Essays on Gun and Marijuana Laws

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Abstract

This dissertation adds to the economic study of gun and marijuana laws. Chapter 1 studies the impact of stand your ground laws (SYGs) on traffic fatalities. Chapter 2 investigates the effects of campus concealed carry laws (CCCs) on higher education outcomes. Chapter 3 looks at the impact of medical marijuana laws (MMLs) on youth crimes.

The first chapter focuses on SYG, a key interest of current gun policy research. SYG is a self-defense law. It allows citizens in the vehicle to use lethal force for self-defense; even retreat is possible. 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 SYGs on traffic fatalities. Using state-level traffic fatality data and an event study approach, I find that the implementation of SYGs is associated with a 3% increase in total traffic fatalities. In addition, the laws are associated with increases in both gun ownership and road rage crimes. These findings are robust to alternative estimation methods addressing staggered policy implementation and heterogeneous treatment effects. The evidence in this paper suggests broader impacts of SYGs on public health than initially considered.

The second chapter studies CCCs, which allow students to carry guns on campus. Utilizing the U.S Department of Education, Federal Bureau of Investigation, and Council for Advancement and Support of Education data, event studies are conducted on the effects of CCCs on various university outcomes, including campus crime rates, alumni donation rate, alumni giving, instructional staff employment, and campus police employment. The results show that the passage

CCCs can increase aggravated assault, and campus police and faculty employment but decrease alumni's donation rate and international student enrollment.

The third chapter intends to explore the impact of MMLs on youth crime. Using UCR state-level arrest data and stacked event study models, the study finds that MMLs' implementation increase youth violent crime by 12-20%. No evidence suggests MMLs increase the overall youth drug crime. The results are robust to a series of robustness and sensitivity checks. States should consider this negative impact when making the future medical marijuana law.

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Chapter 1 Do Stand-Your-Ground Laws Increase or Decrease Traffic Fatalities?

1.1 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). 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.[1] 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. This study aims to examine how SYGs could impact traffic fatality, an extremely pertinent but largely ignored public health outcome for gun policy literature. [2]

It is still uncertain whether SYGs would impact traffic fatalities. Several possible mechanisms may exist to support both traffic fatalities increasing and traffic fatalities decreasing. First, SYGs may increase the presence of guns in the traffic fleet and subsequently alter driving behaviors and traffic

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[1] 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."

[2] No SYGs 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 SYGs were largely regarded as gun laws in the literature (Weaver, 2008; Donohue and Ribeiro, 2012; Crifasi et al, 2018; Smart et al, 2020).

fatalities. The increase of gun ownership in vehicles could be because SYGs provide more legal protections for self-defense in one's vehicle. Also, it is 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 than an unarmed motorist (Miller et al., 2002; Hemenway et al., 2006; Bushman et al., 2017). Thus, traffic fatalities may increase after the passage of the SYGs. 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 SYGs, crash risk and traffic fatalities could rise for both attackers and victims in a road rage incident.[3]

Third, according to Cheng and Hoekstra (2013), "the laws increase the expected cost of committing violent crimes." The perceived risk of committing road rage crime would increase for potential

[3] 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.

assailants as the SYGs protect immediate self-defense using a deadly weapon. On the one hand, the increased expected cost could deter crime (Becker, 1968; Polinsky and Shavell, 1979; İmrohoroğlu et al., 2004; McCrary and Lee, 2009, as cited in DeAngelo and Hansen, 2014). Hence, there would be fewer road rage confrontations and related casualties in the traffic fleet. On the other hand, the increased expected cost could escalate potential aggressive driving behavior into a deadly conflict.[4] 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 SYGs 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 gun laws' impact on traffic fatalities. The current study uses state-level traffic fatality data from the Fatality Analysis Reporting System (FARS) for 1989-2018. During this period, twenty-nine states relieved the duty-to-retreat in one's vehicle or any other places that such person has a legal right to be. An event study analysis confirms that the passage of SYGs is associated with a 3% to 6% increase in traffic fatalities.

Furthermore, the study investigates SYGs' heterogeneous impact on numerous measures of traffic fatality outcomes. The study finds salient heterogeneity by related contributing factors. After the

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[4] 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."

implementation of SYGs, speeding-related fatalities increase by around 17%, non-alcohol-related fatalities rose by around 5%. Additionally, the study identifies law heterogeneity effects by location, timing, gender, and age. Under SYGs, the traffic fatalities in urban areas experience a 13% increase, nighttime traffic fatalities increase by around 4%, weekday traffic fatalities rise by 3%, and male traffic fatalities increase by 4%. Traffic fatalities for ages 15-19 and 20-29 are mostly affected by the laws; the outcomes increase by 7% and 4%, respectively.

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

The remainder of the paper proceeds as follows: Section 1.2 presents background information for SYGs. Section 1.3 describes the data. Section 1.4 explains the empirical methods. Section 1.5 estimates the SYGs' effect on several measures of traffic fatalities outcomes. Section 1.6 provides robustness checks and sensitivity checks for the main findings. Section 1.7 uncovers the mechanisms of how SYGs could influence traffic fatalities, and Section 1.8 concludes.

1.2 Background

1.2.1 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 since "a man's home is his castle" (Catalfamo, 2006). Conversely, for attacks in places other than one's home, states laws impose a duty to retreat for the victims if withdrawal from the scenes is possible. However, since the 1970s, 35 states have expanded the castle doctrine laws to places outside one's own home and lifted the duty-to-retreat requirement. For ease of expression, these laws are referred to as expanded castle doctrine (ECD) laws in the current study.

From 1971 to 1984, nine states have relaxed the "castle doctrine" to include one's workplace.[5] In 1994, Utah was the first state that expanded the castle doctrine beyond home and workplace. No

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

ECD law was implemented until the 2000s. In 2005, an ECD law like Utah's was enacted by Florida and "became the basis for a model law adopted by the American Legislative Exchange Council" (Smart et al., 2020). Since then, 29 more states have followed the legislative efforts.[6][7] The "duty to retreat" requirement has been replaced with the "no duty to retreat" principle to some extent.

The current study only focuses on a sub-group of ECD laws. The laws, referred to as SYGs in the current study, extended castle doctrine laws to one's vehicle or "any place one has a legal right to be" (Cheng & Hoekstra, 2013).[8] Table 1.1shows the detailed effective dates for SYGs for 48 contagious states during 1989-2018. And Figure 1.1 demonstrates the share of SYG-laws state out of the 48 states in the sample.[9] During the time period, 28 states in the sample implemented SYGs. 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" (Cheng & Hoekstra, 2013). 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.

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[6] Arkansas and North Dakota passed SYGs in 2021 and are considered as control states in the sample of the study.
[7] 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 SYGs (Fisher and Eggen, 2012; Jansen and Nugent-Borakove, 2016).
[8] The exact definition of SYGs 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 SYGs 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 SYGs as laws extend castle doctrine to "any place one has a legal right to be" (Cheng & Hoekstra, 2013).
[9] Figure A1 in appendix A visualizes the adoption of SYGs in a geographic way.

1.2.2 Studies on ECD laws

No literature has investigated the effects of ECD laws on traffic safety outcomes. And the ECD studies mainly focus on the laws' impact on crime. Cheng and Hoekstra (2013) is the first empirical study that comprehensively examines the impact of expanded castle doctrine on crimes (McClellan and Tekin, 2017). Specifically, using state-level yearly data from the Uniform Crime Reporting Program (UCR) between 2000 and 2010, and a difference-in-differences method, Cheng and Hoekstra (2013) finds the ECD laws do not reduce burglary, robbery, or aggravated assault, but increase the total of murder and nonnegligent manslaughter by 8%. Using data from UCR and specifications of difference-in-differences and instrumental variables, Gius (2016) identifies no evidence indicating any crime deterrence effect from ECD laws. Additionally, Gius (2016) indicates ECD laws may increase sub-categories of crimes, and whether one sub-category is affected depends on the related specification. McClellan and Tekin (2017) investigates ECD laws' impact on firearm homicides and injuries by using state-month homicides data from the U.S. Vital Statistics, 2000-2010, and difference-in-differences specifications. The findings of McClellan and Tekin (2017) suggests that ECD laws increase the total firearm-related homicide rate by 7.5%. Munasib et al. (2018) studies the impact of ECD laws on non-suicide-related gun death with mortality data from 1999 to 2013 and a difference-in-differences 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 of total homicide increasing and supportive evidence of firearm homicides increasing. However, the directions of the ECD laws' impact on suicide, mass shooting, defensive gun use, and firearm ownership and purchases are identified as inconclusive.[10]

1.2.3 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 phone survey data of over 2400 licensed drivers, Hemenway et al. (2006) finds that making rude gestures and aggressively following other drivers are related to driving with a firearm in the vehicle. Bushman et al. (2017) conducts a laboratory experiment by randomly designating 60 university students to drive with a gun or a tennis racket, aiming to identify the "weapon effect."[11]

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[10] 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).

[11] “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.

The result shows the mere sight of a gun in the vehicle may elicit more aggressive driving behaviors.

1.3 Data

1.3.1 Data on Traffic Fatalities

State-level traffic fatalities are from National Highway Traffic Safety Administration (NHTSA) 's Fatality Analysis Reporting System (FARS) for 1989-2018. FARS is the most comprehensive database for fatal accidents in the United States (Saffer, 1997). The data documented detailed information for each person and each vehicle involved in a deadly traffic accident and the circumstance of the fatal accident. The data have been used to evaluate traffic safety related laws, including Minimum legal drinking age laws (Ruhm, 1996), primary and secondary seat belt laws (Cohen & Einav, 2003), Blood Alcohol Content (BAC) .08 laws (Freeman, 2007), zero-tolerance laws (Darren Grant, 2010), social host law (Dills, 2010), texting ban and hands-free laws (Rocco & Sampaio, 2016), or to evaluate alcohol or drug policy that could potentially affect traffic safety, such as beer tax (Ruhm, 1996), medical marijuana laws (Anderson 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 SYGs on aggressive driving fatalities, I aggregate the traffic fatalities involved in specific behaviors classified by Stuster (2004) as potentially aggressive driving related. The aggressive driving related behaviors in the analysis include speeding, following improperly, improper or erratic lane changing, failure to yield right of way, and failure to obey traffic signs, traffic control devices, or traffic officers, failure to observe safety zone traffic laws.[12] 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 SYGs.

Table 1.2 displays 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. [13] 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, most traffic fatalities happened to the age group 15-39 years old. This is also the age group where people were more often engaged in risky driving practices (Gross, 2016). Table 1.2 also displays the descriptive statistics of outcome variables stratified by the SYG law status. Overall,

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

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

the traffic fatality rates for SYG states are higher than those for states that have not implemented SYGs. These differences necessitate the inclusion of fixed effects in the models.

1.3.2 Data on Gun Ownership

There is no panel data available for gun ownership in the traffic fleet.[14] Thus, the current study examines the impact of SYGs on "vehicular gun ownership" by using corresponding proxies. This study focuses on two proxies, namely, “state-level estimates of household firearm ownership data” from Rand Cooperation (Schell et al., 2020) and firearm suicide data from CDC, 1989-2016. Schell et al. (2020) attempts to address the concerns that current survey data suffers from enormous missing values and household gun ownership proxies may not capture the long-run time trends. Using multi-level regression with poststratification and a structural equation model, Schell et al. (2020) pools four national representative survey data, several related proxies and laws indicators, and demographic data together to construct state-year level household gun ownership data. Household gun ownership is defined as "the annual proportion of adults living in a household with a firearm for each state” (Schell et al., 2020). Before the release of Schell et al. (2020), the gun literature mainly relied on proxies such as the firearm suicide rate to measure the household gun ownership (Kleck, 1997; Cook and Ludwig, 2002; Moody and Marvell, 2003; Cook and Ludwig, 2006). The firearm suicide rate is defined as the percentage of suicides that are committed by guns. Table 1.3 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.

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[14] 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.

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

1.3.3 Data on Crime

Following the literature investigating the ECD laws' impact on violent crimes, the current study uses Uniform Crime Reporting Program (UCR) data, 1989-2018, to examine SYGs' impact on aggravated assault rate and murder rate. UCR data contains monthly crime and arrest information that is voluntarily reported by the individual law enforcement agencies. Table 1.3 reports the average murder rate and aggravated assault rate are 6.61 and 1286 per 100,000 population, respectively.

1.3.4 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” (Covington, 2021). However, there is no administrative data with a concentration on road rage crime. The current study proposes a novel method to approximate the road rage cases using subgroup crime data from the National Incident-Based Reporting System (NIBRS). NIBRS data provides comprehensive information on every crime incidence reported to the police, including victim and offender demographics, whether the assailant was arrested, when the crime happened, the relationship between victims and assailants, and the crime location. Specifically, this study aggregates the crime incidence that occurs on highway/road/alley, contains aggravated assault and murder, contains circumstances as argument and other circumstances, and contains circumstances that all victims are individuals, and all victims are strangers to the offenders. In terms of the weapon used in the crime incident, the crimes are grouped into "road rage using firearm," "road rage using vehicle," and "road rage using other weapons." Table 1.3 presents the estimated road rage cases involving weapons that are firearm and vehicle are 2.29 and 1.69 per 100,000 population, respectively.[15]

1.3.5 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 SYGs on vehicle miles driven (VMT). The last panel in Table 1.3 presents the vehicle miles traveled per 100,000 licensed drivers, stratified by degree of urbanization. Though the total VMT and urban VMT are very similar across SYG states and other states, the rural vehicle miles traveled in SYG states are about twice those in other states.

1.4 Empirical Method

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

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

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

Here, Y_{it} represents the various measures of traffic fatality rate in Table 1.2 for state s at year t, w_t represent the full set of year fixed effects, v_s represent the full set of state fixed effects, SYG_{st} is a dummy variable, indicating when the state has the SYG law. The dummy variable is set equal to the fraction of the year that have SYG. The interested coefficient, β_1, represents the relationship between SYGs and traffic safety outcomes. X_{st} represent a vector of control variables listed in Table 1.2[16]. ε_{st} is an error term. Robust standard errors are clustered at the state level.

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

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

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

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

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

1. 5 Results

1.5.1 The Relationship between SYGs and Traffic Fatalities

Table 1.4 shows the estimates of the impact of SYGs 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.[17] The standard errors are clustered at the state level (Bertrand, Duflo, and Mullainathan 2004). The baseline analysis in column 1 shows that the passage of SYGs increases the fatality rate by 3%.[18] The event study estimates in column 2 and column 3 display a quite persistent effect across years. For column 2, the traffic fatality rate increases by 8% in the first year of legalization. The magnitudes of the rise are steady at 4% until the fourth year of implementation. For four years beyond the legalization, the impacts are positive and statistically insignificant at 3%. Figure 1.2 provides a visualization of the dynamic effect for the specification of column 3. The first year of implementation leads to a 6% increase in traffic fatalities. The estimate for the second year of implementation is positive but statistically insignificant. The estimates for the third and fourth years indicate 3% increases in total traffic fatalities. The magnitude of effect diminishes to an insignificant 2% rise in total traffic fatalities after four years.

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

[18] The passage of SYGs 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.

In Table 1.5, the current study replaces total traffic fatalities with fatalities involving aggressive driving factors. The aggressive driving related factors in the analysis include speeding, following improperly, improper or erratic lane changing, failure to yield right of way, and failure to obey traffic signs, traffic control devices, or traffic officers, failure to observe safety zone traffic laws. Columns 1-3 present the estimates for the relationship between SYGs and traffic fatalities related to any aggressive driving factors. Columns 4-6 show the estimates for the impact of SYGs on speeding fatalities. Though the simple difference-in-differences estimation gives positive insignificant estimates, the results in event studies might suggest SYGs are positively associated with aggressive driving fatalities. Figure 1.3 visualizes the estimates for column 3 and column 6. The left panel shows the legalization is associated with a 6-12% increase in aggressive driving fatalities at 90% confidence level. The right panel displays that the implementation increases speeding-related fatalities by 17% in the first year of implementation. Then the impact wanes to an 8% increase in the third year of implementation. The effects turn into statistically insignificant for the following years.

Yu et al. (2004) surveyed 431 people from “fifty alcoholism and substance abuse treatment facilities across New York State” and found that alcohol problems were associated with aggressive driving, but not road rage behavior. In Table 1.6, the study explores SYGs' impact on alcohol-related and non-alcohol-related traffic fatalities. The difference-in-differences estimate from column 1 shows that SYGs are associated with a 4% increase in traffic fatalities without alcohol involvement. The event study estimates from column 2 and column 3 present the law increase the non-alcohol traffic fatalities by 3-9%. The Estimates for columns 4 -12 do not identify any treatment effect heterogeneity for alcohol-related fatal injuries. Figure 1.4 shows a visual representation of event study results for traffic fatalities stratified by alcohol involvement. The upper-left panel shows the non-alcohol traffic fatality rate rises by 7% in the first year of implementation. For the following years of the law implementation, the increases are around 3%.

According to Smart et al. (2003), road rage victimization is more prevalent among people living in urban area. To explore the treatment effect heterogeneity across regions, this study focuses on the fatalities in urban and rural areas separately.[19] Table 1.7 shows the impact of SYGs on traffic fatalities by the degree of urbanization. The difference-in-differences estimates from column 1 and column 4 show the legalization is associated with an insignificant 2% decrease in rural traffic fatalities and a significant 13% increase in urban traffic fatalities. Columns 2 and 5 show that SYGs do not significantly affect rural traffic fatalities but significantly increase urban traffic fatalities by 9-22%. Figure 1.5 visualizes the estimates from column 3 and column 6. The left panel shows that SYGs decrease traffic fatalities by around 5-10%. However, the estimate for five years before the implementation is negative and statistically insignificant, which may indicate a potential violation of the parallel trend assumption. The right panel presents that the legalization increases urban traffic fatalities by 22% in the first year. The estimate for the second year shows a 9% increase. The estimates for the third and fourth years are positive but insignificant at 7-10%. After four years, the legalization is associated with a 16% rise in traffic fatalities.

[19] 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.

Auto Insurance Center (2016) studies road-rage-related Instagram hashtag words and finds aggressive driving behaviors peak around 6 p.m. Additionally, road rages are most prevalent on Friday and least prevalent on Sunday. Table 1.8 represents the impacts of SYGs on traffic fatalities by time and day. Estimates of columns 1-3 do not identify any significant impacts of SYGs on daytime traffic fatalities. Difference-in-differences estimates from column 4, column 7, and column 10 show that nighttime traffic fatalities, weekday traffic fatalities, and weekend traffic fatalities increase by around 3%. Figure 1.6 visualizes the event study estimates from column 3, column 6, column 9, and column 12. The impacts on daytime traffic fatalities and weekend traffic fatalities are insignificant. SYGs are associated with an initial 7% increase in nighttime traffic fatalities and weekday traffic fatalities. The estimates for the following years for both outcomes are around 3-4%.

The findings concerning gender differences in road rage stressed the prevalence of road rage among male drivers (Evans, 1991; Miller et al., 2002; Sansone and Sansone, 2010; AAA Foundation for Traffic Safety, 2020). Table 1.9 illustrates the relationship between SYGs and traffic fatalities by gender. Simple difference-in-differences analysis shows that male traffic fatalities increase by 4%, while the effect of the SYG law on female traffic fatalities is insignificant. The event studies in column 3 and column 6, visualized in Figure 1.7, suggest that the male traffic fatalities increase by 4-8% following the policy change and the female traffic fatalities experience a 3% increase after the law implementation.

According to AAA Foundation for Traffic Safety (2020), "Male and younger drivers ages 19-39 were significantly more likely to engage in aggressive behaviors.” It could be possible the impact of SYGs on traffic fatalities for ages between 19 to 39 are more saliant than other age groups. Table 1.10 shows the estimated impact of SYGs on traffic fatalities by age group. Difference-in-differences estimates for age groups of 15-19, 20-29, 30-39, 40-49 are statistically significant at conventional levels. The event study estimates in Figure 1.8 show that the traffic fatalities for ages 15-19 increase by around 9-15%, traffic fatalities for ages 20-29 rise by about 7%, and traffic fatalities for ages 50-64 increase by around 4-8%. The estimated coefficients for the rest outcomes are statistically insignificant.

1.6 Robustness Checks and Sensitivity Checks

Recent literature raises the concern that the two-way fixed effect model could potentially be biased for the settings with staggered policy implementation and heterogeneous treatment effect (Borusyak, Jaravel and Spiess, 2021; Bacon, 2021; de Chaisemartin and D'Haultfoeuille, 2020; Sun and Abraham, 2020). The canonical difference-in-differences setting contains a pair of treatment group and control group with pre- and post-treatment time periods. According to Bacon (2021), for settings with staggered policy implementation, the two-way fixed effect estimator is a weighted average of the estimators from all possible canonical difference-in-differences settings within the data. The bias potentially arises when the average treatment effects vary across time within groups and the canonical difference-in-differences setting contains both an "earlier-treated group" and a "later-treated group" (Bacon, 2021). Following Bacon (2021), I decompose the difference-in-differences estimate for the total traffic fatalities analysis (0.034) into three types of canonical settings: the "earlier-treated group" as the control group vs. the "later-treated group" as the treatment group, the "earlier-treated group" as the treatment group vs. the "later-treated" group as the control group, and the treated group vs. the never treated control group.[20] The average estimate for the models comparing treatment groups and never treatment groups is 0.031, with a weight of 0.78. The potential bias raised with heterogeneous treatment effects may not be a major concern.

To formally address the concern of heterogeneous treatment effects, I estimate the effect of SYGs by using the event study method introduced by Sun and Abraham (2020). Sun and Abraham (2020) provides a decomposition of estimators for lead and lags in the traditional event study models to discuss the potential bias and proposes "interaction-weighted" (IW) estimators to perform event studies. The method follows three steps. First, it estimates the two-way fixed effect model controlling for relative time dummies interacted with cohort dummies. The cohort dummies indicate the group of the initial treatment timing. The control group consists of never treated states. Second, it estimates each cohort share in the sample for the relative time period(s). Third, it calculates the weighted average of the estimates from step one with weights of the estimated shares in step 2. Figure 1.9 through Figure 1.15 present the corresponding event study estimates based on the new approach. The results are similar to the two-way fixed effect model estimates. Specifically, the legalization increases total traffic fatalities, female traffic fatalities, and weekday traffic fatalities by around 3-4%. In addition, SYGs are associated with a 17% increase in urban traffic fatalities. The effects on aggressive driving fatalities, speeding fatalities, and non-alcohol-related traffic fatalities are positive but insignificant.

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

Table 1.11 shows additional robustness checks for the total traffic fatalities analysis. Column 1 repeats the baseline event-study model for total traffic fatalities. In columns 2 and 3, this study checks the robustness of the results using alternative treatment indicators. Specifically, Column 2 runs the baseline regression for the sample excluding states that extended castle doctrine to vehicle. Column 3 runs the baseline regression for the sample excluding states that extended castle doctrine to any places one has a legal right to be. The robustness check confirms that SYGs are associated with an increase in total traffic fatalities. The magnitudes of coefficients on two years after and three years after are quite similar.

In Table 1.12, Column 1 repeats the baseline regression result. Column 2 through Column 4 run regressions with the different definitions of the natural log of the traffic fatalities outcome variable. Specifically, column 2 defines the outcome variable as traffic fatalities count. Column 3 defines the outcome variables as traffic fatality count divided by 100,000 licensed drivers. Column 4 defines the outcome variables as traffic fatality count divided by the total vehicle miles traveled (VMT). Column 2 also controls for the natural log of the state population. Among these definitions of the dependent variable, log(count/population) is the most widely used in traffic safety literature. The results across the four specifications are very similar. All results show SYGs increase traffic fatalities by 6% in the first legalization year.

In Table 1.13, 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.[21] People who live in the SYG States may drive to non-SYG states, which could have a potential spillover effect and invalidate the difference-in-differences assumption. To test the potential spillover effect, following Kyiazi (2019), the current study constructs the index below,

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

Therefore, $Spillover_{st}$ is the ratio of the neighboring states under SYGs. Column 8 of Table 1.13 show the regression result for baseline model controls for the spillover index. Adding the spillover index has a minimum impact on the event-study coefficients. And this result further supports the validity of the model. Table 1.14 through Table 1.16 show the robustness checks and sensitivity checks for urban traffic fatalities analysis, and the results are robust across different specifications.

Following Anderson et al. (2013), the current study runs a series of placebo regressions. Each panel in Figure 1.16 shows the histogram of placebo estimates from 1,000 trials of the simple difference-in-differences model of Equation (1). For each trial, I randomly assigned 29 states that

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

legalized SYGs in random years because 29 states legalized SYG during 1989-2018. For each panel, the vertical solid lines indicate the estimated coefficient based on true effective dates, and the area between the dashed lines lies 90% of the coefficients from pseudo effective dates. The major results are robust to permutation tests.

1.7 Mechanism

Table 1.17 shows the regression result by regressing SYG law on two types of gun ownership proxies for 1989-2016. The simple difference-in-differences estimates for both proxies show the SYGs are associated with a 3% increase in gun ownership. Figure 1.17 visualizes the event study estimates from column 3 and column 6. The estimates from the regression of Rand estimated gun ownership is more salient than the estimates from firearm suicide regression. The SYGs increase Rand estimated gun ownership by 11% in the first year of legalization, while the laws increase firearm suicides by 3% in the first year at 90% confidence level and 3% in the fourth year at 95% confidence level.

Next, attention is paid to crime. Chen and Hoeksta (2013) studies extended castle doctrine to places outside the home and finds ECD laws increase murder by 8%. McClellan and Tekin (2017) studied the impact of extended castle doctrine to “any place one has a legal right to be” and finds ECD laws increase total homicide by 7.5% and increase homicides among white total and white males by 12.5% and 15.3% (Cheng & Hoekstra, 2013). But McClellan and Tekin (2017) finds an insignificant impact of ECD laws on African-Americans or blacks. The current study contributes to the literature by studying the effect of extending castle doctrine to one's vehicle and “any place one has a legal right to be” on crime (Cheng & Hoekstra, 2013). Specifically, Table 1.18

summarizes the regression results that analyze SYGs' impact on aggravated assault and murder. The regression is very similar to the baseline regression but replaces the natural log of traffic fatality rate with the log of the crime rate. The simple difference-in-differences estimate from column (4) shows SYGs 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 SYGs states.[22] This is smaller than the finding from McClellan and Tekin (2017), which indicates SYGs cause 600 additional murders each year for the SYGs 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 SYGs after 2010. In addition, the definition of SYGs varies. Figure 1.18 represents the event-study results from column 3 and column 6 in graphs. The estimates from the left panel are statistically insignificant. The right panel presents that the legalization increases the murder rate by14% in the first year of implementation and the impacts are consistent over the years at around 12-18% increases.

Table 1.19 displays the impacts of SYGs on road rage crime proxies from murder and aggravated assault. Given the nature of the data, which contains many zeros, the study applies Poisson fixed effect model. The simple difference-in-differences show that the road rage crime involving

[22] 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 SYGs 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 SYGs states

firearms and vehicles significantly increase by 23% and 27%, respectively[23]. Figure 1.19 visualizes the event study estimates. SYGs are associated with initial increases of 43% for both road rage crime proxies. Back-of-envelope calculations indicate the road rage crimes associated with firearms and the one associated with vehicles increase by 516 cases and 439 cases each year after the SYG law was implemented.[24] By contrast, similar analysis shows SYGs may cause additional 893 traffic casualties each year.[25] This indicates other channels possibly exist between SYGs and traffic fatalities. The current study only focuses on road rage crimes involving murder and aggravated assault. According to Ponomarova (2019), road rage crimes may involve other types of crimes such as hit and run, reckless driving, criminal threats, and vandalism. Due to the data limitation, the current study does not address these potential road rage crimes.

To address the concern that SYGs could potentially make people feel safer and drive more frequently. Table 1.20 presents the impact of SYGs on VMT and Figure 1.20 visualizes the event study estimates. No significant impact was detected for the VMT outcomes, and the results do not identify any evidence to support this anecdotal evidence.

1.8 Conclusion

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[23] The percentage changes are calculated as $(e^{\beta} - 1) \times 100\%$.
[24] 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 SYGs 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 SYGs states.
[25] The difference-in-differences estimates shows SYGs raise total traffic fataltiies by 3%. The average traffic fatalities one year prior to the legalization is 18.06 fatalities per 100,000 population. The 3% increase indicate an increase of 0.54 casualties per 100,000 population. Multiplying the increased share by the SYG states population yields 893 additional deaths per year.

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 SYGs 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 impact of SYGs on traffic safety.

The current study uses data from the Federal Highway Administration and National Highway Traffic Safety Administration to investigate the impact of SYGs on traffic fatalities. The reduced form regressions show that the SYGs 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 SYGs could affect traffic fatalities, I examine SYGs' impact on household firearm ownership, road rage crime, and vehicle miles driven. The results show that firearm ownership increased by 11%, and the road rage crime increased by 23%-27% after passing laws. In other words, the SYGs induce around 450 additional road rage cases per year. No significant impact is identified for vehicle miles driven.

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 SYGs 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 SYGs increase gun ownership. For mitigating the concern of “flow vs. stock,” the SYGs 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 SYGs extend beyond violent crime and suicide.

1.9 Tables and Figures

Table 1.1

SYGs, 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 SYGs in 1989-2018 and their corresponding effective dates. Alaska adopted the SYG law in 2013. Arkansas and North Dakota implemented SYGs in 2021.

Table 1.2

Outcomes for Reduced Form Regressions

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

Notes. This table shows the summary statistics for traffic fatality rates from National Highway Traffic Safety Administration's Fatality Analysis Reporting System (FARS), 1989-2018. The second, third, and fourth columns show the weighted means with standard deviations in parentheses for all states, states that implemented SYGs, and states that have not yet implemented SYGs 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 1.3

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	2.32 (1.63)	2.65 (1.6)	1.54 (1.41)	360
Vehicle Involved	1.71 (1.16)	2.03 (1.16)	0.93 (0.68)	360
Other Weapon Involved	3.93 (2.45)	3.55 (2.04)	4.82 (3.05)	360
Vehicle Miles Traveled				
Total Vehicle Miles Traveled	1412 (207)	1490 (201)	1316 (170)	1440
Urban Vehicle Miles Traveled	914 (187)	891 (196)	942 (173)	1440
Rural Vehicle Miles Traveled	498 (254)	599 (225)	372.61 (231)	1440

Notes. This table shows the summary statistics for gun ownership proxies, road rage crime proxies, and vehicle miles traveled in the sample. Rand estimates of gun ownership, defined as “the percentage of adults living in households with a firearm”, are from Schell et al. (2020), 1989-2016. The firearm suicide rate, defined as the percentage of suicide committed by a firearm, is from the Centers for Disease Control and Prevention Compressed Mortality File, 1989-2016. Murder and aggravated assault, defined as the actual crime divided by the agency reported population, are from the Federal Bureau of Investigation's Uniform Crime Reporting Program, 1989-2018. Road rage proxies are from the Federal Bureau of Investigation's National Incident-Based Reporting System, 2001-2018. Vehicle miles traveled related variables, defined as corresponding vehicle miles traveled divided by 100,000 licensed drivers, are obtained from Federal Highway Administration's Highway Statistics, 1989-2018. The second, third, and fourth columns show the weighted means with standard deviations in parentheses for all states, states which implemented SYGs, and states which not yet implemented SYGs 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 1.4

The Effect of SYGs 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 SYGs, 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 1.5

The Effect of SYGs 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 SYGs, 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 1.6

The Effect of SYGs 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 SYGs, 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 1.7

The Effect of SYGs 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 SYGs, 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 1.8

The Effect of SYGs 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 SYGs, 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 1.9

The Effect of SYGs 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 SYGs, 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 1.10

The Effect of SYGs 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 SYGs, 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 1.10

(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 SYGs, 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 1.11

Robustness Check for Total Traffic Fatalities Analysis

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

Notes. Column (1) repeats the baseline regression result from Column (3) of Table 1.4. Column (2) runs the baseline regression for sample excluding states implemented SYGs which extend castle doctrine to any places one has a legal right to be. Column (3) runs the baseline regression for sample excluding states implemented SGY laws which extend castle doctrine to vehicle. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. All regressions include leads and lags for SYGs, 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 1.12

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 1.4. Column (2) through Column (4) run regressions with the different definitions of the natural log of traffic fatalities outcome variable. Column (2) defines the outcome variable as traffic fatalities count. Column (3) defines the outcome variables as traffic fatality count divided by 100,000 licensed drivers. Column (4) defines the outcome variables as traffic fatality count divided by the total vehicle miles traveled (VMT). All regressions include leads and lags for SYGs, omitting the dummy for one year prior. Column (2) controls for the natural log of the state population in addition to the baseline group of control variables. 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 1.13 Sensitivity Check for Total Traffic Fatalities Analysis - Alternative Covariates

Sensitivity Check for Total Traffic Fatalities Analysis - Alternative Covariates

	(1)	(2)	(3)	(4)	(5)	(7)	(8)	(9)
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 1.4. 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 SYGs, 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 1.14 Robustness Check for Urban Traffic Fatalities Analysis

Robustness Check for Urban Traffic Fatalities Analysis

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

Notes. Column (1) repeats the baseline regression result from Column (6) of Table 1.6. Column (2) runs the baseline regression for sample excluding states implemented SYGs which extend castle doctrine to any places one has a legal right to be. Column (3) runs the baseline regression for sample excluding states implemented SGY laws which extend castle doctrine to vehicle. The dependent variable in all columns is the natural log of traffic fatalities per 100,000 population. All regressions include leads and lags for SYGs, 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 1.15 Sensitivity Check for Urban Traffic Fatalities Analysis - Alternative Functional Form

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 1.7. Column (2) through Column (4) run regressions with the different definitions of the natural log of traffic fatalities outcome variable. Column (2) defines the outcome variable as traffic fatalities count. Column (3) defines the outcome variables as traffic fatality count divided by 100,000 licensed drivers. Column (4) defines the outcome variables as traffic fatality count divided by the total vehicle miles traveled (VMT). All regressions include leads and lags for SYGs, omitting the dummy for one year prior. Column (2) controls for the natural log of the state population in addition to the baseline group of control variables. 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 1.16

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 1.7. 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 SYGs, 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 1.17 The Effect of SYGs on Gun Ownership Proxies

The Effect of SYGs on Gun Ownership Proxies

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

Notes. The dependent variable in columns (1) is the natural log of the percentage of adults living in a household with a gun. The dependent variable in Column (2) is the natural log of the percentage of suicide committed by a gun. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYGs, omitting the dummy for one year prior. 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 1.18

The Effect of SYGs 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 SYGs, 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 1.19

The Effect of SYGs on Road Rage Proxies

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

Notes. Coefficients from the Poisson fixed effect model are reported. The three dependent variables are the crime counts for the proxy road rage cases involving firearms, vehicles, and other weapons. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYGs, omitting the dummy for one year prior. All regressions control for the baseline group of control variables and natural log of agency-reported population. 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 1.20

The Effect of SYGs on Vehicle Miles Traveled (VMT) per Drivers

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

Notes. The three dependent variables from Column (1) through Column (9) are the natural log of total VMT, rural VMT, and urban VMT divided by 100,000 driver population. SYG variable is a dummy variable indicating the share of the year in which an SYG law was implemented. The event studies include leads and lags for SYGs, omitting the dummy for one year prior. 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

Figure 1.1

Share of States with SYGs, 1989-2018

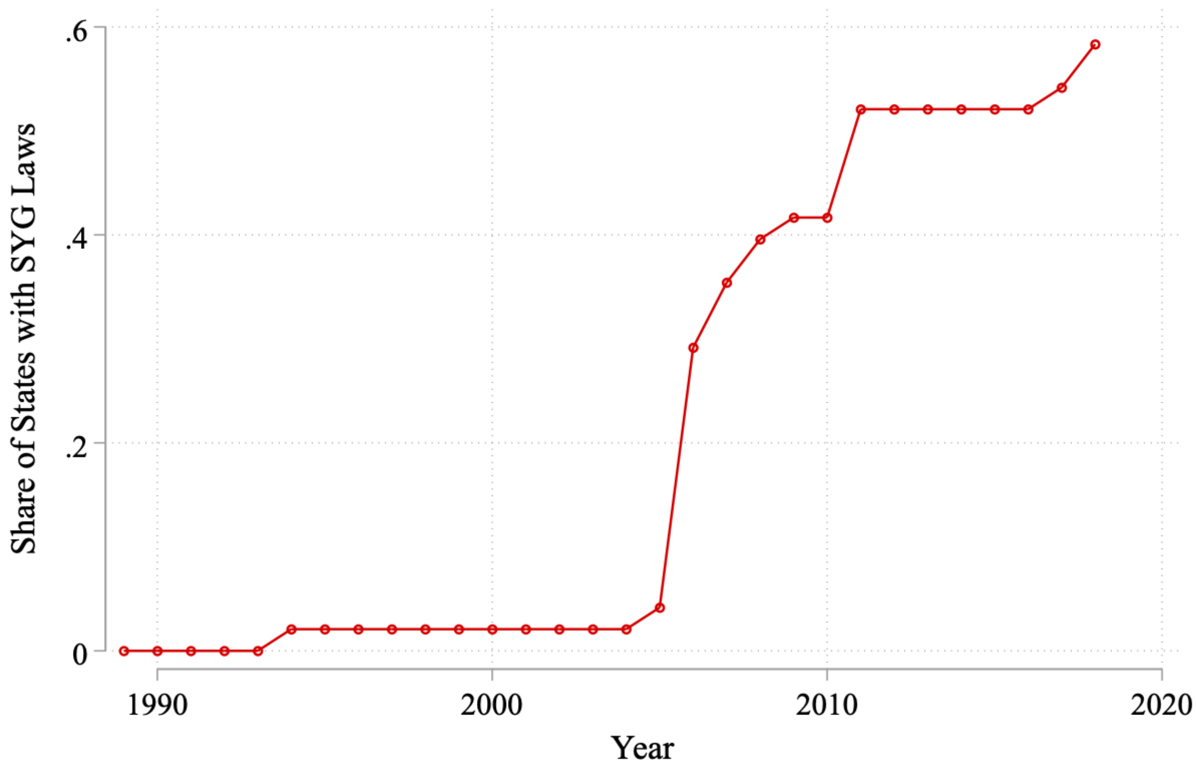


Figure 1.2

SYGs and Total Traffic Fatalities

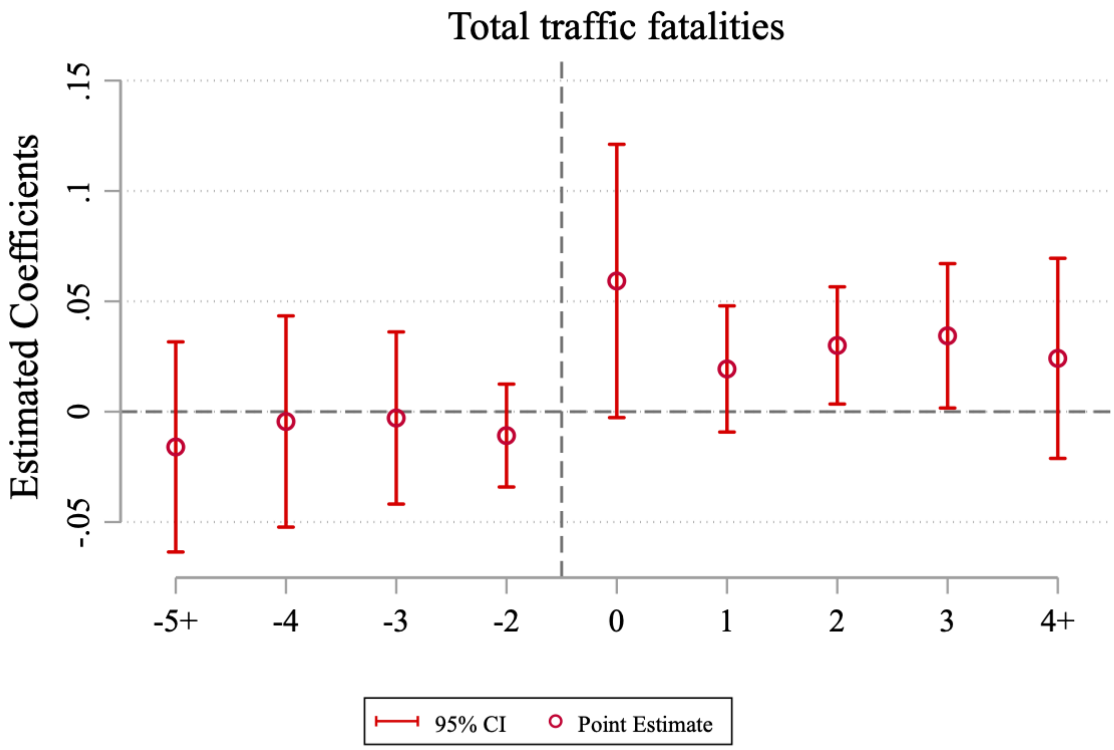


Figure 1.3

Laws and Traffic Fatalities Related to Aggressive Driving

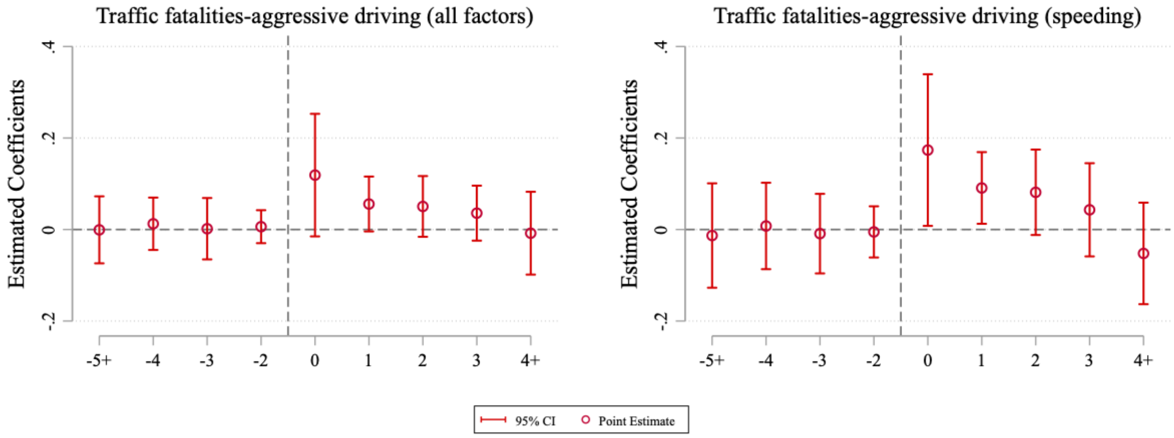


Figure 1.4

Laws and Traffic Fatalities Related to Alcohol Involvement

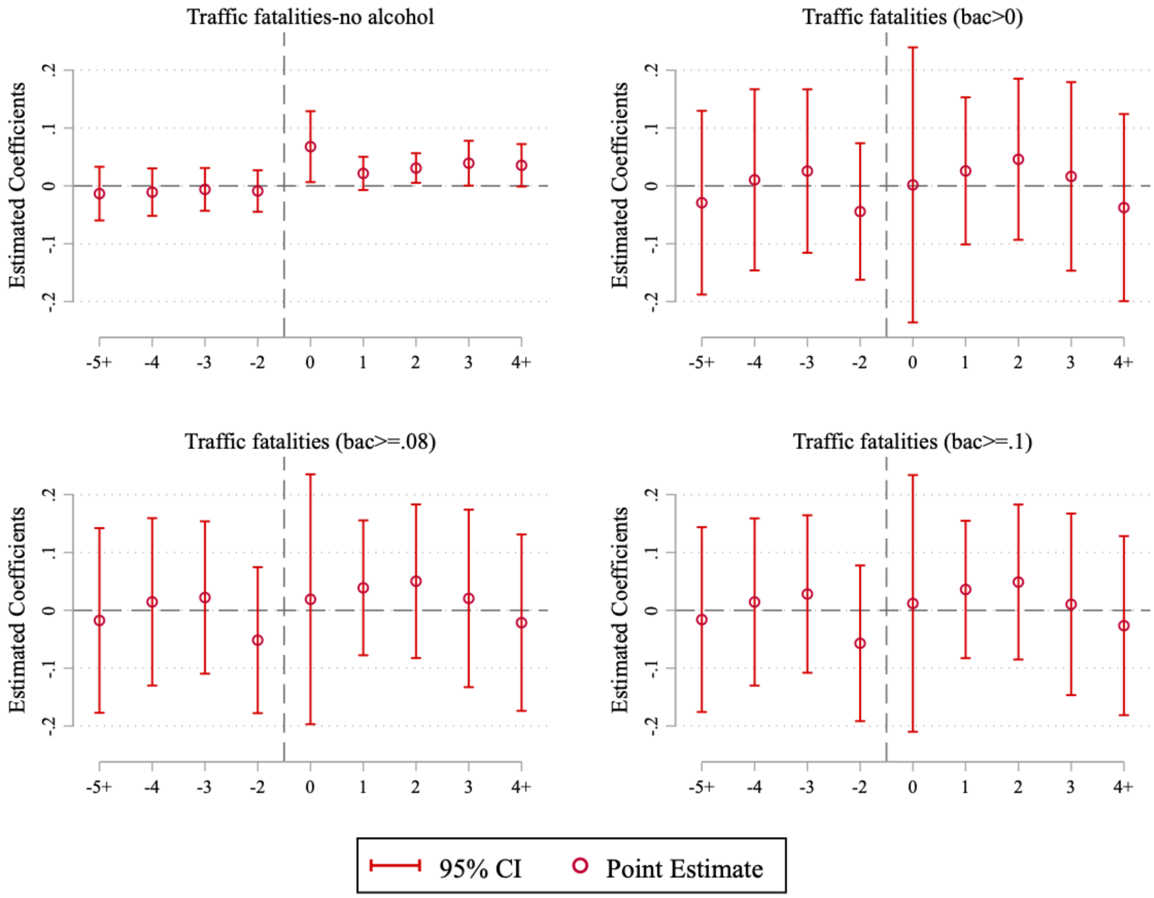


Figure 1.5

SYGs and Traffic Fatalities Related to Location

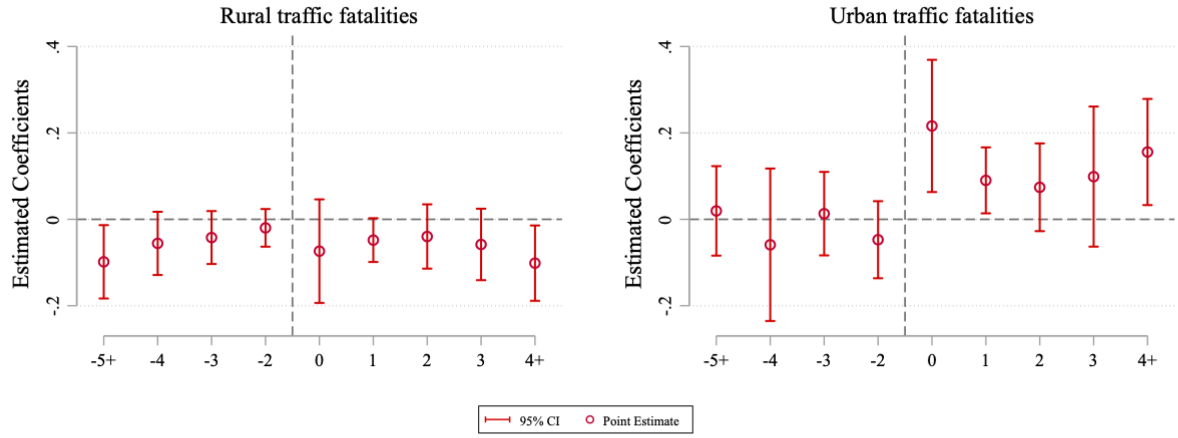


Figure 1.6

SYGs and Traffic Fatalities by Time

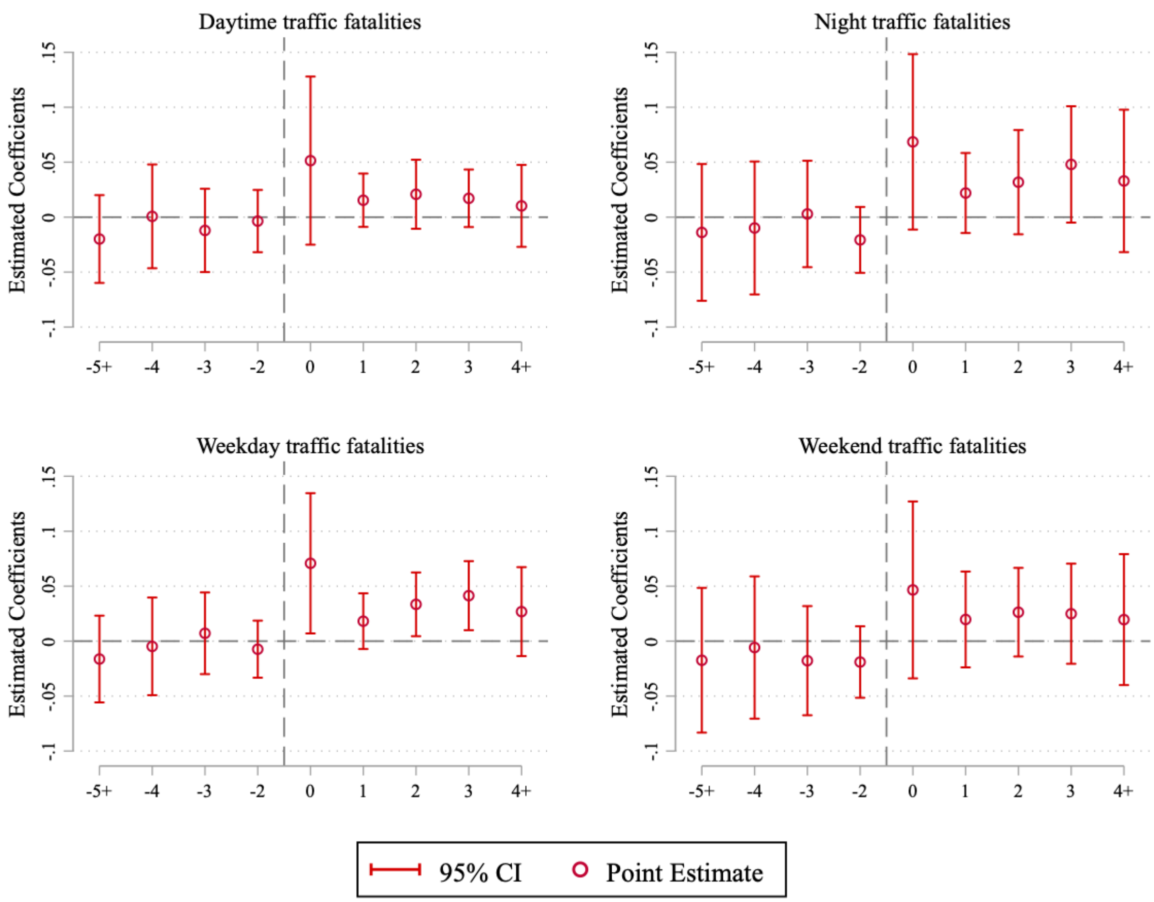


Figure 1.7

SYGs and Traffic Fatalities by Gender

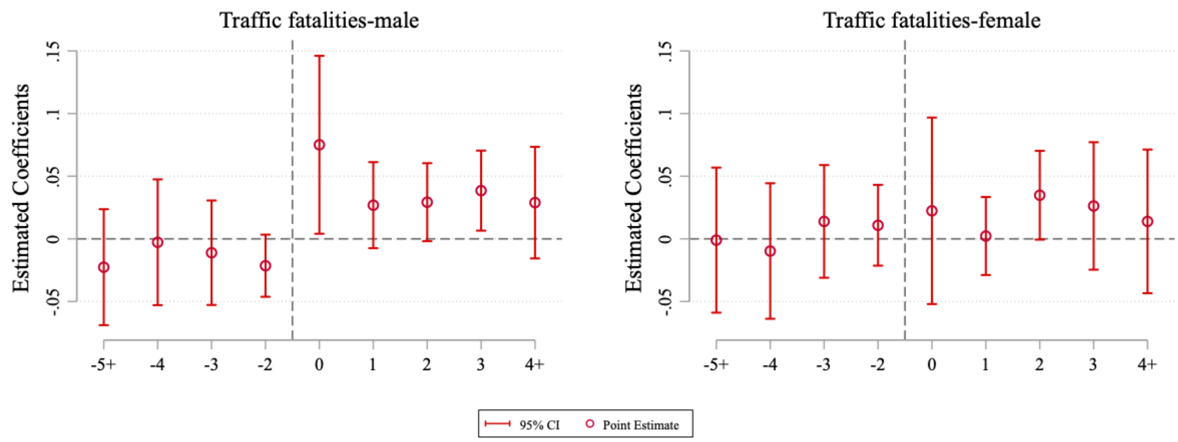


Figure 1.8

SYGs and Traffic Fatalities by Age

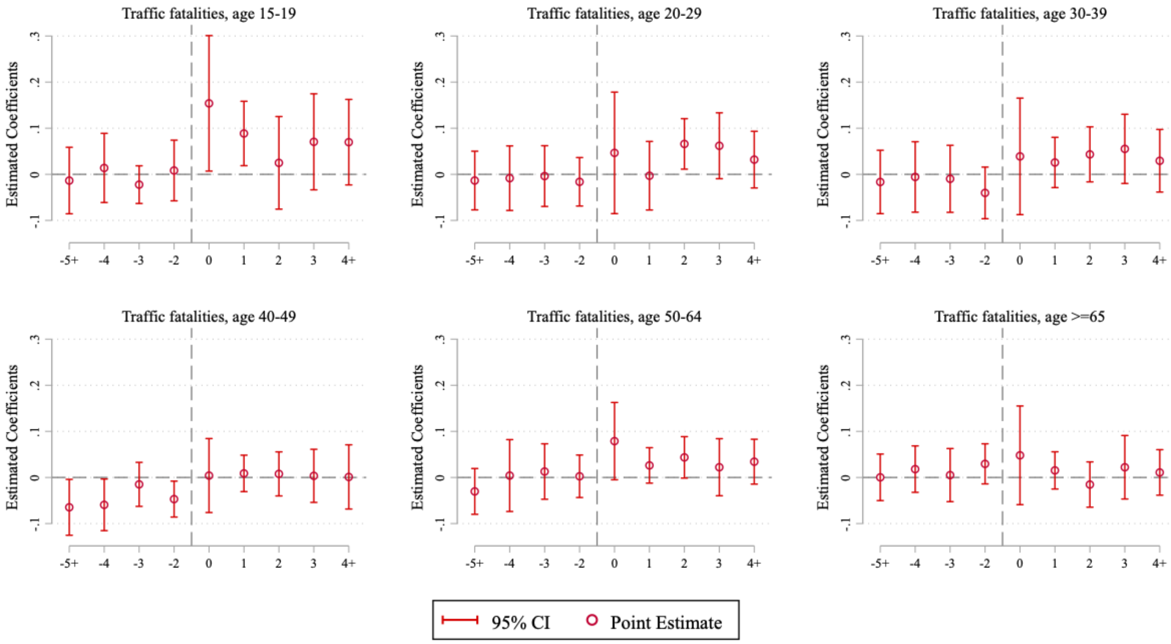


Figure 1.9

SYGs and Total Traffic Fatalities – Sun and Abraham (2020)

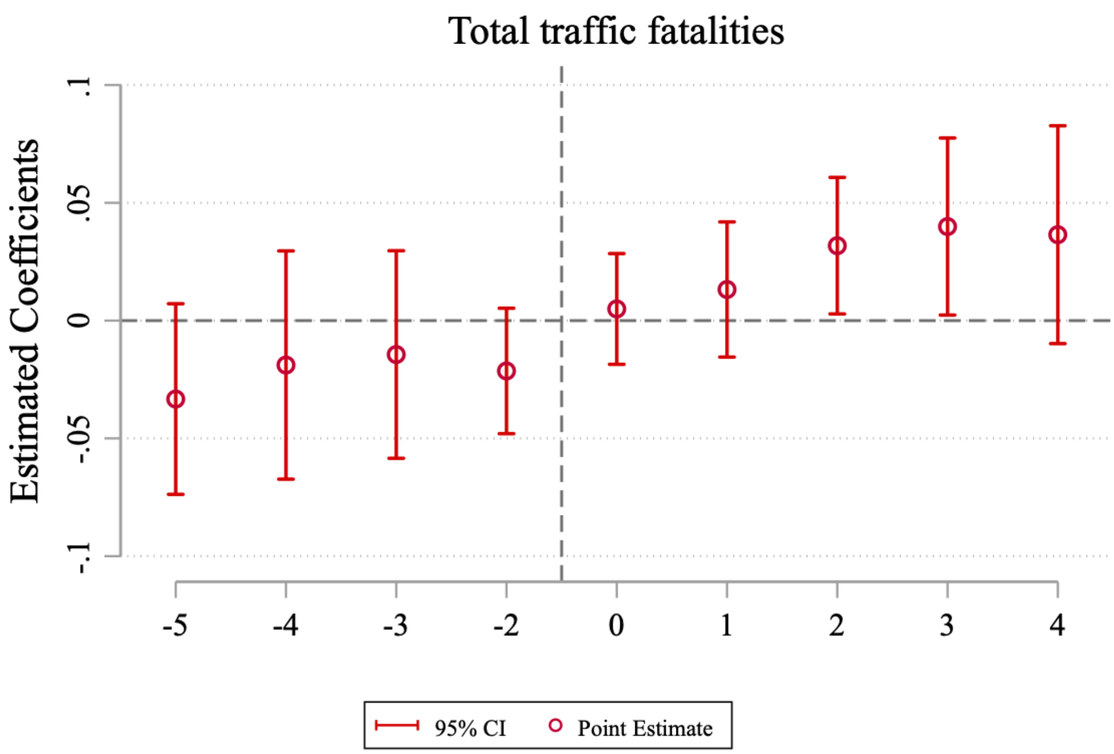


Figure 1.10

SYGs and Traffic Fatalities Related to Aggressive Driving – Sun and Abraham (2020)

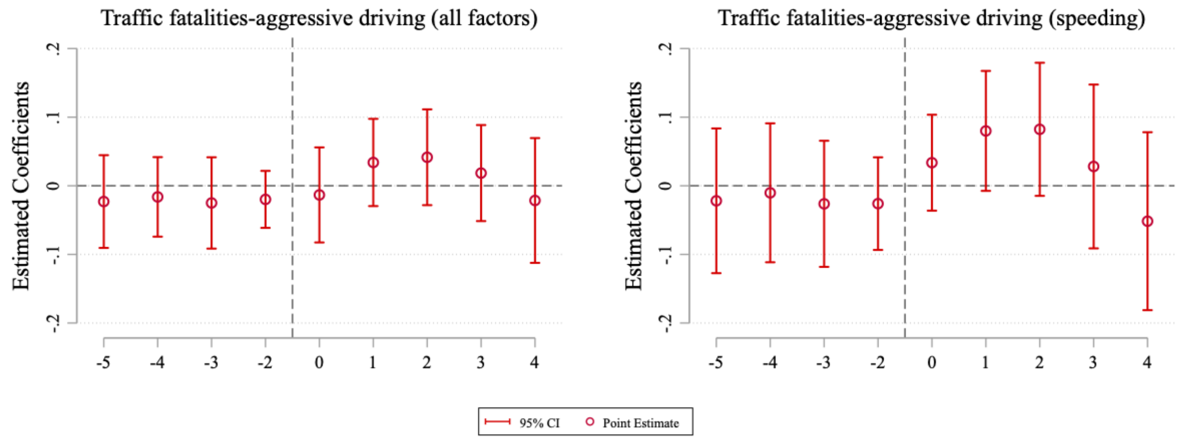


Figure 1.11

Laws and Traffic Fatalities Related to Alcohol Involvement – Sun and Abraham (2020)

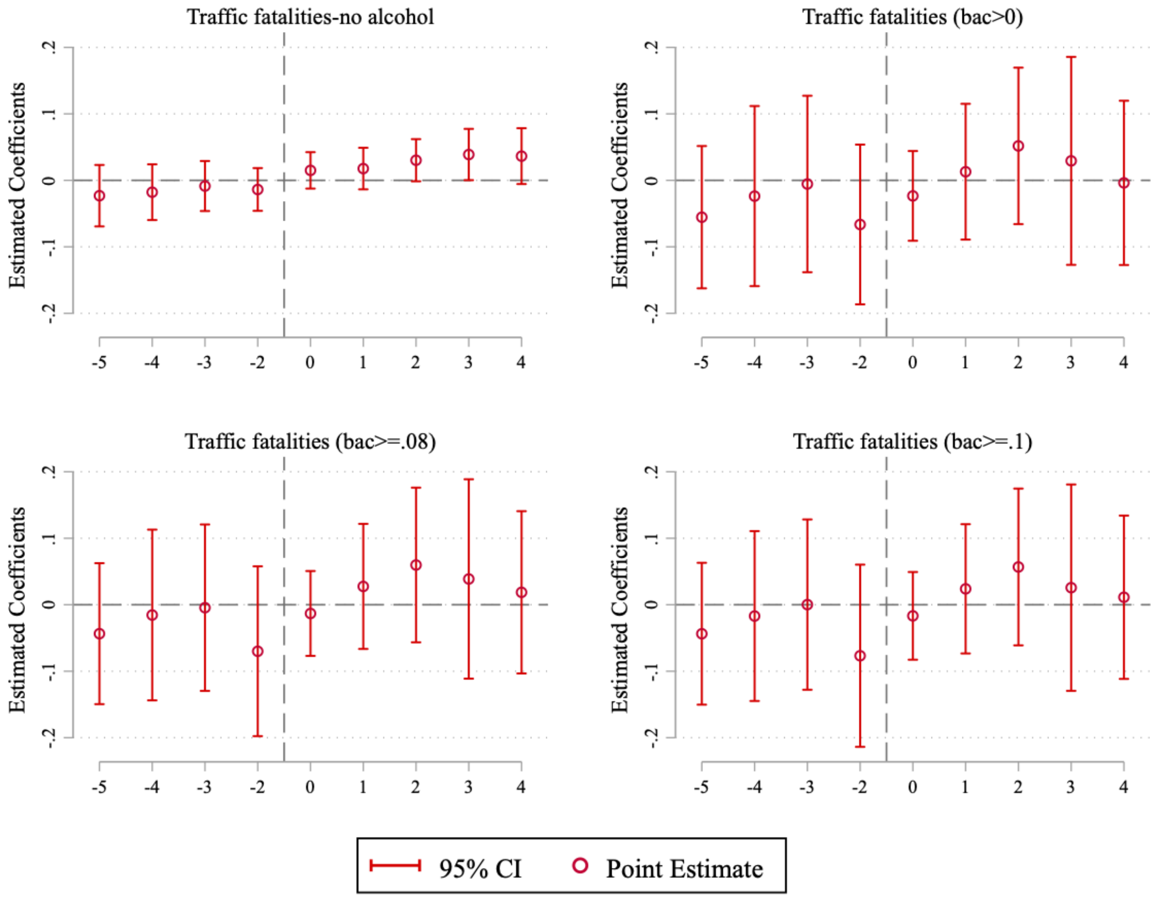


Figure 1.12

SYGs and Traffic Fatalities Related to Location – Sun and Abraham (2020)

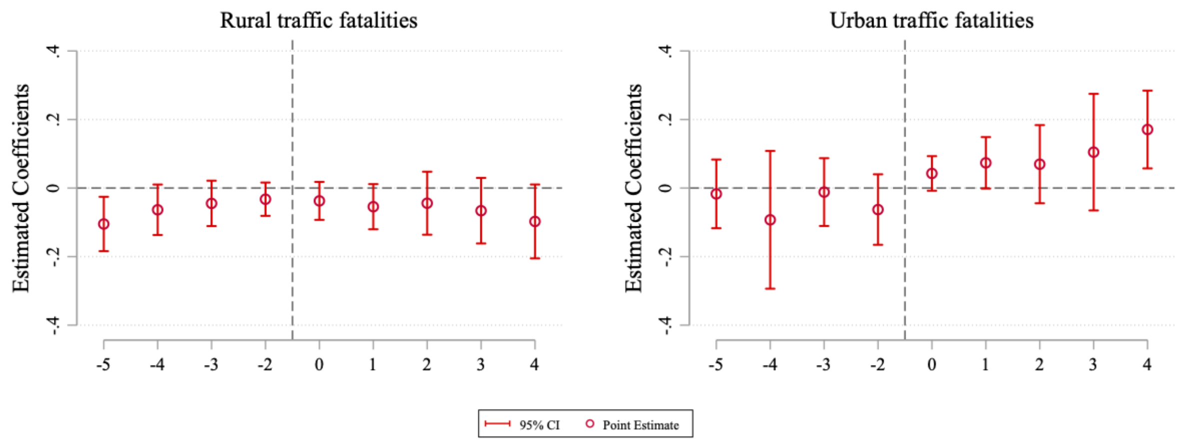


Figure 1.13

SYGs and Traffic Fatalities by Time – Sun and Abraham (2020)

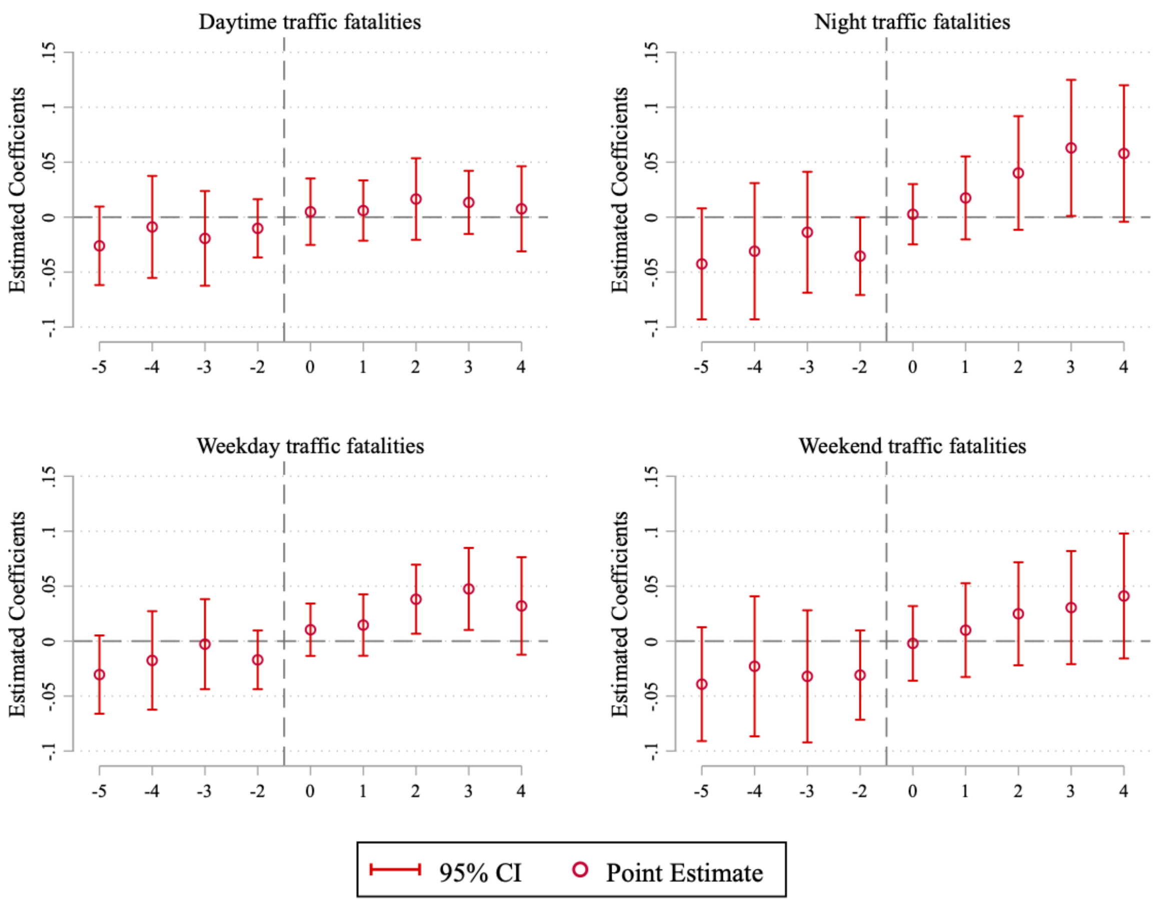


Figure 1.14

SYGs and Traffic Fatalities by Gender – Sun and Abraham (2020)

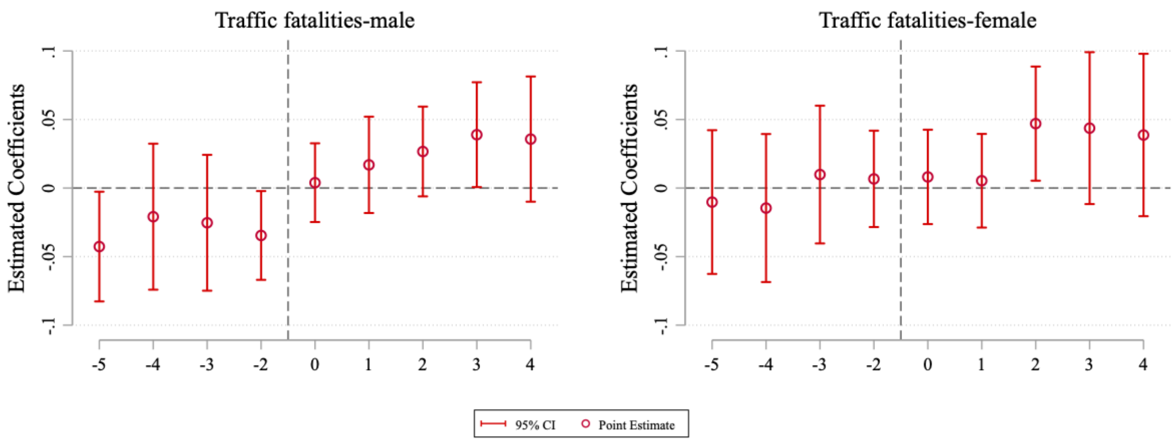


Figure 1.15

SYGs and Traffic Fatalities by Age – Sun and Abraham (2020)

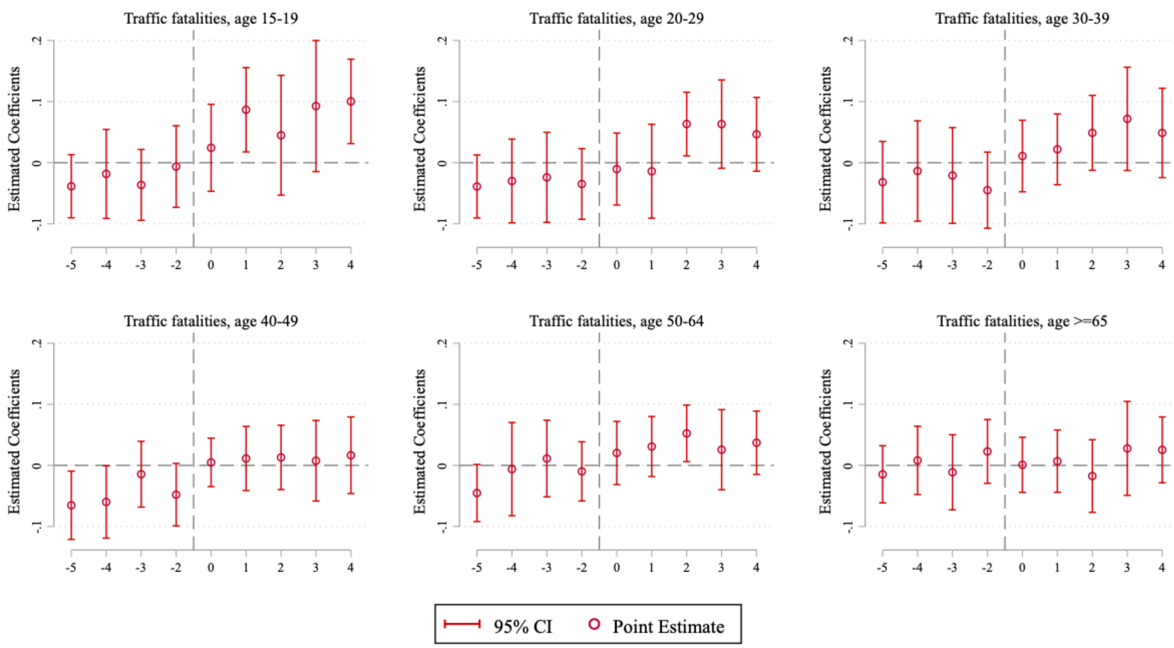


Figure 1.16

Estimated Coefficients of the Placebo SYP

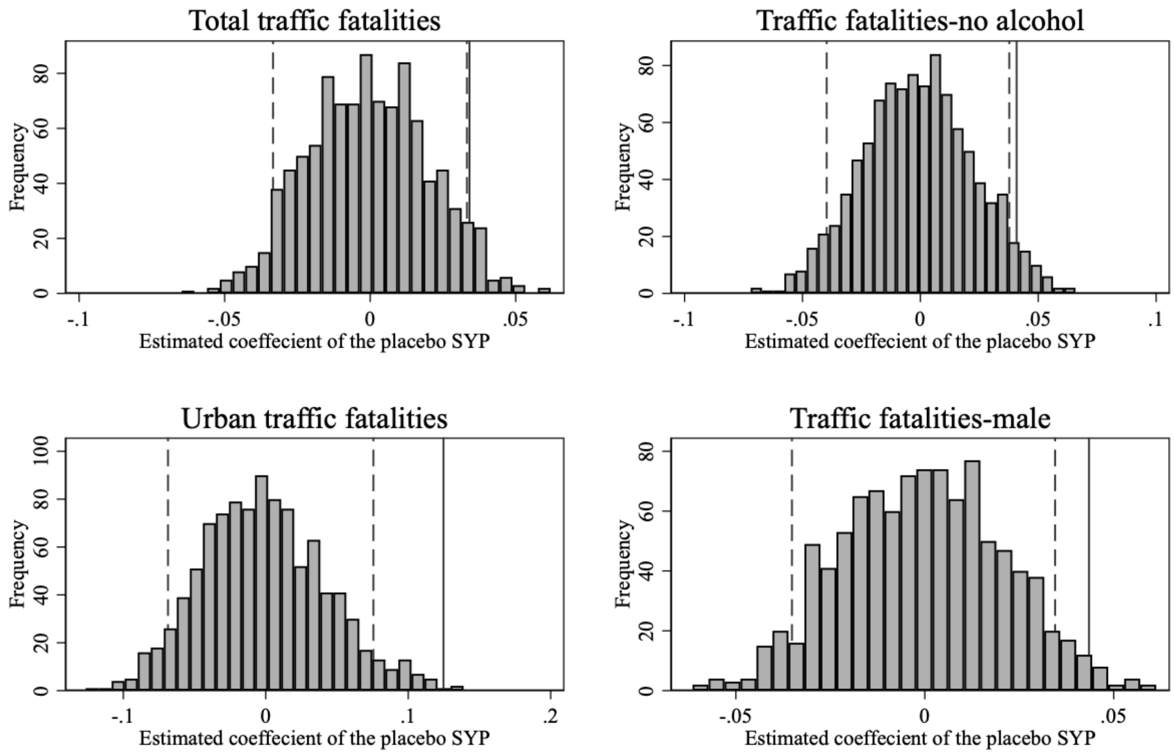


Figure 1.17

SYGs and Gun Ownership Proxies

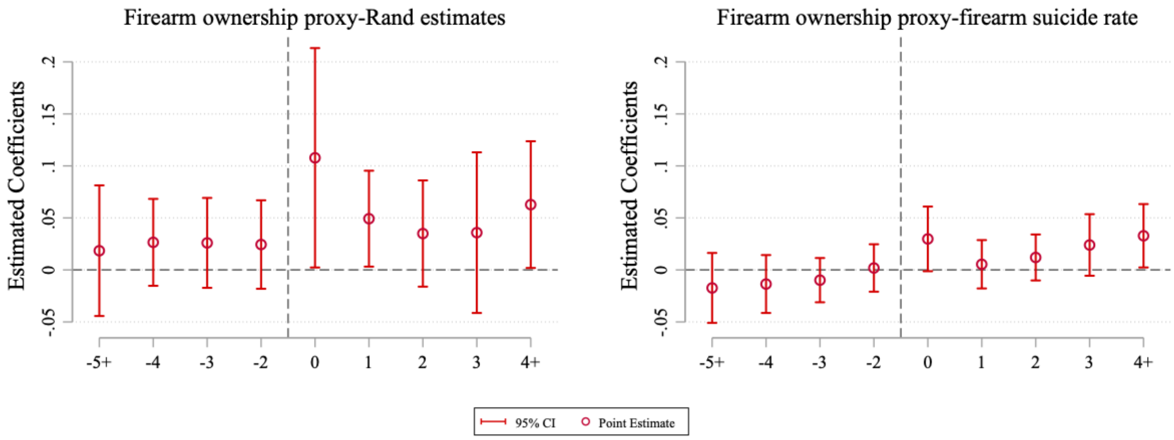


Figure 1.18

SYGs and Crime

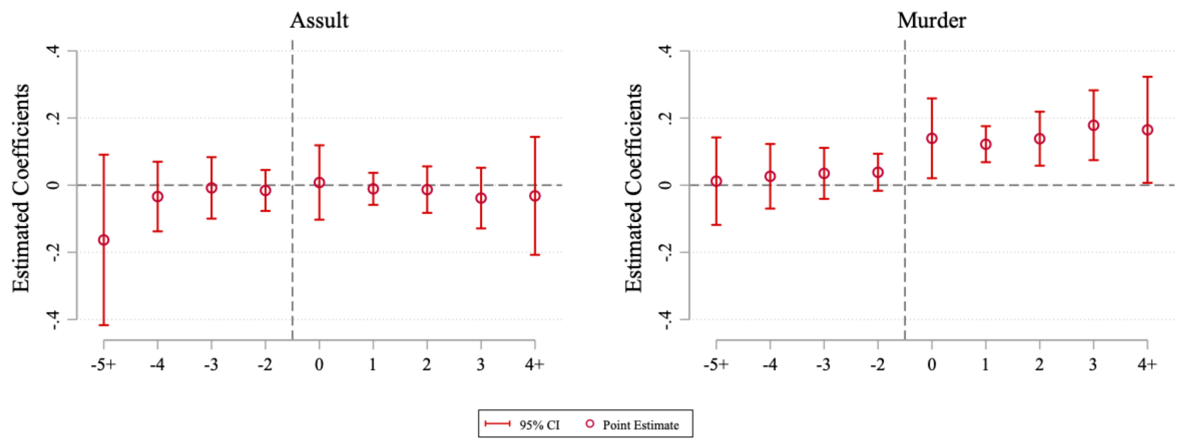


Figure 1.19

SYGs and Road Rage Crime

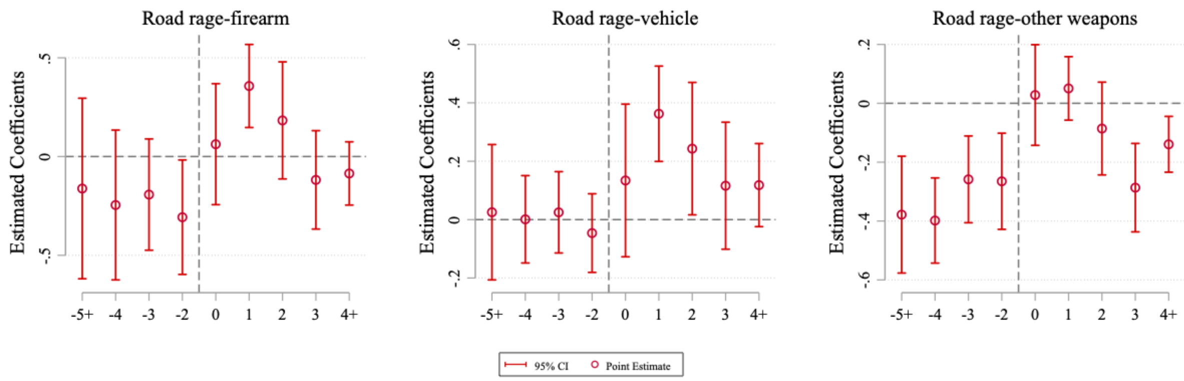
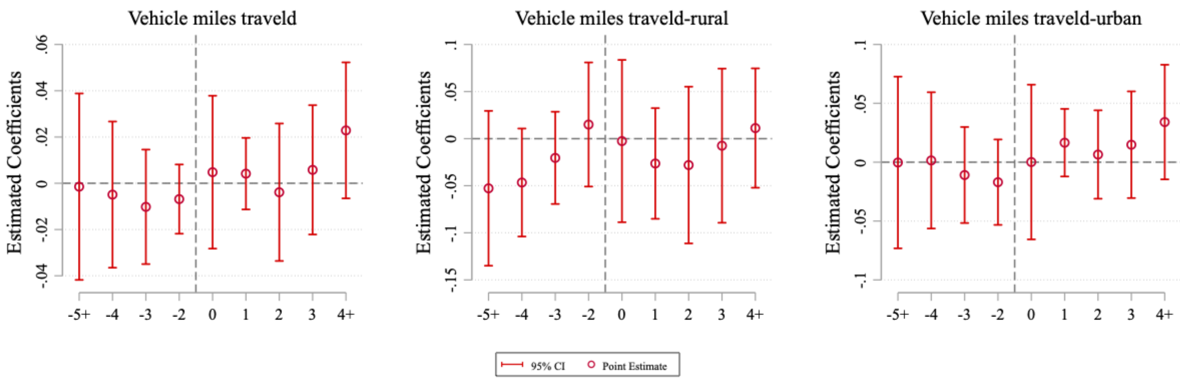


Figure 1.20

SYGs and VMT



Chapter 2 Campus Concealed Carry Laws and Higher Education Outcomes

2.1 Introduction

So far, eight states have implemented campus concealed carry laws (CCCs) allowing college students to carry guns on campus in a concealed way, and more states are considering introducing the legislation. However, the impact of the policy on university environments is still unclear in the current literature.

This study is interested in understanding the impacts of CCCs on higher education outcomes, including campus crimes, police employment, student enrollment, graduation rates, faculty attrition, and alumni donation. Using Campus Safety and Security (CSS) data, Uniform Crime Report (UCR) data, and difference-in-differences analysis, this study reveals that Campus Concealed Carry Laws can increase aggravated assaults by 34%. On average, the campus concealed carry law implementation raise police employment by 9%. Moreover, the current study finds evidence that the law decreases alumni donation and international student enrollment.

There are two novel features of the current study. First, this study highlights extensive original data collection work that provides comprehensive documentation of the campus carry laws. Especially, I obtain the exact effective dates and implementation information from 300 universities by searching the news and law databases, emailing the related officials, or directly submitting public information requests. Second, this paper examines the law’s impact on the full spectrum of higher education outcomes and provides the most comprehensive empirical analysis of the law. Overall, this paper addresses an important topic of university safety and wellbeing and joins emerging literature examining the effects of gun-free zones.

The remainder of the paper proceeds as follows: Section 2.2 presents background information for CCCs. Section 2.3 estimates the CCCs' effect on several measures of university outcomes, and section 2.4 concludes.

2.2 Background

2.2.1 History of campus concealed carry policies

The current study identifies that Brandt (2016) made several inaccurate or incomplete descriptions regarding the effective dates or specific provisions of state concealed carry policies.[26] To ensure the effective dates and provisions are correctly coded, I conduct an extensive legal search via Westlaw, HeinOnline, and Nexis Uni. Additionally, I obtained the CCCs' information for states with unclear law implementations by searching the related news, emailing the related state or university officials, or directly submitting public information requests to the universities.[27] Table 2.1 shows the effective dates of the CCCs. Table 2.2 and Table 2.3 show the summaries of each CCC law.

2.2.2 Literature on concealed carry laws

The impact of concealed carry laws (CCs) on overall crimes have been extensively studied, but there is still no agreement on whether there is crime increasing or crime decreasing. Part of the studies indicated CCs' crime deterrence effect (Bartley & Cohen, 1998; Lott & Whitley, 2003; Lott & Mustard, 1997; Lott, 2010). However, there is also evidence to support the positive

[26] Not only in Brandt (2016), inaccurate description can also be found from both gun advocate groups website (Student for Concealed Carry) and gun control website (Armed Campus).
[27] Emails were sent to university police chiefs, general counsels, student conduct offices, and athletic centers.

relationships between CCs and crime (Donohue et al., 2019; La Valle & Glover, 2011). For example, based on state panel data from 1977 to 2014, Donohue et al. (2019) Donohue et al. (2019) finds that CCs were correlated with higher violent crime rates.

2.2.3 Literature in campus concealed carry laws

Webster et al. (2016) summarized the existing CCs literature and concluded that CCs can increase overall violent crime. There were some specific university characteristics that might make university safety vulnerable to CCCs (Webster et al., 2016). However, some university features that Webster et al. (2016) had not addressed might contribute to the crime deterrence effect of CCCs. On the one hand, there were more safety measures on campus than in other places. On the other hand, the education level of the college students was higher than the overall country level.

Studies have provided insights on campus carry policies. Bouffard et al. (2012) surveyed the students in different school buildings of a public university in Texas. The survey asked them how likely they would apply for a CC license and carry the weapon on campus. The result showed that the effect of the CCC on the number of concealed carry on campuses varied among different school buildings. Thus, the potential impact of the law was still unknown. Bouffard et al. (2012) acknowledged that since the study just focused on “a single, rurally located university in Texas,” the result might not apply to universities in other states.

Gius (2019) studied the impact of CCCs on campus crime based on the data covering the period of 2005 to 2014. The study aggregated the individual university crime data to the state level. It claimed that most university crime data were very small, and it was reasonable to aggregate. In addition, it also claimed that state-level data are compatible with control variables that can only be observed at the state level. The current study does not agree with the simple aggregation of all universities. First, different universities in the same state have intrinsic differences, regarding to size, urbanization, academic status, and demographic composition. Aggregation ignores those differences and brings potential misleading results. Second, in Gius (2019)'s sample period, the law in some states only applies to public colleges and universities. In Colorado, though it had a policy prior to 2008, most universities chose not to follow the law until 2012. Gius (2019) fails to address these issues.

2.2.3 Literature in university outcomes

The current study contributes to studies on campus crime (Aiello, 2020; A. N. Allen, 2017; W. D. Allen, 2013, 2018; Anderson et al., 2019; Arrigo & Acheson, 2016; Barr, 2017; Bartula & Bowen, 2015; Beggan, 2019; Brands et al., 2015; Burdick-Will, 2013; Cannonier et al., 2019; Cavanaugh et al., 2012; Coker et al., 2017; Cramer, 2014; Cunningham et al., 2019; Hassett et al., 2020; Heaton et al., 2016; Heywood & Weber, 2019; Hyclak, 2011; Iheadindu et al., 2019; Lindo et al., 2018), studies on university finance (Alexander & Kern, 2009; Bound et al., 2020; Cai & Heathcote, 2018; Delaney & Kearney, 2015; García-Estévez & Duch-Brown, 2012; Havranek et al., 2018; Lafortune et al., 2018; Webber & Ehrenberg, 2010), and studies on Alumni donations (Lindo et al., 2019; Rooney & Smith, 2019).

2.3 Data

The sample contains 4-year public universities and non-profit private universities with a 2018 enrollment larger than 10,000. The universities with huge online enrollment are dropped. The data analysis of the current study is based on Campus Safety and Security (CSS) data, the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (UCR), Integrated Postsecondary Education Data System (IPEDS), and the Council for Aid to Education's Voluntary Support of Education Survey (VSES). CSS contains the crime data under the category defined by UCR. It also contains enrollment, demography, and academic and financial status information. According to Clery Act, every university has to report its crime data to CSS each year. VSE provides records for U.S university alumni giving.

Both CSS and UCR have campus crime data. The data are different, and CSS crime data is more suitable for this analysis. There are two reasons. First, CSS data are more accurate. UCR data are voluntarily reported by police agencies, while CSS data reporting is mandatory for universities. Second, CSS has detailed location information for campus crime and thus could add more variation in the data. The crime data for 300 universities in 46 states are collected (excluding Oregon, Tennessee, Mississippi, and Wisconsin) from CSS.UCR contains police employment data for university law enforcement agencies. The police employment data covers 181 universities.

2.4 Method

Campus crime rates are often very low or even zero. For the analysis related to campus crimes, the study uses Poisson models and event study design. Poisson model is not subjected to inconsistency caused by the incidental parameter problems.

$$E[\,Y_{it}\,|(\mathrm{CCC}, v_s, w_t)] = \exp\,(\sum_{b=-8;b\neq-1;b\neq-7}^{1} \delta_b CCC_{s,t+b} + v_s + w_t)$$

Here, Y_{it} represents the crime count for university s at time t, w_t represent full set of time fixed effects, v_s represent full set of university fixed effects, CCC_{st} is a dummy variable, indicating when the University has CCC. The study only controls for university fixed effect and time fixed effect. Borusyak et al. (2022) suggested that an event study needed to drop two lag variables to identify the linear trend. So current analysis dropped the lag variable for seven years before implementation and one year before implementation. For outcomes other than campus crime, OLS event studies are applied. Some analyses use two lag variables while others use one lag variable. Because for some outcomes like police employment, data are available until 2019, while other outcomes like campus crime, the data are only available until 2018. Since most CCCs were carried out by the state, the standard errors are clustered at the state level.

2.5 Result

2.5.1 CCC's impact on campus crime

Panel A in Figure 2.1 shows that MMLs are associated with a positive but insignificant increase in robbery immediately after the passage of the laws. Panel B of Figure 2.1 suggests CCCs increase aggravated assault by 34% in the first year of legalization. Figure 2.2 displays that none of the estimates for arson, burglary, motor vehicle theft, or rape is statistically significant.

2.5.2 CCC's impact on campus police employment.

The impacts of CCCs on campus police employment are represented in Panel A and Panel B of Figure 2.3. Both police officers with arrest power and police civilians increase following the implementation of CCC. Specifically, CCCs is associated with 9% increases in police officer employment. However, the pre-trend in Panel B may indicate the estimates for police civilian employment are invalid. After CCC, the University of Kansas hired more safety officers (Williams, 2020). This result reflects that universities are concerned about the potential negative effect of the policies.

2.5.3 CCC's impact on universities outcomes

Panel A and Panel B in Figure 2.4 show that CCCs could not affect either male or female college applications. The result is consistent with Carrico (2016), which shows that campus weapon policy does not change the students' university choice. Figure 2.5 displays the impact of CCCs on student enrollments and faculty employment. Panel A and Panel B of Figure 2.6 find that CCCs increase the completion rates by 5 percent for the female groups. Panel C and Panel D show a 20% drop in international student enrollment and an 8% increase in instructional staff. No evidence suggests a trend of faculty leave after the implementation of CCCs.

2.5.4 CCC's impact on alumni donations.

Panel A in Figure 2.7 suggests that the percentage giving (number of donors divided by the number of alumni) drop by 20%. Figure 2.8 shows no evidence that total giving and the percent of alumni solicited would be affected.

2.6 Conclusion

The impact of RTC laws on the crime rate in counties and states has been extensively studied. However, only two studies address the impact of RTC laws on campus outcomes based on campus-level data, and no study analysis potential broader effect of CCCs. Using Poisson models, this paper finds that the impacts of campus carry laws on aggravated assaults, instructional staff employment, and police employment are all statistically positive. The impacts on alumni donation and international student enrollment are negative.

Donohue et al. (2019) studied CCs and proposed five mechanisms of violent crime increasing: (1) more crime committed by concealed carry permit holders; (2) more gun loss and theft; (3) emerging culture of violence; (4) harsher violence in response to the people who may carry guns; (5) lower police efficiency on the fear of possible gun violence. For colleges and universities, the third mechanism may apply. The law's passage can nurture the culture of violence on campus and make the potential criminals more active.

In response to the campus carry laws, university police choose to increase police employment. After implementing the law, the University of Kansas (KU) public safety hired three additional police officers and three security officers (Williams, 2020). In general, more police should deter crime. My results show that despite the increase in police, crime still increased.

At the University of Kansas, the passage of the campus concealed carry law raises concerns among students and faculties. Some faculties have to take action to protect themselves and their students from the potential gun violence. Due to concealed carry policy, Rtic Rath, a professor of history, move his class online (Hoover, 2018). Kevin Willmott, a professor of film, wore a bulletproof vest while teaching (Lodos, 2017). In Kansas, at least two professors resigned due to the concern about CCCs (Dorman, 2017; Tobias, 2017). However, the current study does not find an increase in job attrition.

There are a few limitations in this study. First, both CSS and UCR data sets only show the crime reported to the police. Crime such as sexual assault could be largely underreported (Fisher et al., 2010). Future studies can analyze the data from victimization surveys. Second, for some universities, the number of sexual assault cases reported could raise by half during the audit period (Yung, 2015). Third, the implementation of the Violence against Women Act (2013) could largely affect violent crime cases. Gavran (2017) observes a hike in total violent crime reported in 2014, the year after adopting the Violence against Women Act. Future studies may address these limitations.

2.7 Tables and Figures

Table 2.1

CCCs, 2004-2019

State	Effective Date
Arkansas	9/1/17
Colorado	3/5/12
Georgia	7/1/17
Idaho	7/1/14
Kansas	7/1/17
Tennessee	7/1/16
Texas	8/1/16
Utah	9/8/06

Table 2.2

CCCs - People Who Can Carry on Campus

State	Enhanced CCP holder who is campus visitor	Enhanced CCP holder who is student or faculty	Reciprocal out-of-state CCP holder / enhanced CCP holder who is campus visitor	CCP holder who is campus visitor	CCP holder who is fulltime employee	CCP holder who is student	People without CCP who can legally process a firearm
Arkansas	Yes	Yes	No	No	No	No	No
Colorado	NA	NA	Yes	Yes	Yes	Yes	No
Georgia	NA	NA	Yes	Yes	Yes	Yes	No
Idaho	Yes	Yes	No	No	No	No	No
Kansas	NA	NA	Yes	Yes	Yes	Yes	Yes
Tennessee	NA	NA	No	No	Yes	No	No
Texas	NA	NA	Yes	Yes	Yes	Yes	No
Utah	NA	NA	Yes	Yes	Yes	Yes	No

Table 2.3

CCCs - Places That Campus Concealed Carry is Allowed

State	Campus ground	University vehicle	Faculty office	Student dormitory or residence hall	Classroom	Disciplinary Hearings	Athletic sporting events	Public daycare facility	Laboratories
Arkansas	Yes	Yes	Yes	Yes	Yes	No	No	No	Yes
Colorado	Yes	Yes	Yes	No	Yes	Yes	Yes/No	Yes	Yes
Georgia	Yes	Yes	No	No	Yes	No	No	No	Yes
Idaho	Yes	Yes	Yes	No	Yes	Yes	Yes/No	Yes	Yes
Kansas	Yes	Yes	Yes	Yes	Yes	Yes	Yes/No	Yes/No	Yes
Tennessee	Yes	Yes/No	Yes	Yes	Yes	No	No	No	Yes
Texas	Yes	Yes	Yes/No	No8	Yes	No	No	Yes	Yes
Utah	Yes	Yes	Yes	No	Yes	Yes	No	Yes	Yes

Figure 2.1

Effects of CCCs on Robbery and Aggravated Assault

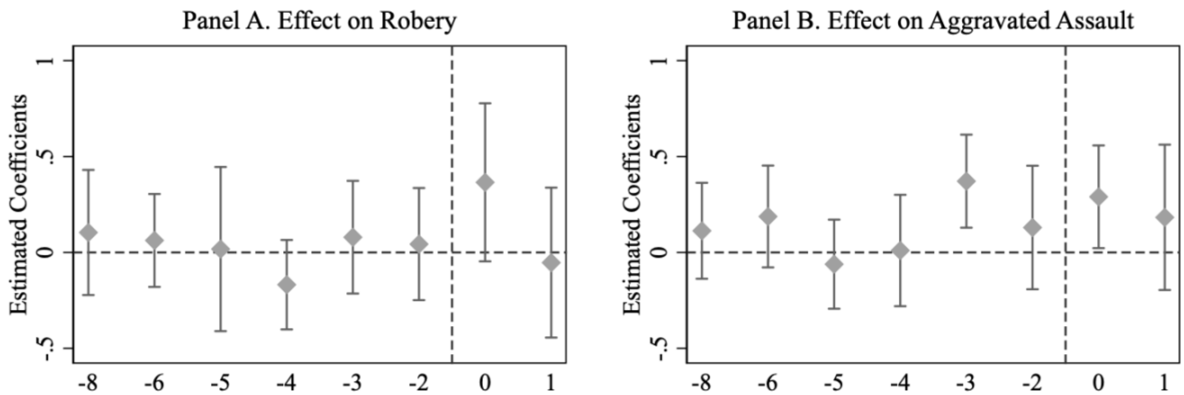


Figure 2.2

Effects of CCCs on Property Crimes and Rape

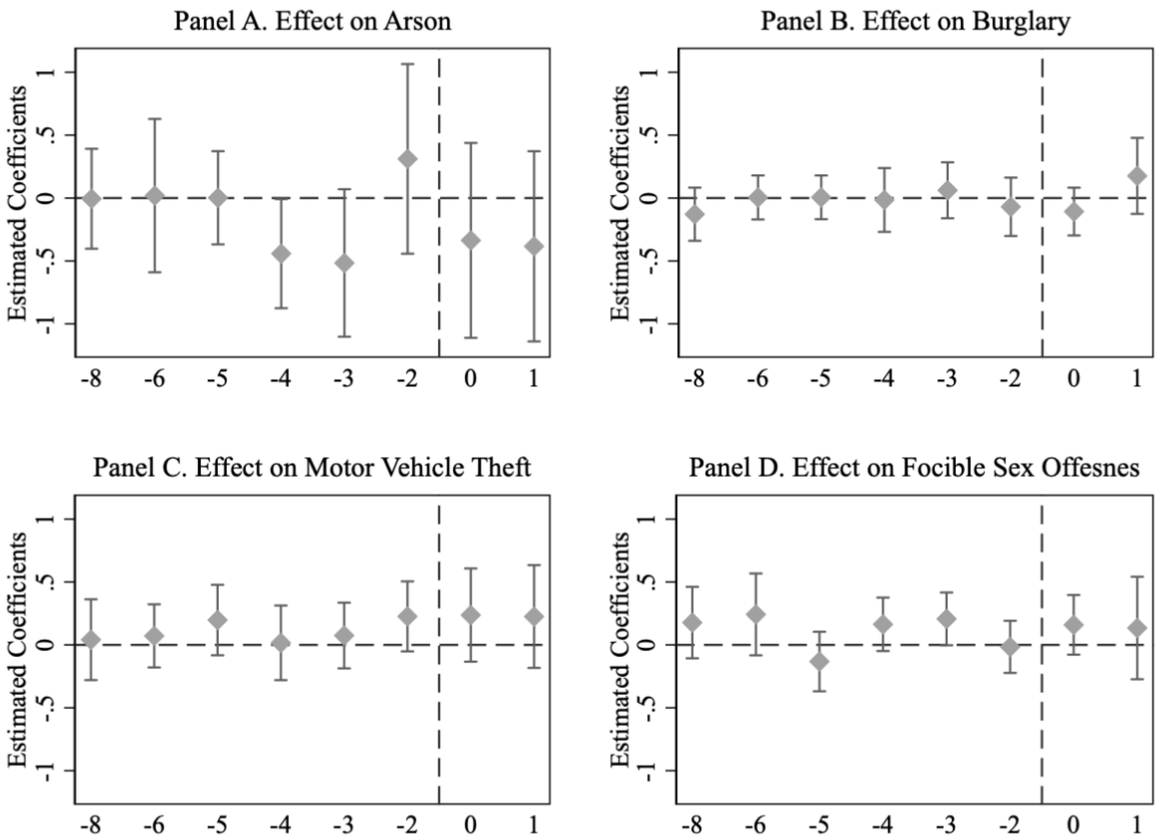


Figure 2.3
Effects of CCCs on Police Employment

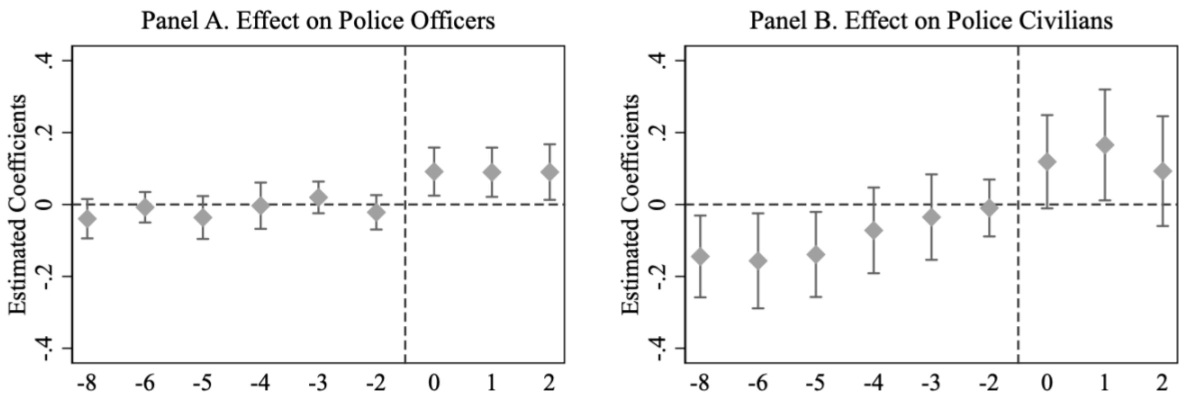


Figure 2.4

Effects of CCCs on College Applications

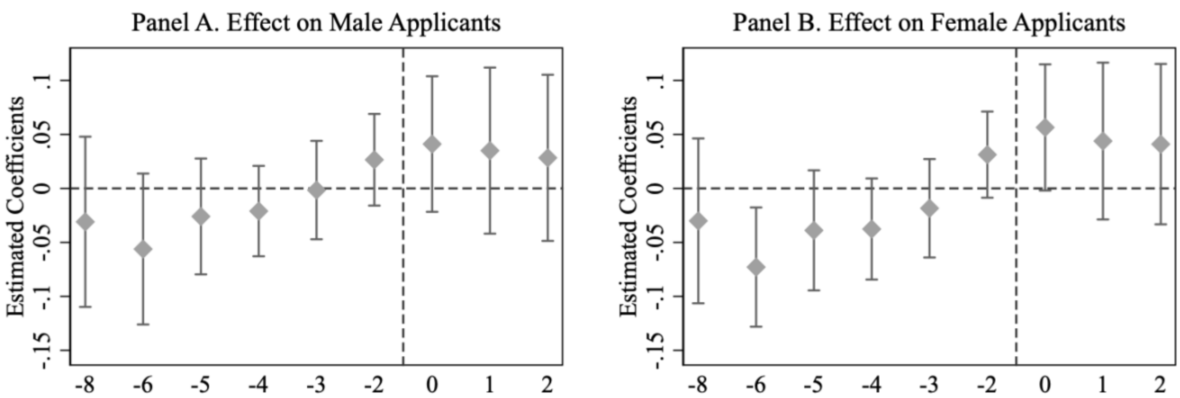


Figure 2.5

Effects of CCCs on Student Enrollment and Faculty Employment

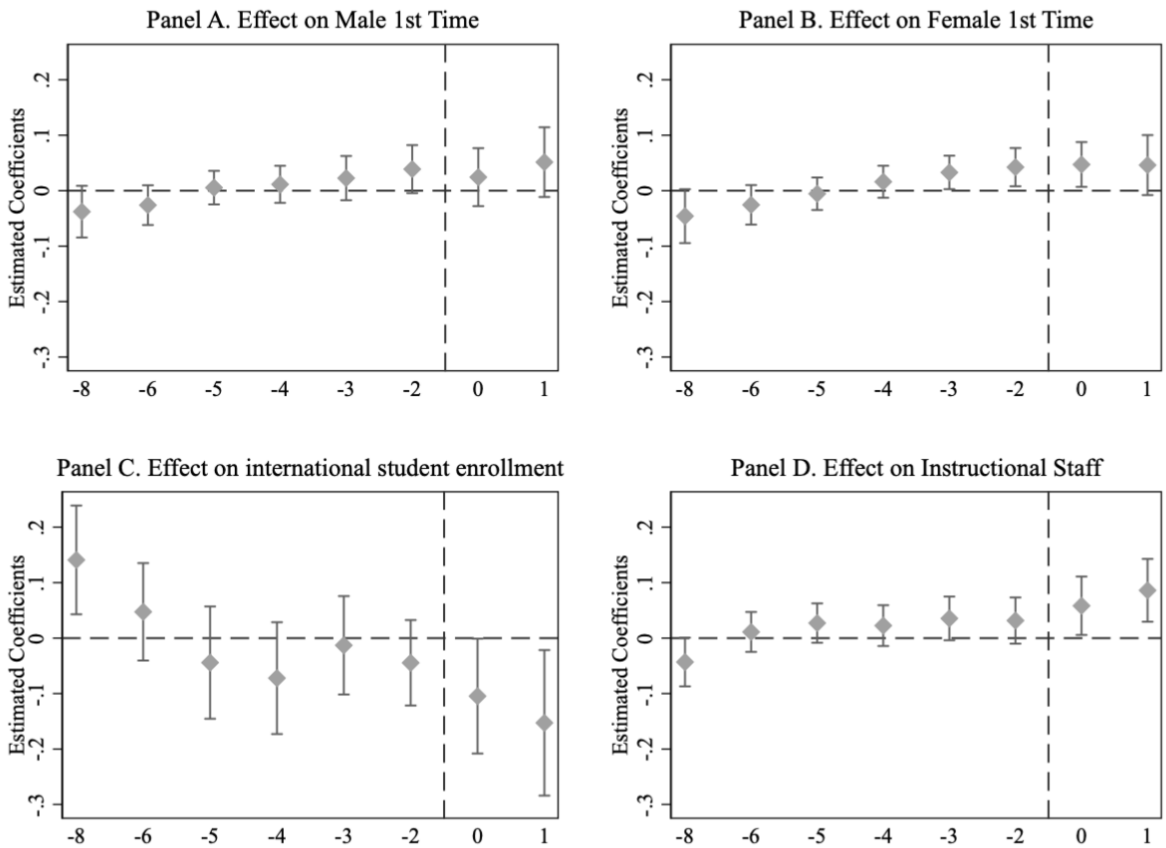


Figure 2.6

Effects of CCCs on Completion Rates

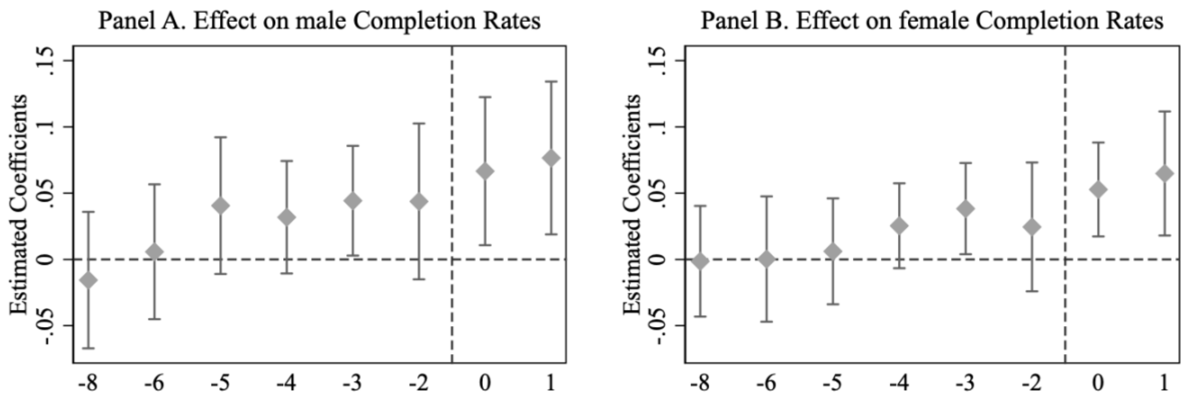


Figure 2.7

Effects of CCCs on Percent Giving and Ratio of Donors to Solicited Alumni

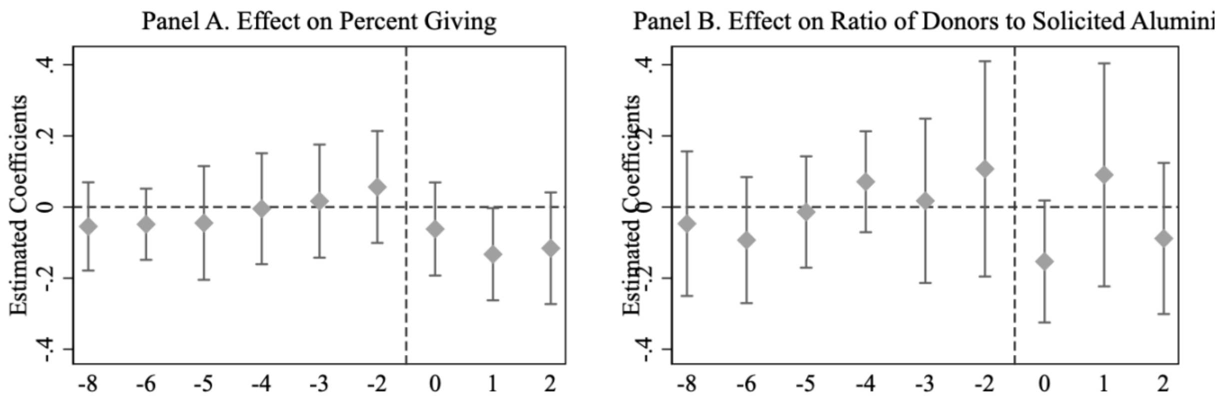
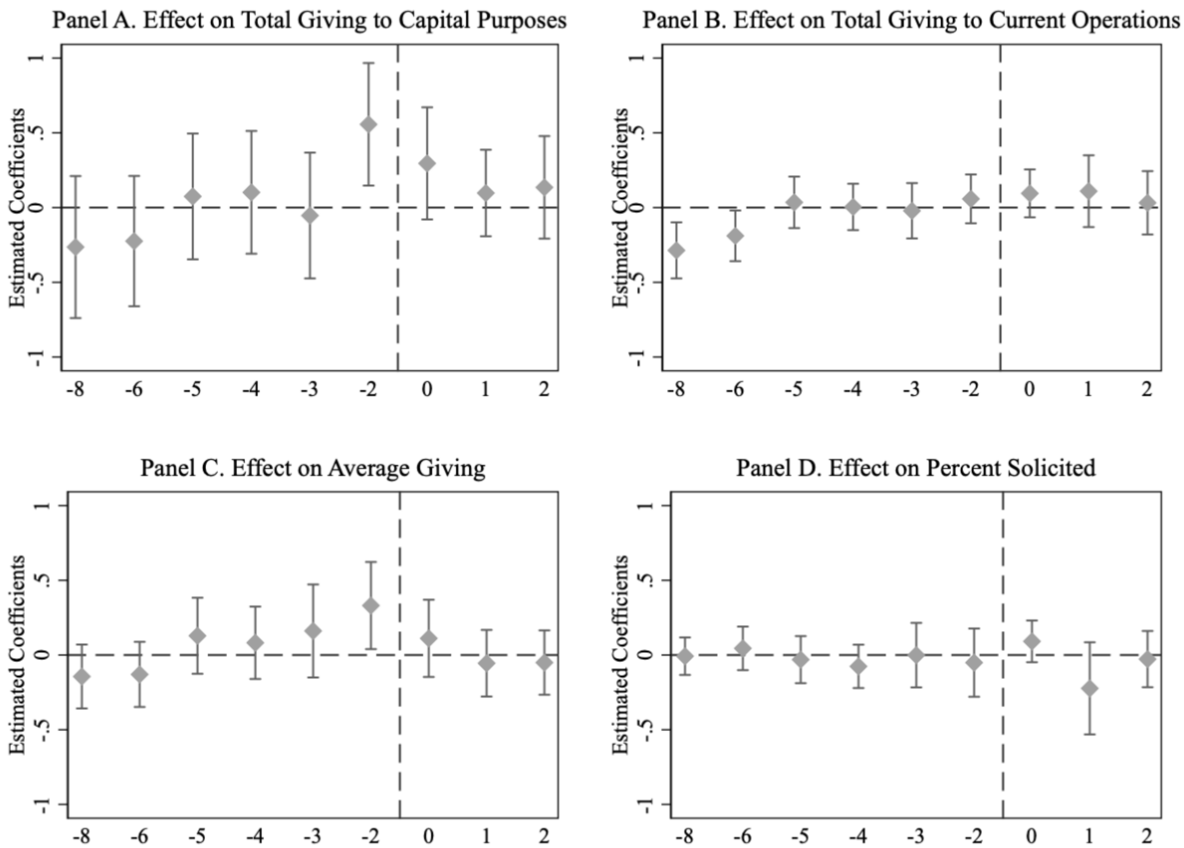


Figure 2.8

CCCs, Alumni Giving, and Donation Solicited



Chapter 3 Medical Marijuana Laws and Youth Crime

3.1 Introduction

Though marijuana remains an illegal substance at the federal legislation level, 37 states have legalized its medical or recreational use over the past two decades (National Conference of State Legislatures, 2022). Medical Marijuana Laws (MMLs), which legalize the medical use of marijuana, have been the subject of a heated debate considering their potential impact on various public health issues. According to National Institutes of Health (2012), marijuana contains chemicals that can potentially affect the human body's nervous, cardiovascular, respiratory, digestive, and reproductive systems. On the one hand, marijuana use may be associated with heart, lung, mental health, and reproductive problems. On the other hand, the medical use of marijuana is promising in relieving health issues such as nausea, pain, epilepsy, and Crohn's disease (National Institutes of Health, 2020; Mayo Clinic, 2021). Furthermore, the drug effect of marijuana on human body health may indirectly impact other public health outcomes, such as traffic fatalities, crimes, suicide, labor market outcomes, and educational outcomes (Anderson et al., 2013, 2018; Anderson & Rees, 2021; Chu & Townsend, 2019a; Plunk et al., 2016; Sabia & Nguyen, 2018; Ullman, 2017).

One of the major areas of MMLs studies focuses on the laws' impact on crime.[28] However, the existing MMLs and crime literature mainly emphasize policy impacts on the overall crime but draw little attention to youth crime. Multiple mechanisms may exist to explain how MMLs affect youth crime: MMLs may (a) increase marijuana accessibility for the youth, (b) establish medical

[28] For example, see Chu & Townsend (2019), Huber III et al. (2016) and Morris et al. (2014).

marijuana dispensaries with a high stock of crime-attracting marijuana and cash, (c) move police focus on combating marijuana crime to non-marijuana law enforcement, and (d) alter parental influence on youth behavior.

MMLs could potentially expand the youth's access to marijuana and affect crime.[29] First, marijuana might be more harmful to juveniles than adults regarding the impact on brain development, and the affected brain development may cause violent behaviors (Dellazizzo et al., 2020). Second, marijuana may serve as a substitute for alcohol, and thus, marijuana liberalization can deter youth crime related to alcohol abuse (Caulkins et al., 2016; Mark Anderson et al., 2013; Matthay et al., 2021). Third, according to the gateway drug hypothesis, marijuana consumption could be the first step toward using other "harder" drugs, such as cocaine or heroin (National Institute on Drug Abuse, 2020; Secades-Villa et al., 2015). The "harder" drugs, which are more addictive, may lead to more crime. Last, the expansion of marijuana access could deter the crimes related to the marijuana black market (Anderson & Rees, 2021; Chu & Townsend, 2019a).

Since marijuana is illegal at the federal level and financial services are subject to federal regulations, most marijuana dispensaries operate as cash-only (Hill, 2015). Marijuana consumers and dispensaries could be high-value targets for crime activities for cash and marijuana, such as robbery and theft. (Anderson & Rees, 2021; Morris et al., 2014). Nevertheless, the crime-attracting features of the marijuana business entail heavy security measures in the store or nearby areas,

[29] According to Insurance Institute for Highway Safety (2022), 23 out of 35 MMLs states allow youth medical marijuana use. But even in MMLs states that prohibited youth medical marijuana use, the youth access to marijuana likely increases as a result of the legal marijuana market's expansion (Smart, 2015).

which could decrease crimes (Chu & Townsend, 2019a). Besides, marijuana dispensaries may deter crime by attracting marijuana consumers from “dark alley” to safer places with “eyes upon the street” (Anderson & Rees, 2021; Chang & Jacobson, 2017; Jacobs, 1961).

Police efforts in marijuana crime reduction freed from MMLs could be used for non-marijuana law enforcement (Adda et al., 2014; Anderson & Rees, 2021; Chu & Townsend, 2019a; Stohr et al., 2020). Therefore, crimes such as violent crime, property crime, and “harder drug” crime are likely to drop, and the related arrests may increase. However, the marijuana crime, especially youth marijuana crime, may increase because of the reallocation of police resources.

Parent medical marijuana use could possibly affect youth criminal behavior. The presence of marijuana at home may alter youth perception towards drugs, engage youth in future drug use, and subsequently deter or increase crime (Broman, 2016). Besides, as marijuana may substitute alcohol use, a common factor in domestic violence, there could be less domestic violence at home and thus less youth crime (Kaplan & Goh, 2022; Rebellon & Van Gundy, 2005).

Given the unclear mechanisms, the current study empirically examines the impact of MMLs on youth crime by using the Uniform Crime Reporting (UCR) Program arrest data and difference-in-differences methods. Specifically, this study focuses on violent crime, property crime, drug crime, and the related sub-category crimes by different young age groups and gender groups. This study starts with a simple “traditional” difference-in-differences event study model focusing on police arrests for 42 states from 1999 through 2018. It concludes that MMLs increase certain age category youth violent crime, property crime, and drug crime by around 10-20%.

Considering the validity of the “traditional” difference-in-differences setting with staggered treatment timing and heterogeneous treatment effect, the primary analysis of the current study implements a stacked event study model following Cengiz et al. (2019), Deshpande & Li (2019), and Ruhm & Mathur (2022). For the model implementation, the subsequent analyses tailor the sample to adolescent arrests in 28 states from 2003 to 2018. The results show that the violent crime in the age group 16-17 years old and 18-20 years old increase by around 15%. However, no significant effect of MMLs on overall youth drug crime or property crime was detected. The current study also investigates MMLs’ impact on sub-category crime of violent crime, property crime, and drug crime.

The current study performs robustness checks by investigating the MMLs impact on university crime. Using U.S. Department of Education’s Campus Safety and Security (CSS) data and a stacked event study model, the result shows that MMLs increase campus violent crime by 25% in the first year of implementation, but the result finds no evidence of campus property crime increasing or decreasing. Additionally, the results from the main analysis are robust to sensitivity checks by progressively adding covariates and applying different types of clustering.

In terms of the contribution, this paper is the first to empirically study the impact of MMLs on youth crime and find significant increases in youth violent crime and drug crimes by using both “traditional” and stacked difference-in-differences event study methods. Furthermore, unlike previous empirical MMLs and crime literature focusing on UCR offense data, this paper provides analyses of UCR arrest data to estimate the related crime incidences (Chu & Townsend, 2019a; Gavrilova et al., 2019; Huber III et al., 2016; Morris et al., 2014).

The remainder of the paper proceeds as follows: Section 3.2 presents background information for MMLs. Section 3.3 describes the data. Section 3.4 explains the empirical methods. Section 3.5 estimates the MMLs' effect on multiple measures of youth crime. Section 3.6 provides robustness checks and sensitivity checks for the main findings. Section 3.7 concludes.

3.2 Backgrounds

3.2.1 MMLs

Table 3.1 shows the effective dates and applicable age groups for MMLs in 35 states during 1996-2019. Notably, California was the first to legalize MMLs in 1996. The increase in the number of MMLs states has been relatively stable across the years. There are only 6 out of 24 years with no MML implementation, and the highest number of new MML implementations in a year is 4 for 2016. For laws' affected age groups, 8, 1, and 3 states' MMLs apply to people older than 18, 19, and 21 years old, respectively. Twenty-three states' MMLs have no age limit.

3.2.2 Literature on MMLs and Youth Marijuana Use

When MMLs with different provisions are considered similar laws, which is common in MMLs literature, the evidence of MMLs' impact on youth marijuana use is mixed. Most related literature did not detect any significant changes in youth marijuana use (Anderson & Rees, 2021; Sarvet et al., 2018; Anderson et al., 2015, 2021; Choo et al., 2014; Coley et al., 2021; Dills et al., 2017; Pacula et al., 2015; Wen et al., 2015). For instance, Anderson et al. (2021) used Youth Risk

Behavior Survey (YRBS) data from 1993 to 2017 and difference-in-differences models to investigate the marijuana use among high school students and found insignificant impacts on "current or frequent marijuana use." In addition to the literature with insignificant results, Coley et al. (2019), Harper et al. (2012), and Johnson et al. (2017, 2021) identified negative relationships between MMLs and youth marijuana use, while Hollingsworth et al. (2020) supported a positive relationship (Anderson & Rees, 2021; Hollingsworth et al., 2020).[30]

When MMLs are considered heterogeneous, most related studies found the direction of the relationships varies by the restrictiveness of the laws. Pacula et al. (2015) found MMLs requiring patient registry systems were associated with about a 9% increase in marijuana use in the past month, while MMLs allowing home cultivation were associated with a 4% decrease in the same measure. Using YRBS data during 1991-2011 and logistic regression, Johnson et al. (2017) distinguished MMLs by different provisions and focused on the laws' impact on past-month "use and heavy marijuana use." Johnson et al. (2017) categorized permissive provisions of MMLs as "(a) Dispensaries for profit OR dispensaries active in 2011, (b) Home cultivation ≥10 plants allowed, (c) Possession: ≥2.5usable ounces allowed, (d) Caregivers allowed 5 patients, and (e) Patient registry voluntary." The level of permissibility as measured by the number of permissible provisions is negatively related to the odds of youth past-month marijuana use, whereas MMLs with voluntarily registration provisions increase the odds of youth past-month marijuana use (Johnson et al., 2017). In addition, the higher the possession limit in an MML state, the higher the odds of youth past-month marijuana use (Johnson et al., 2017). Based on states MMLs provisions,

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[30] Hollingsworth et al. (2020) identified a positive relationship between MMLs and past-month teen marijuana use, but found an insignificant relationship between MMLs and past-year teen marijuana use.

Neeley & Richardson (2022) created three ordinary variables to indicate the levels of laws' effort to promote (a) pharmaceutical use, (b) recreational use, and (c) state fiscal revenue. The evidence suggests more provisions to encourage pharmaceutical marijuana use is associated with lower youth marijuana use; however, more provisions to promote recreational use or fiscal revenue is positively related to youth marijuana use (Neeley & Richardson, 2022).

3.2.2 Literature on MMLs and Crime

A meta-analysis conducted by Anderson & Rees (2021) identified four papers investigating the impact of MMLs on crime (Chu & Townsend, 2019a; Gavrilova et al., 2019; Huber III et al., 2016; Morris et al., 2014). Using Uniform Crime Reporting offense data and difference-in-differences methodology, all these papers did not find positive relationships between MMLs and crimes (Anderson & Rees, 2021).

To my knowledge, there is no paper investigating the relationship between MMLs and youth crime. Considering papers investigating other marijuana legalizations' impact on youth crime, Plunk et al. (2016) used UCR arrest data aggregated by state and year for 2000-2016 and the difference-in-differences method to investigate the association between marijuana decriminalization, recreational marijuana laws, and youth arrests for cannabis possession. Plunk et al. (2016) only identified that marijuana decriminalization decreased the arrest rate for youth marijuana possession. There are several limitations associated with this study. First, the study aggregated the arrest counts for all agencies within each state. According to Kaplan (2021), about half of the agencies failed to report their arrest statistics for each month in a given year. Thus, aggregating all the reporting agencies may create too much noise for the analysis. Furthermore, the number of agencies reported for all 12 months varies across years for each state, which may necessitate the inclusion of either the number of reporting agencies or the total population covered by the reporting agency for each year to the specifications. Second, Plunk et al. (2016) only coded states that decriminalized marijuana after the 2000s, and missed that 11 states decriminalized marijuana during the 1970s, which should be coded as "always-treated" states in the analysis or completely dropped from the analysis considering the "always-treated" states failed to provide useful information for the pre-treatment periods (Baker, 2022; de Chaisemartin & D'Haultfoeuille, 2022; Kim & Lee, 2019; Marcus & Sant'Anna, 2021).

3.3 Data

3.3.1 UCR Arrests Data

The data for the major analysis of the current study is from the FBI Uniform Crime Reporting (UCR) Program's Arrests by Age, Sex and Race dataset, also known as the "arrests data" (Kaplan, 2021). Previous MMLs and Crime literature mainly use the UCR Program's Offenses Known and Clearances by Arrest dataset, described in Kaplan (2021) as the "crime dataset." The "crime dataset" covered the monthly agency-level counts of crime incidences reported to the police, and those cleared by the police.[31] By contrast, the "arrests data" had detailed age-group-level and gender-level counts of arrestees. To investigate the policy impacts on youth crime, crime literature mainly relies on the "arrest data', which has the age and gender information of the arrestees (Anderson, 2014; Corman et al., 2017; Gunadi & Shi, 2022).

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[31] According to Kaplan (2021), "A crime is cleared when an offender is arrested or when the case is considered cleared by exceptional means."

Based on the data available in the UCR "arrest dataset," I constructed several groups of youth outcome variables, including violent crime, male violent crime, index violent crime, simple assault, property crime, drug abuse violation, drug sale or manufacturing violation, drug possession violation, marijuana abuse violation, and non-marijuana abuse violation. Among them, violent crime consists of index violent crime and simple assault, and index violent crimes include murder, rape, robbery, and aggravated assault.[32] Drug abuse violation can be divided into drug sale or manufacturing violation and drug possession violation. At the same time, it can also be divided into marijuana abuse violation and non-marijuana abuse violation.

Though the UCR "arrests data" provides the most comprehensive administrative arrest records, it is subject to several drawbacks, including missing data, underreporting, and overreporting (Loftin & McDowall, 2010). To circumvent these issues, following Corman et al. (2017), the current study only focuses on "agencies that cover at least 50,000 individuals and reported arrests for all 12 months of the year."[33]

The current study models the arrests data for "traditional" difference-in-differences analysis and staggered difference-in-differences analysis. For the "traditional" differences-in-differences, this study further aggregated the large agencies' full-reported "arrests data" into state-year level data covering 42 states from 1999 to 2018. The descriptive statistics for the major outcomes can be found in Table 3.2. The first column indicates the major outcome variables in the analysis. The

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[32] Kaplan (2021) has a detailed discussion about the importance of including simple assault as part of violent crime for crime literature.
[33] According to Maltz (1999, 2006), compared with small agencies, statistics from agencies covering large population and are better supported by statistical technology and are often subject to more auditing from the FBI.

second column lists the mean followed by the standard deviation in the parentheses for the related variables. The third and fourth columns show the mean and standard deviation for the associated variables within MMLs states and control states, respectively. The number of observations for the analysis is listed in the last column. By contrast, to perform the stacked difference-in-differences analysis, the current study trimmed the previously aggregated data into a dataset covering 28 states from 2003 to 2018. The descriptive statistics for the major outcomes in the resulted dataset can be found in Table 3.3. [34]

3.3.2 CSS Data

To support the UCR analysis of MMLs and youth crime, the current study uses the U.S. Department of Education's Campus Safety and Security (CSS) data to examine the impact of MMLs on university crime. The CSS data contains university-year-level counts for four major categories of crimes, including criminal offenses, hate crimes, VAWA offenses, and arrests and referral for disciplinary action. Unlike police agencies that can voluntarily report their data to UCR, universities participating in HEA's Title IV student financial assistance programs are required by the Clery Act to report the campus crime statistics. To construct the campus crime dataset, I first restrict the sample to only include the on-campus crime statistics for 4-year public and non-profit universities with Fall 2020 enrollment greater than 10,000. Then, I drop the universities with huge online enrollment.[35]

[34] The detailed method to construct the staked dataset is discussed in section 3.4.2.
[35] The detailed method to construct the staked dataset is discussed in section 3.4.2.

3.3.3 Independent Variables

Both UCR and CSS analysis control for potential confounding factors. All analyses control for minimum wages, unemployment rate, poverty rate, beer tax, and 1-year-lagged state police employment. Minimum wages come from University of Kentucky Center for Poverty Research (2022). The Bureau of Labor Statistics (BLS) provides the unemployment rates. Poverty rates, beer taxes, and state police employment come from the Census Bureau, Beer Institute (2019), and FBI Law Enforcement Officers Killed and Assaulted (LEOKA) Program, respectively. In addition, following Corman et al. (2017), the UCR analysis control for the total population covered by the sampled agencies within each state. This variable is crucial for the UCR analysis to control for the arrests' variation caused by the fact that not every agency reports all years in the sample. Table 3.4 provides the descriptive statistics for the dependent variables awaiting to be stacked for the UCR stacked difference-in-differences analysis.

3.4 Empirical Methods

3.4.1 "Traditional" Differences-in-differences Event Study

The current study first uses a "traditional" difference-in-differences model to estimate the effect of MMLs on youth crime. The following equation is estimated:

$$Ln(Y_{sta}) = \sum_{b=-4;b\neq-1}^{4} \delta_b MML_{sa,t+b} + X_{sta}\beta_2 + v_{sa} + w_{ta} + \varepsilon_{sta} \tag{1}$$

where Y_{sta} is the arrest rate of age group a for state s at year t, which is calculated as the arrest count divided by 1,000 relevant age group a population; w_{ta} and v_{sa} denote the year fixed effects and state fixed effects, respectively; X_{sta} represent the control variables listed in Table 3.5. The treatment dummy, $MML_{sa,t+b}$, takes the value of 1 if MML was implemented b years from year t, and zero otherwise. δ_b measures the relationship between MMLs and youth crime. ε_{st} is an error term. Standard errors are clustered at the state level.

3.4.2 Stacked Differences-in-differences Event study

A recent wave of literature questioned the validity of the "traditional" difference-in-differences method when one or more key assumptions are relaxed (Baker, 2022; Borusyak et al., 2022; Callaway & Sant'Anna, 2021; Goodman-Bacon, 2021; Roth et al., 2022; Sun & Abraham, 2021; Wooldridge, 2021). Following Cengiz et al. (2019), Deshpande & Li (2019), and Ruhm & Mathur (2022), the current study implemented a stacked difference-in-differences event study model to address the concern of heterogeneous treatment effects with staggered timing.

The stacked difference-in-differences method's idea is to create multiple sub-datasets with control states and non-staggered treated states, stack all the observations, and run fixed effect regression. First, based on the timing of MML implementations and data availability, the current study set 2003-2018 as the time range and deleted states implementing MML before 2006 and after 2015. This deletion kept the states that at least can be observed within a 9-year time window around the implementation date. Thus, all treated states can provide complete information about the pre- and post-treatment periods. Second, the dataset was split into multiple sub-datasets. Each sub-dataset only contains one cohort of states treated in the same year and all states that have not yet been treated until 2018. Third, each sub-dataset dropped the observations outside the 9-year time window around the cohort-specific treated year. Forth, this study renumbered the years in the time window by the position of the year in ascending order. So, every cohort covers the period from year 1 to year 9. Fifth, the current study combined all the sub-dataset and applied the following fixed effect model:

$$Ln(Y_{stac}) = \sum_{b=-4;b\neq-1}^{4} \delta_b MML_{sac,t+b} + X_{stac}\beta_2 + w_{tac} + v_{sac} + \varepsilon_{stac} \quad (2)$$

Here, Y_{stac} means the arrest rate of age group a for state s in cohort c at year t, which is calculated as the arrest count divided by 1,000 relevant age group a population. The state fixed effects and year fixed effects in equation (1) were replaced by cohort-by-year fixed effect, w_{tac}, and cohort-by-state fixed effect, v_{sac}, respectively. X_{stac} represent the control variables listed in Table 3.4. The treatment dummy, $MML_{sac,t+b}$, takes the value of 1 if MML was implemented b years from year t in cohort c, and zero otherwise. δ_b measures the relationship between MMLs and youth crime. ε_{stac} is an error term. Standard errors are clustered at the state level.

For the university crime analysis, I followed the same sprit of constructing stacked youth crime sample to create stacked campus crime sample. Then I estimated the following Poisson model:

$$E[\,Y_{utc}|(\text{MML}, X_{utc}, v_{uc}, w_{tc})] = \exp\,(\sum_{b=-4;b\neq-1}^{4} \delta_b MML_{uc,t+b} + X_{utc}\beta_2 + w_{tc} + v_{uc}) \quad (3)$$

Here, Y_{utc} means the crime count for university u in cohort c at year t . The equation also control for cohort-by-year fixed effect, w_{tc}, and cohort-by-university fixed effect, v_{uc}, respectively. X_{utc} represent the control variables listed in Table 3.4. The treatment dummy, $MML_{uc,t+b}$, takes the value of 1 if MML was implemented b years from year t in cohort c, and zero otherwise. δ_b

measures the relationship between MMLs and campus crime. Standard errors are clustered at the state level.

3.5 Results

3.5.1 "Traditional" Difference-in-Differences Event Studies-UCR

Figure 3.1 displays the "traditional" event study estimates for the impact of MMLs on violent crime arrests by four different youth age groups. Panel A shows the impacts on violent crime committed by 13-15 years old group are insignificant. Panel B, Panel C, and Panel D display similar patterns of impacts, indicating the laws significantly increase the relevant age group's violent crime arrests by 12-22% during the second and third full year of legalization.

The impacts of MMLs on youth property crime arrests estimated by the "traditional" difference-in-differences models are shown in Figure 3.2. All panels follow similar patterns, where there are statistically significant increases started as late as the second full year of implementation at 13-24% increases. The significant increases continue until the fourth full year of implementation for age groups 13-15, 16-17, and 18-20 years old, while significant increases stop at the third post-legalization year for the age group 21-24 years old.

In Figure 3.3, Panel A, Panel B and Panel C show the laws have insignificant effects on the drug crime arrest for 13-15 years old, 16-17 years old and 18-20 years old groups, respectively. By contrast, the laws increase 21-24 years old drug crime arrests by 14-22% in the four-year post-legalization window.

3.5.2 Stacked Difference-in-Differences Event Studies-UCR

The current study considers the stacked difference-in-differences method as a preferred way of modeling the impacts of MMLs because of the staggered nature of the law implementation and the possibility of heterogeneous treatment effects. Unlike “traditional” difference-in-differences models concluding MMLs increase violent crime, property crime, and drug crime arrests for some age groups, stacked difference-in-differences models suggest the impact on the overall property crimes might not be valid. The sub-sections in this section discuss the laws’ impact on the overall crimes and the associated sub-category crimes in stacked difference-in-differences settings.

3.5.2.1 Violent Crime and Property Crime

Figure 3.4 plots the staked difference-in-differences estimates of the relationship between MMLs and violent crimes. Panel A and panel D finds insignificant impacts of MMLs on violent crimes among 13-15 years old and 21-24 years old groups. Panel B and Panel C indicate that MMLs increase youth crime among people ages 16-17 years old and 18-20 years old by 11-20%.

Figure 3.5 displays that the youth male violent crime arrests for 16-17 years old groups increase by around 15%. Estimates in Figure 3.6 show that the index violent crime arrests for people ages 21-24 years old increase by 13% in the first full year of MML legalization. By contrast, in Figure 3.7, simple assaults arrests for all age groups have 8-20% increases. Panel A through Panel D suggest that simple arrests for people ages 13-15, 16-17, 18-20, and 21-24 years old increase by around15%. The effects of MMLs on simple assault dominate the effects on index violent crime. This result may suggest that MMLs may be more influential on less serious violent crimes among youth. In terms of property crimes, Figure 3.8 and Figure 3.9 find no evidence that MMLs affect youth property crime arrests and youth male property crime arrests.

3.5.2.2 Drug Crime

The association between MMLs and youth drug crime is represented in Figure 3.10. The estimates suggest that MMLs' impacts are not statistically significant. The same pattern applies to Figure 3.11, which displays youth male drug crime arrests estimates. Figure 3.12 and Figure 3.13 show the estimates for drug sale crime and drug possession crime, respectively. For the drug sale crime arrests, the 13-15 years old cohort experiences 18-30% increases, and other age groups show no significant change. These estimates may suggest that 13-15 years old have more access to illegal drugs or the police tends to focus on the younger age groups who are less likely to be affected by MMLs. For the drug possession crime arrests, all estimates are insignificant.

Figure 3.14 focus on the relationships between MMLs and marijuana abuse violations. Only the estimates in Panel A are significant and indicate 20% increases in marijuana abuse arrest among 13-15 years old. No significant effects are observed for other age groups. As for non-marijuana drug abuse violation, Figure 3.15 suggests the law increase the related arrests for 16-17 and 21-24 years old by around 10% in the first year of implementation.

3.6 Robustness and Sensitivity Checks

3.6.1 Stacked Difference-in-Differences Event Studies-CSS

Figure 3.16 displays the effects of MMLs on campus violent crime and property crime. The left panel shows the campus violent crime increase by 28% in the first year of legalization, but the effects dissipate immediately and trending downwards in the following years. In the fourth full year of implementation, the effect changes to negative indicating a 22% decrease in campus violent crime. The right panel displays no significant impact of MMLs on campus property crimes.

3.6.2 Sensitivity Checks

The current study runs a series of sensitivity checks to gauge whether the major results are sensitive to alternative specifications. First, the impacts on youth total violent crime, female violent crime, and simple assault arrests are re-estimated using different controls. Each graph from Figure 3.17 through Figure 3.19 shows four groups of estimates resulting from progressively adding control variables to the basic specification controlling for MMLs dummies and state agency-reported population. The main results are robust to alternative controls.

Cengiz et al. (2019) suggests clustering the errors at the state by cohort level, while Deshpande & Li (2019) and Ruhm & Mathur (2022) suggest clustering the errors at the state level. Wing (2021) runs a series of Monte Carlo experiments and concludes that "Stacked DID with clustering at the Unit Level works pretty well." Thus, the main analysis of the current study cluster the error at the state level. Nonetheless, I test whether clustering at state by cohort level will affect the main results. Figure 3.20 to Figure 3.22 show the related estimates for youth violent crimes, male violent crime, and simple assault regressions, and the main results are not sensitive to the alternative clustering.

3.7 Conclusion

Though the impact of MMLs on crime and the effect of MMLs on youth marijuana use draw considerable attention from MMLs literature, little studies investigate the relationships between MMLs and youth crimes (Anderson et al., 2015, 2021; Choo et al., 2014; Chu & Townsend, 2019b; Coley et al., 2019; Morris et al., 2014). However, the law could affect youth crime by increasing youth access to marijuana, setting up marijuana dispensaries that can attract crime, altering police crime efforts towards non-marijuana crime, and changing parents' behaviors.

Using both the "traditional" and stacked difference-in-differences methods, I find that the laws have positive impacts on youth violent crimes, especially for the violent crimes that are less severe. In addition, I identify that the laws particularly affect 13-15 years old youth's drug-related criminal behaviors. The impact of MMLs on violent crimes is more profound than the impact on drug-related crimes, which show relative temporal increases.

The current study is the first to investigate the relationship between MMLs and youth crimes and found positive impacts on violent crimes and drug crimes. Policymakers may consider these important connections when making future MMLs. Just like the police-reported crime counts, the arrest count may not serve as a perfect proxy for the true number of crimes. Future studies may consider doing the analysis for self-reported survey data.

3.8 Tables and Figures

Table 3.1

Medical Marijuana Laws, 1996-2019

State	Effective Date	Age Limit
Alaska	3/4/99	N
Arizona	4/14/11	N
Arkansas	11/9/16	21
California	11/6/96	N
Colorado	6/1/01	18
Connecticut	8/20/14	18
Delaware	6/26/15	18
District Of Columbia	7/30/13	N
Florida	7/26/16	N
Hawaii	12/28/00	N
Illinois	11/9/15	21
Louisiana	8/6/19	21
Maine	12/22/99	N
Maryland	12/2/17	N
Massachusetts	1/1/13	N
Michigan	12/4/08	N
Minnesota	7/1/15	N
Missouri	10/17/20	18
Montana	11/2/04	18
Nevada	10/1/01	N
New Hampshire	5/1/16	N
New Jersey	12/6/12	N
New Mexico	7/1/07	N
New York	1/8/16	N
North Dakota	3/1/19	19
Ohio	1/16/19	N
Oklahoma	7/26/18	N
Oregon	12/3/98	18
Pennsylvania	1/17/18	N
Rhode Island	1/3/06	N
Utah	3/2/20	18
Vermont	7/1/04	N
Virginia	10/17/20	N
Washington	11/3/98	18
West Virginia	8/22/17	N

Source: Insurance Institute for Highway Safety (2022).

Table 3.2

Outcomes for UCR Analysis, 1999-2018

Outcome Variables	Mean All	MMLs States	Other States	Observations
Violent Crime, 13-15 Years Old	5.07 (3.41)	5.25 (3.74)	4.68 (2.52)	840
Violent Crime, 16-17 Years Old	6.99 (4.68)	7.44 (5.14)	5.99 (3.25)	840
Violent Crime, 18-20 Years Old	7.04 (4.5)	7.66 (4.93)	5.67 (2.93)	840
Violent Crime, 21-24 Years Old	7.66 (4.6)	8.24 (5.04)	6.35 (3.07)	840
Property Crime, 13-15 Years Old	6.76 (4.94)	6.92 (5.41)	6.41 (3.68)	840
Property Crime, 16-17 Years Old	10.22 (6.47)	10.36 (7.14)	9.92 (4.62)	840
Property Crime, 18-20 Years Old	8.76 (4.98)	8.96 (5.47)	8.3 (3.65)	840
Property Crime, 21-24 Years Old	5.64 (3.07)	5.91 (3.31)	5.03 (2.34)	840
Drug Crime, 13-15 Years Old	1.89 (1.65)	1.96 (1.84)	1.73 (1.12)	840
Drug Crime, 16-17 Years Old	5.45 (3.99)	5.42 (4.41)	5.51 (2.83)	840
Drug Crime, 18-20 Years Old	8.49 (5.5)	8.41 (6.11)	8.64 (3.82)	840
Drug Crime, 21-24 Years Old	6.89 (4.32)	6.86 (4.74)	6.95 (3.19)	840

Table 3.3

Outcomes for UCR Analysis, 2003-2018

Outcome Variables	Mean All	MMLs States	Other States	Observations
Violent Crime, 13-15 Years Old	4.61 (2.55)	5.06 (2.38)	4.27 (2.62)	448
Violent Crime, 16-17 Years Old	6.14 (3.38)	6.95 (3.21)	5.53 (3.38)	448
Violent Crime, 18-20 Years Old	6.1 (3.32)	7.23 (3.46)	5.25 (2.94)	448
Violent Crime, 21-24 Years Old	6.78 (3.47)	7.95 (3.61)	5.91 (3.1)	448
Property Crime, 13-15 Years Old	5.74 (3.73)	5.83 (3.96)	5.67 (3.56)	448
Property Crime, 16-17 Years Old	9.24 (5.24)	9.47 (5.7)	9.06 (4.87)	448
Property Crime, 18-20 Years Old	8.09 (4.44)	8.67 (4.93)	7.66 (4)	448
Property Crime, 21-24 Years Old	5.2 (2.78)	5.79 (3)	4.76 (2.52)	448
Drug Crime, 13-15 Years Old	1.65 (1.31)	1.7 (1.46)	1.61 (1.18)	448
Drug Crime, 16-17 Years Old	5.05 (2.93)	5.06 (2.8)	5.04 (3.02)	448
Drug Crime, 18-20 Years Old	8.22 (4.36)	8.65 (4.56)	7.9 (4.19)	448
Drug Crime, 21-24 Years Old	6.68 (3.43)	7.05 (3.38)	6.41 (3.44)	448

Table 3.4

Independent Variables, 2003-2018

Variables	Mean	Observation
Total Agency Population	2,680,000 (3,300,000)	448
State Minimum Wage	2.99 (0.44)	448
Unemployment Rate	5.72 (2.05)	448
Poverty Rate	13.39 (3.5)	448
Beer Tax	0.13 (0.11)	448
Police Employment	240.17 (57.83)	448

Figure 3.1

MMLs and Youth Violent Crime, 1999-2018

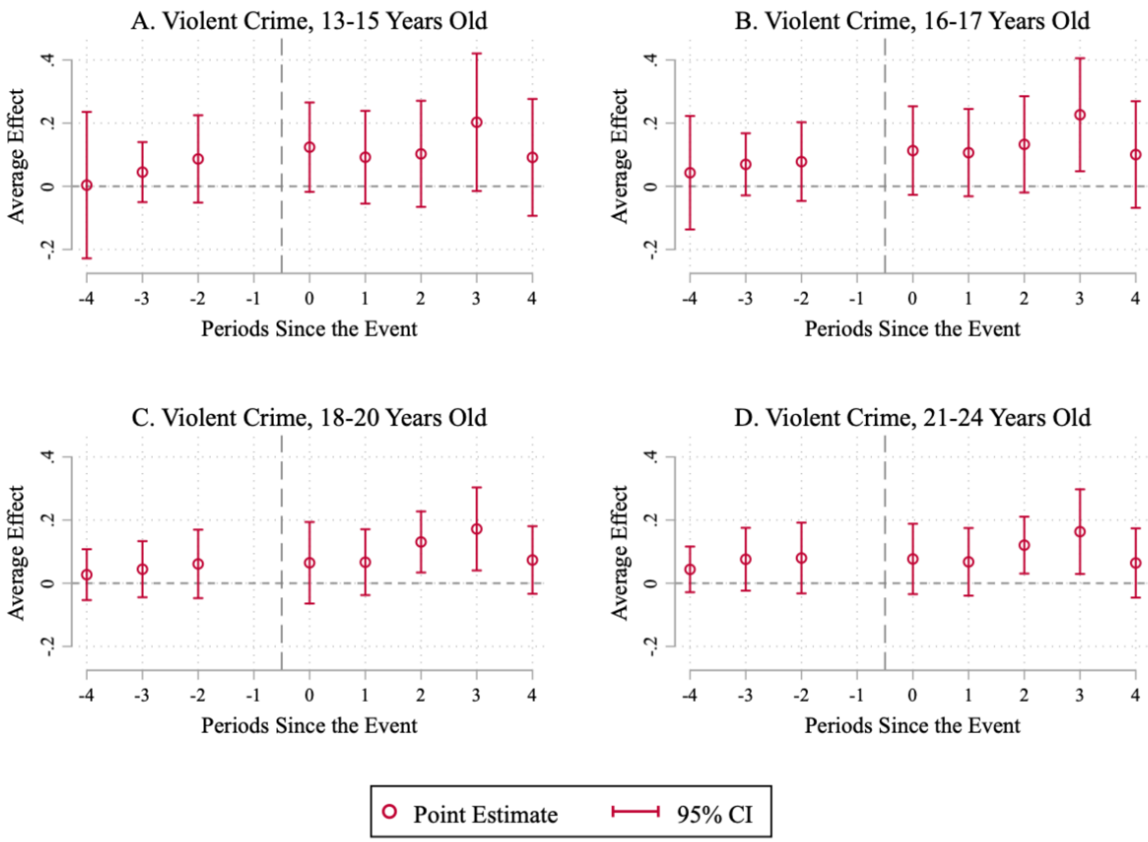


Figure 3.2

MMLs and Youth Violent Crime, 1999-2018

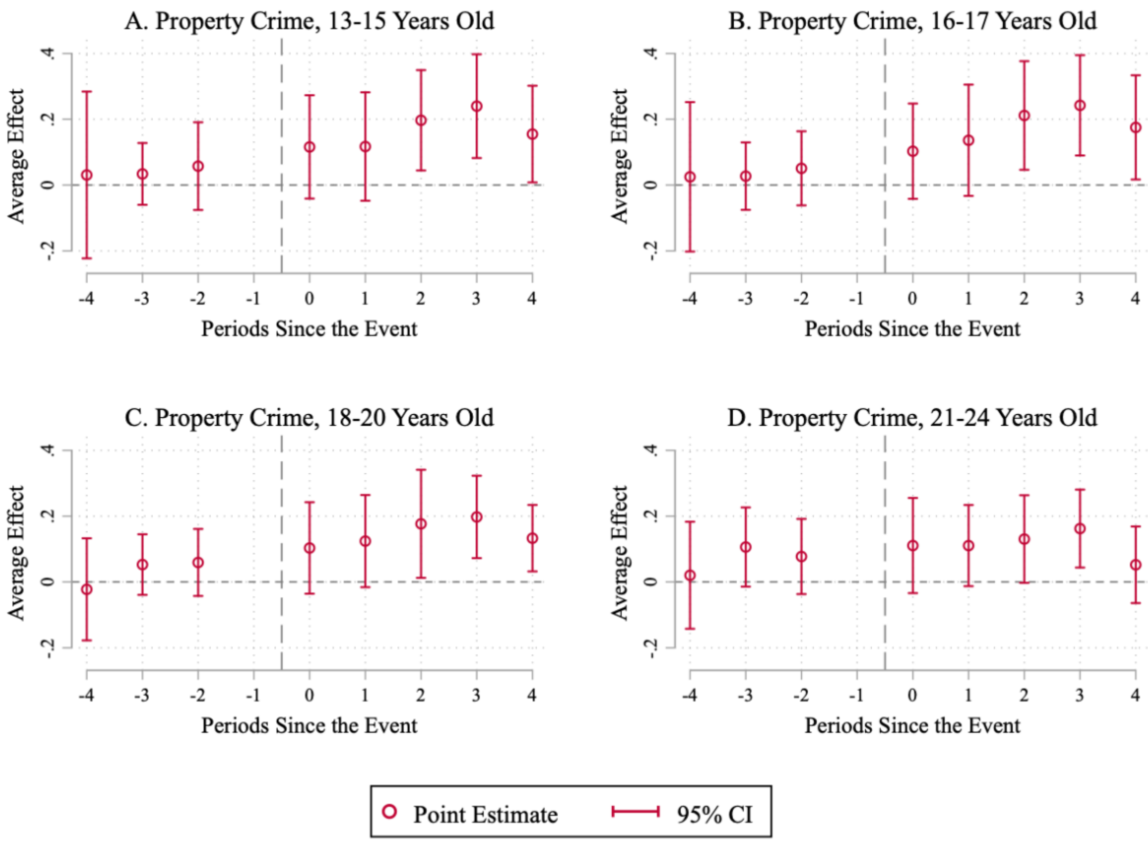


Figure 3.3

MMLs and Youth Drug Crime, 1999-2018

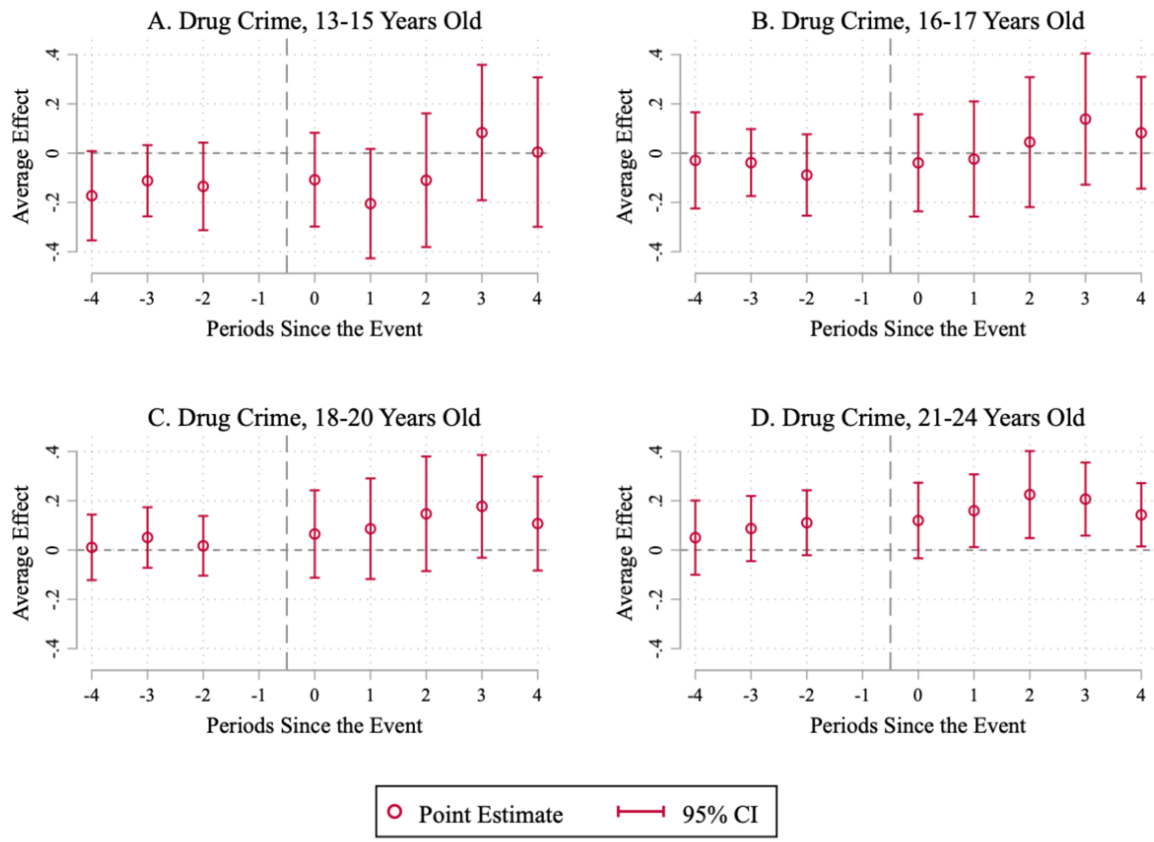


Figure 3.4

MMLs and Youth Violent Crime, 2003-2018

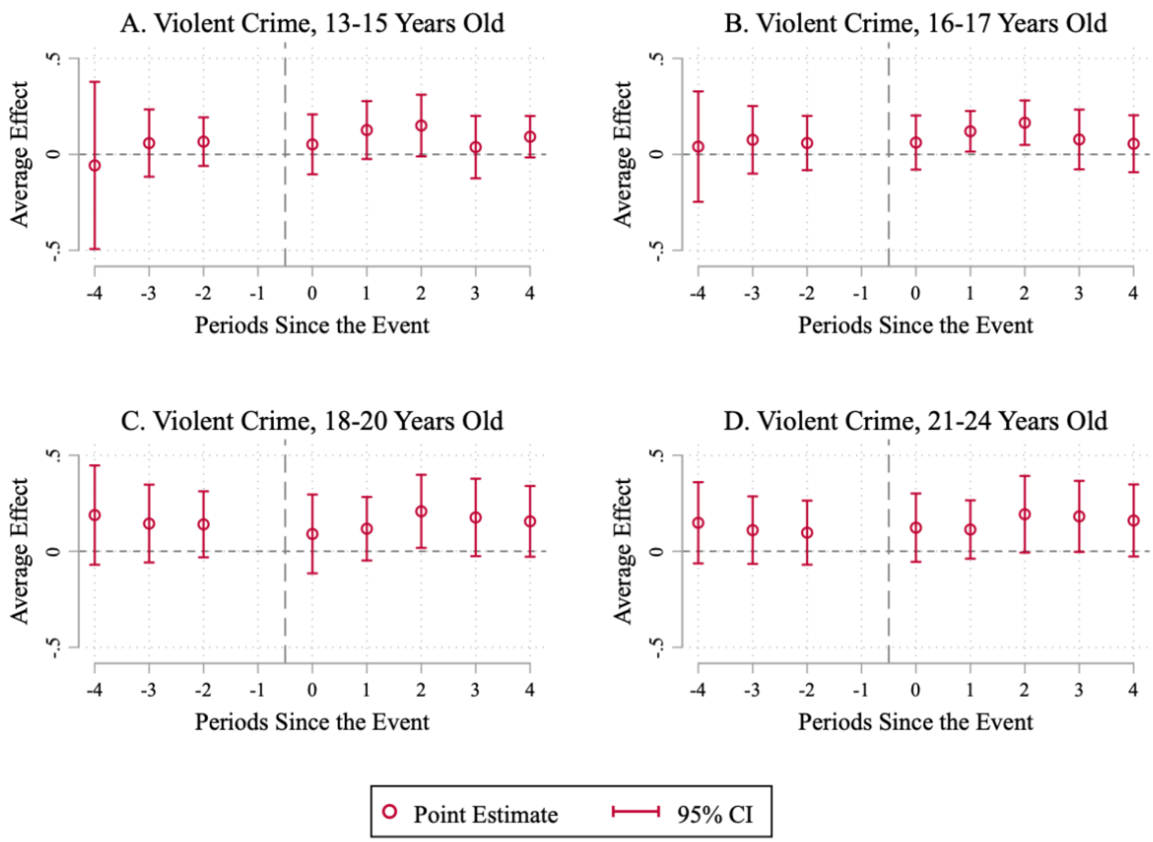


Figure 3.5

MMLs and Youth Male Violent Crime, 2003-2018

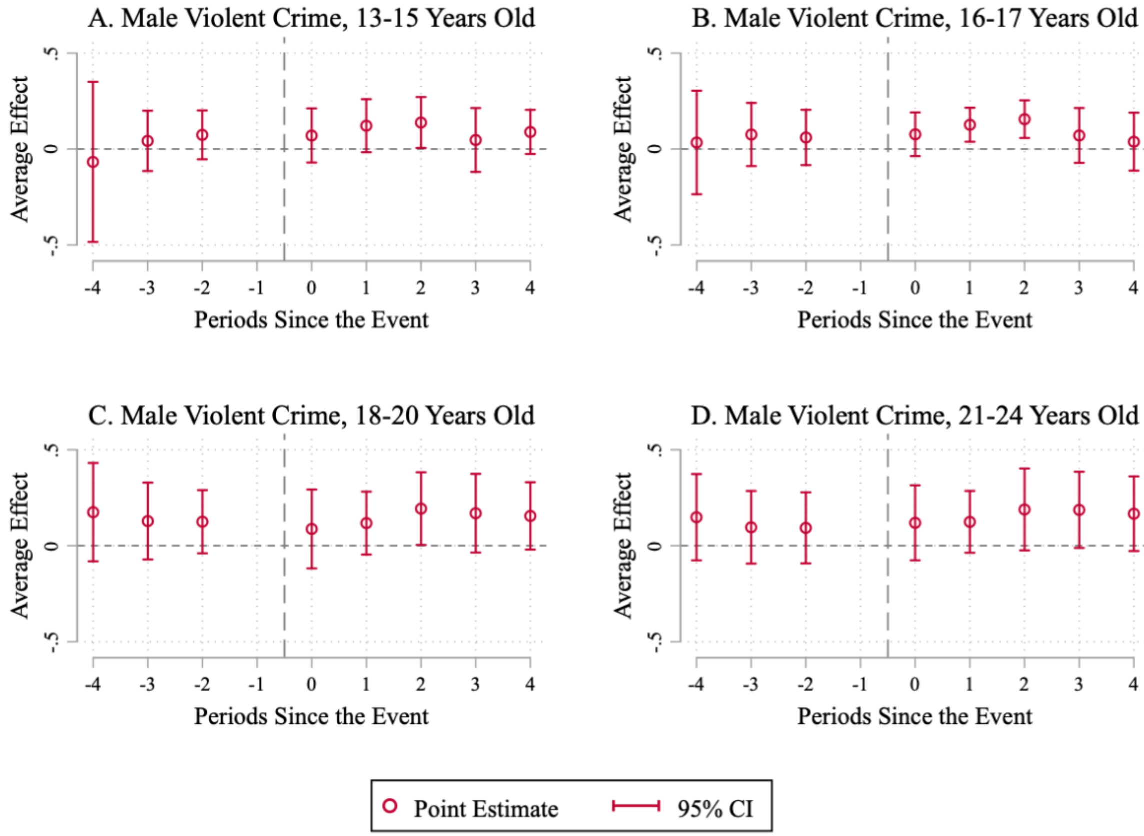


Figure 3.6

MMLs and Index Violent Crime, 2003-2018

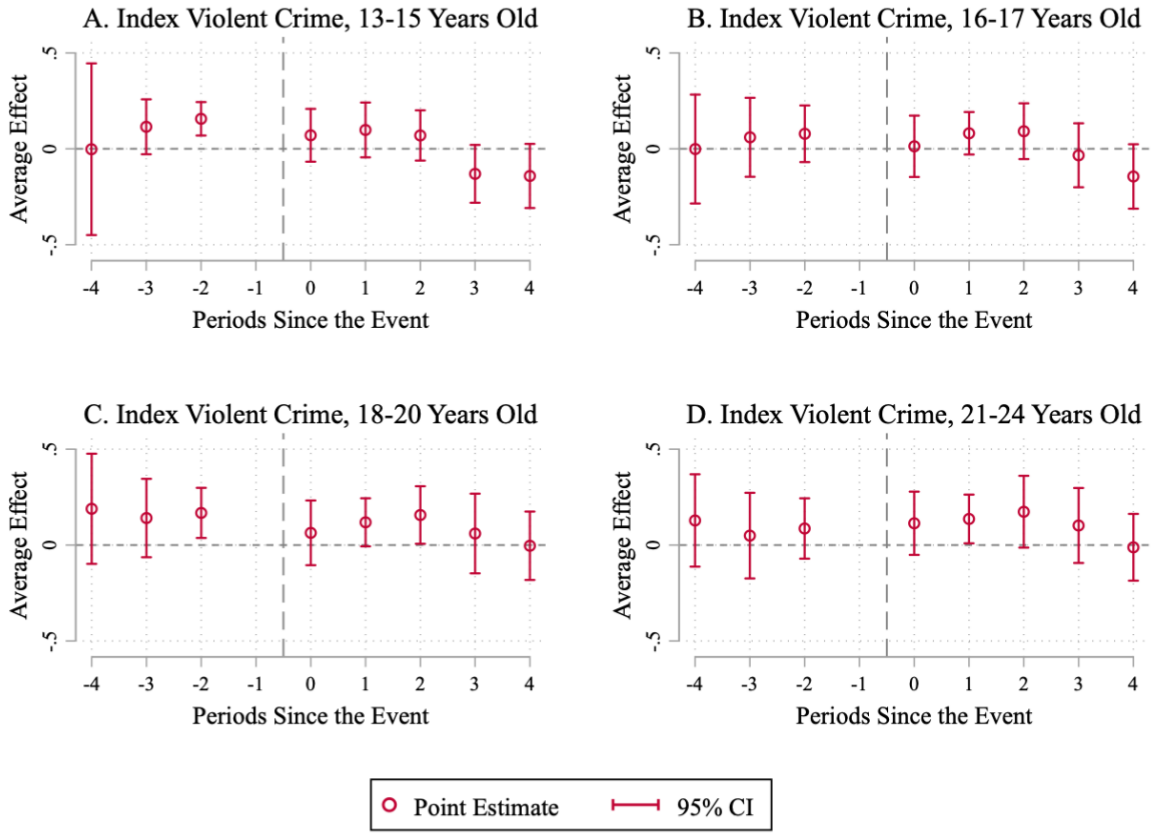


Figure 3.7

MMLs and Youth Simple Assault

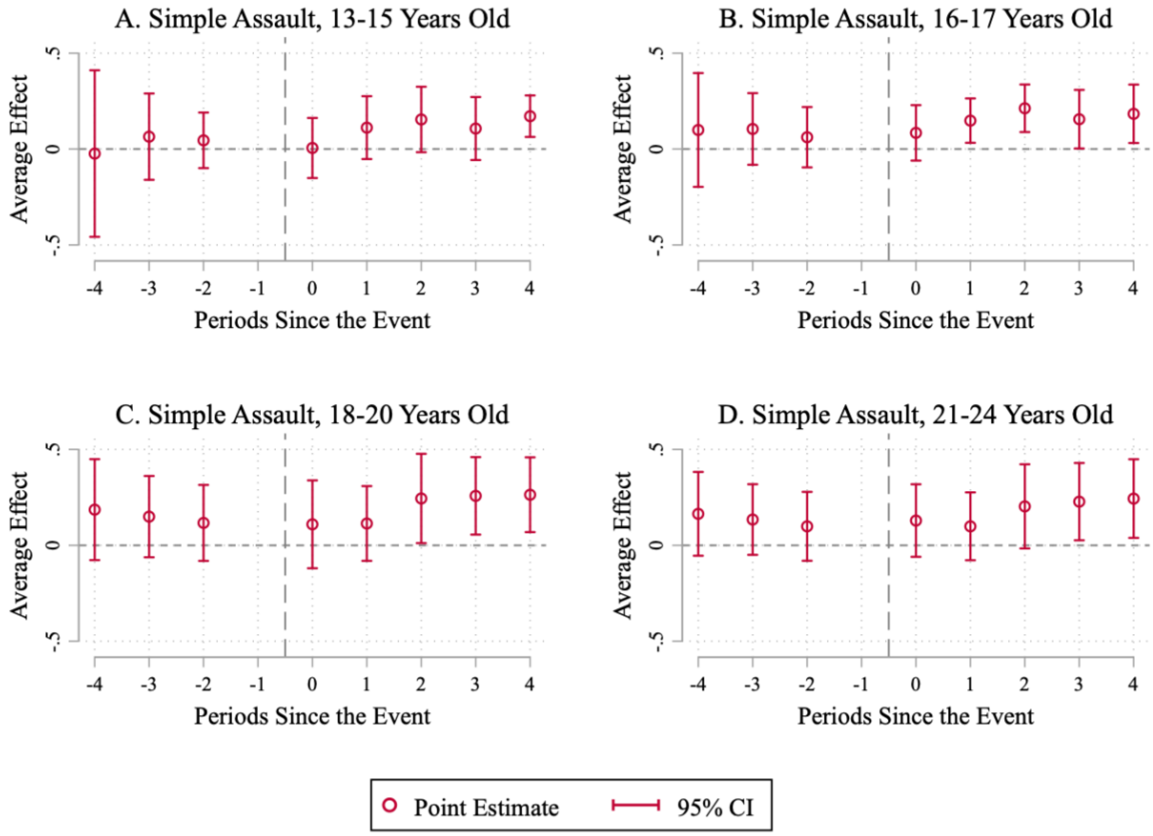


Figure 3.8

MMLs and Youth Property Crime, 2003-2018

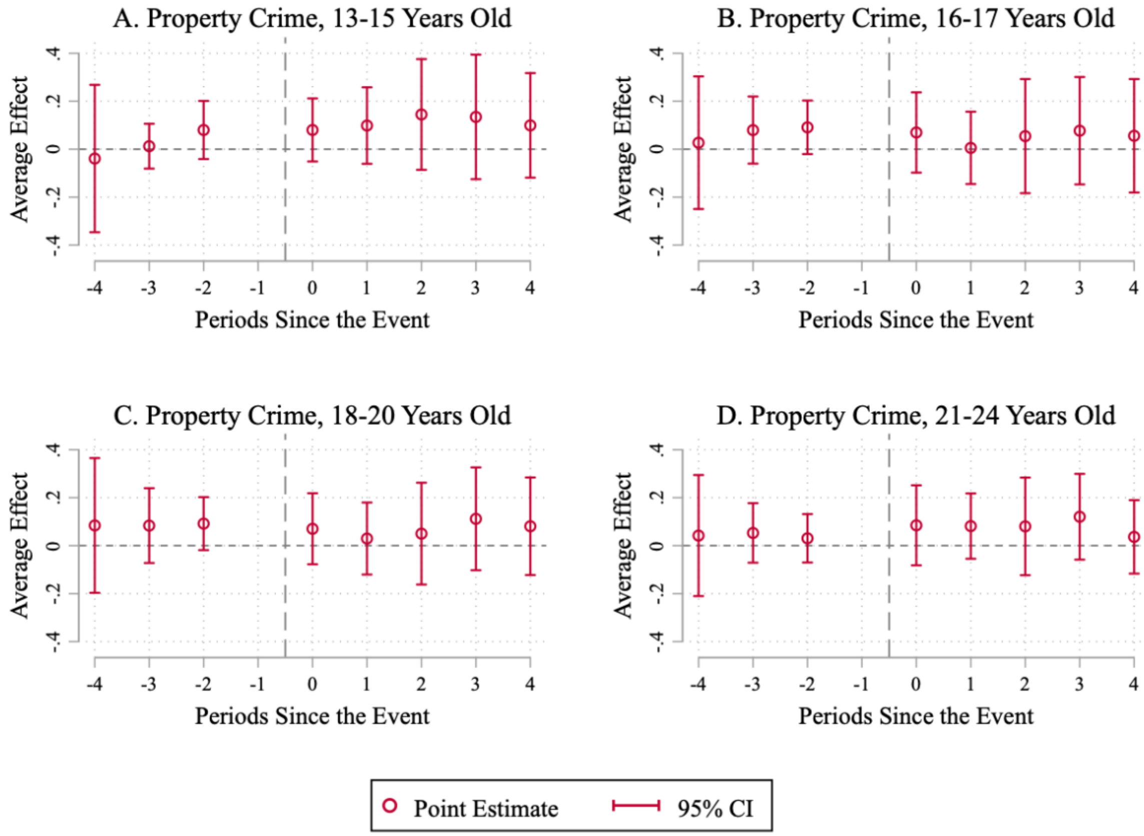


Figure 3.9

MMLs and Youth Male Property Crime, 2003-2018

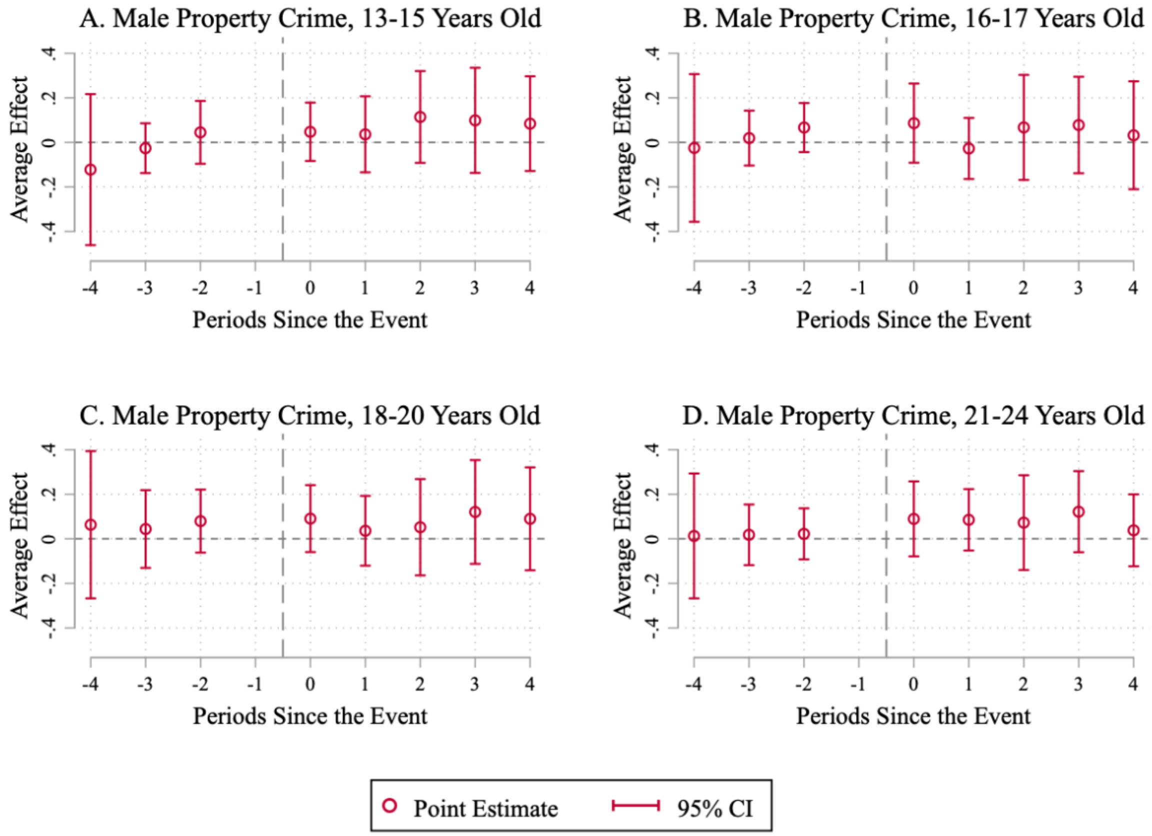


Figure 3.10

MMLs and Youth Drug Crime, 2003-2018

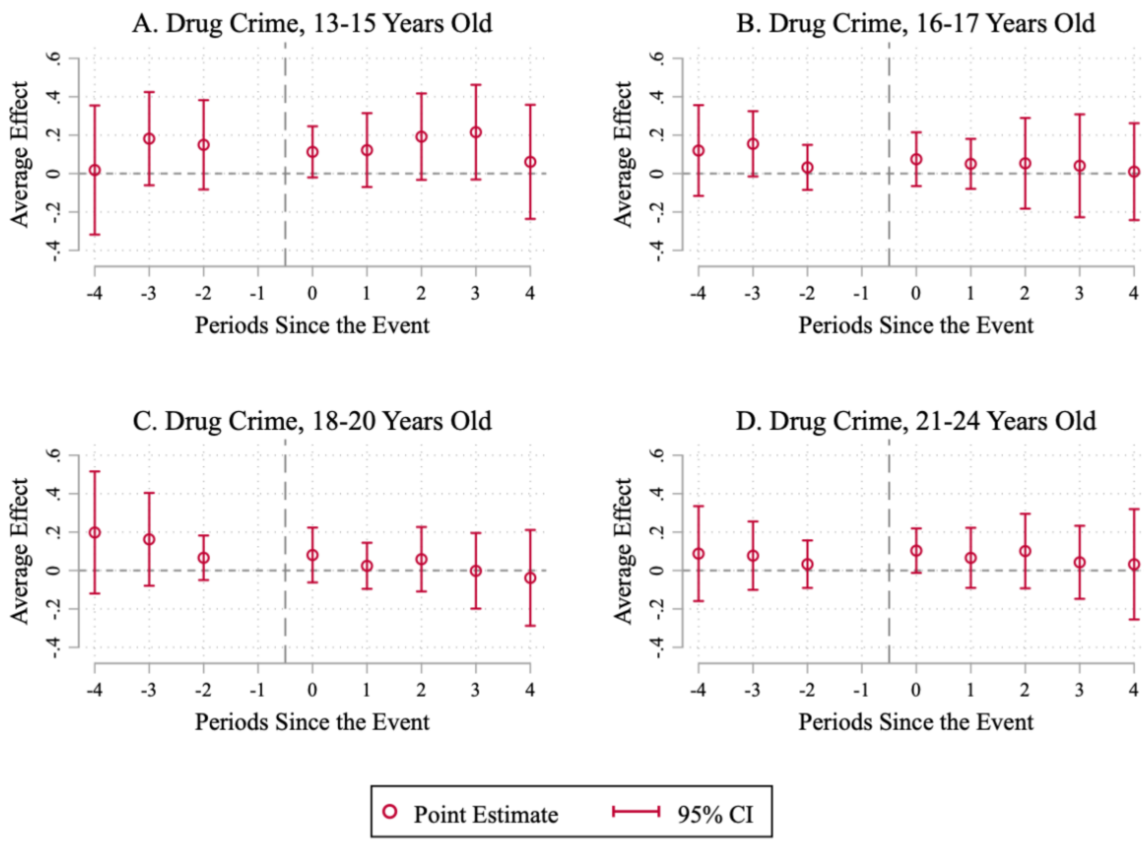


Figure 3.11

MMLs and Youth Male Drug Crime

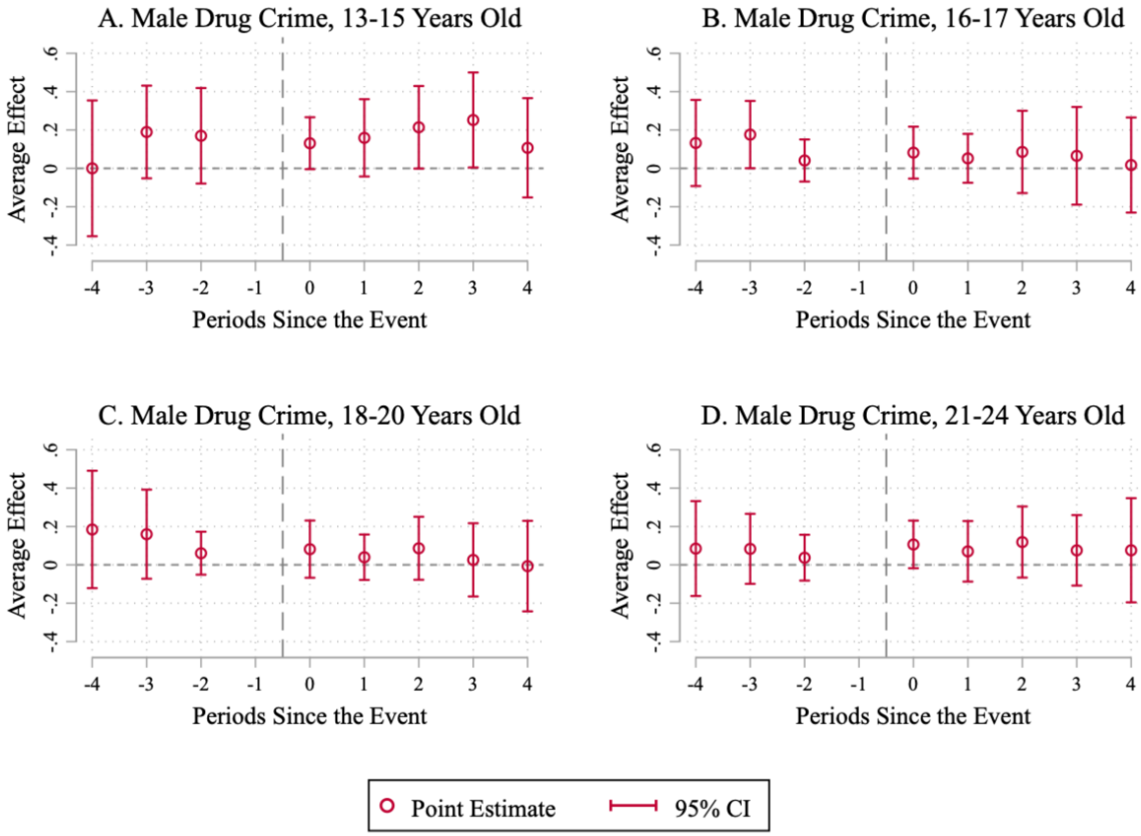


Figure 3.12

Youth Drug Sale Crime

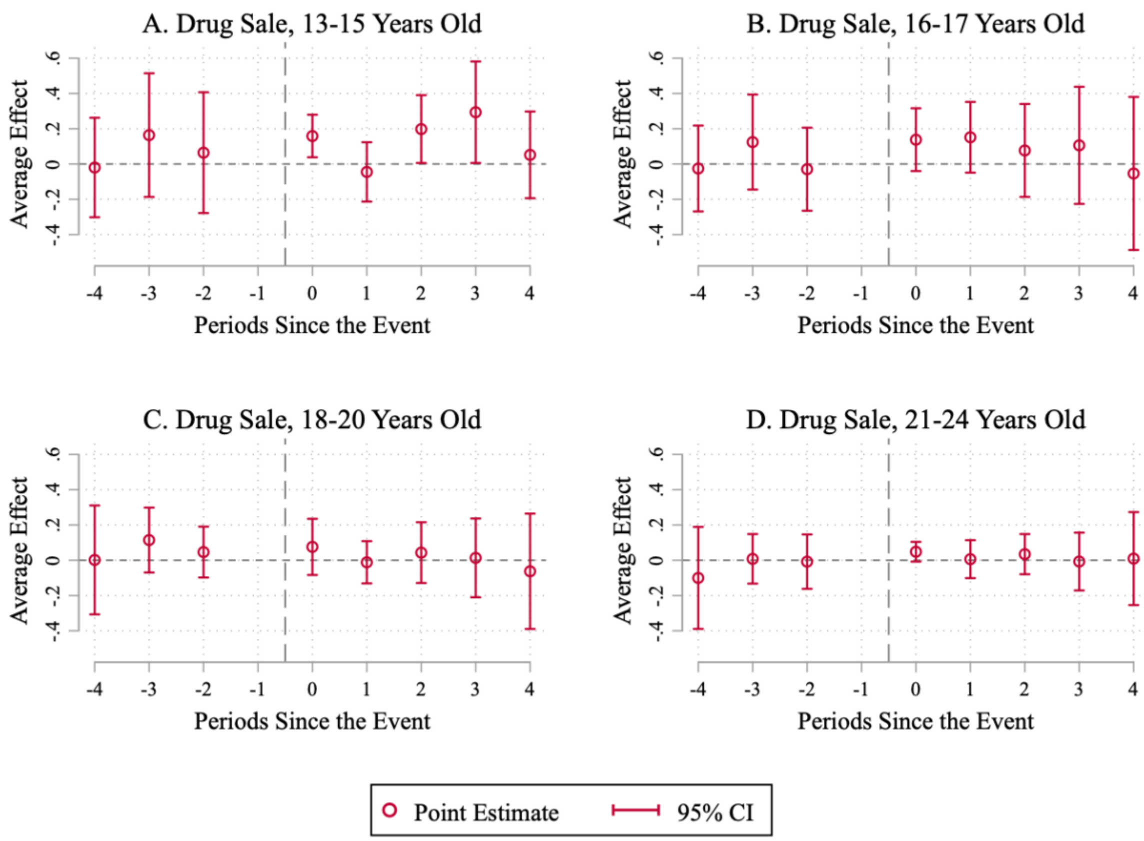


Figure 3.13

MMLs and Youth Drug Possession Crime

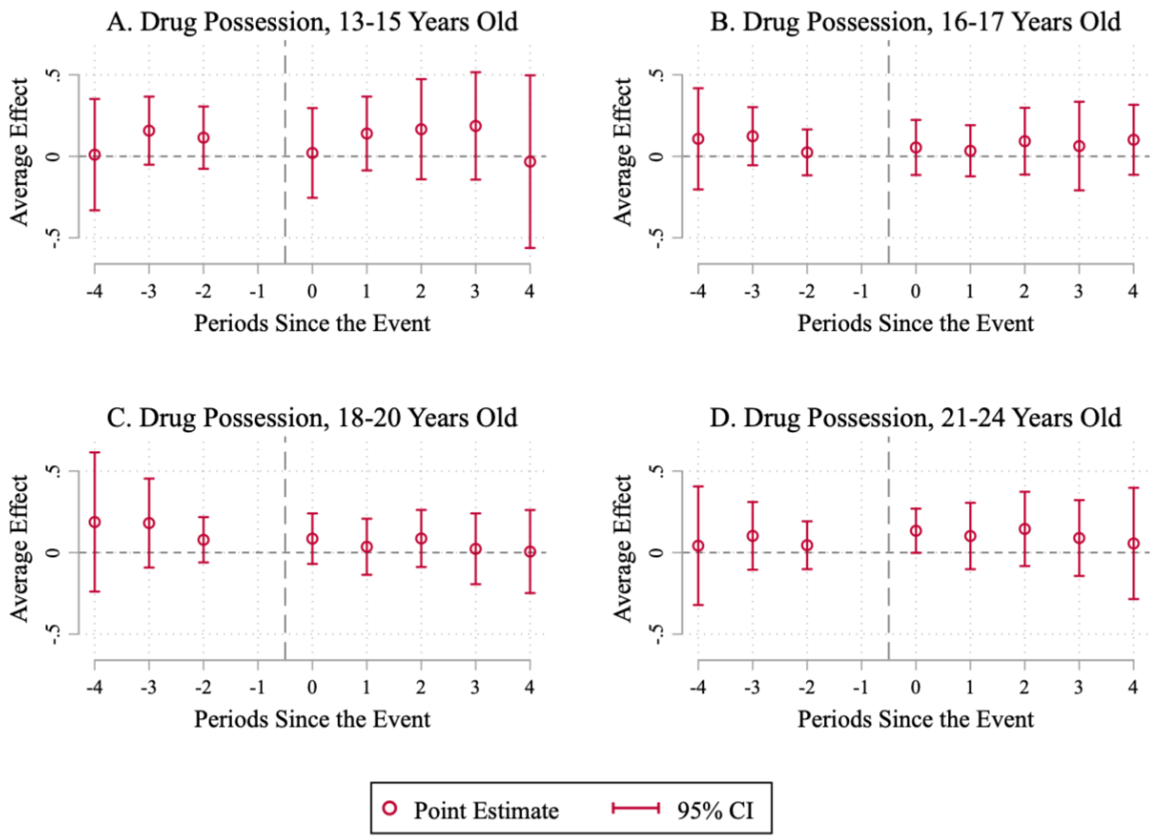


Figure 3.14

MMLs and Marijuana Abuse

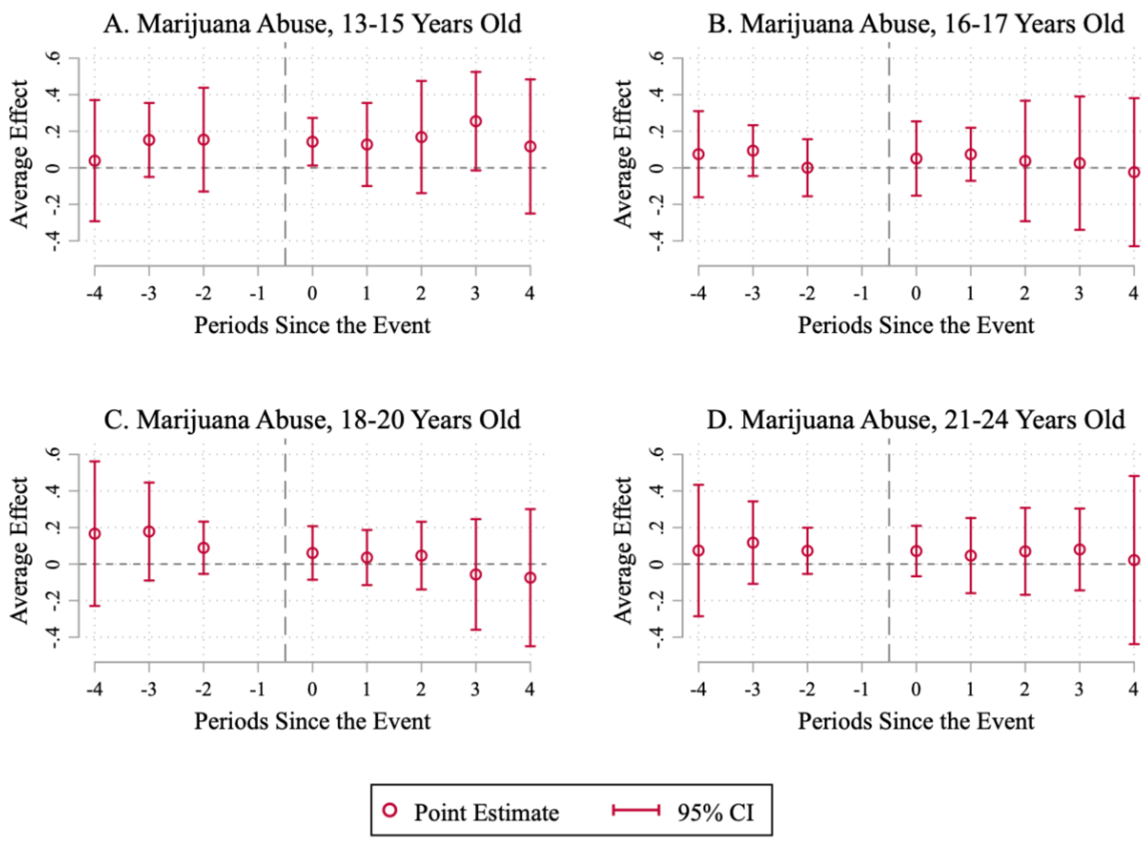


Figure 3.15

MMLs and Non-Marijuana Drug Abuse

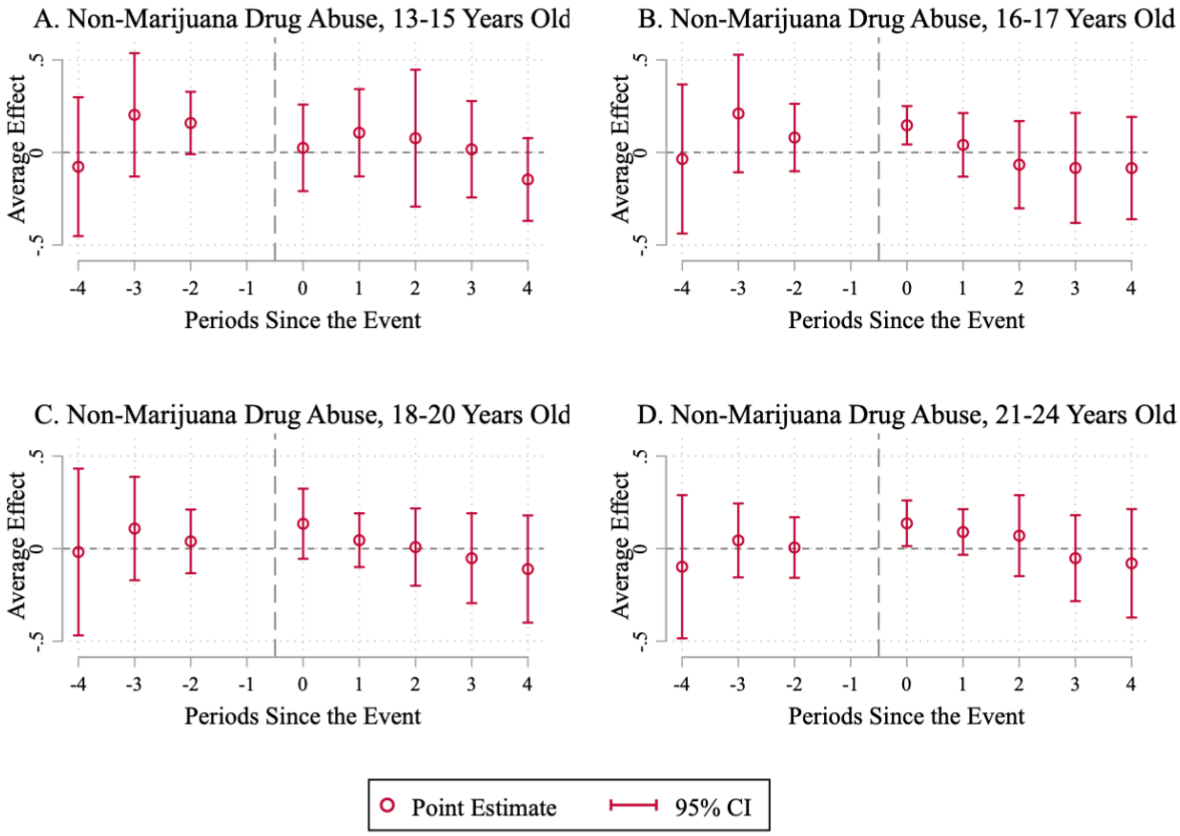


Figure 3.16

MMLs and University Crimes

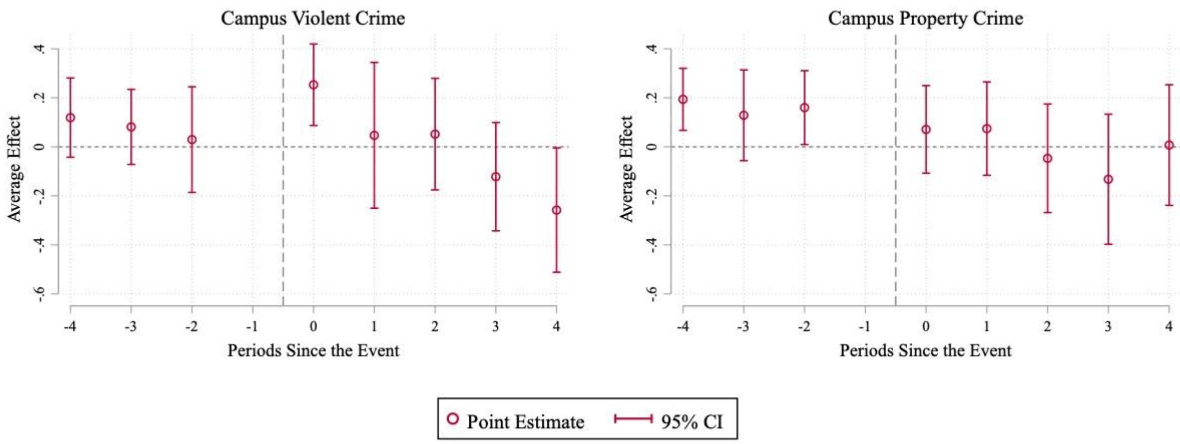


Figure 3.17

Sensitivity Check: MMLs and Youth Violent Crime - Progressively Adding Controls

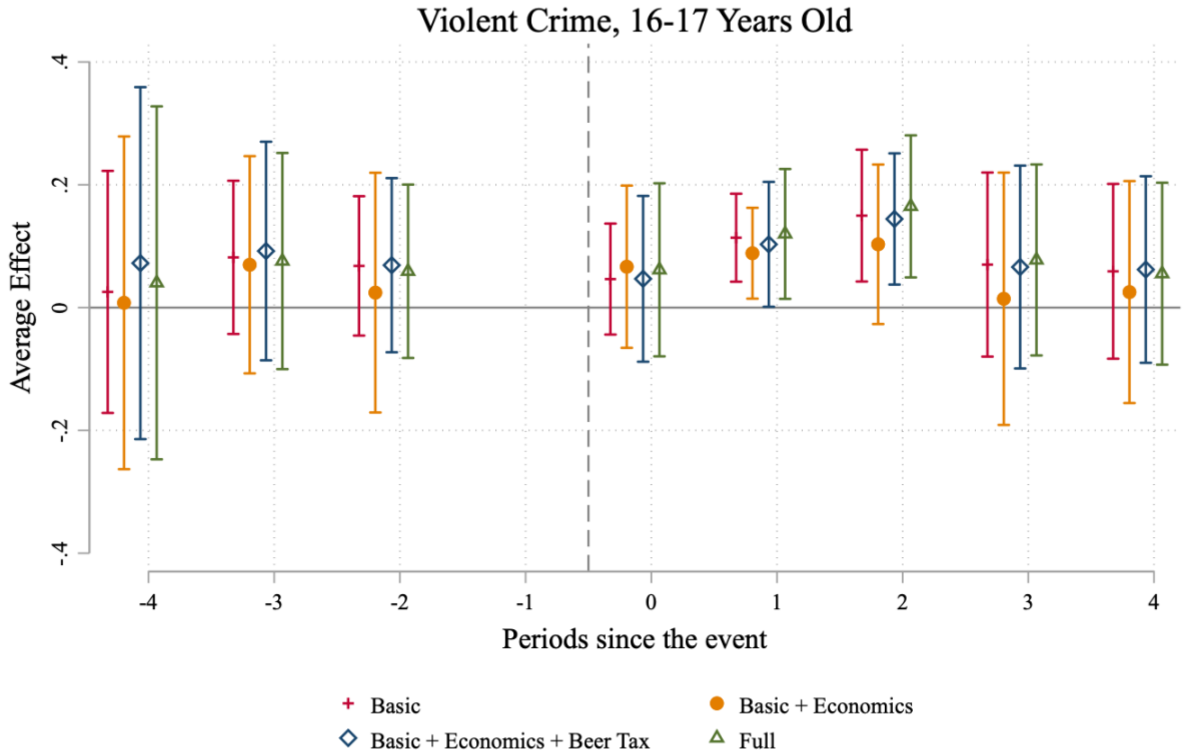


Figure 3.18

Sensitivity Check: MMLs and Youth Male Violent Crime - Progressively Adding Controls

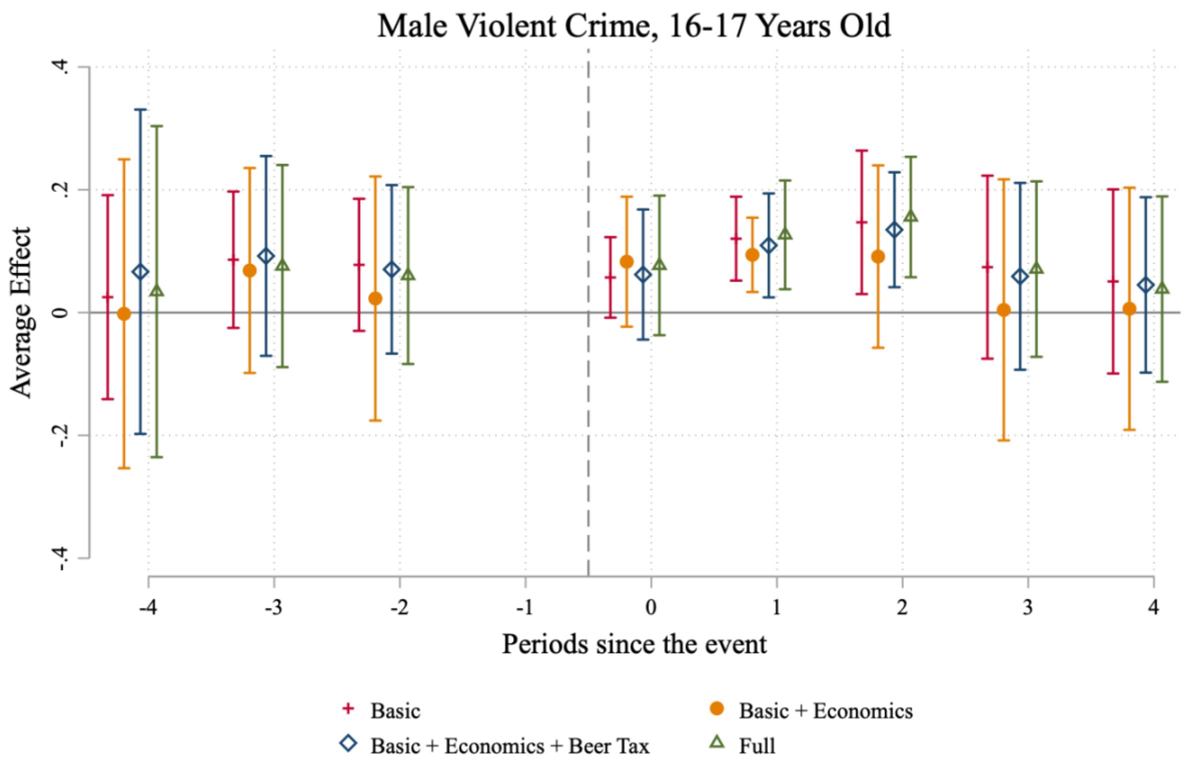


Figure 3.19

Sensitivity Check: MMLs and Youth Simple Assault - Progressively Adding Controls

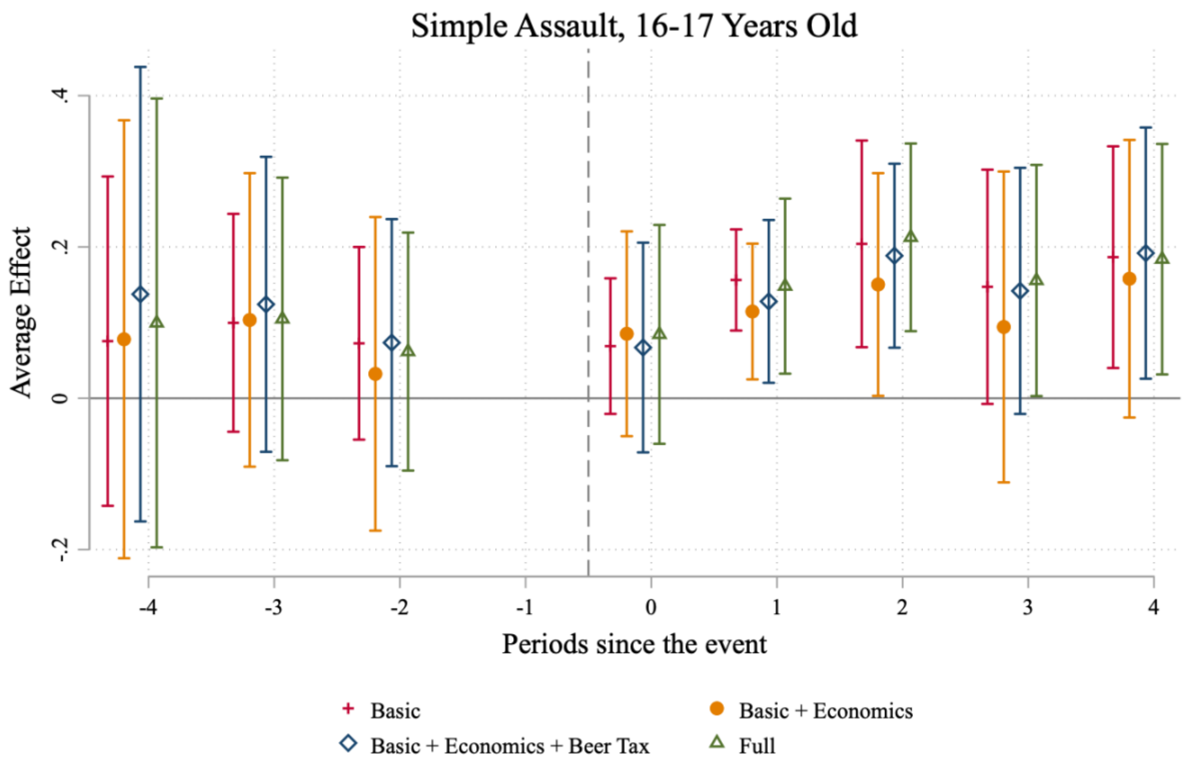


Figure 3.20

Sensitivity Check: MMLs and Youth Violent Crime - Alternative Clustering

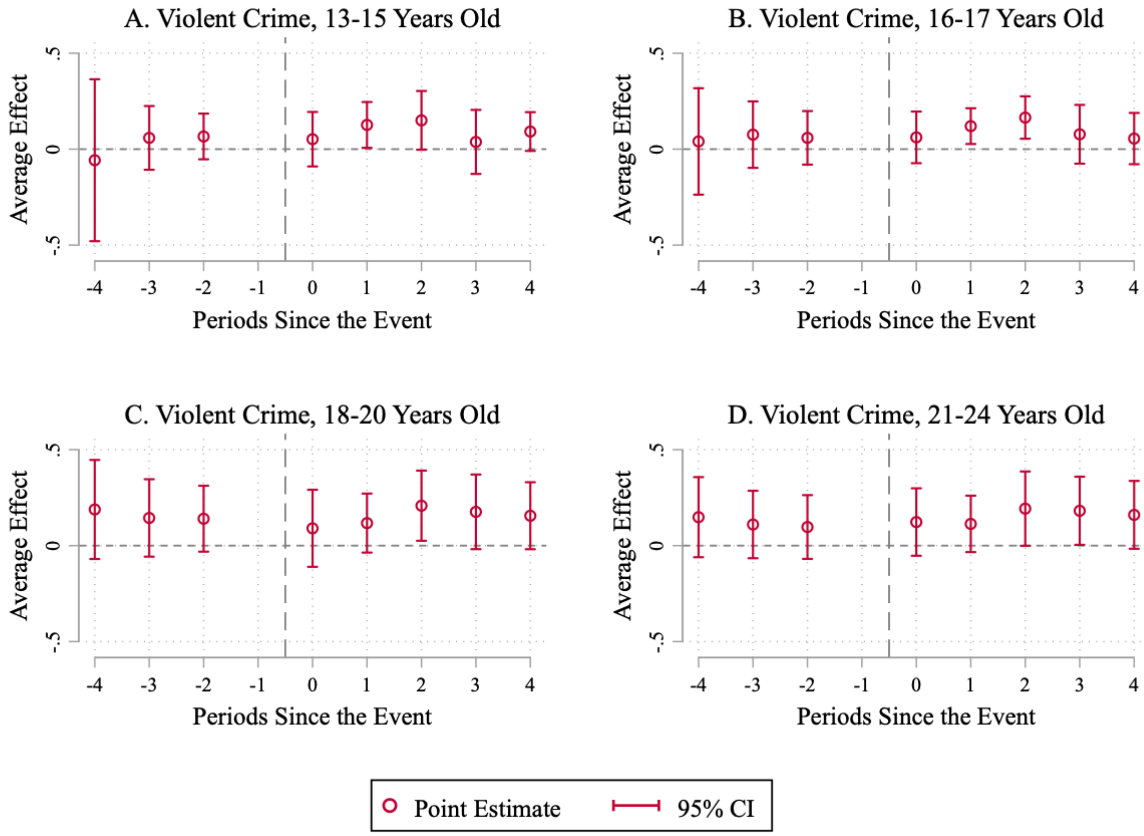


Figure 3.21

Sensitivity Check: MMLs and Youth Male Violent Crime - Alternative Clustering

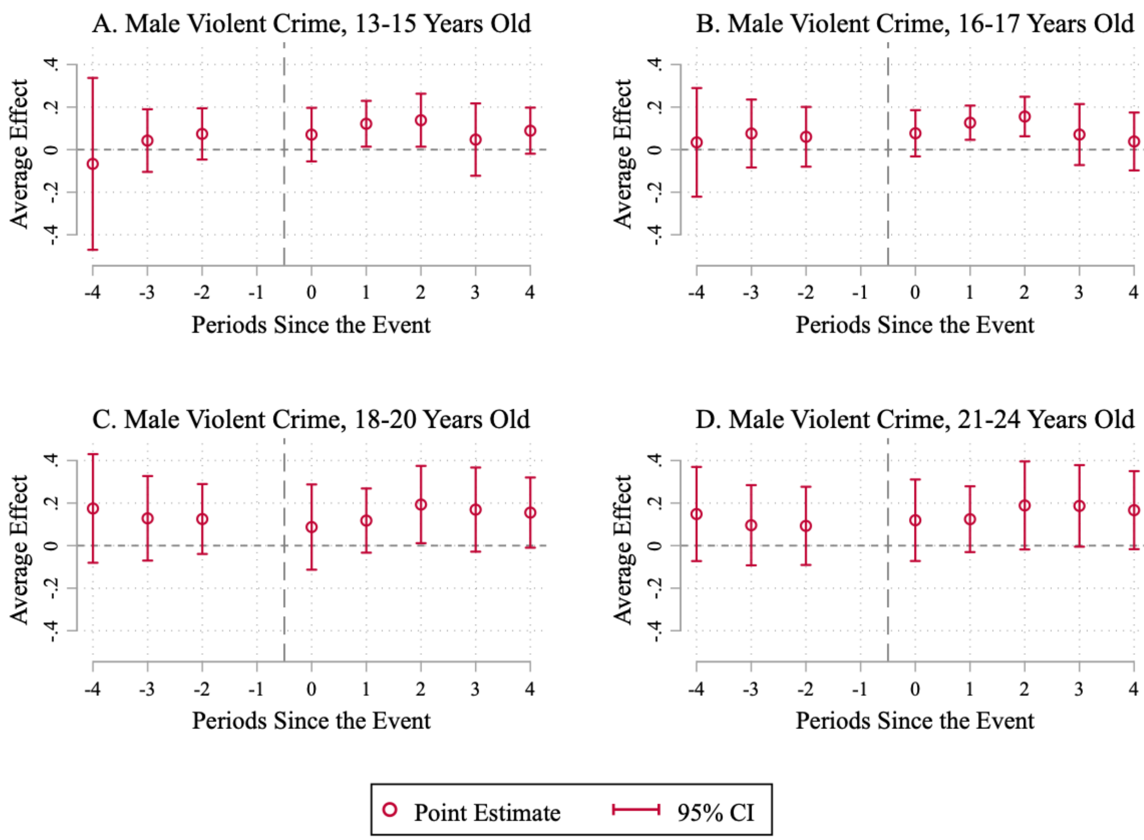
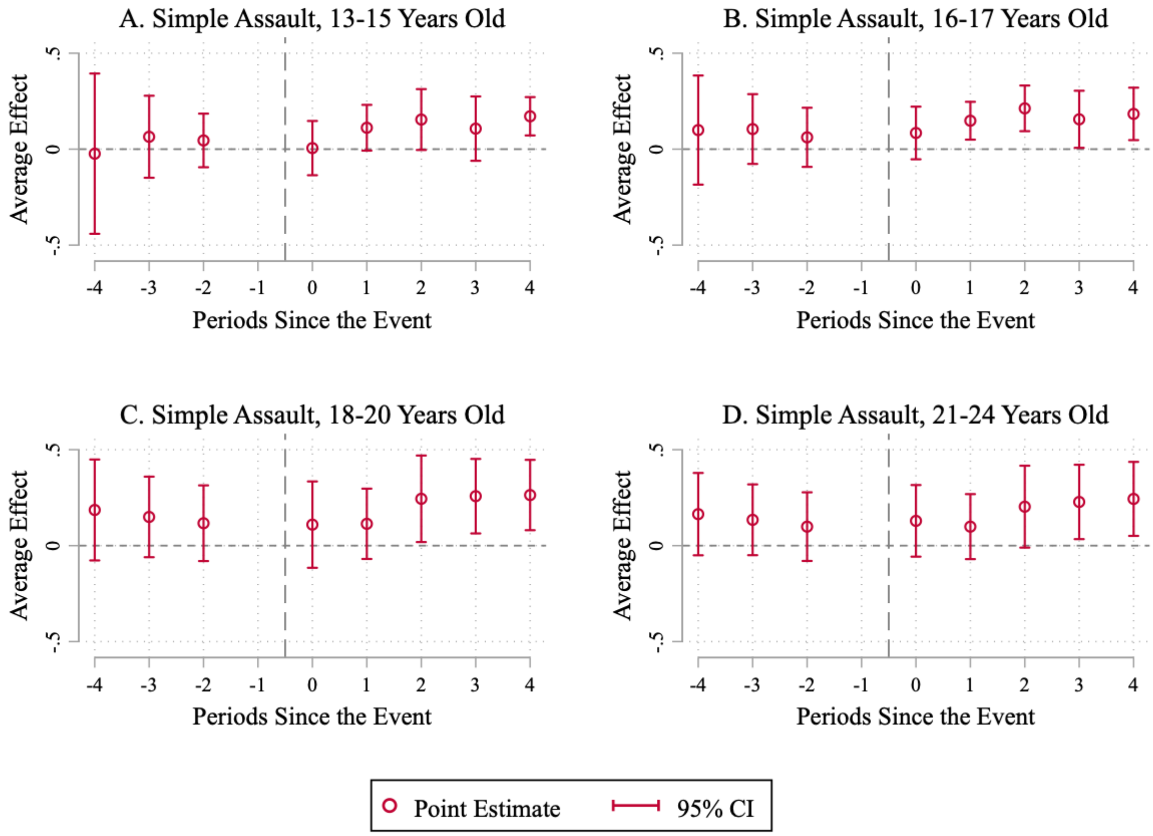


Figure 3.22 MMLs and Youth Simple Assault- Alternative Clustering

Sensitivity Check: MMLs and Youth Simple Assault - Alternative Clustering



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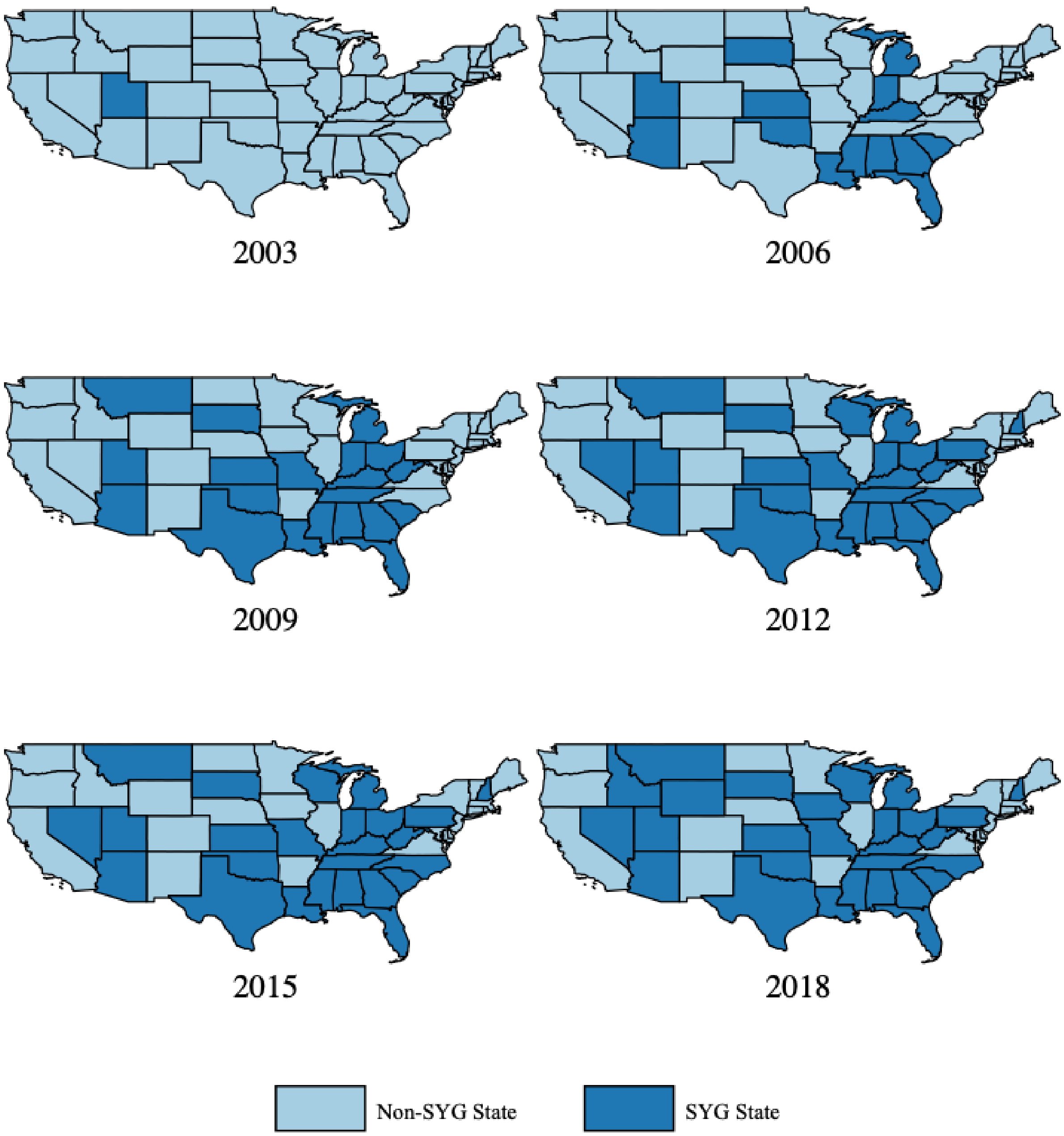
Appendix A:

Chapter 1 Descriptions for Reduced Form Regressions' Outcome Variables



Appendix B:

Chapter 1 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 fatailties 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 does 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 area (per 100,000 state population)
Urban	5.89 (2.08)	Traffic fatalities in urban area (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+)

Appendix C:

Chapter 1 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 (Schell et al., 2020).
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 assult 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 driver)
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)

Appendix D:

Chapter 1 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 populaiton (per 100,000 state populaiton)
Personal Income	17982.63 (3194.07)	Real personal income (1982$ per 100,000 state populaiton)
Unemployment Insurance	76.22 (57.39)	Real unemployment insurance compensation (1982$ per 100,000 state populaiton)
Income Maintenance	287.81 (85.72)	Real income maintenance benefits (1982$ per 100,000 state populaiton)
Retirement Payments	17838.48 (3407.3)	Real retirement payments (1982$ per 100,000 state populaiton over 65)
Unemployment Rate	5.93 (1.91)	State unemployment rate (1982$ per 100 state populaiton)
State Minium Wage	3.06 (0.46)	Real state minimum wage (1982$)
Poverty Rate	13.35 (3.09)	Population in poverty (per 100 state populaiton)
Beer Tax	0.13 (0.11)	Real beer tax, (1982$ 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.
Graduated Driver Licensing 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 Metropolitan Statistical Areas	82.17 (14.34)	Population in MSA counties (per 100 state population)
White Population	81.1 (7.74)	White population (per 100 state populaiton)

Appendix E:

Chapter 1 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-Suicde Rate	1989-2016	CDC Compressed Mortality File
Overall Crime Rate	1989-2018	Uniform Crime Reporting Program
Road Rage Crime Proxies	2001-2018	National Incident-Based Reporting System
Vehicle Miles Traveled	1989-2018	Highway Statistics
Licensed Driver Population	1989-2018	Highway Statistics
SYG variables	1989-2018	Cherney et al. (2018)
RTC variables	1989-2018	Donohue et al. (2019)
Police Employment	1989-2018	Uniform Crime Reporting Program
Personal Income and Transfer Payments	1989-2018	U.S. Bureau of Economic Analysis
Unemployment rate	1989-2018	U.S. Bureau of Labor Statistics
State Minimum Wage	1989-2018	University of Kentucky Center for Poverty Research
Poverty rate	1989-2018	Census Bureau
Beer tax	1989-2018	Silver and Macinko (2014); Beer Institute
Consumer price index	1989-2018	U.S. Bureau of Labor Statistics
Seat belt laws	1989-2018	Insurance Institute for Highway Safety
Graudated 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