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

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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 ownerships and road rage crimes. These findings are consistent with estimates produced 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

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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).[2] 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.[3] 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. [4]

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

[2] 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)

[3] 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.”

[4] 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).

prevalent for 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 compared with 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.[5]

Third, according to Cheng and Hoekstra (2013), "the laws increase the expected cost of committing violent crimes." For potential assailants, the perceived risk of committing road rage crime would increase 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; Lee and McCrary, 2009, as cited in DeAngelo and Hansen, 2014). Hence, there would be fewer road rage confrontations and related casualties in

[5] 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 traffic fleet. On the other hand, the increased expected cost could escalate potential aggressive driving behavior into a deadly conflict.[6] 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 the years 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

[6] 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."

and ages 20-29 are mostly affected by the laws; the outcomes increase by 7% and 4%, respectively.

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 as well as 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 increase by 3% and the approximated road rage incidences increase by around 25% after the implementation of the laws. A back-of-envelop analysis indicates road rage crimes increase by around 450 cases per year after the implementation of 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 estimates the SYG laws' effect on several measures of traffic fatalities outcomes. Section 5 provides robustness checks for the

main findings. Section 6 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, 2007). 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.[7] 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 which is 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.[8,9] The

[7] According to Cherney et al. (2019), 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.

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

[9] 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 (Jansen and Nugent-Borakove, 2007; Fisher and Eggen, 2012).

requirement of "duty to retreat" 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, which are referred to as SYG laws in the current study, extended castle doctrine laws to one's vehicle or any place one has a legal right to be. [10] 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.[11] 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

[10] 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.

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

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 the impact of ECD laws 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 and 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.[12]

[12] 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)"

### 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) 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. (2005) 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."[13] The result shows the mere sight of a gun in the vehicle may elicit more aggressive driving behaviors.

## 3 Data and Empirical Method

### 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 the period 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 fatal traffic accident and the circumstance of the fatal accident. The data have been used to evaluate traffic

[13] "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.

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, 2015), or to evaluate alcohol or drug policy that could potentially affect traffic safety, such as beer tax (Ruhm, 1996), medical marijuana laws (Anderson 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 that are classified by NHTSA (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.[14] 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.

[14] NHTSA (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 NHTSA (2004) make up 55.7% of total traffic fatalities from 2003 to 2007. Thus, the current study's selection of related factors is representative.

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. [15] 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 more than twice the female traffic fatality rate. For the traffic fatality rate broken down by age group, the majority of traffic fatalities happened to 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 the ones 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.[16] 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-

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

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)" Prior to 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, 2005; Cook and Ludwig, 2006). The firearm suicide rate is defined as the percentage of suicides that are 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 the household gun ownership as well as an approximation of vehicular gun ownership.

### 3.3 Data on Crime

Similar to the literature investigating the ECD laws' impact on violent crimes, the current study uses Uniform Crime Reporting Program (UCR) data, 1989-2018, to investigate SYG laws' impact on aggravated assault and murder rate. 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 a seven-year period in the United States (The Zebra, 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 by 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 happens 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.

### 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 the ones in other states.

## 4 Empirical Method

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

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

Here, $Y_{it}$ represents the various measures of traffic fatality rate in table 2 for state $i$ at time $t$, $w_t$ represent full set of time fixed effects, $v_s$ represent full set of state fixed effects, $SYG_{st}$ is a dummy variable, indicating when the state has SYG. The dummy variable is set equal to the fraction of the effective year, and equal to one for all the following years. The interested coefficient is $\beta_1$. $X_{it}$ represent a vector of control variables listed in Table C1[17]. $\varepsilon_{it}$ is an error term.

In addition to equation (1), the following two event study specifications are estimated.

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

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

Both equation (2) and equation (3) replacing the dummy variable $SYG_{st}$ in equation (1) with a series of indicator variables that take the value 1 if SYG laws has been in effect for b periods,

[17] The list of data source could be found in Appendix table A1

and 0 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.

## 5 Results

### 5.1 The Relationship between SYG Laws and Traffic Fatalities

Table 6 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.[18] The standard errors are clustered at the state level (Betrand, Duflo, and Mulainathan 2004). The baseline analysis shows that the passage of SYG laws increases the fatality rate by 3%.[19] Column 2 and column 3 in Table 4 shows that the traffic fatality rate increases in the first year of passage of the law. After the first full year of passage, the traffic fatalities rise by around 3-4%. The effect is quite persistent across years. Figure 2 provides a visualization of this effect.

The study then focuses on the impact of SYG laws on aggressive-driving-related traffic fatalities. In table 5, the current study replaces total traffic fatalities with fatalities related to all aggressive driving factors as well as speeding. Though the simple difference in difference gives positive insignificant estimates, the results in event studies might suggest SYGs are related to traffic fatalities through the behavior of aggressive driving. The legalization of SYG is associated

[18] 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), CottiCotti & Walker (2010), Kaestner & Yarnoff (2011), Coas et al. (2003), Grant (2010), Cotti & Teffe (2011), Anderson & Rees (2013), Abouk & Adams (2013), Anderson & Rees (2015), Santaella-Tenorio et al (2017), French & Gumus (2018), Sabia & Argys (2018), Ruhm (2000), and French & Gumus (2014),

[19] 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.

with a 12% increase in fatalities involving all aggressive driving factors, and a 17% increase in fatalities involving speeding. Figure 3 visualizes the event-study results.

Yu et al. (2004) surveyed 432 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 on road rage behavior. In Table 6, the study explores SYG laws' impact on alcohol-related and non-alcohol-related traffic fatalities. No treatment effect heterogeneity is identified for alcohol-related fatal injuries. The event-study analysis in column 3 shows the non-alcohol-related traffic fatality rate increased by 7% in the first year of implementation. Figure 4 shows a visual representation for all event-study results for traffic fatalities stratified by alcohol involvement.

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 that occur in urban areas and rural areas separately. 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. The result from Table 7 and Figure 5 shows that urban traffic fatalities increase by 9-22% following SYG laws.

Auto Insurance Center (2016) analyzed more than 65,000 Instagram posts with the hashtag "#RoadRage" to find out 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 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. On average, the nighttime traffic fatalities increase by around 5-7%, and weekday traffic fatalities increase by around 3-7%. Figure 6 visualizes the dynamic effect of SYG laws on nighttime fatalities.

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, 2010; AAA Foundation, 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 study in Figure 7 suggests that the male traffic fatalities increase by 4-8% following the policy change, and the female traffic fatalities experience a 3% increase after the first full year of implementation.

According to AAA Foundation, 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 15-19, 20-29, 30-39, 40-49 age groups are statistically significant at conventional levels. The event study in Figure 8 shows that the traffic fatalities for age 15-19 increase by around 9-15%. And traffic fatalities for age 20-29 increase by about 7%.

## 6 Robustness Checks and Sensitivity Checks

Table 11 shows the robustness check 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 implemented SYG laws which extend castle doctrine to vehicle. Column (3) runs the baseline regression for the sample excluding states that implemented SGY laws which extend castle doctrine to any places one has a legal right to be. Column (4) contains estimates from the event study approach proposed by Sun and Abraham (2020). Unlike other regressions with the dummy variable takes on fraction value for partial year implementation, the policy variable in column 4 is a pure dummy with a value equal to zero or one. This is because Sun & Abraham (2020) only discuss the setting with pure dummy variables. The robustness check confirms that SYG laws are associated with an increase in total traffic fatalities. The magnitude of coefficients on 2 years after and 3 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. 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.[20] People live in the SYG States may drive to non-SYG states, and this 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 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.

[20] 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 (2019) observes unit-specific trend over control for time-varying treatment effects. This is consistent with Lee and Solon(2011), Meer and West (2013), 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."

Following Anderson (2013), the current study runs a series of regressions in which a placebo SYG was randomly assigned to each control state. Because 29 states legalized SYG during 1989-2018, the study assigned 29 placebos per trial. And there are 1000 trials. Figure 8 plots the histogram of the placebo estimates. The vertical solid lines represent the actual difference-in-difference estimates, and 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. 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 corresponding gun ownership proxies. The effect for Rand estimates is more salient than the CDC data. The SYG laws increase gun ownership by 11%, followed by the implementation of SYG law. The gun ownership estimates from CDC data increase by about 3% after SYG laws. Figure 9 visualizes the effects.

Next, attention is paid to crime. Chen and Hoeksta (2013) studied 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 impact of extend 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 analyzing the impact of SYG laws 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. 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.[21] 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 definition of SYG laws varies. Figure 10 represents the event-study results in graphs.

Table 19 display the impact of SYG laws on road rage crime proxies. Given the nature of the data, which contains many zeros, the study applies Poisson fixed effect model. Road rage proxies using firearm and vehicle significantly increase by 23% and 27%, respectively[22]. Figure 11 visualizes the event study estimates. A back-of-envelope analysis indicate the road rage crimes associated with firearm and vehicle increase by 516 cases and 439 cases each year after the SYG law was implemented.[23] By contrast, similar analysis shows SYG laws may cause additional 893 traffic casualties each year.[24] This indicates other channels possibly exist between

[21] 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

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

[23] 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.

[24] 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%

SYG laws and traffic fatalities. Table 20 presents the results for the impact of SYG laws on VMT. No significant impact was detected for the VMT outcome.

## 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 polices 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 the passage of laws. In

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.

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 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. 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, which is defined as percentage of adults living in a household with a firearm, is from Schell et al. (2020), 1989-2016. Firearm suicide rate, which is defined as the percentage of suicide committed by a firearm, is from 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 Federal Bureau of Investigation's National Incident-Based Reporting System, 2001-2018. Vehicle miles traveled related variables, which is defined as corresponding vehicle miles traveled divided by 100,000 licensed drivers, is 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 | Sun & Abraham (2020) |
|---|---|---|---|---|

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| 5+ years before | -0.02 | -0.03 | -0.02 | -0.03 |
| | (0.02) | (0.03) | (0.03) | (0.02) |
| 4 years before | -0.00 | -0.01 | -0.01 | -0.02 |
| | (0.02) | (0.04) | (0.03) | (0.02) |
| 3 years before | -0.00 | -0.03 | 0.00 | -0.01 |
| | (0.02) | (0.03) | (0.03) | (0.02) |
| 2 years before | -0.01 | -0.01 | -0.01 | -0.02 |
| | (0.01) | (0.03) | (0.01) | (0.01) |
| Year 0 for SYG | 0.06* | 0.05 | 0.05 | 0.00 |
| | (0.03) | (0.05) | (0.04) | (0.01) |
| 1 years after | 0.02 | 0.02 | 0.02 | 0.01 |
| | (0.01) | (0.02) | (0.02) | (0.01) |
| 2 years after | 0.03** | 0.03* | 0.03 | 0.03* |
| | (0.01) | (0.02) | (0.02) | (0.01) |
| 3 years after | 0.03** | 0.03 | 0.04* | 0.04* |
| | (0.02) | (0.03) | (0.02) | (0.02) |
| 4+ years after | 0.02 | 0.04 | 0.02 | 0.04 |
| | (0.02) | (0.03) | (0.03) | (0.02) |
| Observations | 1,440 | 810 | 1,230 | 1,440 |
| R-squared | 0.89 | 0.91 | 0.89 | 0.97 |

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 sates implemented SGY laws which extend castle doctrine to vehicle. Column (4) contains estimates from the event study approach proposed by Sun and Abraham (2020). 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. For Column (1) through Column (3), year 0 dummies contain the values equal to the fraction of the year for which the SYG law was initiated, and zero otherwise. For Column (4), year 0 dummies equal to ones for the year the SYG laws was initiated, and zero otherwise. 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 definition 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 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 | Sun & Abraham (2020) |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 5+ years before | 0.02 | -0.10 | 0.02 | -0.02 |
| | (0.05) | (0.12) | (0.05) | (0.05) |
| 4 years before | -0.06 | -0.22 | -0.02 | -0.09 |
| | (0.09) | (0.28) | (0.07) | (0,10) |
| 3 years before | 0.01 | 0.04 | -0.00 | -0.01 |
| | (0.05) | (0.09) | (0.06) | (0.05) |
| 2 years before | -0.05 | -0.11** | -0.03 | -0.06 |
| | (0.04) | (0.05) | (0.05) | (0.05) |
| Year 0 for SYG | 0.22*** | 0.25 | 0.20** | 0.04 |
| | (0.08) | (0.28) | (0.08) | (0.03) |
| 1 years after | 0.09** | 0.13* | 0.08 | 0.07* |
| | (0.04) | (0.06) | (0.05) | (0.04) |
| 2 years after | 0.07 | 0.11 | 0.08 | 0.07 |
| | (0.05) | (0.07) | (0.06) | (0.06) |
| 3 years after | 0.10 | 0.14 | 0.10 | 0.10 |
| | (0.08) | (0.11) | (0.10) | (0.09) |
| 4+ years after | 0.16** | 0.19 | 0.16** | 0.18*** |
| | (0.06) | (0.11) | (0.07) | (0.06) |
| Observations | 1,438 | 808 | 1,229 | 1438 |
| R-squared | 0.37 | 0.45 | 0.48 | 0.77 |

Notes. Column (1) repeats the baseline regression result from Column (6) of Table 7. 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 sates implemented SGY laws which extend castle doctrine to vehicle. Column (4) contains estimates from the event study approach proposed by Sun and Abraham (2020). 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. For Column (1) through Column (3), year 0 dummies contain the values equal to the fraction of the year for which the SYG law was initiated, and zero otherwise. For Column (4), year 0 dummies equal to ones for the year the SYG laws was initiated, and zero otherwise. 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 definition 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 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 percentage of adults living in a household with a gun. The dependent variable in Column (2) is the natural log of 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 Poisson fixed effect model are reported. The three dependent variables are the crime counts for the proxy road rage cases involving firearms, vehicle 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 variable 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

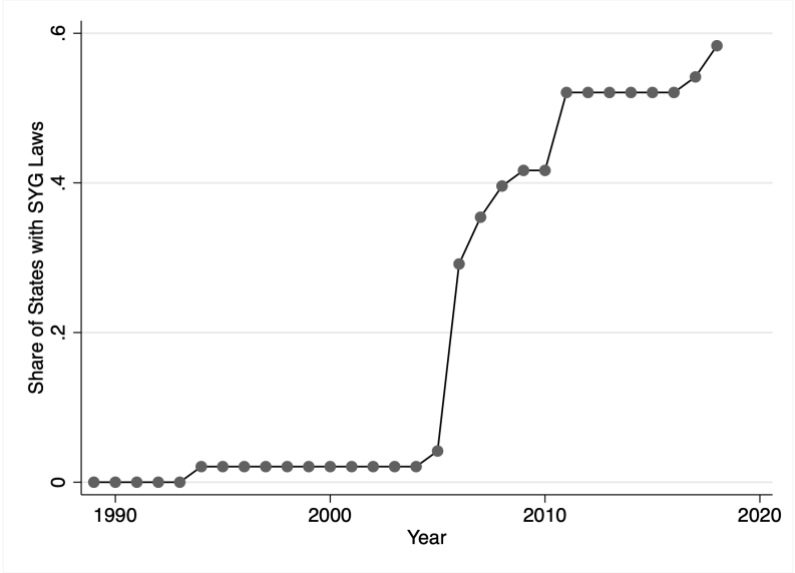


**Figure 1. Share of States with SYG Laws, 1989-2018**

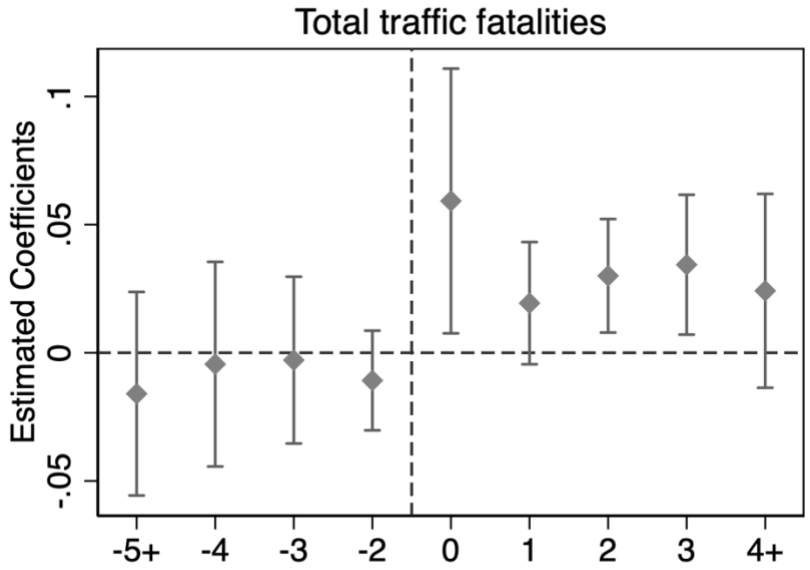


**Figure 2. SYG Laws and Total Traffic Fatalities**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

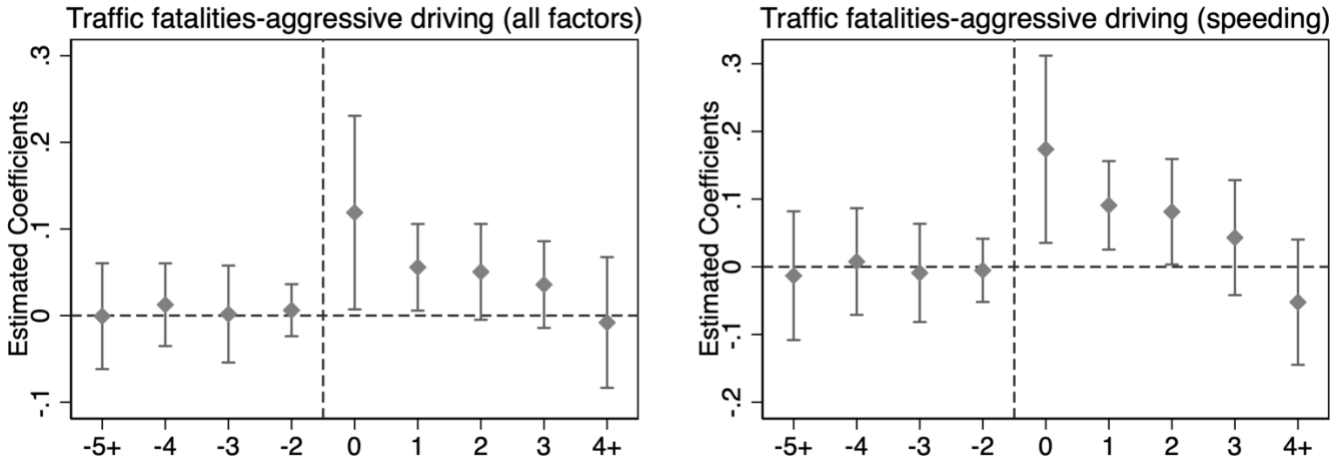


**Figure 3. SYG Laws and Traffic Fatalities Related to Aggressive Driving**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

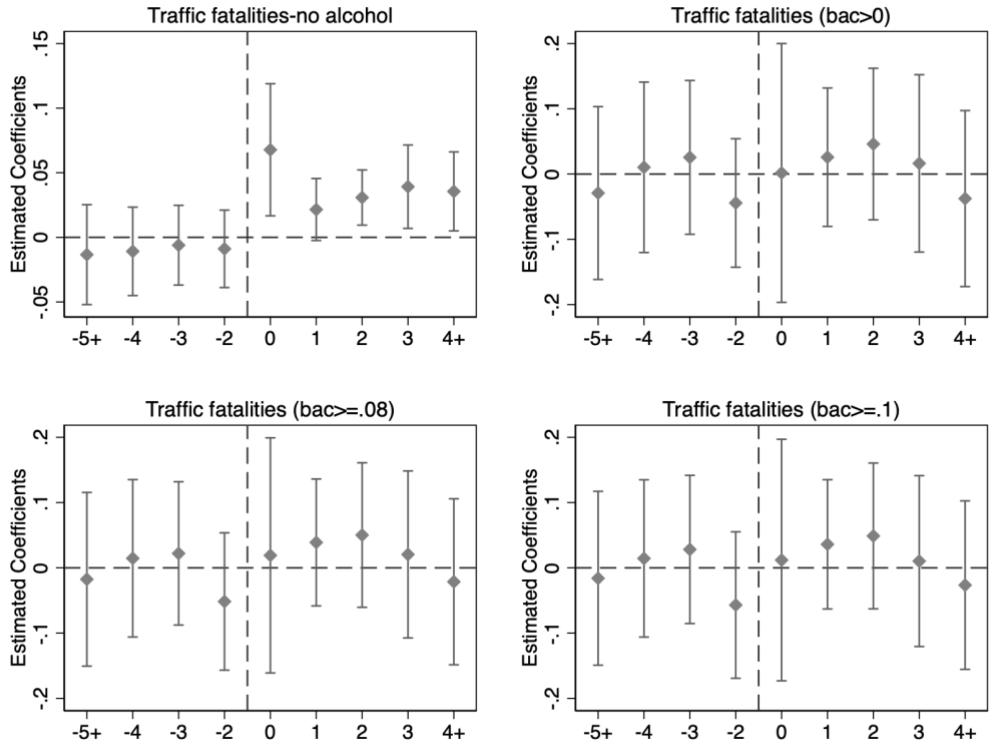


**Figure 4. SYG Laws and Traffic Fatalities Related to Alcohol Involvement**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

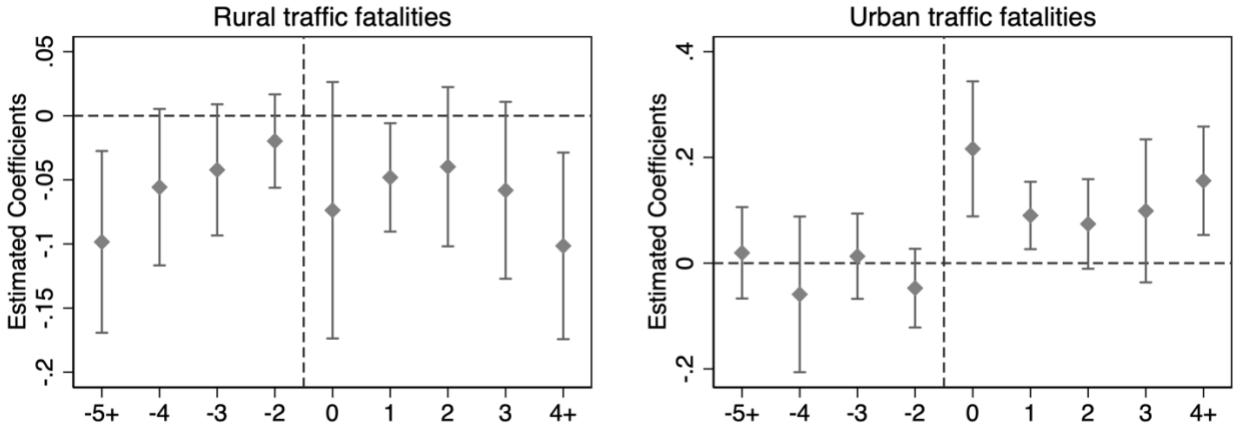


**Figure 5. SYG Laws and Traffic Fatalities Related to Location**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

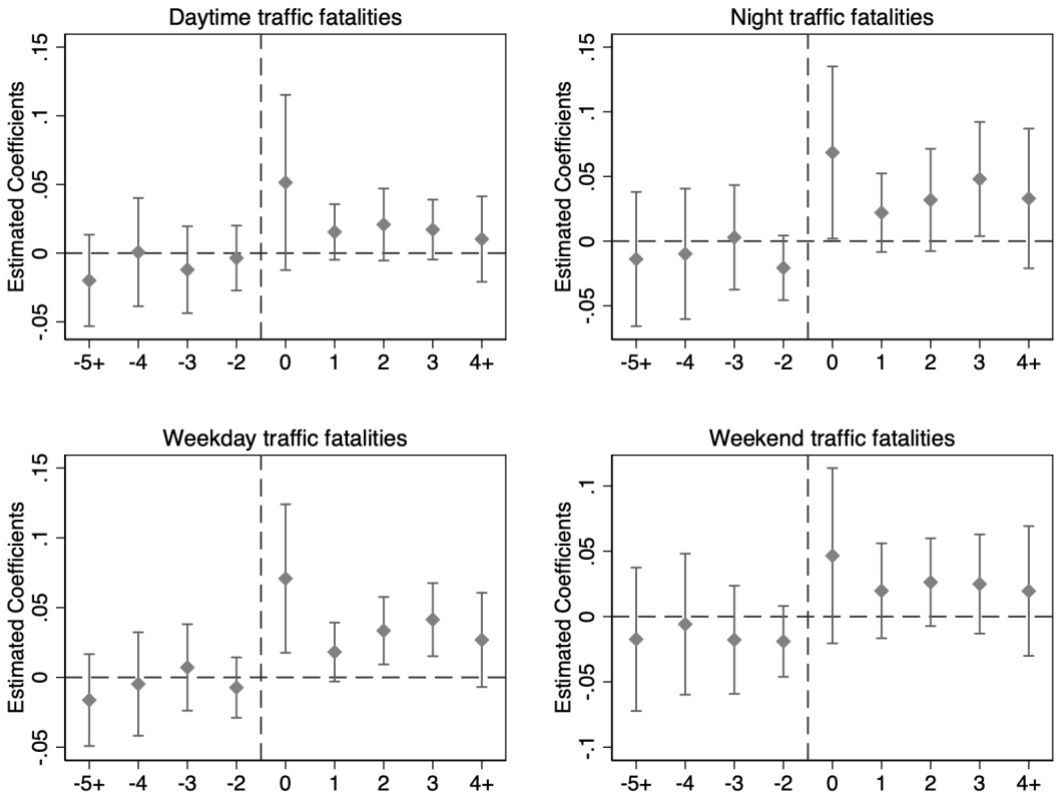


**Figure 6. SYG Laws and Traffic Fatalities by Time**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

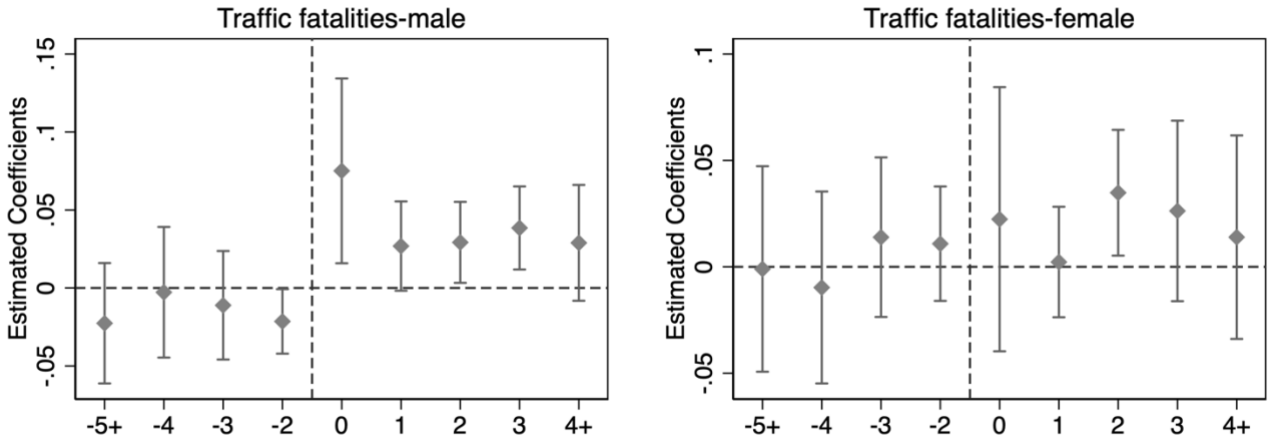


**Figure 7. SYG Laws and Traffic Fatalities by Gender**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

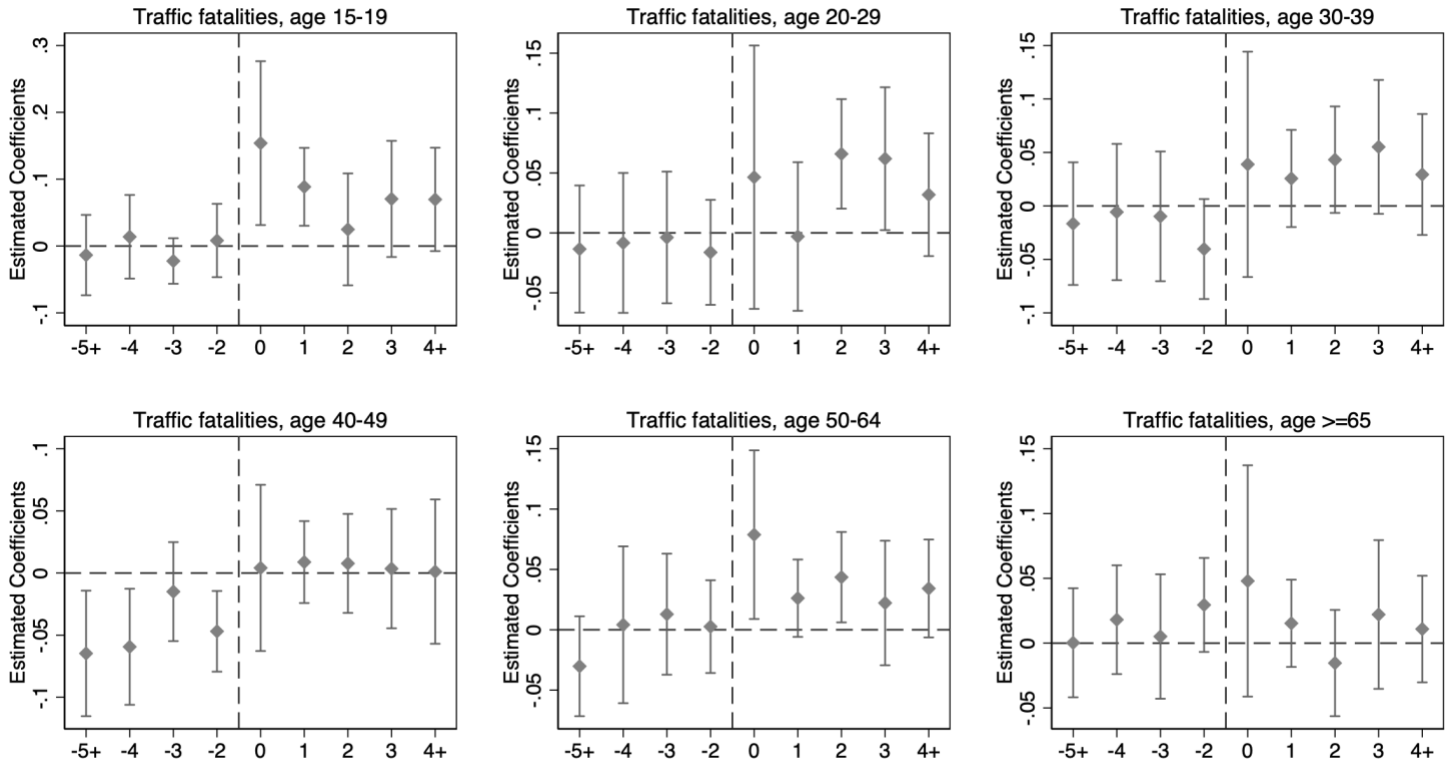


**Figure 7. SYG Laws and Traffic Fatalities by Age**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

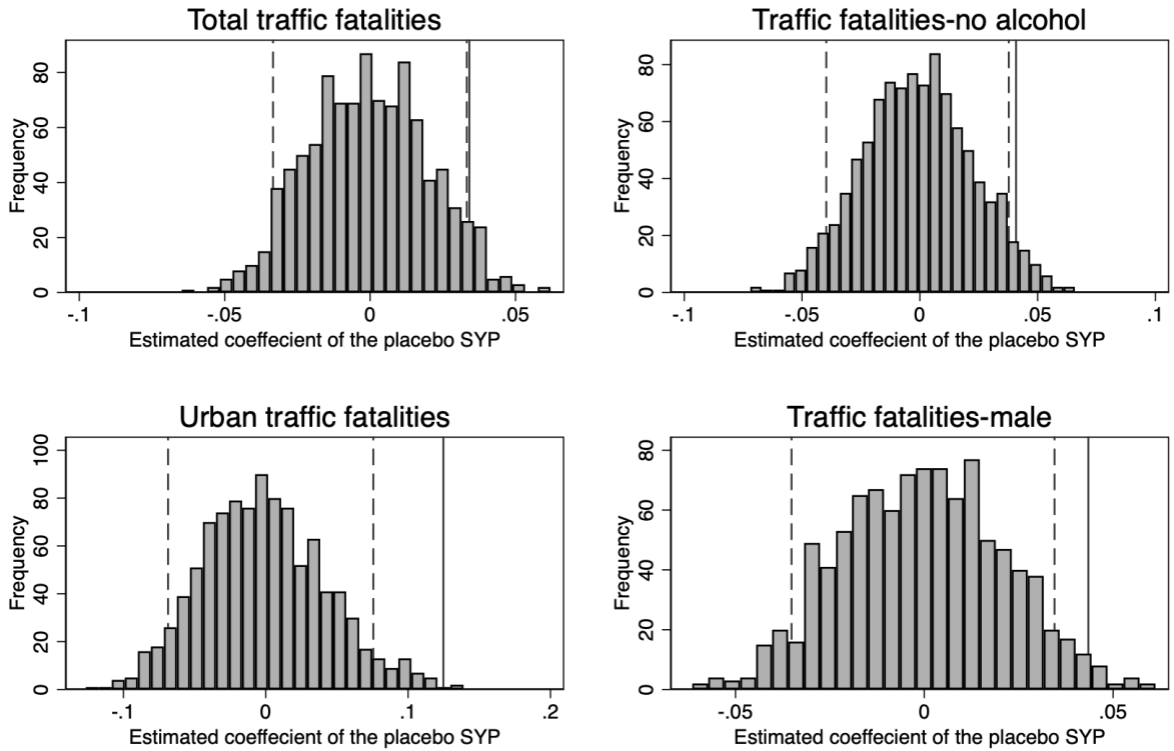


**Figure 8. Estimated Coefficients of the Placebo SYP**

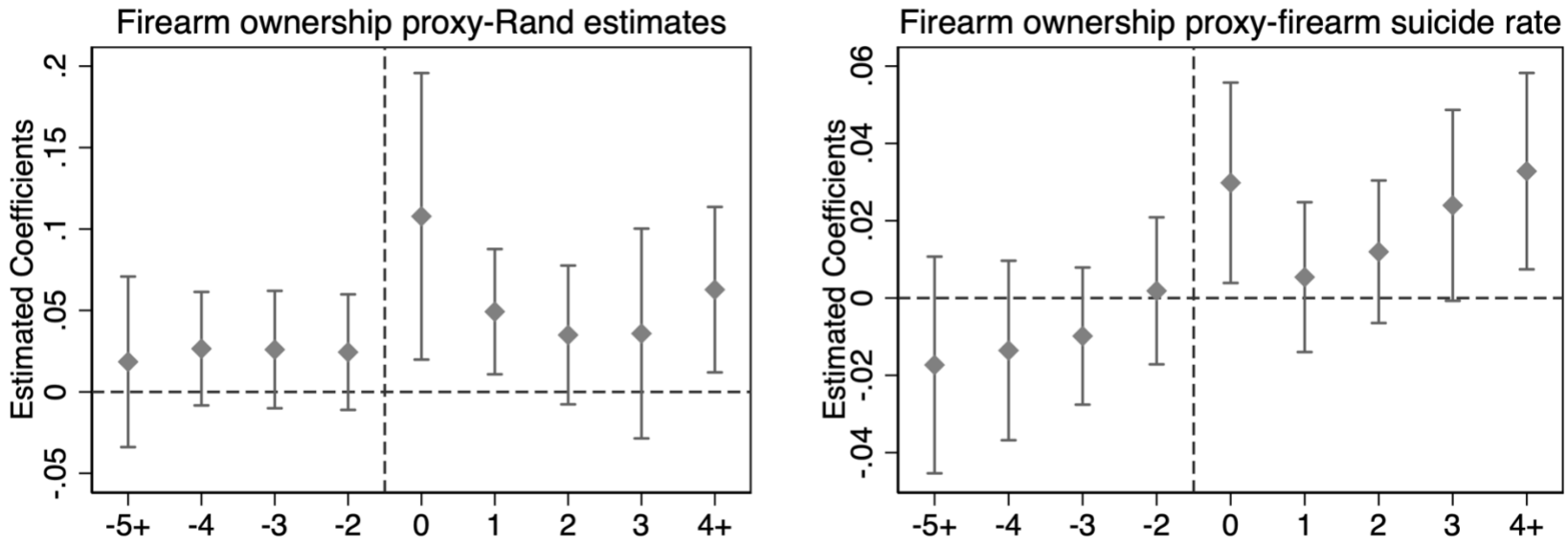


**Figure 9. SYG Laws and Gun Ownership Proxies**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

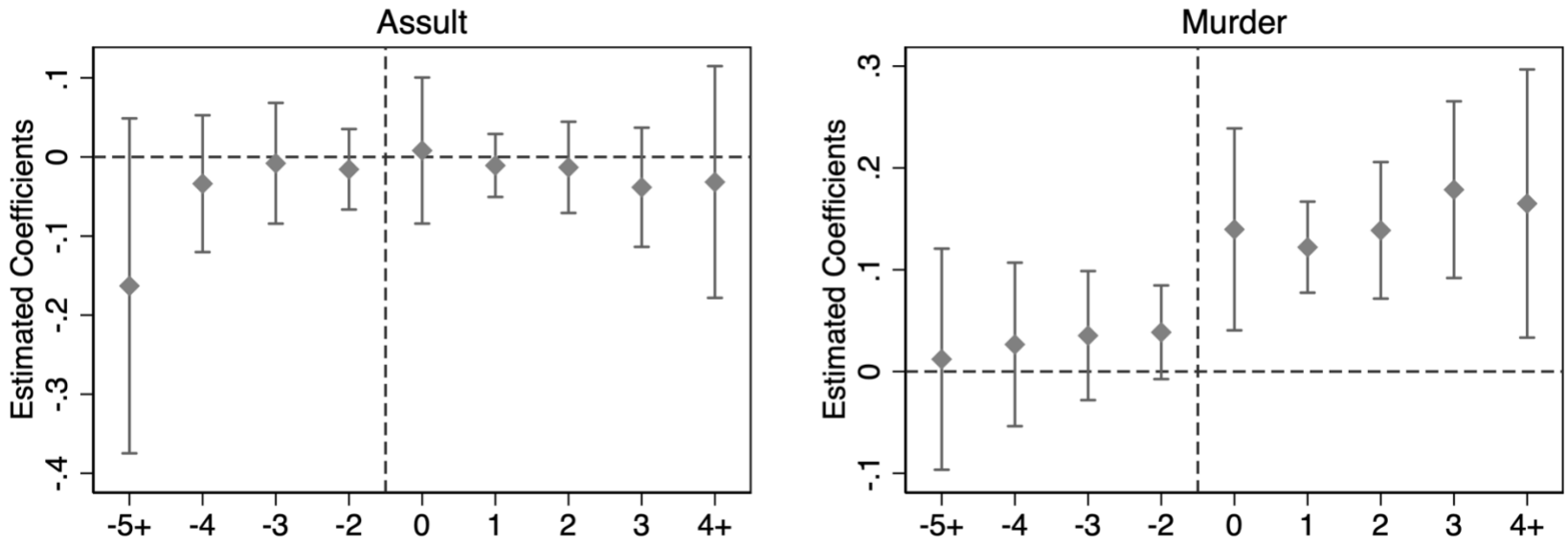


**Figure 10. SYG Laws and Crime**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

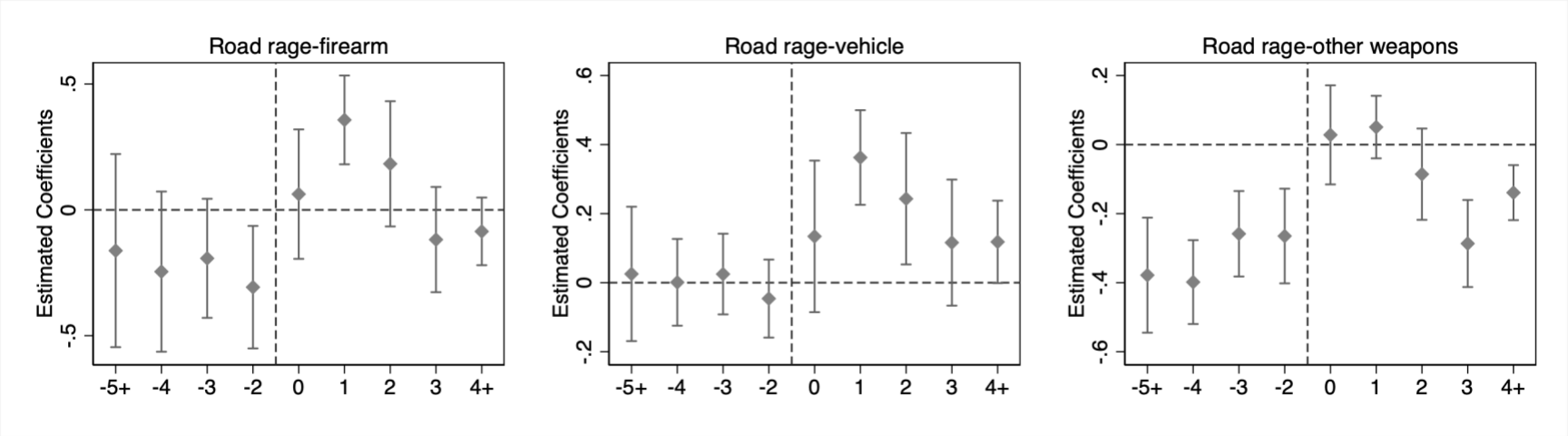


**Figure 11. SYG Laws and Road Rage Crime**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

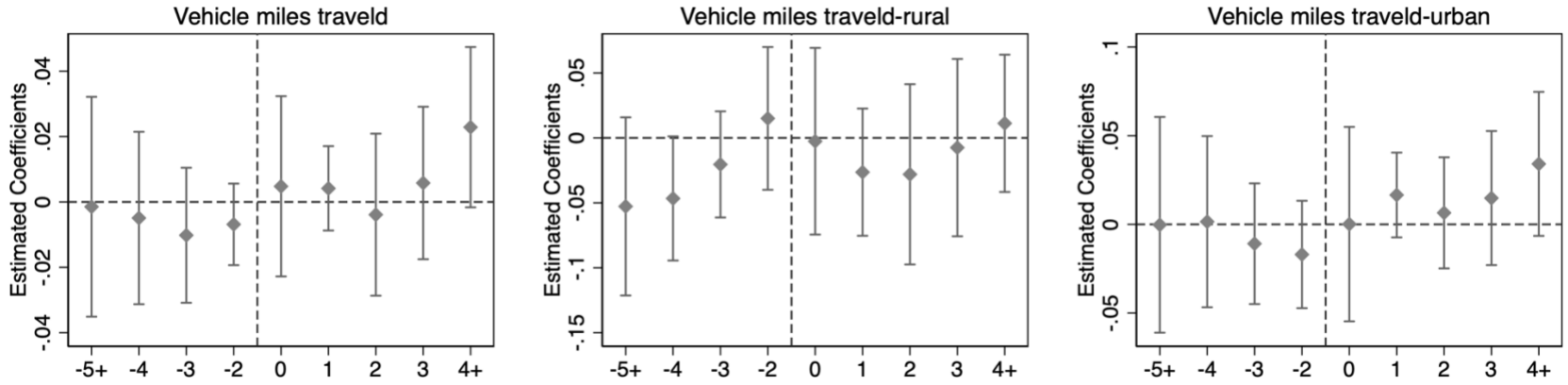


**Figure 12. SYG Laws and VMT**

Note: Unit of observation is state-year. Population weights applied. Event study dummy variable 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 with 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.

**Appendix A: Implementation of SYG Laws**

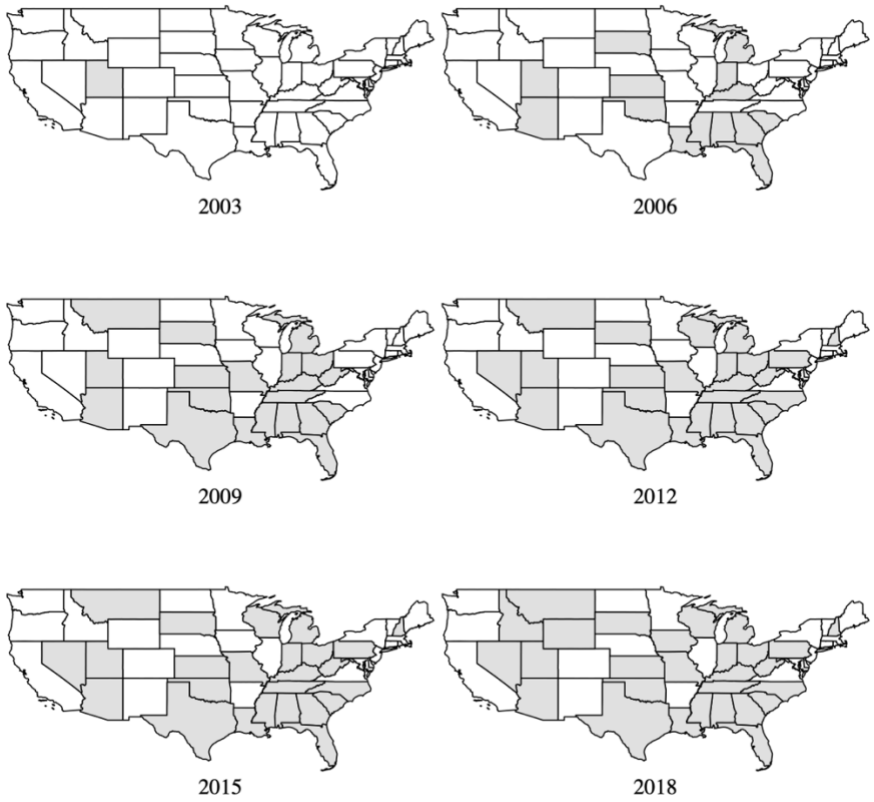


Figure A1 Maps of SYG Laws, 1989-2018

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 | UU.S.Bureau of Economic Analysis |
| Unemployment rate | 1989-2018 | UU.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 | UU.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, as well as the available years for the data.