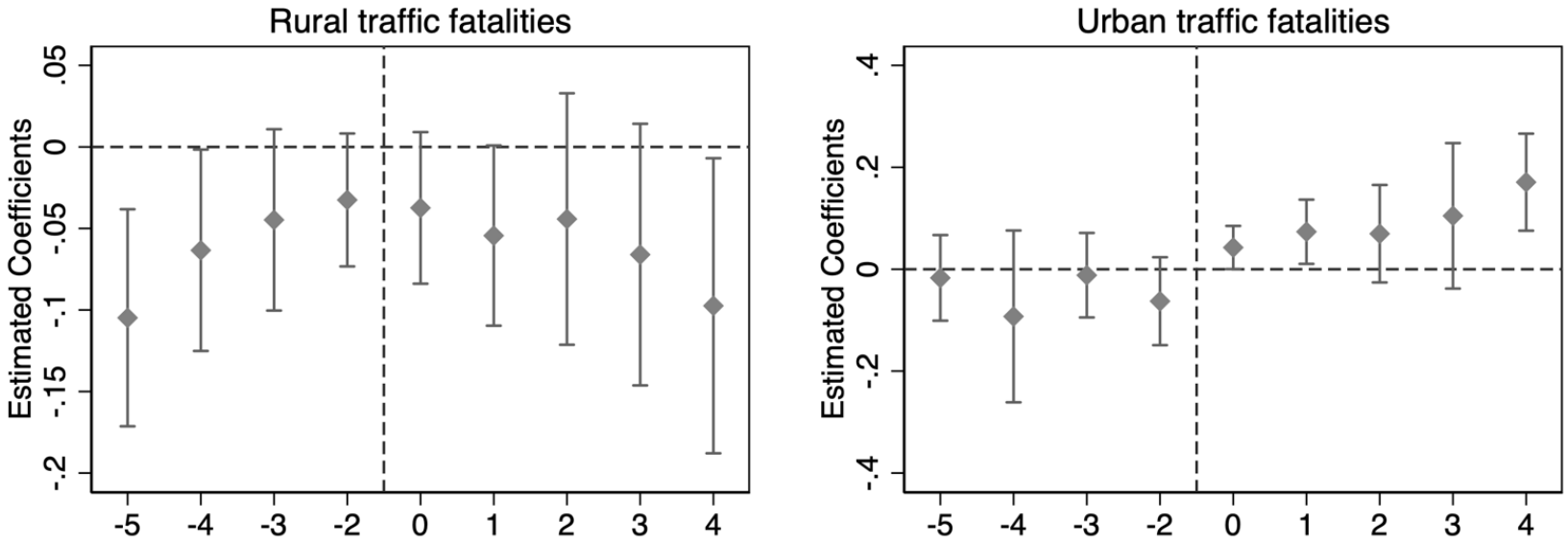


**Figure 12. SYG Laws and Traffic Fatalities Related to Location – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

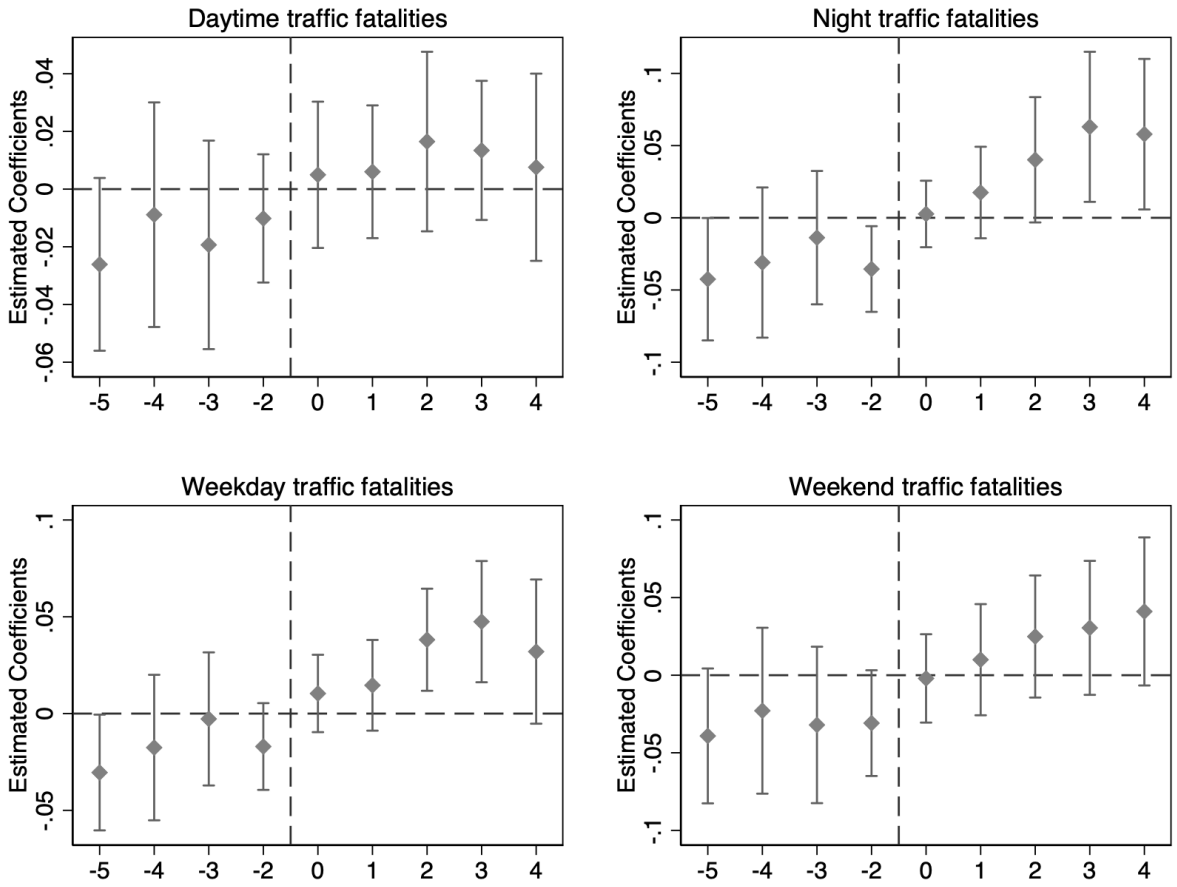


**Figure 13. SYG Laws and Traffic Fatalities by Time – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

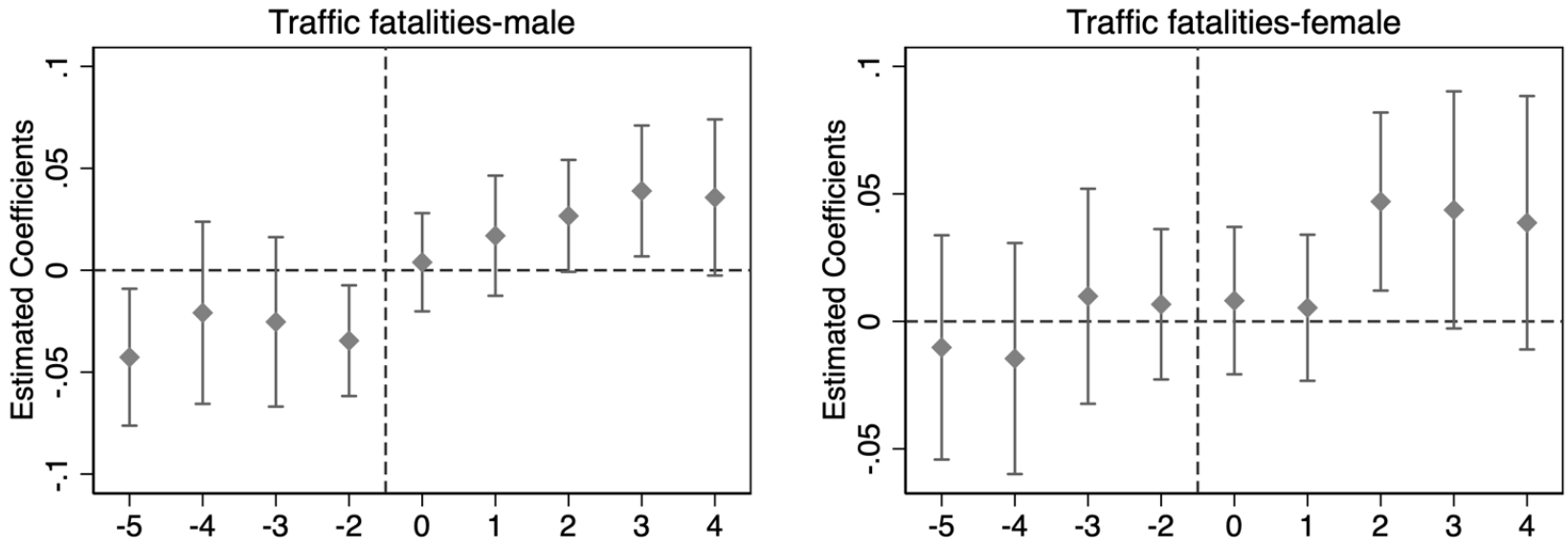


**Figure 14. SYG Laws and Traffic Fatalities by Gender – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

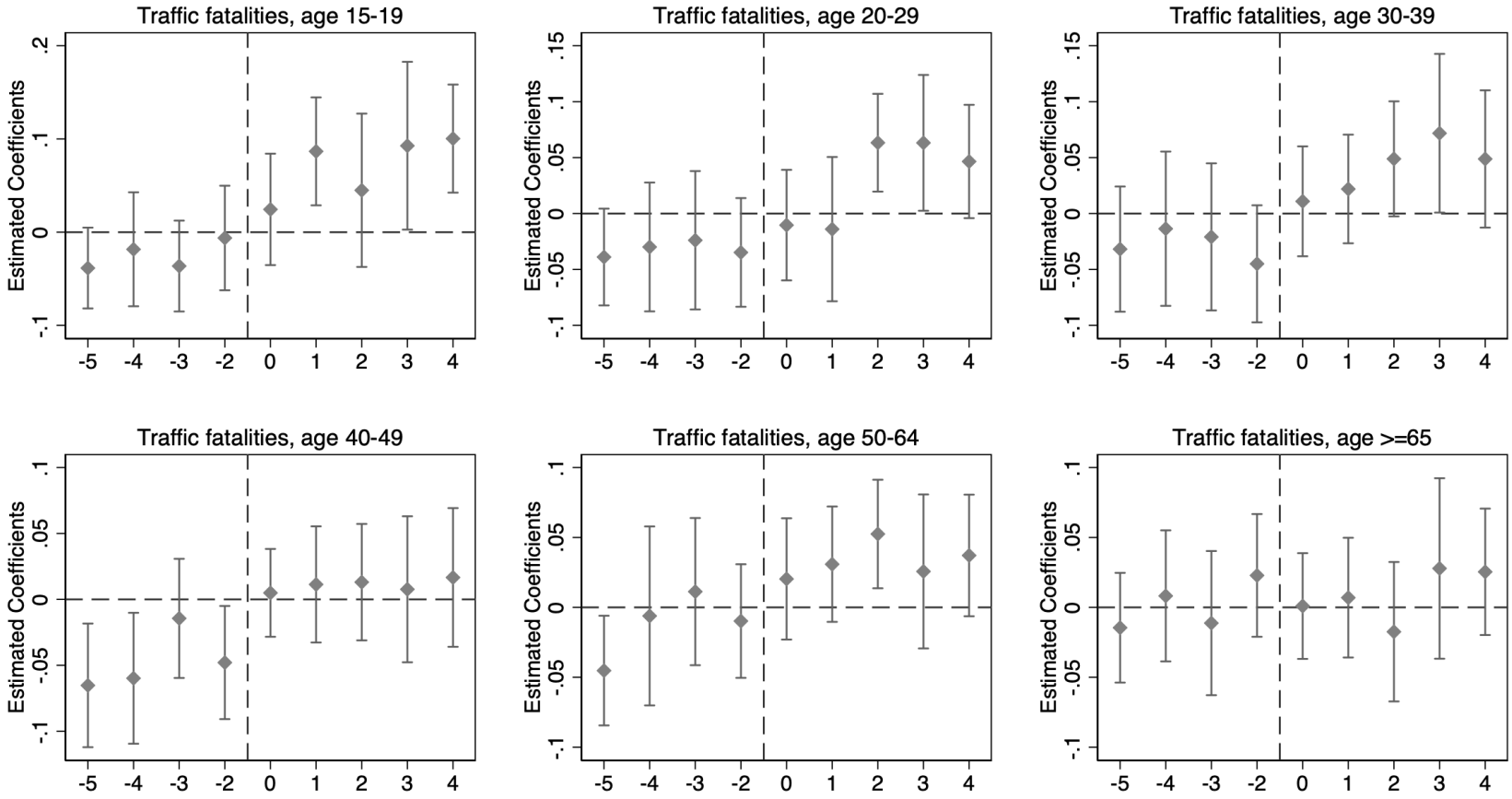


**Figure 15. SYG Laws and Traffic Fatalities by Age – Sun and Abraham (2020)**

Note: Unit of observation is state-year. Population weights are applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. The estimates are based on the event study model proposed by Sun and Abraham (2020), controlling for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

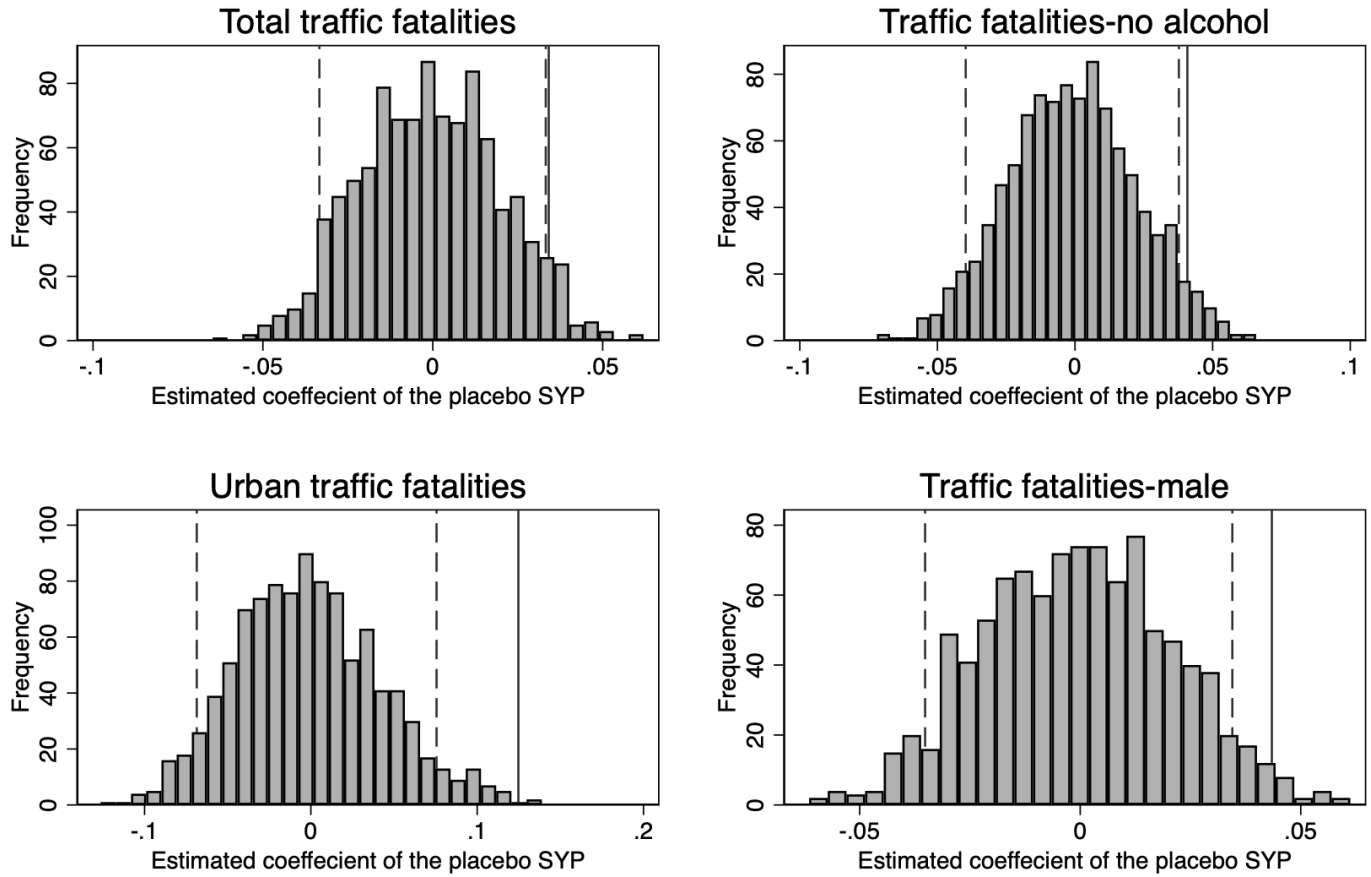


**Figure 16. Estimated Coefficients of the Placebo SYP**

Note: Each panel shows the histogram of placebo estimates from 1,000 trials of the simple difference-in-differences model of Equation (1). For each trial, I randomly assigned 29 states that legalized SYG laws in random years. The vertical solid lines represent the actual difference-in-differences estimates, and the dashed lines are 5$^{th}$ and 95$^{th}$ percentile of the placebo estimates.

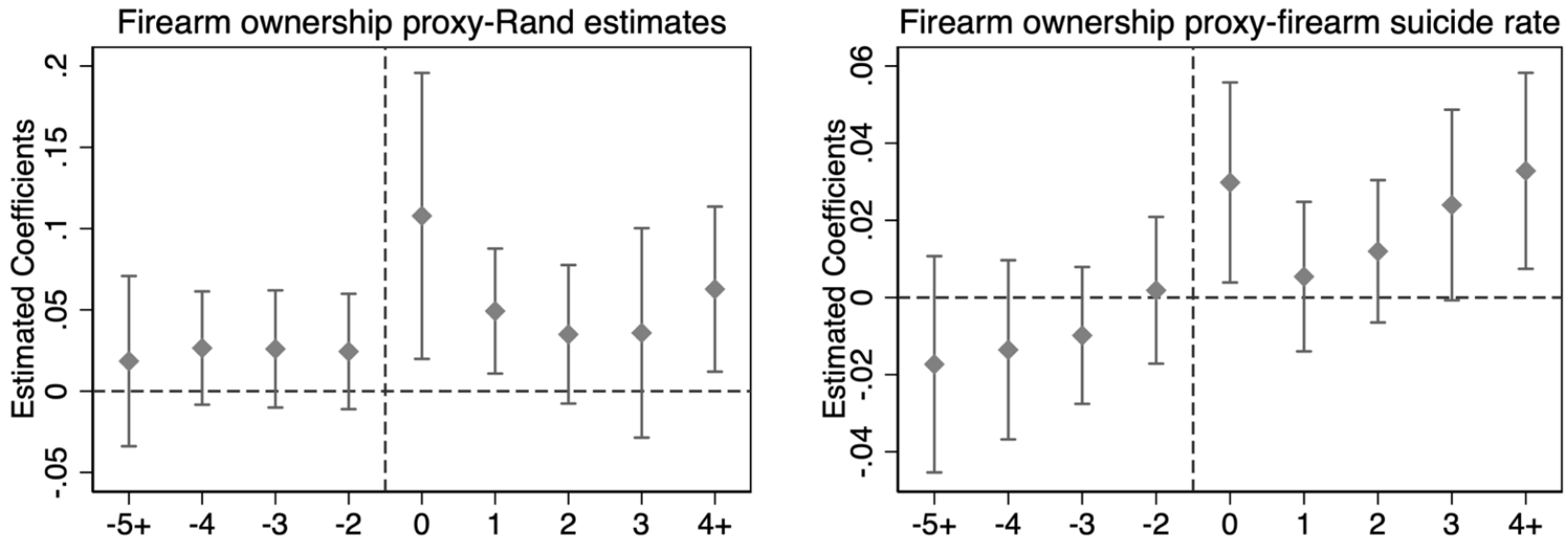


**Figure 17. SYG Laws and Gun Ownership Proxies**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. Both models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

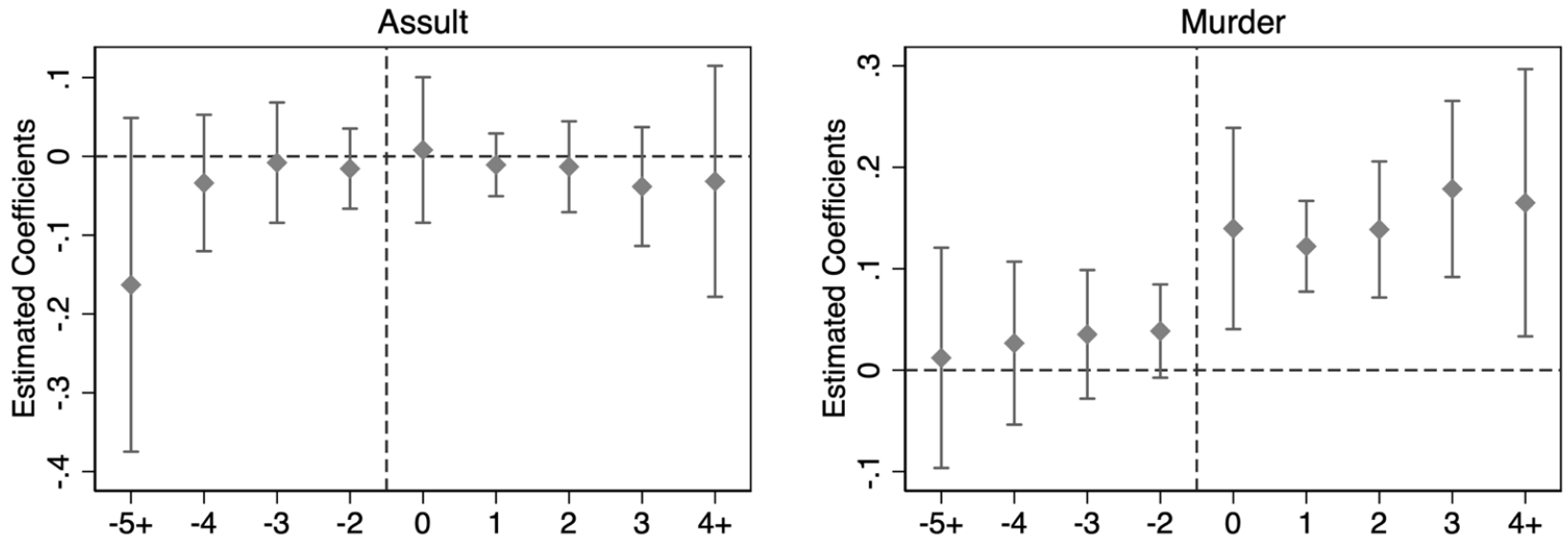


**Figure 18. SYG Laws and Crime**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. Both models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

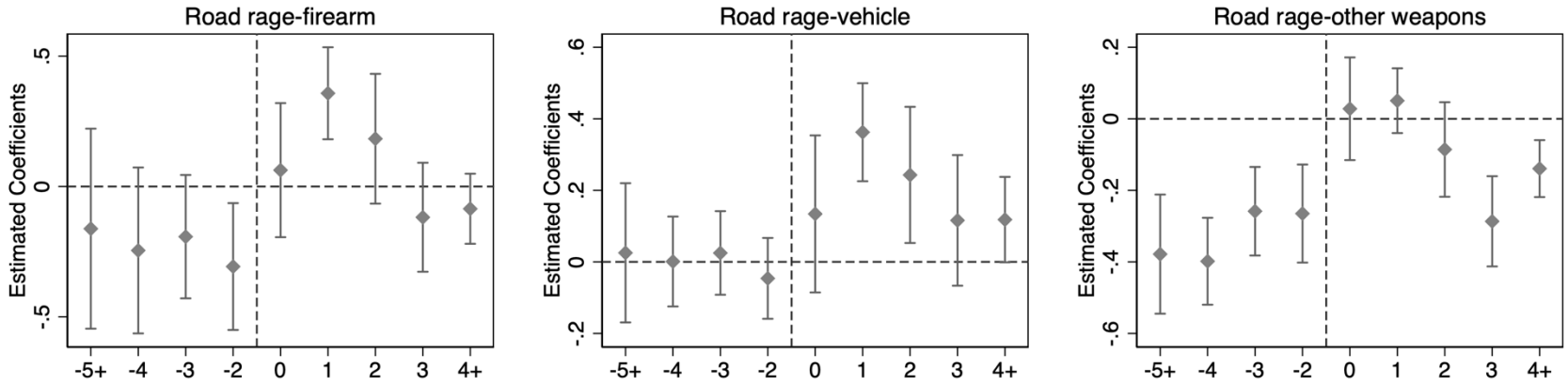


**Figure 19. SYG Laws and Road Rage Crime**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. All models are Poisson and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

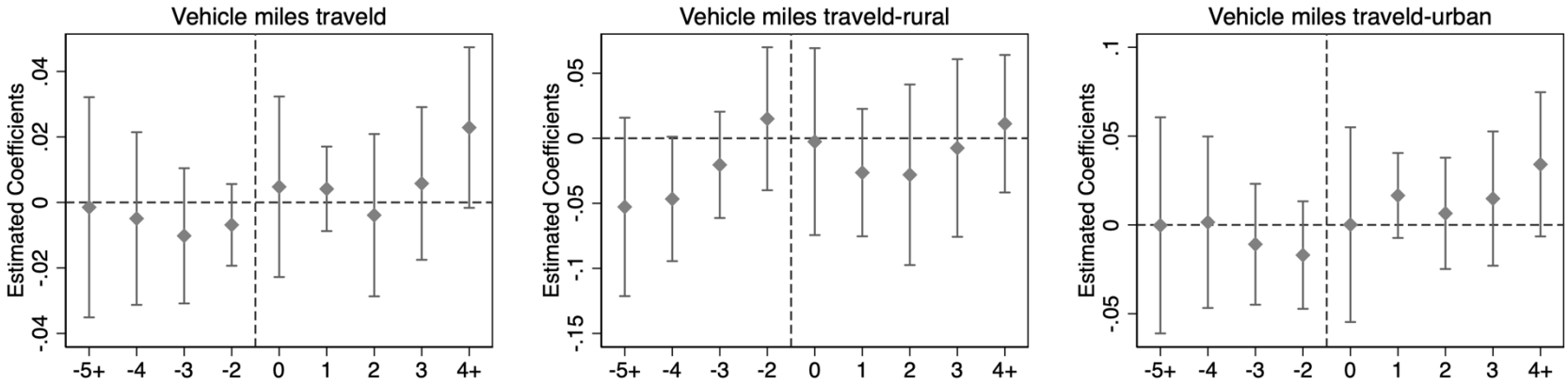


**Figure 20. SYG Laws and VMT**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variables include 1 to 5 years pre-law and 1 to 4 years post-law. The omitted category is 1 year pre-law. All models are estimated by OLS and control for state law dummies, state economic indicators, state demographics, state and year fixed effects. 90% confidence intervals account for state-level clustering are reported in vertical bars.

# Appendix A: Implementation of SYG Laws

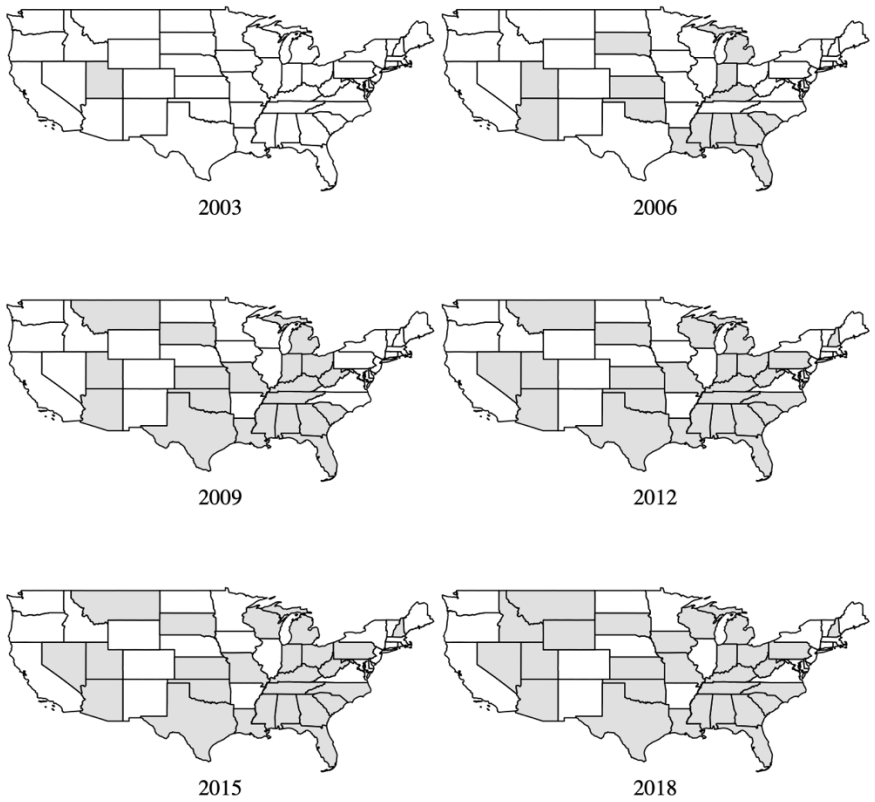


**Figure A1 Maps of SYG Laws, 1989-2018**

Notes: This graph shows the SYG laws' implementations in six different years. Shaded in grey indicates the states that implemented SYG laws by the year corresponding to each panel.

## Appendix B: Descriptions for Dependent Variables

**Table B1 Descriptions for Reduced Form Regressions' Outcome Variables**

| Outcome Variables | Mean All | Description |
|---|---|---|
| Total Fatalities | 13.65 (5.04) | Traffic fatalities (per 100,000 state population) |
| Aggressive Driving | 6.49 (2.77) | Traffic fatalities for incidences related to factors that potentially implicate aggressive driving behavior (per 100,000 state population) |
| Speeding | 4.2 (2.11) | Traffic fatalities for incidences related to speeding (per 100,000 state population) |
| No Alcohol Involved | 9.25 (3.4) | Traffic fatalities for incidences do not involve alcohol (per 100,000 state population) |
| Alcohol Involved (BAC ≥ 0) | 3.7 (1.72) | Traffic fatalities for incidences with at least one driver tested a blood alcohol concentration (BAC) > 0 (per 100,000 state population) |
| Alcohol Involved (BAC ≥ .08) | 3.01 (1.46) | Traffic fatalities for incidence with at least one driver tested a BAC ≥ 0.08 (per 100,000 state population) |
| Alcohol Involved (BAC ≥ .1) | 2.9 (1.41) | Traffic fatalities for incidences with at least one driver tested a BAC ≥ 0.10 (per 100,000 state population) |
| Rural | 7.71 (4.72) | Traffic fatalities in rural areas (per 100,000 state population) |
| Urban | 5.89 (2.08) | Traffic fatalities in urban areas (per 100,000 state population) |
| Daytime | 6.58 (2.55) | Traffic fatalities in the daytime (per 100,000 state population) |
| Nighttime | 6.97 (2.62) | Traffic fatalities in the nighttime (per 100,000 state population) |
| Weekday | 7.83 (2.86) | Traffic fatalities on weekdays (per 100,000 state population) |
| Weekend | 5.8 (2.24) | Traffic fatalities on weekends (per 100,000 state population) |
| Male | 19.23 (7.03) | Traffic fatalities, males (per 100,000 state population, males) |
| Female | 8.28 (3.27) | Traffic fatalities, females (per 100,000 state population, females) |
| Ages 15-19 | 20.88 (10.37) | Traffic fatalities, ages 15-19 (per 100,000 state population, ages 20-29) |
| Ages 20-29 | 21.8 (8.32) | Traffic fatalities, ages 20-29 (per 100,000 state population, ages 20-29) |
| Ages 30-39 | 14.66 (6.33) | Traffic fatalities, ages 30-39 (per 100,000 state population, ages 30-39) |
| Ages 40-49 | 13.38 (5.5) | Traffic fatalities, ages 40-49 (per 100,000 state population, ages 40-49) |
| Ages 50-64 | 12.97 (4.59) | Traffic fatalities, ages 50-64 (per 100,000 state population, ages 50-64) |
| Ages 65+ | 17.31 (5.71) | Traffic fatalities, ages 65+ (per 100,000 state population, ages 65+) |

Notes: This table shows the summary statistics for traffic fatality rates from National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS), 1989-2018. The second columns show the weighted means with standard deviations in parentheses for all states in the sample. The last columns display the description for the related variables. All means are weighted by the state-by-year population.

**Table B2 Descriptions for Outcomes for Regressions Testing Mechanisms**

| Outcome Variables | Mean All | Description |
|---|---|---|
| **Gun Ownership Proxies** | | |
| Rand Estimates | 36.23 (12.95) | Percentage of adults living in a household with a firearm. |
| Firearm Suicide Rate | 52.72 (12.31) | Percentage of suicide committed by a firearm. |
| **Crime** | | |
| Murder | 6.61 (3.57) | Murder rate (per 100,000 state population) |
| Aggravated Assault | 1286 (480) | Aggravated assault rate (per 100,000 state population) |
| **Road Rage Crime Proxies** | | |
| Firearm Involved | 2.29 (1.62) | Possible road rage cases involved firearms (per 100,000 agency-reported population) |
| Vehicle Involved | 1.69 (1.15) | Possible road rage cases involved vehicles (per 100,000 agency-reported population) |
| Other Weapon Involved | 3.91 (2.42) | Possible road rage cases involved other weapons (per 100,000 agency-reported population) |
| **Vehicle Miles Traveled** | | |
| Rural Vehicle Miles Traveled | 498 (254) | Rural vehicle miles traveled (per 100,000 licensed drivers) |
| Urban Vehicle Miles Traveled | 914 (187) | Urban vehicle miles traveled (per 100,000 licensed driver) |
| Total Vehicle Miles Traveled | 1412 (207) | Vehicle miles traveled (per 100,000 licensed driver) |

Notes. This table shows the summary statistics for gun ownership proxies, crime rates, road rage crime proxies, and vehicle miles traveled in the sample. The second columns show the weighted means with standard deviations in parentheses for all states in the sample. The last columns display the description for the related variables. All means are weighted by the state-by-year population.

## Appendix C: Descriptions for Independent Variables

**Table C1 Independent variables**

| Variables | Mean | Description |
|---|---|---|
| Stand-Your-Ground Laws | 0.22 (0.41) | Dummy variable for stand-your-ground laws. |
| Right-to-Carry Laws | 0.56 (0.49) | Dummy variable for right-to-carry laws. |
| Police Employment | 336.79 (76.75) | Lagged police population (per 100,000 agency-reported population) |
| Incarceration Rate | 410.52 (144.36) | Prisoner population (per 100,000 state population) |
| Personal Income | 17982.63 (3194.07) | Real personal income (1982$ per 100,000 state population) |
| Unemployment Insurance | 76.22 (57.39) | Real unemployment insurance (1982$ per 100,000 state population) |
| Income Maintenance | 287.81 (85.72) | Real income maintenance benefits (1982$ per 100,000 state population) |
| Retirement Payments | 17838.48 (3407.3) | Real retirement payments (1982$ per 100,000 state population over 65) |
| Unemployment Rate | 5.93 (1.91) | State unemployment rate (1982$ per 100 state population) |
| State Minimum Wage | 3.06 (0.46) | Real state minimum wage (1982$) |
| Poverty Rate | 13.35 (3.09) | Population in poverty (per 100 state population) |
| Beer Tax | 0.13 (0.11) | Real beer tax, (1982$ per gallon) |
| Primary Seat Belt Laws | 0.54 (0.49) | Dummy variable for seat belt laws with primary enforcement. |
| Secondary Seat Belt Laws | 0.98 (0.14) | Dummy variable for seat belt laws with secondary enforcement. |
| GDL Laws | 0.13 (0.33) | Dummy variable for graduated driver licensing laws. |
| Zero Tolerance Laws | 0.81 (0.39) | Dummy variable for zero tolerance laws. |
| Hands-free Laws | 0.14 (0.34) | Dummy variable for hands-free laws. |
| Medical Marijuana Laws | 0.2 (0.4) | Dummy variable for medical marijuana laws. |
| Population in MSA | 82.17 (14.34) | Population in MSA counties (per 100 state population) |
| White Population | 81.1 (7.74) | White population (per 100 state population) |

Note. All dummy variables contain state-year observations indicating the share of the year in which the related state law was implemented. All means are weighted by the state-by-year population. Standard deviations are in parentheses.

## Appendix D: Data Sources

**Table D1 Data Sources**

| Variables | Years | Source |
|---|---|---|
| Traffic Fatalities | 1989-2018 | Fatality Analysis Reporting System (FARS) |
| Gun Ownership Proxies-Rand | 1989-2016 | Schell et al. (2020) |
| Gun Ownership Proxies-Suicide Rate | 1989-2016 | CDC Compressed Mortality File |
| Overall Crime Rate | 1989-2018 | Uniform Crime Reporting Program |
| Road Rage Crime Proxies | 2001-2018 | National Incident-Based Reporting System |
| Vehicle Miles Traveled | 1989-2018 | Highway Statistics |
| Licensed Driver Population | 1989-2018 | Highway Statistics |
| SYG variables | 1989-2018 | Cherney et al. (2018) |
| RTC variables | 1989-2018 | Donohue et al. (2019) |
| Police Employment | 1989-2018 | Uniform Crime Reporting Program |
| Personal Income and Transfer Payments | 1989-2018 | U.S.Bureau of Economic Analysis |
| Unemployment rate | 1989-2018 | U.S.Bureau of Labor Statistics |
| State Minimum Wage | 1989-2018 | University of Kentucky Center for Poverty Research |
| Poverty rate | 1989-2018 | Census Bureau |
| Beer tax | 1989-2018 | Silver and Macinko (2014); Beer Institute |
| Consumer price index | 1989-2018 | U.S.Bureau of Labor Statistics |
| Seat belt laws | 1989-2018 | Insurance Institute for Highway Safety |
| Graduated Driver Licensing Laws | 1989-2018 | Gilpin (2019); Deza (2019); Dee et al. (2005); Srinivasan and Kishnani (2002); Insurance Institute for Highway Safety |
| Zero Tolerance Laws | 1989-2018 | HeinOnline; Hingson et al. (1994); Digest of Impaired Driving and Selected Beverage Control Laws; |
| Hands-free Laws | 1989-2018 | Insurance Institute for Highway Safety |
| Medical Marijuana laws | 1989-2018 | Anderson and Rees (2021) |
| Population | 1989-2018 | National Cancer Institute's Surveillance, Epidemiology, and End Results Program |
| Population in metropolitan statistical areas | 1989-2018 | Donohue et al. (2019); Federal Bureau of Investigation's Uniform Crime Reporting Program |

Notes. This table presents the data sources for the variables used in this paper and the available years for the data.