# The Effect of Permitless Carry Laws on Homicide and Suicide

Jinlin Li Jeff DeSimone*

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**Abstract**

Since 2003, 28 U.S. states have enacted permitless carry laws, allowing concealed carry of handguns by legal gun owners without a specific permit to do so. Employing a difference-in-differences approach in annual state panel data spanning 1999 to 2020, we find that permitless carry adoption substantially raised rates of homicide and suicide by firearm without impacting non-firearm rates of either or reducing other types of crime. Firearm-related death increases are statistically significant and large even relative to a baseline of "shall issue" laws, which required a permit for concealed carry but with minimal costs and standards beyond those of gun ownership. Corresponding positive effects on federal background checks for handgun purchases suggest that permitless carry laws elevate the perceived net benefit of handgun ownership.

**JEL Codes: K14, K42, I18**

*Li: Department of Economics, University of Southern California, jinlinli@usc.edu; DeSimone, Department of Economics, Duke University, jeffrey.desimone@duke.edu. We thank Phil Cook and Jeff Weaver for suggestions and encouragement.

# 1 Introduction

The United States has the highest rates of gun ownership and gun-related crime among all developed countries worldwide, with over 48,000 documented deaths by firearm in 2022 alone (CDC, 2022). The nature of the relationship between gun control policies and violent crime has been extensively and contentiously debated. One set of regulations that have been frequently studied are those governing the concealed carry of handguns, specifically the widespread adoption since the 1980s of "shall issue" laws that facilitate the ability for legal gun owners to obtain concealed carry permits. The stated justification for shall issue legislation was to provide law-abiding citizens with the means to defend themselves, thereby serving as a mechanism to deter criminals. While early research on shall issue laws produced inconsistent evidence regarding their effectiveness (Bartley & Cohen, 1998; Benson & Mast, 2001; Donohue, 2004; Lott & Mustard, 1997; Ludwig, 1998; Rubin & Dezhbakhsh, 2003), more recent studies suggest that they are associated with higher rates of homicide and other violent crime (Donohue et al., 2023; Donohue et al., 2019; Luca et al., 2017; Siegel et al., 2017) .

Notwithstanding such evidence, since 2003, more than half of U.S. states have moved beyond shall issue by enacting permitless carry legislation that allows lawful gun owners to carry concealed handguns in public without the need for a specific permit to do so. Through 2024, 29 states – containing nearly 47% of U.S. residents and covering over two-thirds of its land area – have transitioned to permitless carry regimes. The associated further reduction in the cost of concealed carry raises questions about the potential impacts on firearm-related mortality and criminal victimization. In contrast to the extensive literature on shall issue laws, however, research on the impact of permitless carry laws is scarce.

This paper explores how firearm-related death and crime rates have evolved in states that have adopted permitless carry legislation relative to states that have not, addressing the notable gap in research that has persisted as the relevance of the policy has rapidly accelerated. Permitless carry laws are germane to the behavior and incentives of various actors

potentially involved with gun violence, including criminals, law-abiding citizens, and law enforcement agencies. As has been argued to motivate shall issue laws, the greater prevalence of lawful gun carrying likely to accompany permitless carry might deter criminality by raising the likelihood that an offender will encounter armed resistance (Lott & Mustard, 1997). The reduced administrative burden on law enforcement from no longer having to process or check for permits also makes available additional resources for deterring and solving crimes (Donohue et al., 2023; Donohue et al., 2019).

Meanwhile, several other considerations are prospectively have an offsetting effect in creating, rather than avoiding, gun deaths and crime. If police devote less effort to checking for gun violations, criminals face lower expected costs of carrying firearms (Donohue et al., 2023). Moreover, increased gun carrying in public – including firearms being left unattended in vehicles and unlocked at home to allow for greater accessibility – creates additional opportunities for gun theft, which is thought to be the primary method by which legally owned firearms come to be used in the commisson of crime (Billings, 2023; Cook, 2018; Donohue et al., 2023; Khalil, 2017).

It is further possible that the shift towards more permissive gun policy will increase demand for guns by law-abiding individuals. Previous research has demonstrated that changes in gun regulation can directly influence firearm demand (Depetris-Chauvin, 2015; Moshary et al., 2023). In the context of permitless carry, eliminating the monetary and inconvenience costs of permit acquisition removes barriers to legal gun carrying. The predicted consequence is an outward shift in gun ownership among those who consider gun carrying to be a substantive associated benefit, including people who are particularly concerned about self-defense or personal safety. A body of research showing a strong connection between legal gun ownership and rates of firearm homicide and suicide suggests that gun deaths might rise in response (Briggs & Tabarrok, 2014; Cook & Ludwig, 2006; David C. Vitt & Sawyer, 2018; Duggan, 2001; Johnson & Robinson, 2021; Lang, 2013; Tannenbaum, 2020).

In sum, the interplay between various agents with differing incentives combined with the

multifaceted mechanisms discussed above implies that the consequences of permitless carry adoption are not obvious a priori. This motivates our empirical investigation of whether permitless carry laws encourage homicide and suicide, and whether they discourage other types of crime. We then explore whether a concordant shift in firearm demand might be one of the underlying mechanisms driving these outcomes.

Our analysis uses annual state-level data from 1999-2020 to estimate the effects of permitless carry laws on firearm-related deaths and crime as well as the demand for handguns. We exploit the differential timing in law adoption across states to apply a difference-in-differences (DiD) identification strategy, comparing within-state changes in outcomes for states that adopted permitless laws to the analogous changes experienced by non-adopting states over the same period. To isolate effects attributable to permitless carry laws, we control for the presence of shall issue laws, which prevailed in every state that subsequently switched to permitless carry but were not universal in other states by the end of our sample period. Beyond two-way fixed effects to account for unobserved determinants of outcomes and law adoption that do not vary within states over time or across states at a specific time, our preferred models also include state-specific quartic time trends to account for smooth but nonlinear temporal variation in outcomes that could be correlated with adoption timing – though our results are robust to the simplifying assumption of linear state trends.

Following Cheng and Hoekstra (2013), we also include region-by-year fixed effects using the four U.S. census regions. This restricts comparisons to occur between states within the same census region. The implicit assumption is that in the absence of permitless carry, adopting states would have experienced changes in the outcomes studied similar to non-adopting states in the same region. Limiting the counterfactual outcome set to those of states in the same region is potentially important in our setting, given strong regional variation in attitudes and preferences surrounding firearms (Cheng & Hoekstra, 2013; Cook & Ludwig, 2006).

We further show that our results are unaffected by the inclusion of time-varying con-

trols for demographics, policing, and economic conditions. In addition, falsification tests verify that permitless carry laws affect neither deaths by longgun or non-firearm means nor crime unrelated to firearms. Moreover, to address potential policy endogeneity, we conduct event studies that provide graphical evidence indicating an absence of pre-adoption trends in firearm-related outcomes within states that implemented permitless carry laws relative to other states.

Our primary analysis relies on data from the National Vital Statistics System (NVSS) to estimate the effects of permitless carry laws on firearm-related mortality, overall and specifically by homicide and suicide. We find that in response to law adoption, rates of homicide and suicide by firearm increase by 23–26 percent and 5–7 percent, respectively, compared with rates in "may issue" or "no issue" states that issue concealed carry permits only with considerable discretion or not at all. These magnitudes are sufficient to make the impact of permitless carry laws statistically distinguishable from that of shall issue laws, which we in turn estimate to increase firearm homicides and suicides, significantly so in the former case.

Additional results leveraging data from the FBI Uniform Crime Reporting (UCR) Return A files ((Kaplan, 2021)) imply that permitless carry laws do not deter crime. Specifically, we estimate small and statistically insignificant effects of law adoption on rates robbery or aggravated assault, two forms of crime that seem especially susceptible to deterrence by a greater likelihood of would-be victims being armed. In contrast, we observe a positive, albeit statistically insignificant, uptick in firearm-involved robberies and aggravated assaults upon transition to permitless carry.

Finally, utilizing data on background checks for handgun purchases recorded through the National Instant Criminal Background Check System (NICS), we present evidence that handgun sales also increased significantly following the enactment of permitless carry legislation. This finding is consistent with the premise that the perceived benefit derived from handgun ownership is increased by the ability to concealed carry in public, for which the

cost of gaining status to legally do so is lowered considerably under permitless carry. This provides a potential mechanism for the observed increases in homicide and suicide rates once the requirement to obtain a permit in order to concealed carry is removed.

This paper contributes to various strands of literature in economics, criminology, and public health. First, our analysis improves the understanding of how concealed carry weapon (CCW) permitting laws impact gun violence. To the best of our knowledge, we are the first study in economics to analyze implications of the transition from shall issue to permitless carry laws. While the deregulation of CCW policy from the default of no or may issue began nearly half a century ago, laws specifically removing the need for permitting in order to concealed carry has emerged only in the last 15 years. Consequently, the existing research on CCW deregulation has focused on the move from no/may issue to shall issue policies (Barati, 2016; Bartley & Cohen, 1998; Benson & Mast, 2001; Donohue, 2004; Lott & Mustard, 1997; Luca et al., 2017; Ludwig, 1998; Rubin & Dezhbakhsh, 2003; Siegel et al., 2017). Recent work has provided consistent evidence that the implementation of shall issue laws is associated with intensified violent crime (Donohue et al., 2023; Donohue et al., 2019; McElroy & Wang, 2017) . By extending the analysis of CCW policy from shall issue to permitless carry laws, our paper substantially advances this line of inquiry, offering a comprehensive assessment of CCW deregulation.

Second, we contribute to the broader literature on the social costs of gun ownership. Previous research has consistently linked increases in gun prevalence with higher rates of suicide, homicide, and accidental shooting deaths (Balestra, 2018; Billings, 2023; Briggs & Tabarrok, 2014; David C. Vitt & Sawyer, 2018; Depetris-Chauvin, 2015; Duggan, 2001; Johnson & Robinson, 2021; Koenig & Schindler, 2023; Levine & McKnight, 2017; Tannenbaum, 2020). Additionally, various studies have shown that other policies beyond shall issue laws that ease restrictions on firearm access or use, including Castle doctrine laws (Cheng & Hoekstra, 2013; McClellan & Tekin, 2017), the absence of mandatory purchase delays (Edwards et al., 2018; Koenig & Schindler, 2023; Luca et al., 2017), child access prevention laws

(Anderson et al., 2021), and weaker gun regulations in general (Depetris-Chauvin, 2015) , predict higher rates of homicide and/or suicide by firearm. Our study adds to the evidence on these relationships, and is the first to highlight the role of permitless carry laws in this regard.

Third, our evaluation of impacts on background checks for handgun purchases complements the literature on the determinants of the demand for firearms. In particular, research by (Depetris-Chauvin, 2015), (Depew & Swensen, 2018), (Koenig & Schindler, 2023), (Levine & McKnight, 2017), and (Moshary et al., 2023) identify significantly greater responsiveness to exogenous shocks that shift gun demand outward in areas where gun policy is more permissive or attitudes towards guns are more favorable. Our paper likewise reveals that the implementation of less restrictive CCW permitting laws leads to a rise in handgun purchase rates.

The paper continues as follows. Section 2 presents background information on permitless carry laws and describes our data in further detail. Section 3 elaborates on our identification strategy. We present our main results and robustness checks in section 4, and investigate an increase in handgun demand as a potential mechanism in section 5. Finally, we offer concluding thoughts in section 6.

# 2 Concealed Carry Weapons Laws and Data

## 2.1 Concealed Carry Weapons Laws in the United States

The U.S. has undergone a major shift in concealed gun carrying policy over the last half century. Prior to the late 1970s, most states had either "no issue" laws that did not allow private citizens to carry concealed weapons in public for any reason or "may issue" laws that empower local authorities to decide whether to issue concealed carry permits to legal gun owners. Under a may issue regime, permit applicants must demonstrate "proper cause," or a need for safety concerns, to legally carry a weapon in public, with law enforcement

agencies granted substantial discretion in their evaluation of applications. Subsequently, an extensive campaign by gun rights advocates led to a widespread shift towards shall issue laws (Donohue et al., 2023), which remove the need to show proper cause and mandate that local authorities issue a permit to essentially all applicants who are not legally prohibited from possessing a gun, pay the associated fee, and complete any required safety training.

Beginning in 2003 with Alaska, and at an accelerated pace since 2015, states have further deregulated their concealed carry policies by transitioning from shall issue to permitless carry legislation, thereby allowing lawful gun owners to freely carry concealed handguns in public [1]. Permitless carry laws specifically eliminate mandatory fees, federal background checks, and safety training before being legally allowed to carry a concealed firearm. As of January 2025, 29 states do not require a permit for concealed firearm carrying, while 21 states issue permits on a shall issue or "may issue" basis.[2][3] Table1 lists the 14 states that transitioned from shall issue to permitless carry along with the 11 that switched from "may issue" or "no issue" to shall issue during our sample period of 1999-2020.[4]

[Insert Table 1 here]

[1]Vermont never had a law regulating concealed weapon carrying so has always been considered a permitless carry state.

[2]Washington, D.C., now operates under shall issue laws.

[3]Adding to the national trend of deregulating concealed carry regulation, in June 2022 the Supreme Court, in a 6-3 decision on New York State Rifle & Pistol Association v. Bruen, struck down New York's 111 year-old statute requiring individuals to show "proper cause" to obtain a concealed carry license, despite recent research demonstrating that this law, the Sullivan Act, reduced firearm-involved deaths (Depew & Swensen, 2022). The Bruen decision potentially sets a precedent for shall issue laws to become universally applied across the U.S., further underscoring the relevance of our study.

[4]Kansas and Missouri each progressed from "no issue" to shall issue and ultimately permitless carry during our sample period. Kansas switched from "no issue" to shall issue in 2007 and then to permitless carry in 2015; Missouri made the analogous changes in 2003 and 2017, respectively.

## 2.2 Data

### 2.2.1 Firearm Deaths

Our outcomes of primary interest come from the NVSS, which encompasses all U.S. death certificates so is considered to be the most comprehensive source for homicide and suicide data.[5] Using the Centers for Disease Control and Prevention (CDC) Wonder retrieval mechanism (CDC, 2020), we compile annual data on the number of homicides, suicides, and total deaths, separately by firearm and all other methods combined, in each state from 1999 to 2020.[6] We end our sample period in 2020 for two reasons. Importantly, 2020 is a year in which no new permitless carry laws took effect, whereas in contrast 10 states switched to permitless carry over the previous five years (including three in 2019) and another 14 did so the following four years (including five in 2021). In addition, this avoids any confounding impacts that the onset of the COVID-19 pandemic and the nationwide protests sparked by the death of George Floyd, both of which occurred during 2020, might have on our outcomes through any longer-term evolution of either that happens to be correlated with the adoption timing of subsequent permitless carry laws. Thus, 2020 provides a "clean avenue" for examining the impact of permitless carry laws in states that adopted them by 2019.

Using multiple cause of death data and ICD-10 codes from the NVSS,[7] for each mortality outcome we also separate deaths known to occur by long gun from other firearm-involved deaths. Given that long guns cannot be carried concealed, beyond any second-order substitution toward handguns, permitless carry laws should not directly affect mortality from or demand for long guns. Excluding deaths involving long guns from our data should therefore augment the power of our statistical analysis. Homicides and suicides by long gun also provide another set of falsification test outcomes, accompanying deaths of each type by means

[5] An alternative source of homicide data is the FBI's UCR Program, which provide the crime measures we later analyze, but UCR homicides are known to be a slight undercount.

[6] While the vast majority of gun deaths are by homicide or suicide, total firearm-related deaths also include those from accidental shootings, by legal intervention, and of undetermined intent.

[7] The NVSS contains ICD-10 codes for the underlying cause of each death, as well as decedent demographics, county of residence, and injury circumstances for deaths from an external cause.

other than firearm, to assess whether our findings might represent other factors correlated with homicide or suicide more generally rather than causal effects of permitless carry statues.

Figures 1 and 2 each display two time series of population-weighted firearm-related homicides and suicides per 100,000 individuals from 1999 to 2020, one representing treated states that adopted permitless carry during the study period and the other representing control states that did not. Before states besides Alaska enacted permitless carry laws, the trends in firearm homicides and suicides in treated and control states move in lockstep, supporting the parallel trends assumption of our eventual DiD analysis. As additional states switched to permitless carry, the trends in the two series increasingly diverge, with treated states ultimately experiencing pronounced increases in both homicides and suicide relative to control states.

[Insert Figure 1 & 2 here]

### 2.2.2 Robbery and Aggravated Assault

Our crime data come from the FBI Uniform Crime Reports (UCR) and cover all 50 states. Specifically, we utilize the UCR Return A files compiled by Jacob Kaplan (Kaplan, 2021), focusing on two categories of crime, robbery and aggravated assault. Although the UCR data are widely used to study crime, they are known to suffer from reporting issues (Koenig & Schindler, 2023; Targonski, 2011). The FBI collect the data monthly from local law enforcement agencies, some of which do not provide crime data for all 12 months of the year, leading to undercounts even without taking into account that some crime is unobserved by agencies. As a result, we include only reporting agencies that provide crime data for all 12 months of the year in our sample, and construct crime rates based on the population covered by these agencies each year rather than the overall state population.

We restrict attention to robberies and aggravated assaults, the two crimes that comprise the bulk of the UCR violent crime category (which also include homicides and rapes). Because both involve personal attacks with potentially severe immediate consequences, they

seem especially susceptible to increases in the likelihood of encountering armed resistance in states that allow permitless carry, and therefore to potentially be deterred by such legislation. This is especially true if victims are more likely to engage in armed resistance when the attempted crime itself involves firearm use, since beyond homicide these are the two types of crime most commonly committed with guns. Indeed, these are the the only two crime categories besides homicide for which the UCR, in their Return A files, specifically identifies incidents that involved firearm use. This enables us to analyze not only whether permitless carry laws have a deterrent effect on crime, but also whether firearm-related crime increases in adopting states, thereby offering more comprehensive insight into the impact of the laws. [8] For these reasons, robbery and aggravated assault are the crimes most widely studied by previous research in testing for deterrent effects of permissive concealed carry laws (Barati, 2016; Donohue et al., 2023; Donohue et al., 2019; Lott & Mustard, 1997). Table 2 presents summary statistics for robbery and aggravated assault, by firearm and overall, in both control and treated states.

Figures 3 and 4 display separate time series over our sample period for population-weighted rates of overall robbery and aggravated assault per 100,000 residents separately for treated and control states. For both crime categories, rates were initially higher in control states, with the gap closing – slightly for robbery and fully for aggravated assault – by the time states began to adopt permitless carry in earnest. As this occurred throughout the 2010s, the robbery gap continued to narrow, while aggravated assault rates in treated states eventually exceeded those in control states by about the same amount as they trailed them when the sample began. This suggests that at the very least, permitless carry laws may have limited deterrent effects on these two types of crime.

[Insert Figure 3 & 4 here]

[8] An alternative source of data used to study crime is the National Incident-Based Reporting System (NIBRS), which systematically categories crime by weapons involvement. However, relatively few states had fully converted from UCR to NIBRS until either late in our sample period or after it ended.

### 2.2.3 Handgun Sales

To explore one potential mechanism explaining the rise in homicides and suicides in states adopting permitless carry laws, we investigate whether their adoption raises the demand for handguns among law-abiding individuals. Transitioning from shall issue to permitless carry might not only increase concealed carrying among gun owners, but could also spur additional handgun purchases among individuals for whom the ability to concealed carry comprises a relatively large share of the benefit of handgun ownership. For some households, eliminating the monetary fees and inconvenience associated with obtaining a permit will lower the overall cost of acquiring a handgun that can be legally carried concealed, which now consists entirely of the initial handgun purchase, to a level below the corresponding perceived benefit. If for some of these households, this same cost exceeded the benefit of ownership without the ability to carry, then the reduced financial and procedural barriers accompanying permitless carry would increase handgun demand. A parallel argument can be made for additional handgun purchases by gun owners without permits to carry in response to the cost and complexity of the permitting process being eliminated. As the corresponding literature has consistently demonstrated that increased gun ownership and prevalence predicts higher rates of homicide and suicide, an increase in guns owned might potentially explain some of the homicide and suicide rate responses in adopting states (Briggs & Tabarrok, 2014; Cook & Ludwig, 2006; David C. Vitt & Sawyer, 2018; Duggan, 2001; Lang, 2013; Tannenbaum, 2020).

Our background check analysis employs state-level handgun check data from the FBI's National Instant Criminal Background Check System (NICS) as a proxy for gun sales. Since its establishment in late 1998, the NICS has been responsible for overseeing background checks for all firearm transactions. Federal regulations require that a NICS electronic background check (BGC) be performed for every firearm transaction conducted through a Federal Firearm Licensee (FFL). Given the lack of a centralized firearm sales database or alternative sources of information on gun prevalence at a level more disaggregated than the biannual Census division estimates of gun ownership available through the General Social Survey,

recent research has relied extensively on publicly available NICS data as a proxy for gun sales (Depetris-Chauvin, 2015; Koenig & Schindler, 2023; Lang, 2013; Levine & McKnight, 2017)

We compile NICS data on BGCs for sales of handguns and long guns in order estimate the impact of permitless carry laws on the demand for each gun type.[9]. Similarly to our approach for homicides and suicides, we employ long gun checks as a falsification test for our analysis of handgun demand: if anything, permitless carry would be expected to induce substitution from long guns to handguns, though any such shift would likely be second order in magnitude. Focusing exclusively on handgun and long gun checks also alleviates concerns raised by Koenig and Schindler (2023) related to including permit check counts when calculating guns sold. Specifically, our strategy avoids the potential for severe inflation of estimated handgun sales resulting from states conducting rechecks on existing permit holders, which several states implemented, often temporarily, during our sample period.[10][11]

Though BGCs are the best proxy for handgun sales of which we are aware, we nonetheless acknowledge their limitations. As a flow measure, BGCs do not reflect the total stock of handguns in circulation or rates of gun ownership. Further, BGCs are known to underestimate sales: not only is it almost certain that more BGCs are associated with purchases of multiple guns than are failed or otherwise do not involve a purchase, but moreover BGCs are

[9]The NICS handgun series lacks data for Connecticut in 2000. Additionally, for Pennsylvania, we suspect that handgun checks were falsely categorized as long gun checks from 1999 to 2014. The specifics of how we addressed these data anomalies are detailed in Appendix Section B.

[10]Some states do not require a separate BGC to purchase a firearm once a concealed carry permit is issued. In most of the permitless carry states, existing permit holders can still use concealed carry permits as alternatives to BGCs even after the adoption of permitless carry laws. Consequently, the handgun check variable may fail to capture increases in gun demand for existing permit holders who bypass BGCs using their permits.

[11]Another potential issue associated with bypassing handgun BGCs is that, when states adopt permitless carry, gun demand previously reported as permit-related BGCs may instead be categorized as handgun BGCs. This could mechanically inflate the number of observed handgun BGCs upon adoption, even if actual handgun demand remains unchanged. However, we believe this issue would have only minimal impact on our estimates. Among the 14 adopting states in our sample, six did not accept a concealed carry permit as a substitute for a background check at the time of purchase, eliminating this concern. In most of the remaining states, applicants were required to complete a state-approved firearms training course and obtain a certificate of completion – typically necessitating a handgun – before obtaining a concealed carry permit. Thus, we argue that most potential buyers were more likely to purchase their handgun before applying for a concealed carry permit, rather than the reverse, even before the permit requirement was lifted.

required for private transactions only in states with corresponding laws.[12] Despite these considerations, we believe that BGCs represent the most reliable substitute measure of handgun sales, and as such their changes in response to permitless carry adoption reasonably reflect changes in handgun sales.

### 2.2.4 Summary Statistics and Controls

We present summary statistics for our study variables in Table 2. Our time-varying controls include demographic information from the US Census on the share of the state population which is male, White, Black, and in several different age groups. To capture effects of deterrence by law enforcement and incapacitation through incarceration, we control for the number of police (Uniform Crime Reports 1999–2020) and state prison inhabitants (Bureau of Justice Statistics Bulletin 1999-2020) per 100,000 residents in each state. We also hold constant the state median household income and poverty rate to control for economic conditions.

[Insert Table 2 here]

To ensure that our results are not in fact partly attributable to other gun legislation, we also control for Stand Your Ground (SYG) laws, Child Access Prevention (CAP) laws, and mandatory handgun purchase delays laws, each of which previous research has demonstrated to influence firearm-involved deaths (Anderson et al., 2021; Cheng & Hoekstra, 2013; DeSimone et al., 2013; Edwards et al., 2018; Koenig & Schindler, 2023; McClellan & Tekin, 2017). To account for the fact that more background checks will be mechanically recorded in states that require them for all gun purchases, even those from unlicensed sellers, we likewise control for private sales background check laws, Our estimated permitless carry effects are therefore independent of any impacts that these additional legislative measures might have on firearm mortality, crime, and sales over our sample period.

[12]Miller et al. (2017) document that about 22% of firearm sales occur between private parties in states that do not require background checks for private transactions. As described in the Empirical Strategy section, all regression models control for the presence of state private sales background check laws.

# 3 Empirical Strategy

To study the impact of permitless carry laws on firearm-related mortality and crime, we leverage the within-state variation arising from the transition of 14 states from shall issue to permitless carry laws between 1999 and 2020. We employ a difference-in-difference (DiD) research design with staggered events to test whether gun deaths and crime exhibit greater changes in states that adopt permitless carry laws compared with those that do not, and focus primarily on within-Census region comparisons.

Formally, we estimate two-way fixed effects models using panel data encompassing the 50 U.S. states over the 22-year period from 1999 to 2020. The regression model is:

$$\ln(\text{outcome}_{it}) = \alpha + \beta_1 \text{Permitless}_{it} + \beta_2 \text{ShallIssue}_{it} + \Theta X_{it} + \gamma_i + \tau_t \times \rho_t + \gamma_i \times t + \epsilon_{it} \quad (1)$$

where $\ln(\text{outcome}_{it})$ is the natural log of our dependent variables per 100,000 population,[13] $X$ is the vector of controls outlined in the Data Section, $\gamma$ is a set of year fixed effects, $\tau$ is a set of state fixed effects, $\rho$ is a set of region by year fixed effects,[14] and $\gamma_i \times t$ are state-specific linear time trends. We also incorporate an additional specification that includes state-specific quartic trends to capture potential changes in the outcome variables over the period that are smooth but nonlinear[15]. As demonstrated in our Results Section, our estimates using state-specific quartic trends are qualitatively similar to, but more precise than, those using state-specific linear trends. All regressions are weighted by state population and specify standard errors that are robust to heteroskedasticity and clustered at the state level.

The specific variation we exploit is reflected by $\text{Permitless}_{it}$, a binary indicator that equals 1 in year $t$ if state $i$ adopts permitless carry legislation before July 1 of year $t$. If the law

[13] In the extremely rare case of zero homicide or suicide deaths of a specific type in a state during a year, we code log deaths as zero. Our next version of the paper will instead use the inverse hyperbolic sine transformation for any mortality measure that takes on a zero value for any observation.

[14] Permitless carry-adopting states are spread throughout the four Census regions: Alaska, Arizona, Idaho, and Wyoming in the West, Arkansas, Kentucky, Mississippi, Oklahoma, and West Virginia in the South, Kansas, Missouri, and South Dakota in the Midwest, and Maine and New Hampshire in the Northeast.

[15] We also run regressions with state-specific quadratic and cubic trends, the results for which are included in the Appendix.

is adopted on or after July 1, we code $\text{Permitless}_{it}$ to equal 1 starting in year $t+1$. This coding strategy effectively categorizes a state as allowing permitless carry if it does so for at least half of the year. Permitless carry adoption dates are listed in Table 1.

To distinguish the effects of permitless carry laws from those of shall issue laws, all models also control for the presence of the latter through the binary indicator $\text{ShallIssue}_{it}$. In states transitioning from shall issue to permitless carry, the value of $\text{ShallIssue}_{it}$ changes from 1 to 0 concurrently with that of $\text{Permitless}_{it}$ changing from 0 to 1. Additionally, our sample includes 11 states that transitioned from no or may issue to shall issue laws within our study period. For these states, the value of $\text{ShallIssue}_{it}$ switches from 0 to 1 in adoption year $t$ or $t+1$ depending on whether or not the date during $t$ is before July 1, mimicking our just-described approach for permitless carry law coding.

Our identification of $\beta_1$ relies on comparing changes in rates of mortality and crime in states that adopted permitless carry laws with contemporaneous analogous changes in non-adopting states. For $\beta_1$ to reflect the causal impact of permitless carry laws, we must assume that in the absence of permitless carry legislation, mortality and crime changes in adopting states would have been identical to those in nonadapting states from the same Census region. To investigate this assumption, we first present graphical evidence in Figure 1, 2, 3, and 4 to illustrate that the trends in homicides, suicides and crimes are substantively parallel in adopting and non-adopting states before the former transitioned to permitless carry. We then offer more formal evidence by estimating an event-study specification that includes 10 years of leading treatment variable indicators, along with 5 years of lagging indicators to examine dynamic law effects. This framework tests whether pre-adoption outcome trends confound our difference-in-difference results. The event study model is a modification of (1):

$$\begin{aligned}\ln(\text{outcome}_{it}) = \alpha + \sum_{\substack{\tau=-10 \\ \tau\neq -1}}^{5} \xi_\tau \text{PostPermitless}_{i,t-\tau} + \beta_2 \text{ShallIssue}_{it} + \Theta X_{it} \\ + \ \gamma_i + \tau_t + \rho_{rt} + \gamma_i \times t + \epsilon_{it}\end{aligned} \tag{2}$$

where $\ln(\text{outcome}_{it})$ is an outcome of interest in state $i$ and year $t$, and $\text{PostPermitless}_{i,t-\tau}$ is a set of binary indicators equal to 1 for year $\tau$ before ($\tau>0$) or after ($\tau<0$) law adoption. The year prior to permitless carry adoption is the omitted category. We present the results of our event study analysis in the robustness checks section.

Lastly, a growing body of recent empirical policy evaluation literature has raised concerns about potential biases in DiD estimates, particularly when adoption timing is staggered. In particular, the use of already-treated states as controls for later-adopting states could impart bias if the treatment effect is temporally heterogeneous (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). To address these concerns, we first perform the decomposition of treatment effects estimated by our DiD model using the methods proposed by Goodman-Bacon (2021). We then estimate our DiD model using the method proposed by de Chaisemartin and D'Haultfœuille (2020), which shows the immediate impact of law adoption. These results are also presented in our robustness check section.

# 4 Results

## 4.1 Homicide, Suicide, and Total Gun Deaths

Our results for homicide, suicide, and total firearm deaths are reported in Table 3,, Table 4, and Table 5, respectively. In these and subsequent tables, each column represents the DiD estimate from a unique specification, with all regressions controlling for state and year fixed effects, state specific linear trends, and a set of gun law controls. Our preferred models are those contained in columns 3 and 5. We add region-by-year fixed effects and time varying controls to the baseline model in column 3, and further add state-specific quartic trends (including quadratic and cubic trend terms) in column 5.

Table 3 indicates that regardless of specification, permitless carry laws consistently have a large and statistically significant positive effect on homicides by firearm. The effect strengthens when homicide counts exclude those committed by long gun. Specifically, we find that

permitless carry laws increase firearm-related homicides overall by about 21% and those excluding long guns by about 24% in our preferred specifications, regardless of whether the nonlinear trend terms are included.

In alignment with recent estimates of the relationship (Donohue et al., 2023; Donohue et al., 2019; Luca et al., 2017; McElroy & Wang, 2017; Siegel et al., 2017), our findings indicate that shall issue laws lead to a 14–15% increase in firearm homicides using the column 3 model. Allowing for quartic state trends in column 5, the effect diminishes in size to 9–10% but remains statistically significant.

We then employ a Wald test to investigate whether the impact on gun-related homicides of permitless carry laws are statistically distinct from that of shall issue laws. For overall firearm homicides, the effects of permitless carry and shall issue laws are statistically different at the 10% significance level conditional on quartic state trends in column 5. For firearm homicides excluding long guns, the effect of permitless carry is statistically greater than that of shall issue at the 10% level with just linear trends in column 3, but at the 5% level with nonlinear trends in column 5. Compared with shall issue laws and quartic state-specific trends, firearm homicide rates rise upon transitioning from shall issue to permitless carry laws by a statistically significant 13–14%.

[Insert Table 3 here]

While homicide, as the most socially costly form of gun crime, has long been a focus in the shall issue literature, suicide has received substantially less attention by research on CCW permitting. Both Luca et al. (2017) and DeSimone et al. (2013) reported indeterminate effects of shall issue laws, on total and firearm-related suicides among adults in the former case, and nonfatal self-inflicted gun injuries among youth aged 17 or younger in the latter. However, both studies were oriented toward estimating the impact of a different law – mandatory purchase delays and CAP laws, respectively – with shall issue law indicators included as a control in both cases. Similarly, in Table 4, our various specifications uniformly indicate positive but statistically insignificant effects of shall issue laws on rates of suicide by

gun regardless of whether the outcome measure includes long gun suicides, with magnitudes that never exceed 3%.

In contrast, our analysis in Table 4 reveals that permitless carry laws do have a positive and significant impact on firearm-related suicides. Regardless of the control variable set, we find that overall firearm suicides increase significantly upon permitless carry adoption, by 5–6% with linear state trends and 7% with quartic trends. Moreover, these effects statistically exceed those of shall issue laws at the 1% level in column 3 and the 5% level in column 5, indicating a 4–6% rise in gun suicides upon transitioning from shall issue to permitless carry. Excluding long guns from the suicide count, effects are slightly smaller and insignificant with linear trends, but rise to a statistically significant 9% upon controlling for quadratic trends.

[Insert Table 4 here]

Finally, in Table 5 we present the results for total firearm-related deaths, which along with homicides and suicides include fatalities from shootings that were accidental, resulted from legal intervention, and had undetermined intent. Similar to what we find in Table 3 and 4, permitless carry laws exhibit a consistently positive and significant impact on firearm-related deaths. Specifically, columns 3 and 5 suggest that relative to no and may issue regimes, permitless carry laws lead to an increase in firearm-related deaths of 10% overall and 11–13% excluding long guns. We also find that shall issue laws increase firearm-related deaths by 6% overall and 7% excluding long guns in column 3 with linear state trends, though both of these effects diminish in magnitude and become statistically insignificant when quartic state trends are included in column 5.

We again utilize a Wald test to determine whether the effects of permitless carry and shall issue laws statistically differ. The results indicate that in both columns 3 and 5 and regardless of whether long gun deaths are incorporated into the outcome, effects of permitless carry laws are statistically larger than those of shall issue laws at the 5% level. The corresponding estimates reveal that the switch from shall issue to permitless carry increases gun-involved deaths by 4–8%.

[Insert Table 5 here]

## 4.2 Crime Deterrence

Our deterrence results are reported for robberies in Table 6 and aggravated assaults in Table 7. Both tables are organized analogously to those for firearm-related deaths presented in the previous section.

The upper panel Table 6 indicates an absence of evidence that permitless carry laws deter overall robberies. None of the permitless carry coefficients are statistically significant. Including only linear state trends, estimates are positive and as large as 6.6%, though standard errors are considerably larger. Conditioning on quartic state trends flips the signs of the coefficients, but their magnitudes are below 1% and remain considerably smaller than the associated standard errors. Similarly, shall issue coefficients are never statistically significant, with estimated effects shifting even more dramatically from 8–10% with linear state trends to -4–5% with quartic trends, though the latter are again smaller than their respective standard errors. While the lower bounds of the confidence intervals for the column 5 estimates are consistent with sizable deterrence effects, the upper bounds instead suggest substantial jumps in robberies upon implementing shall issue and then permitless carry legislation.

We report the results for firearm-related robberies in the lower panel of Table 6. With linear state trends in Column 3, we observe that permitless carry laws bring about a 26.3% increase, which is statistically significant at the 10% level. With the non-linear time trend in Column 5, the estimate falls in magnitude and loses statistical significance, but remains large at 19.1%. The pattern is similar for shall issue laws, which we estimate to significantly increase firearm robberies by 20% controlling for linear trends but by a smaller and no longer significant 11% when nonlinear trends are added to the model.

To summarize, we fail to find convincing evidence that permitless carry laws deter overall robberies, with estimates suggesting that if anything their adoption led to more firearm-related robberies.

[Insert Table 6 here]

Results for aggravated assaults are presented in Table 7. For overall aggravated assaults, we observe a positive yet statistically insignificant estimate of 10.6% for permitless carry laws with linear state trends in column 3. With quartic state trends in column 5, the coefficient decreases substantially to 1.7% and remains statistically insignificant. As for shall issue laws, the results indicate an 11.5% increase with linear trends, which is statistically significant at the 10% level. However, with quartic trends, the effect diminishes to 0.5% and loses statistically significance. In sum,the results again fail to indicate deterrence effects of either law type.

Turning to aggravated assaults involving firearms, effects of both laws are uniformly statistically insignificant, but also uniformly positive with magnitudes of 10–16% for permitless carry and 10–11% for shall issue. Once again, if there is any effect of permitless carry legislation, it appears to be an increase rather than decline in aggravated assaults.

[Insert Table 7 here]

Summarizing the results for the two crime categories, we find little evidence of deterrence effects of permitless carry laws on violent crime. In contrast, gun-related violent crime is observed to rise, albeit statistically insignificantly, in permitless carry environments.

# 5 Robustness Checks

In this section, we perform robustness checks for the previouisly presented results. Our focus is primarily on firearm-related homicides, suicides, and total deaths, as the findings from the previous section provide strong evidence that permitless carry laws are associated with increases in these outcomes. Specifically, we first perform an event study analysis to examine the dynamic effects of permitless carry laws. Next, we conduct a falsification check to ensure that our findings on homicides and suicides are not driven by types of incidents that should

remain unaffected by permitless carry laws. Finally, we perform a Bacon decomposition (Goodman-Bacon, 2021) to verify that our DiD estimates are based on valid comparisons between treated and control groups. We also use an alternative estimator proposed by (de Chaisemartin & D'Haultfœuille, 2020) to account for multiple groups and periods with heterogeneous treatment effects in the DiD design.

## 5.1 Event Study Analysis

As highlighted in the Empirical Strategy section, our identification strategy hinges on the validity of the parallel trends assumption that in the absence of permitless carry laws, adopting states would have experienced changes in firearm-related outcomes similar to non-adapting states in the same Census region. To address this concern, we study the dynamic effects of permitless carry laws on total firearm-related deaths, homicides and suicides by estimating Equation (2), which is based on the specification presented in column 3 of our previous tables.[16] Figures 5, 6 and 7 illustrate the 10 year leading and 5 year lagging dynamic effects of permitless carry laws on total firearm-related deaths, homicides and suicides, with point estimates and standard errors reported in the Appendix.

The estimated pre-adoption coefficients in Figure 5 for overall firearm-related homicides show that, before the adoption periods, the 9 year leading coefficients are all statistically insignificant (the year before adoption is the omitted year). In contrast, the post-adoption coefficients are consistently positive and significant at the 5% level, with some significant at the 1% level. Additionally, the effect of the law exhibits a gradual increase in magnitude over time, ranging from 24.4% to 41.2% in the 1 to 5 years following adoption.

[Insert Figure 5 here]

For overall firearm-related suicides, we similarly find no statistically significant relationship between permitless carry laws and suicide outcomes prior to adoption, as demonstrated

[16] We also perform the same analysis for total firearm-related deaths, homicides and suicides excluding long guns, results for which can be found in the Appendix.

in Figure 6. For post-adoption periods, the estimated coefficients are again consistently positive in the 1 to 5 years after the law's adoption. The effect is statistically significant at the 10% level in the adoption period and 1% in the first year following adoption. The estimated effect rises from 5.4% in the adoption period to 8.4% one year after adoption.

[Insert Figure 6 here]

Figure 7 presents the results of the event study analysis for total firearm-related deaths. The event study plot closely resembles the pattern observed for firearm-related homicides. During the pre-adoption periods, all coefficients are statistically insignificant, supporting the parallel trends assumption. In the post-adoption periods, the estimates are consistently positive, ranging from 12% to 20% and are statistically significant at the 1% level in all periods other than four years post-adoption, in which it is significant at the 5% level.

[Insert Figure 7 here]

Together, these results suggest that permitless carry laws lead to a persistent increase in total firearm-related gun deaths and homicide rates in adopting states. While the long-term results for firearm-related suicides are less robust, there is nonetheless clear evidence that permitless carry adoption is associated with an immediate increase in firearm-related suicide rates.

## 5.2 Falsification Checks

Since permitless carry laws specifically regulate the concealed carrying of handguns, we posit that these laws should have minimal influence on non-gun and long-gun related deaths. To test this hypothesis and reinforce the validity of our previous findings, we employ non-gun and long-gun homicides and suicides as our falsification check outcomes.

Estimates examining the relationship between permitless carry laws and both non-gun and long-gun homicides are presented in Table 8. Our analysis reveals no statistically significant effects of permitless carry laws. Similarly, the results reported in Table 9 also show

no evidence of a statistically significant relationship between permitless carry laws and either non-gun or long gun suicides. Taken together, these results suggest that our previous findings for firearm-related homicides and suicides are unlikely to be driven by the types of incidents that should be unaffected by permitless carry laws, thus strengthening the validity of our main results.

[Insert Table 8 here]

[Insert Table 9 here]

## 5.3 Bacon Decomposition and Alternative Estimator

One important concern about the two-way fixed effects estimator raised by the recent econometric literature on staggered DiD is that the estimated parameter of interest ($\beta_1$ in our case) can be biased in the presence of staggered treatment timing (de Chaisemartin & D'Haultfœuille, 2020; Goodman-Bacon, 2021). In the context of our study, the DiD estimator is potentially biased when the treatment effect varies over time or across states due to comparisons between states that adopted the law earlier as controls versus later-adopting treated states.[17]

To test for this potential bias, we perform the decomposition from Goodman-Bacon (2021) and report the weight each comparison group receives in the Appendix.[18] The Bacon decomposition shows that the vast majority (95.4%) of our DiD estimates stem from comparisons between states that adopted permitless carry and those that never did (treated vs. never treated). Callaway and Sant'Anna (2021) point out the TWFE DiD estimator performs well when "never treated" groups are used as comparison groups to "eventually treated"

[17] A similar potential source of bias is from the use of always treated states within the control group for adopting states. However, this has minimal relevance for our study, as Vermont is the only always treated state in our sample.

[18] The treatment effect coefficients for each outcome variable are not reported as the Goodman-Bacon decomposition is most accurate when excluding covariates. Thus, the treatment effects from this exercise may not accurately reflect the impact of permitless carry laws. Nonetheless, we do obtain positive and significant results for all gun homicides, suicides, and total deaths variables.

groups. Thus, our DiD estimates are unlikely affected by issues raised in Goodman-Bacon (2021).

In addition, we use an alternative estimator to accommodate the possibility of differential treatment effects across time and states. Specifically, we estimate the model in (1) using the DiD design with multiple groups and periods with heterogeneous treatment effects proposed by de Chaisemartin and D'Haultfœuille (2020). The results are reported in Table 10. We see that our previous results are robust to the alternative estimator, with permitless carry laws bringing about statistically significant increases in overall firearm-related homicides, suicides, and total deaths. The magnitudes of the effects observed with the alternative estimator are similar to those obtained in our main analysis.

[Insert Table 10 here]

Based on the results from the Goodman-Bacon (2021) decomposition, which are further confirmed by the consistency of coefficients between our main findings and the results from the alternative estimator proposed by de Chaisemartin and D'Haultfœuille (2020), we believe that our standard DiD estimates accurately reflect the impact of permitless carry laws.

# 6 Mechanism

In this section, we analyze one mechanism that may explain the effect of permitless carry laws on firearm-related deaths. While an extensive body of literature has studied the effects of CCW permitting laws, and recent studies have largely reached a consensus that less restrictive permitting laws increase violent crime, there is little empirical evidence on the precise mechanisms that drive this increase (Donohue et al., 2023). In this paper, we focus on one simple but pivotal factor that might cause the rise in homicides and suicides in states adopting permitless carry laws: an increase in handgun ownership in those states. Previous research consistently shows that easier access to firearms and higher gun ownership rates are linked to increased rates of suicide, homicide, and accidental shootings (Anderson et al.,

2021; Billings, 2023; Cook & Ludwig, 2006; David C. Vitt & Sawyer, 2018; DeSimone et al., 2013; Duggan, 2001; Koenig & Schindler, 2023; Lang, 2013; Tannenbaum, 2020). We posit that the transition from shall issue to permitless carry laws may increase law-abiding citizens' demand for handguns since the law eliminates the costs associated with obtaining a permit and reduces the financial and procedural barriers to carry a handgun.

Formally, we test this mechanism by employing the same DiD framework from our main analysis and use FBI handgun background checks as a proxy for handgun demand to examine whether permitless carry laws increase handgun purchases in adopting states. Similar to the previous research design, we use FBI long gun background checks as the falsification test outcome for our proposed mechanism. We report the result of this analysis in Table 11. In our preferred specifications in columns (3) and (5), we observe that both permitless and shall issue laws have a positive and significant impact on the number of FBI handgun background checks, with the increase being more pronounced for permitless carry laws, which are associated with an 64% and 39% increase in handgun demand with linear (column 3) and quartic (column 5) state trends. In comparison, shall issue laws are linked to a smaller yet significant increase of 51% and 28% in handgun demand, respectively.

Contrary to our findings for handgun checks, our analysis does not reveal any evidence that permitless carry laws have an effect on long gun purchases. The estimates for permitless carry on long gun background checks reported in Table 11 are small and insignificant with both linear and quartic state trends. For shall issue laws, we similarly find no evidence of their impact on long gun demand with linear trends. However, with quartic trends, we observe a negative and marginally significant coefficient, suggesting potential substitution from long guns to handguns when concealed carry of the latter becomes less costly.

[Insert Table 11 here]

To examine the dynamic effects of permitless carry laws on gun ownership, we perform the same event study analysis as in the Robustness Check section, using FBI handgun checks as the outcome. Formally, we estimate equation (2) with the dependent variable being the

natural log of the number of FBI Handgun Background Checks per 100,000 population. The resulting event study plot is presented in Figure 8, with point estimates reported in the appendix. We observe that the pattern shown in Figure 8 closely resembles the event study analysis plots for firearm-related homicides and total firearm-related deaths in Figures 5 and 7. This strengthens the evidence for our hypothesis that the increase in firearm-related deaths is driven by the rise in handgun ownership following the adoption of permitless carry laws. Figure 8 shows that the estimated coefficients are all statistically insignificant prior to the adoption of permitless carry laws. Following the adoption, there is a clear upward trend in handgun ownership, with estimated coefficients ranging from 52% to 91% over one to five years. These coefficients are statistically significant at the 10% level in the current and post 3 periods, and at the 5% level in post 1,2,4 and 5 periods.

[Insert Figure 8 here]

Summarizing these results, we observe that permitless carry laws have a positive and persistent impact on handgun demand but no impact on long gun demand. As a potential mechanism for our findings on gun deaths, these results are consistent with the previous literature regarding the impact of gun ownership on gun related deaths. Thus, we present evidence suggesting that increase in handgun ownership is a contributing factor to the rise in firearm-related suicides, homicides, and total deaths in states that have adopted permitless carry laws.

# 7 Conclusion

This paper presents the first detailed analysis in the economic literature on the effects of permitless carry laws on firearm-related deaths and crime. Using state-level fatality and crime data from 1999 to 2020, we examine the impacts of these laws on firearm-related homicides, suicides, and total deaths along with robberies and aggravated assaults. Our findings indicate that permitless carry laws increase firearm-related homicides by approximately 21% to

24%, firearm-related suicides by 5.4% to 8.8%, and total firearm-related deaths by 9.9% to 12.7%, when compared to "may issue" and "no issue" laws. These effects of permitless carry laws are generally statistically larger than those of shall issue laws. In contrast, our analysis reveals no evidence that permitless carry laws impact non-gun and long-gun homicides, suicides, and total deaths. This falsification check result strengthens the validity of our main findings, as permitless carry laws specifically regulate the carrying of handguns and should have minimal impact on non gun and long gun related outcomes. Our study also reveals no evidence that permitless carry laws deter robberies or aggravated assaults, either overall or involving firearms.

We then investigate plausible mechanisms that may explain the rise in firearm-related deaths caused by permitless carry laws. Employing FBI handgun background checks as a proxy for handgun purchases, we find that permitless carry laws have a positive and significant impact on handgun ownership in adopting states. We believe that this increase in handgun ownership is an important contributing factor to the rise in firearm-related deaths in states that adopted permitless carry legislation, as previous research consistently shows that easier access to firearms and higher gun ownership rates are linked to increased rates of suicide, homicide, and accidental shootings (Anderson et al., 2021; Billings, 2023; Cook & Ludwig, 2006; DeSimone et al., 2013; Duggan, 2001; Koenig & Schindler, 2023; Lang, 2013).

In interpreting results from this study, it is important to recognize that the adopting states in our sample have relatively small populations. Despite this limitation, we believe our study carries significant policy implications, especially as an increasing number of states, including some of the most populous states in the country, have adopted permitless carry laws since 2020[19]. Though the exact effect of permitless carry laws in these states remains unknown due to data availability, the most conservative estimate derived from our results suggests that permitless carry laws might cause a 9.9% increase in firearm-related deaths in states that adopted permitless carry laws after 2020. These estimates underscore the necessity

[19]These include Texas in 2021, Ohio in 2022, and Florida in 2023.

for policymakers to consider the potential impacts on public safety when contemplating the deregulation of concealed handgun carry laws.

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# Figures

Figure 1: Firearm-related Homicides in Treated vs. Control States (1999-2020)

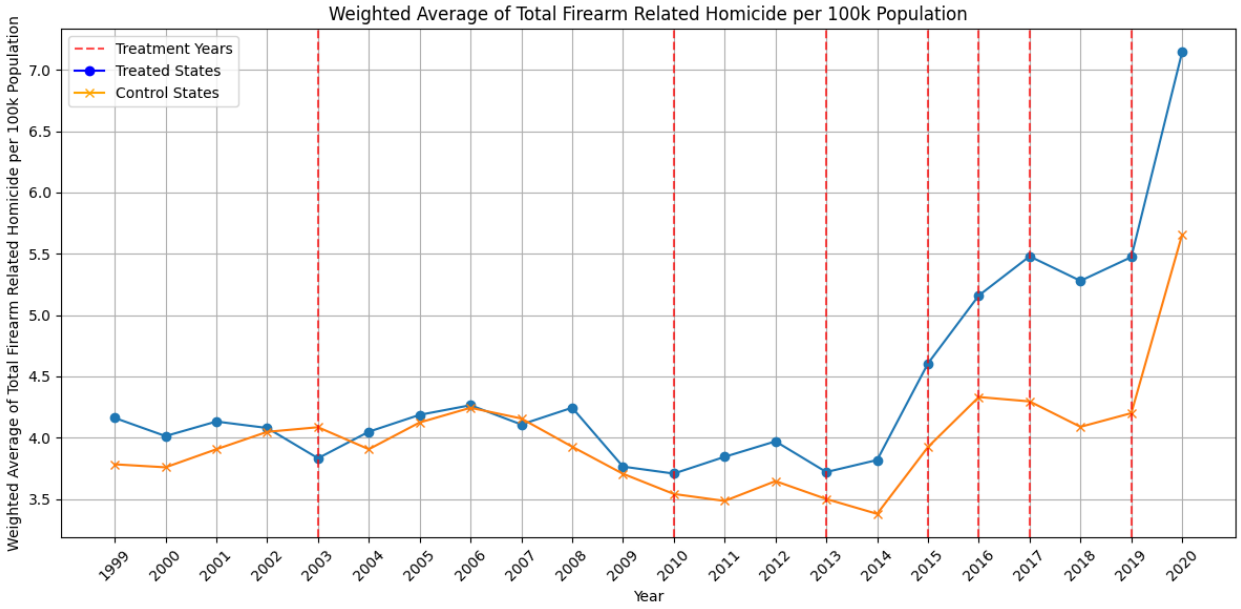



Figure 2: Firearm-related Suicides in Treated vs. Control States (1999-2020)

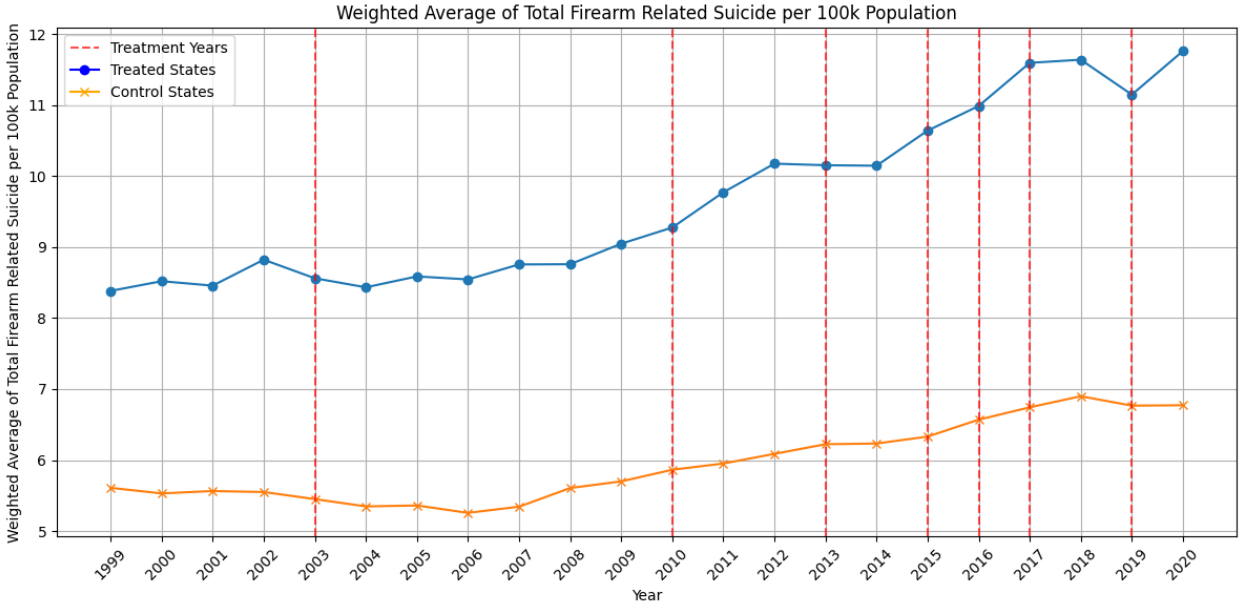


Figure 3: Robberies in Treated vs. Control States (1999-2020)

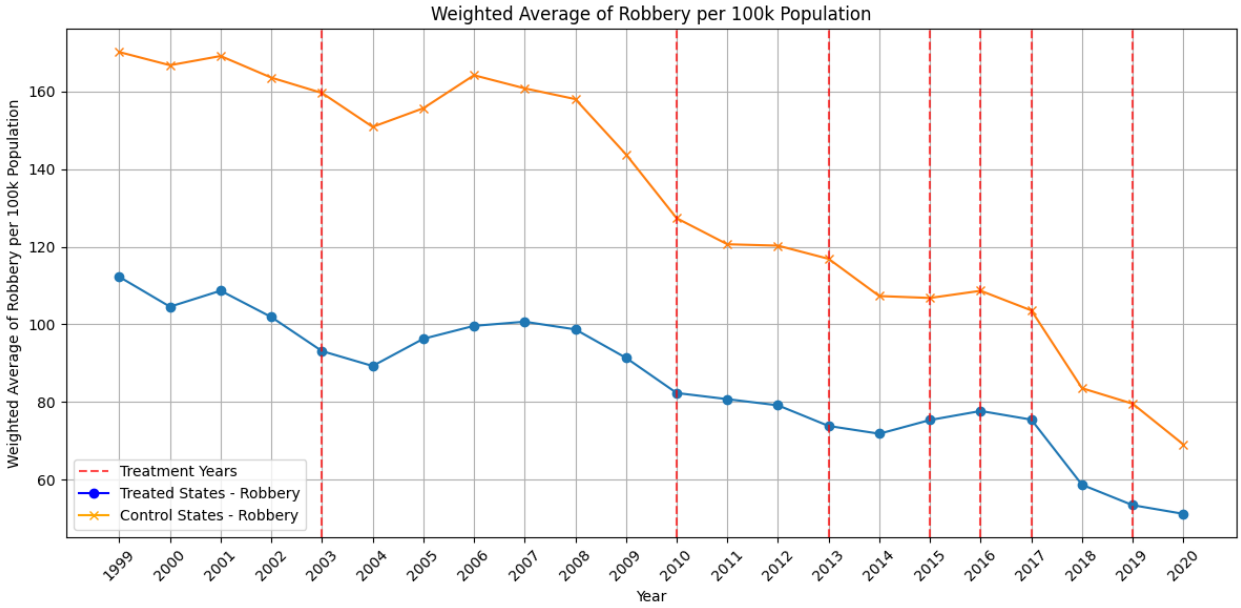



Figure 4: Aggravated Assaults in Treated vs. Control States (1999-2020)

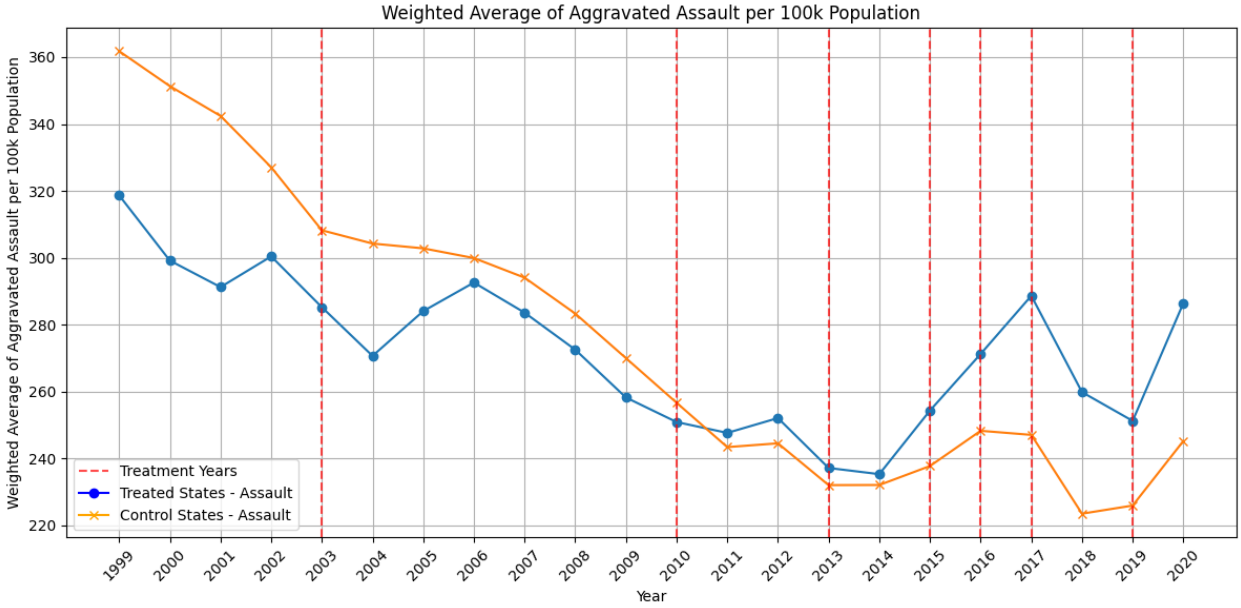
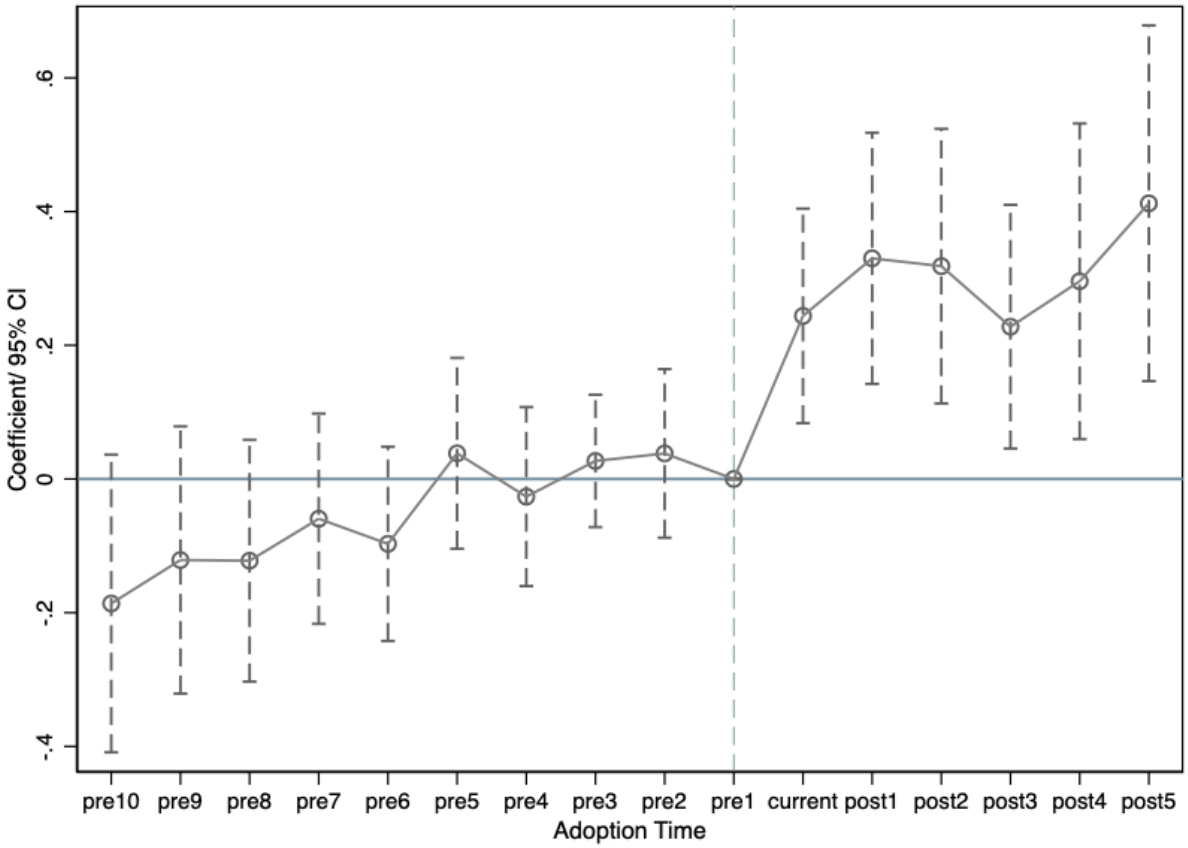
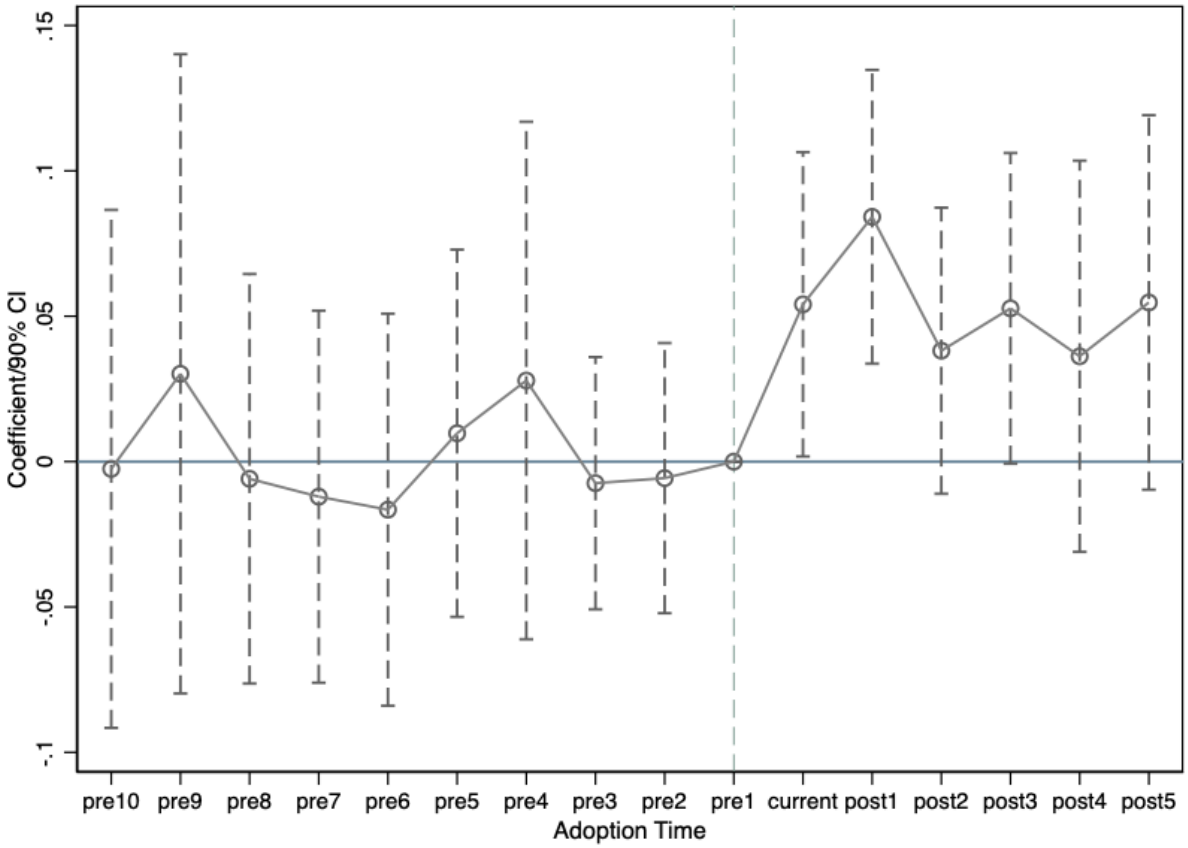


Figure 5: Dynamic effect of permitless carry laws on overall firearm-related homicides



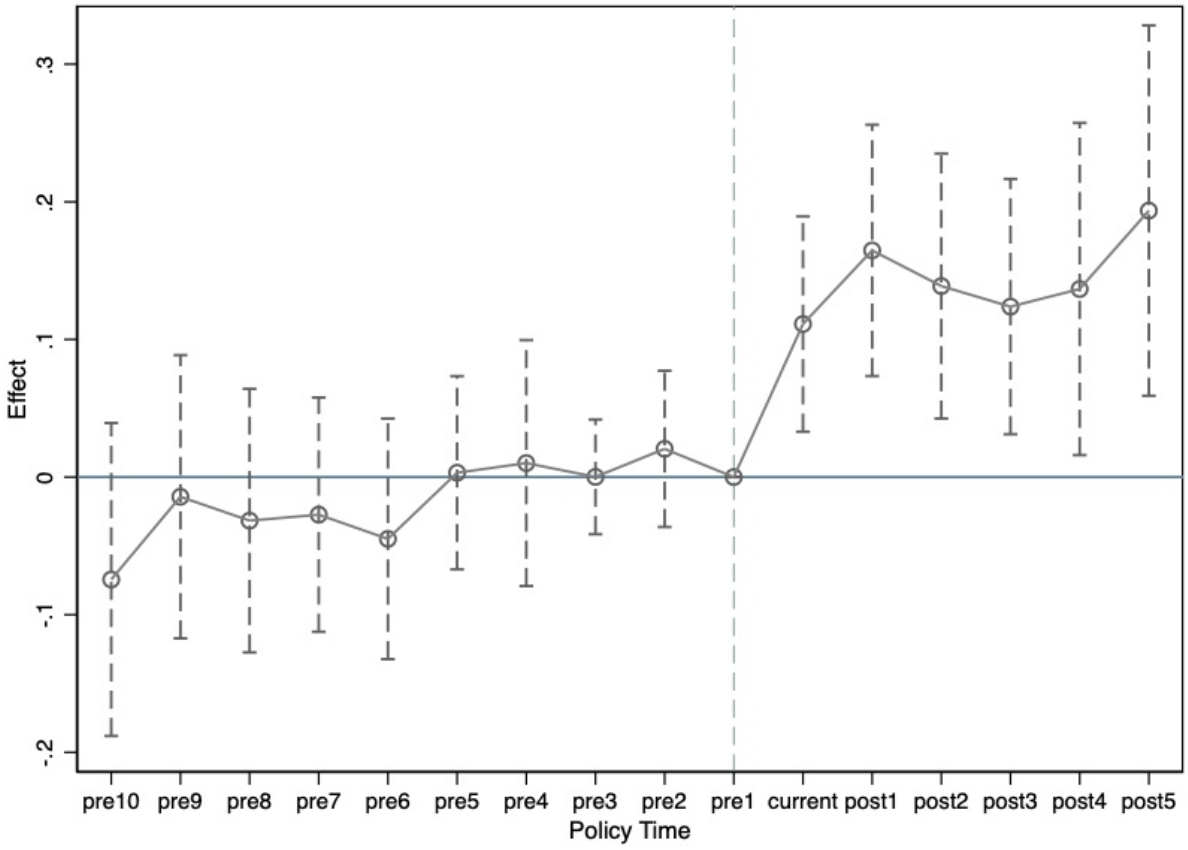

Note: The dots represent coefficients and the bands represent the 95% confidence interval.

Figure 6: Dynamic effect of permitless carry laws on overall firearm-related suicides



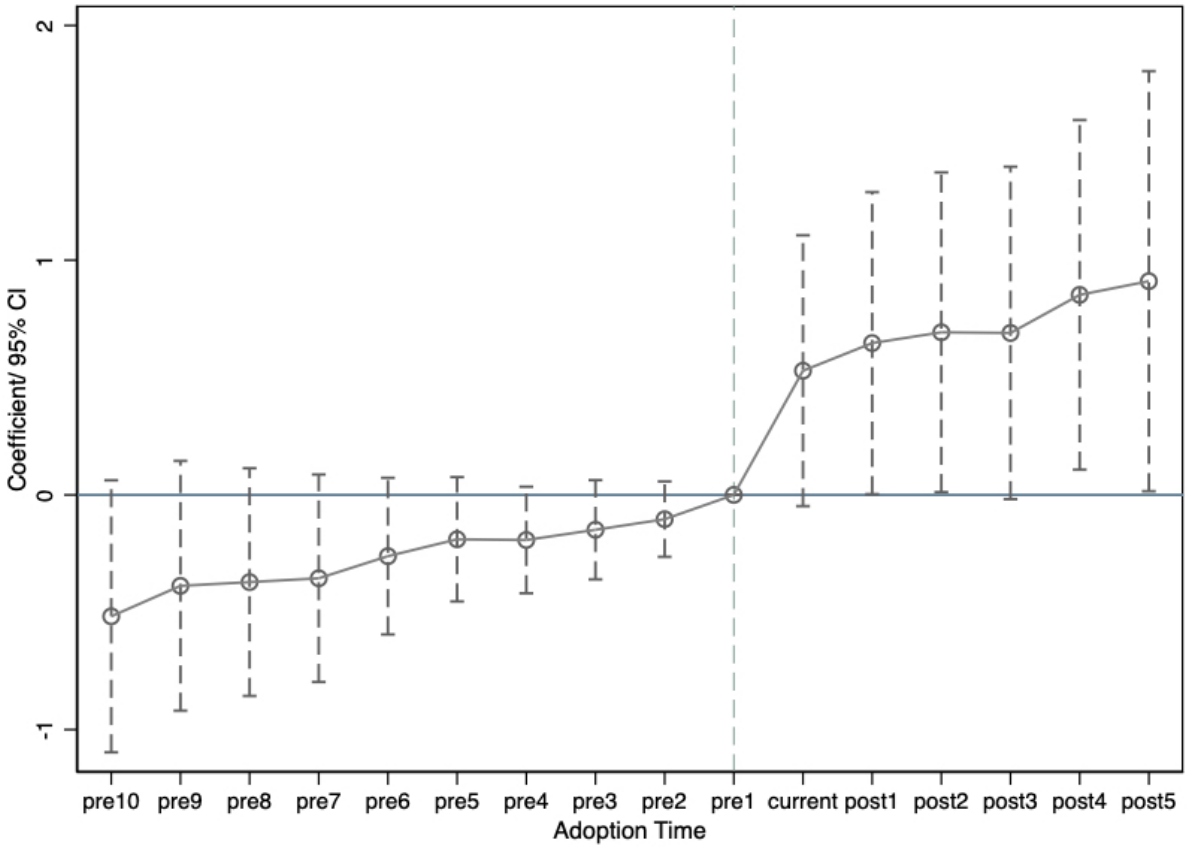

Note: The dots represent coefficients and the bands represent the 90% confidence interval.

Figure 7: Dynamic effect of permitless carry laws on total firearm-related deaths




Note: The dots represent coefficients and the bands represent the 95% confidence interval.

Figure 8: Dynamic effect of permitless carry laws on FBI handgun background checks




Note: The dots represent coefficients and the bands represent the 95% confidence interval.

# Tables

Table 1: Year of enactments of permitless carry and shall issue laws

| State | Effective Date |
|---|---|
| **"Shall issue" to Permitless** | |
| Alaska | 09/09/2003 |
| Arizona | 07/29/2010 |
| Arkansas | 08/16/2013 |
| Idaho | 07/01/2016 |
| Kansas | 07/01/2015 |
| Kentucky | 06/27/2019 |
| Maine | 10/12/2015 |
| Mississippi | 07/01/2015 |
| Missouri | 01/01/2017 |
| New Hampshire | 02/22/2017 |
| Oklahoma | 11/01/2019 |
| South Dakota | 07/01/2019 |
| West Virginia | 05/26/2016 |
| Wyoming | 07/01/2011 |
| **"May/No issue" to "Shall issue"** | |
| Colorado | 05/17/2003 |
| Illinois | 07/09/2013 |
| Iowa | 01/01/2011 |
| Kansas | 01/01/2007 |
| Michigan | 07/01/2001 |
| Minnesota | 04/29/2003 |
| Missouri | 10/11/2003 |
| Nebraska | 01/01/2006 |
| New Mexico | 01/01/2004 |
| Ohio | 04/01/2004 |
| Wisconsin | 11/01/2011 |

Table 2: Descriptive Statistics

| | **Treated States** | **Control States** |
|---|---|---|
| | Mean (Weighted by Population) | |
| **Dependent Variables** (Per 100,000 Population) | | |
| Total Gun Deaths | 14.72 | 10.34 |
| | (3.63) | (3.60) |
| Total Gun Deaths Excluding Long Guns | 12.75 | 9.28 |
| | (3.69) | (3.51) |
| Gun Homicides | 4.39 | 4.00 |
| | (2.42) | (1.80) |
| Gun Homicides Excluding Long Guns | 4.06 | 3.81 |
| | (2.36) | (1.75) |
| Gun Suicides | 9.63 | 5.96 |
| | (2.00) | (2.44) |
| Gun Suicides Excluding Long Guns | 8.06 | 5.11 |
| | (2.09) | (2.27) |
| Robberies | 83.51 | 135.13 |
| | (43.93) | (70.49) |
| Robberies-Gun | 37.78 | 46.54 |
| | (22.52) | (25.18) |
| Aggravated Assaults | 265.59 | 279.11 |
| | (108.22) | (120.68) |
| Aggravated Assaults-Gun | 65.23 | 54.02 |
| | (35.41) | (35.10) |
| Non-gun Homicides | 1.92 | 1.78 |
| | (0.66) | (0.83) |
| Non-gun Suicides | 6.22 | 5.93 |
| | (1.61) | (1.42) |
| Long Gun Homicides | 0.32 | 0.18 |
| | (0.20) | (0.11) |
| Long Gun Suicides | 1.56 | 0.85 |
| | (0.81) | (0.51) |
| FBI Handgun Checks | 2,357 | 1,467 |
| | (1,239) | (1,228) |
| FBI Long Gun Checks | 2,908 | 1,564 |
| | (902) | (973) |
| **Control Variables** | | |
| Median Household Income ($) | 46,489 | 52,939 |
| | (10,024) | (10,887) |
| Police per 100,000 Population | 212 | 245 |
| | (28) | (52) |

| | **Treated States** | **Control States** |
|---|---|---|
| | Mean (Weighted by Population) | |
| Prisoners per 100,000 Population | 493 | 419 |
| | (127) | (140) |
| Unemployment Rate (%) | 5.54 | 5.97 |
| | (1.75) | (2.15) |
| Poverty Rate(%) | 14.01 | 12.74 |
| | (3.63) | (2.84) |
| % White | 85.87 | 79.92 |
| | (9.26) | (7.74) |
| % Black | 10.55 | 14.32 |
| | (9.41) | (7.69) |
| % Male | 50.45 | 50.29 |
| | (1.21) | (7.68) |
| % Aged 21-39 | 25.88 | 26.79 |
| | (1.33) | (1.73) |
| % Aged 40-59 | 26.88 | 27.24 |
| | (1.66) | (1.40) |
| % Aged 60+ | 19.88 | 18.89 |
| | (2.89) | (3.32) |

Note: Each cell contains the mean with the standard deviation in parentheses. All variables except robbery and aggravated assault with guns have 1,100 observations. Data on whether robbery and aggravated assault were committed with a gun were missing from 1999 to 2005 for Illinois.

Table 3: Permitless Carry on Gun Homicides

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **All Gun Homicides** | | | | | |
| Permitless Carry | 0.159* | 0.208*** | 0.214*** | 0.203*** | 0.212*** |
| | (0.081) | (0.076) | (0.076) | (0.071) | (0.068) |
| Shall Issue | 0.075 | 0.131*** | 0.137*** | 0.091** | 0.085** |
| | (0.062) | (0.047) | (0.049) | (0.041) | (0.041) |
| **Gun Homicides Excluding Long Guns** | | | | | |
| Permitless Carry | 0.183** | 0.235*** | 0.243*** | 0.232*** | 0.243*** |
| | (0.079) | (0.075) | (0.075) | (0.064) | (0.063) |
| Shall Issue | 0.087 | 0.145*** | 0.154*** | 0.110** | 0.103** |
| | (0.065) | (0.049) | (0.049) | (0.045) | (0.045) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of gun homicides/gun homicides excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 4: Permitless Carry on Gun Suicides

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **All Gun Suicides** | | | | | |
| Permitless Carry | 0.051* | 0.058** | 0.054** | 0.073** | 0.069** |
| | (0.028) | (0.029) | (0.025) | (0.034) | (0.034) |
| Shall Issue | 0.014 | 0.025 | 0.017 | 0.020 | 0.021 |
| | (0.027) | (0.025) | (0.021) | (0.028) | (0.028) |
| **Gun Suicides Excluding Long Guns** | | | | | |
| Permitless Carry | 0.031 | 0.045 | 0.037 | 0.092** | 0.088** |
| | (0.032) | (0.030) | (0.030) | (0.037) | (0.036) |
| Shall Issue | 0.003 | 0.021 | 0.015 | 0.029 | 0.030 |
| | (0.029) | (0.023) | (0.023) | (0.031) | (0.031) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of gun suicides/gun suicides excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 5: Permitless Carry on Total Gun Deaths

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **All Gun Deaths** | | | | | |
| Permitless Carry | 0.086** | 0.106*** | 0.105*** | 0.098*** | 0.099*** |
| | (0.037) | (0.036) | (0.037) | (0.036) | (0.033) |
| Shall Issue | 0.031 | 0.060* | 0.059* | 0.036 | 0.033 |
| | (0.040) | (0.033) | (0.032) | (0.026) | (0.025) |
| **All Gun Deaths Excluding Long Guns** | | | | | |
| Permitless Carry | 0.081* | 0.110*** | 0.107*** | 0.125*** | 0.127*** |
| | (0.042) | (0.037) | (0.039) | (0.034) | (0.031) |
| Shall Issue | 0.031 | 0.067** | 0.070** | 0.048* | 0.045 |
| | (0.044) | (0.030) | (0.030) | (0.028) | (0.027) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of total gun deaths/gun deaths excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 6: Permitless Carry on Robberies

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Robberies** | | | | | |
| Permitless Carry | 0.025 | 0.066 | 0.041 | -0.009 | -0.008 |
| | (0.102) | (0.096) | (0.096) | (0.065) | (0.066) |
| Shall Issue | 0.077 | 0.099 | 0.084 | -0.041 | -0.048 |
| | (0.077) | (0.073) | (0.067) | (0.044) | (0.050) |
| **Robberies-Gun** | | | | | |
| Permitless Carry | 0.225 | 0.260* | 0.263* | 0.195 | 0.191 |
| | (0.148) | (0.144) | (0.152) | (0.149) | (0.143) |
| Shall Issue | 0.232** | 0.221** | 0.204** | 0.113 | 0.107 |
| | (0.114) | (0.090) | (0.096) | (0.134) | (0.135) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations (Robbery) | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |
| Observations (Robbery-Gun) | 1,093 | 1,093 | 1,093 | 1,093 | 1,093 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of robberies/robberies with a gun per 100,000 population and the robust standard errors are clustered at the state level. Data on whether robbery and aggravated assault were committed with a gun were missing from 1999 to 2005 for Illinois. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 7: Permitless Carry on Aggravated Assaults

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Aggravated Assaults** | | | | | |
| Permitless Carry | 0.074 | 0.095 | 0.106 | 0.015 | 0.017 |
| | (0.084) | (0.066) | (0.065) | (0.036) | (0.034) |
| Shall Issue | 0.098 | 0.127** | 0.115** | 0.009 | 0.005 |
| | (0.067) | (0.061) | (0.051) | (0.019) | (0.019) |
| **Aggravated Assaults-Gun** | | | | | |
| Permitless Carry | 0.130 | 0.102 | 0.123 | 0.159 | 0.157 |
| | (0.118) | (0.113) | (0.109) | (0.102) | (0.104) |
| Shall Issue | 0.114 | 0.103 | 0.098 | 0.103 | 0.101 |
| | (0.092) | (0.080) | (0.077) | (0.095) | (0.095) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations (Aggravated Assaults) | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |
| Observations (Aggravated Assaults-Gun) | 1,093 | 1,093 | 1,093 | 1,093 | 1,093 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of aggravated assaults/aggravated assaults with a gun per 100,000 population and the robust standard errors are clustered at the state level. Data on whether robbery and aggravated assault were committed with a gun were missing from 1999 to 2005 for Illinois. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 8: Permitless Carry on Non-gun and Long Gun Homicides

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Non-gun Homicides** | | | | | |
| Permitless Carry | -0.077 | -0.084 | -0.095 | 0.084 | 0.031 |
| | (0.060) | (0.058) | (0.060) | (0.051) | (0.033) |
| Shall Issue | 0.017 | 0.006 | 0.000 | 0.036 | 0.033 |
| | (0.040) | (0.030) | (0.032) | (0.026) | (0.025) |
| **Long Gun Homicides** | | | | | |
| Permitless Carry | 0.012 | -0.035 | -0.036 | -0.220 | -0.236 |
| | (0.149) | (0.142) | (0.146) | (0.201) | (0.198) |
| Shall Issue | -0.045 | -0.027 | -0.026 | -0.057 | -0.064 |
| | (0.049) | (0.056) | (0.061) | (0.139) | (0.139) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of non-gun homicides/non-gun homicides excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 9: Permitless Carry on Non-gun and Long Gun Suicides

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Non-gun Suicides** | | | | | |
| Permitless Carry | 0.034 | 0.018 | 0.003 | 0.029 | 0.034 |
| | (0.035) | (0.036) | (0.036) | (0.044) | (0.046) |
| Shall Issue | 0.013 | 0.006 | 0.001 | 0.022 | 0.024 |
| | (0.022) | (0.021) | (0.021) | (0.037) | (0.037) |
| **Long Gun Suicides** | | | | | |
| Permitless Carry | 0.069 | 0.040 | 0.077 | 0.009 | -0.001 |
| | (0.099) | (0.098) | (0.091) | (0.093) | (0.094) |
| Shall Issue | 0.049 | 0.038 | 0.022 | -0.046 | -0.047 |
| | (0.057) | (0.045) | (0.046) | (0.057) | (0.059) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of non-gun suicides/non-gun suicides excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 10: Permitless Carry on Gun Homicide, Suicide, Total Death: Alternative Estimator

| | **Gun Homicide** | **Gun Suicide** | **Total Gun Death** |
|---|---|---|---|
| **Post-Treatment** | | | |
| *Including Long Guns* | 0.173** | 0.099** | 0.111*** |
| | (0.076) | (0.045) | (0.039) |
| *Excluding Long Guns* | | | |
| | 0.186** | 0.084 | 0.117*** |
| | (0.075) | (0.054) | (0.037) |
| **Observations** | 1,100 | 1,100 | 1,100 |

Note: The Table shows results from an OLS estimation of Equation 1 using methods for Difference-in-Difference design with multiple groups and periods with heterogeneous treatment effects proposed by de Chaisemartin and D'Haultfœuille (2020). The coefficients reflect the immediate impact of permitless carry laws on each variable. Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of various gun death variables and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table 11: Permitless Carry on FBI Handgun and Long Gun Background Checks

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| **Handgun Background Checks** | | | | | |
| Permitless Carry | 0.538 | 0.541* | 0.638* | 0.368** | 0.393** |
| | (0.327) | (0.279) | (0.323) | (0.179) | (0.189) |
| Shall Issue | 0.529* | 0.517** | 0.512* | 0.270* | 0.276* |
| | (0.283) | (0.228) | (0.266) | (0.150) | (0.155) |
| **Long Gun Background Checks** | | | | | |
| Permitless Carry | 0.037 | 0.018 | 0.026 | -0.053 | -0.040 |
| | (0.061) | (0.053) | (0.061) | (0.059) | (0.062) |
| Shall Issue | -0.052 | -0.044 | -0.033 | -0.080* | -0.081* |
| | (0.045) | (0.053) | (0.037) | (0.044) | (0.044) |
| State and Year FE | Y | Y | Y | Y | Y |
| State Specific Linear Trend | Y | Y | Y | Y | Y |
| Region-by-Year FE | N | Y | Y | Y | Y |
| State Specific Quartic Trend | N | N | N | Y | Y |
| Time Varying Controls | N | N | Y | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of FBI Long Gun/Handgun Background Checks per 100,000 population and the robust standard errors are clustered at the state level. All specifications include state and year fixed effects, state-specific linear trend, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

# Appendix

## A: Supplementary Regression Results

This appendix provides supplementary regression results that complement the analyses presented in the main sections of the paper.

Table A1: Permitless Carry on Gun Homicides, Linear to Quartic Time Trends

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **All Gun Homicides** | | | | |
| Permitless Carry | 0.214*** | 0.100 | 0.190** | 0.212*** |
| | (0.076) | (0.059) | (0.072) | (0.068) |
| Shall Issue | 0.137*** | 0.104** | 0.100** | 0.084** |
| | (0.049) | (0.042) | (0.041) | (0.041) |
| **Gun Homicides Excluding Long Guns** | | | | |
| Permitless Carry | 0.243*** | 0.123** | 0.203*** | 0.242*** |
| | (0.075) | (0.055) | (0.068) | (0.063) |
| Shall Issue | 0.154*** | 0.127*** | 0.116*** | 0.103** |
| | (0.049) | (0.042) | (0.042) | (0.045) |
| State Specific Linear Trend | Y | Y | Y | Y |
| State Specific Quadratic Trend | N | Y | Y | Y |
| State Specific Cubic Trend | N | N | Y | Y |
| State Specific Quartic Trend | N | N | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of gun homicides/gun homicides excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table A2: Permitless Carry on Gun Suicides, Linear to Quartic Time Trends

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **All Gun Suicides** | | | | |
| Permitless Carry | 0.054** | 0.048** | 0.062** | 0.069** |
| | (0.025) | (0.023) | (0.027) | (0.034) |
| Shall Issue | 0.017 | 0.006 | 0.006 | 0.021 |
| | (0.021) | (0.015) | (0.018) | (0.028) |
| **Gun Suicides Excluding Long Guns** | | | | |
| Permitless Carry | 0.038 | 0.042 | 0.064** | 0.088** |
| | (0.029) | (0.033) | (0.030) | (0.037) |
| Shall Issue | 0.015 | 0.012 | 0.020 | 0.030 |
| | (0.023) | (0.023) | (0.021) | (0.031) |
| State Specific Linear Trend | Y | Y | Y | Y |
| State Specific Quadratic Trend | N | Y | Y | Y |
| State Specific Cubic Trend | N | N | Y | Y |
| State Specific Quartic Trend | N | N | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of gun suicides/gun suicides excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table A3: Permitless Carry on Total Gun Deaths, Linear to Quartic Time Trends

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **All Gun Deaths** | | | | |
| Permitless Carry | 0.105*** | 0.056** | 0.090*** | 0.099*** |
| | (0.037) | (0.025) | (0.031) | (0.033) |
| Shall Issue | 0.059** | 0.034 | 0.030 | 0.034 |
| | (0.032) | (0.022) | (0.020) | (0.025) |
| **All Gun Deaths Excluding Long Guns** | | | | |
| Permitless Carry | 0.107*** | 0.063** | 0.098*** | 0.127*** |
| | (0.040) | (0.029) | (0.031) | (0.031) |
| Shall Issue | 0.070** | 0.049** | 0.044** | 0.045 |
| | (0.030) | (0.025) | (0.022) | (0.027) |
| State Specific Linear Trend | Y | Y | Y | Y |
| State Specific Quadratic Trend | N | Y | Y | Y |
| State Specific Cubic Trend | N | N | Y | Y |
| State Specific Quartic Trend | N | N | N | Y |
| Observations | 1,100 | 1,100 | 1,100 | 1,100 |

Note: Each column represents a unique regression. Each observation is at the state-year level. The dependent variable is the natural log of the number of total gun deaths/gun deaths excluding long guns per 100,000 population and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table A4: Dynamic Effect of Permitless Carry Laws on Gun Homicides, Suicides, and Total Deaths

| | **Homicide** | **Suicide** | **Total Death** |
|---|---|---|---|
| **Years before the law** | | | |
| 10+ years prior | -0.186 | -0.003 | -0.074 |
| | (0.111) | (0.053) | (0.057) |
| 9 years prior | -0.121 | 0.030 | -0.014 |
| | (0.099) | (0.066) | (0.051) |
| 8 years prior | -0.122 | -0.006 | -0.032 |
| | (0.090) | (0.042) | (0.048) |
| 7 years prior | -0.059 | -0.012 | -0.027 |
| | (0.078) | (0.038) | (0.042) |
| 6 years prior | -0.097 | -0.016 | -0.045 |
| | (0.072) | (0.040) | (0.043) |
| 5 years prior | 0.038 | 0.009 | 0.003 |
| | (0.071) | (0.038) | (0.035) |
| 4 years prior | -0.026 | 0.028 | 0.010 |
| | (0.067) | (0.044) | (0.044) |
| 3 years prior | 0.027 | -0.007 | 0.000 |
| | (0.049) | (0.028) | (0.021) |
| 2 years prior | 0.038 | -0.006 | 0.021 |
| | (0.063) | (0.028) | (0.028) |
| 1 year prior (omitted) | . | . | . |
| | (.) | (.) | (.) |
| **Years after the law** | | | |
| current year | 0.244*** | 0.054* | 0.111*** |
| | (0.080) | (0.031) | (0.039) |
| 1st year | 0.330*** | 0.084*** | 0.165*** |
| | (0.093) | (0.030) | (0.045) |
| 2nd year | 0.318*** | 0.038 | 0.139*** |
| | (0.102) | (0.029) | (0.048) |
| 3rd year | 0.228** | 0.053 | 0.124*** |
| | (0.091) | (0.032) | (0.046) |
| 4th year | 0.296** | 0.036 | 0.137** |
| | (0.117) | (0.040) | (0.060) |
| 5th year | 0.412*** | 0.055 | 0.194*** |
| | (0.132) | (0.038) | (0.067) |

Note: Each column represents a regression for different outcome variables. Each observation is at the state-year level. The dependent variable is the natural log of the number of various gun death variables and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table A5: Dynamic Effect of Permitless Carry Laws on Gun Homicides, Suicides, and Total Deaths (Excluding Long Guns)

| | **Homicide** | **Suicide** | **Total Death** |
|---|---|---|---|
| **Years before the law** | | | |
| 10+ years prior | -0.210* | -0.034 | -0.104 |
| | (0.123) | (0.077) | (0.070) |
| 9 years prior | -0.142 | 0.019 | -0.026 |
| | (0.103) | (0.062) | (0.053) |
| 8 years prior | -0.148 | 0.001 | -0.028 |
| | (0.104) | (0.056) | (0.054) |
| 7 years prior | -0.059 | -0.026 | -0.027 |
| | (0.087) | (0.046) | (0.042) |
| 6 years prior | -0.111 | -0.022 | -0.046 |
| | (0.087) | (0.043) | (0.046) |
| 5 years prior | 0.016 | 0.010 | 0.006 |
| | (0.083) | (0.047) | (0.036) |
| 4 years prior | -0.010 | 0.017 | 0.009 |
| | (0.072) | (0.053) | (0.043) |
| 3 years prior | 0.025 | -0.007 | 0.004 |
| | (0.053) | (0.032) | (0.024) |
| 2 years prior | 0.029 | -0.004 | 0.027 |
| | (0.076) | (0.034) | (0.029) |
| 1 year prior (omitted) | . | . | . |
| | (.) | (.) | (.) |
| **Years after the law** | | | |
| current year | 0.258*** | 0.041 | 0.120*** |
| | (0.078) | (0.032) | (0.038) |
| 1st year | 0.374*** | 0.079** | 0.185*** |
| | (0.090) | (0.039) | (0.049) |
| 2nd year | 0.369*** | 0.036 | 0.162*** |
| | (0.100) | (0.043) | (0.056) |
| 3rd year | 0.261*** | 0.048 | 0.142** |
| | (0.094) | (0.042) | (0.055) |
| 4th year | 0.348*** | 0.008 | 0.142** |
| | (0.124) | (0.047) | (0.065) |
| 5th year | 0.472*** | 0.010 | 0.194** |
| | (0.134) | (0.079) | (0.085) |

Note: Each column represents a regression for different outcome variables. Each observation is at the state-year level. The dependent variable is the natural log of the number of various gun death variables(excluding long guns) and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

Table A6: Goodman-Bacon Decomposition Weights

| Comparion Group | Weight |
|---|---|
| Timing Groups | 0.044 |
| Always vs. Timing | 0.002 |
| Never vs. Timing | 0.954 |

Note: The Table shows results from decomposition of difference- in-difference estimator into weight each treatment effect receives using method proposed by Goodman-Bacon (2021). Timing Groups include Early Treated Group vs. Later Treated Groups and Later Treated Groups vs. Early Treated Groups. For details refer to Goodman-Bacon (2021).

Table A7: Dynamic Effect of Permitless Carry Laws on FBI Handgun Checks

| | **FBI Handgun Checks** |
|---|---|
| **Years before the law** | |
| 10+ years prior | -0.517 |
| | (0.298) |
| 9 years prior | -0.387 |
| | (0.265) |
| 8 years prior | -0.372 |
| | (0.241) |
| 7 years prior | -0.355 |
| | (0.220) |
| 6 years prior | -0.261 |
| | (0.166) |
| 5 years prior | -0.189 |
| | (0.132) |
| 4 years prior | -0.192 |
| | (0.113) |
| 3 years prior | -0.149 |
| | (0.105) |
| 2 years prior | -0.103 |
| | (0.080) |
| 1 year prior (omitted) | . |
| | (.) |
| **Years after the law** | |
| current year | 0.523** |
| | (0.287) |
| 1st year | 0.647** |
| | (0.320) |
| 2nd year | 0.693** |
| | (0.339) |
| 3rd year | 0.690* |
| | (0.352) |
| 4th year | 0.852** |
| | (0.371) |
| 5th year | 0.911** |
| | (0.445) |

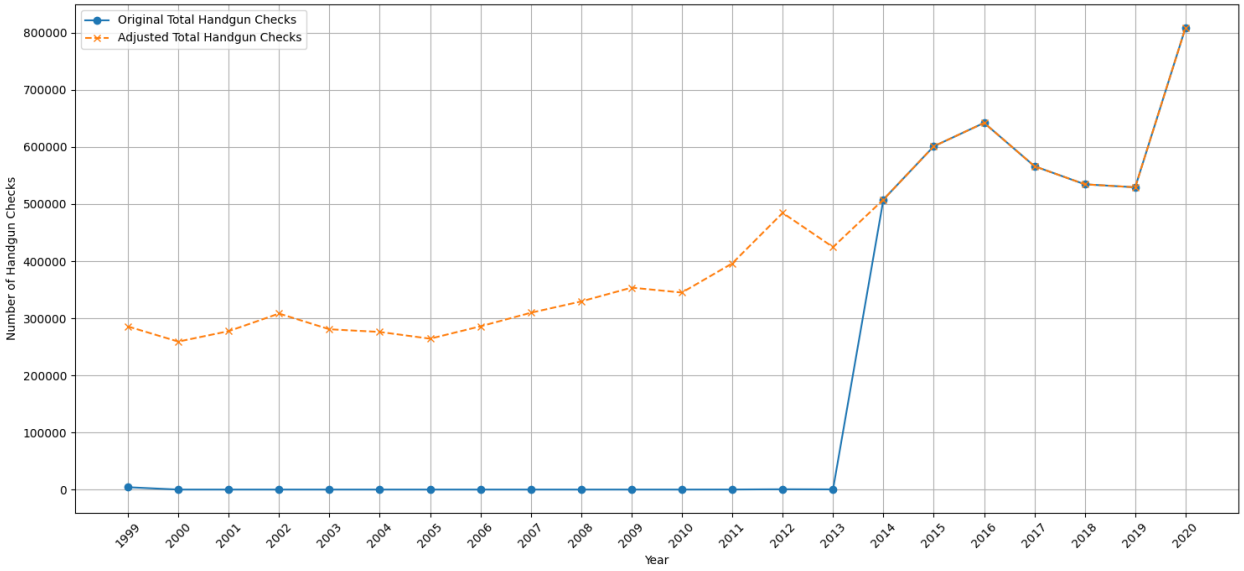
Note: Each observation is at the state-year level. The dependent variable is the natural log of the number of FBI handgun checks per 100,000 population, and the robust standard errors are clustered at the state level. Each regression includes state and year fixed effects, region by year fixed effect, state-specific linear trend, time-varying controls, and a set of gun law controls. All regressions are weighted by state population. *p $<$ 0.10, **p $<$ 0.05, ***p $<$ 0.01.

## B: Processing FBI Background Check Data

Upon analyzing the state level handgun and long gun data series from NICS, we noticed that the handgun check count for Connecticut is zero for 2000 and remained suspiciously low in 2001 relative to the remaining series. Additionally, for Pennsylvania, the absence of handgun checks until 2015 leads us to suspect that handgun checks were falsely categorized as long gun checks from 1999 to 2014.

To adjust for the missing handgun checks through 2014 in Pennsylvania, we first calculated the average handgun to total checks ratio from 2015 to 2020, a period when data recording became reliable. This ratio was then applied to the total check data from 1999 to 2014 to estimate the handgun check figures for those years. Figure A1 illustrates the original and adjusted total handgun check series for Pennsylvania from 1999 to 2020.

Figure A1: Original and adjusted total handgun check series for PA from 1999 to 2020.




Similarly for Connecticut, we first calculated the average handgun to total checks ratio from 2002 to 2005 then applied this ratio to the total check data from 2000 to 2001 to estimate the handgun check in those years. Figure A2 illustrates the original and adjusted total handgun check series for Connecticut from 1999 to 2020.

Figure A2: Original and adjusted total handgun check series for CT from 1999 to 2020.

