The Effect of Permitless Carry Laws on Gun Deaths and Crime

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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 2023, we find that relative to a baseline of shall issue laws, which require a permit for concealed carry but with minimal costs and qualifications beyond those of gun ownership, permitless carry adoption has raised handgun death rates by a statistically significant 3.2 percent on average. This response is attributable primarily to a rise in handgun homicides, though unintentional handgun deaths likewise are significantly increased, while long gun and nongun deaths are unaffected. The significant handgun homicide effect is observed in both CDC and FBI data, and is accompanied by a nearly nine percent increase in aggravated assaults by gun and large rises in forced entry burglaries and overall theft. Corresponding jumps in the prevalence of licensed gun dealers and background checks for handgun purchases, but not non-dealer/firearm licensees or long gun checks, suggest that the laws elevate the perceived net benefit of handgun ownership.

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1 Introduction

The U.S. has the highest rates of gun ownership and gun-related crime among all developed countries worldwide, with nearly 189,000 documented deaths by firearm during 2020-2023 alone (CDC, 2023). 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 is 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., 2019, 2025; 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 (including Vermont, which has never required a permit for concealed carrying) – 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 carrying 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 residents, and law enforcement agencies. As has been argued to motivate moving from may- to shall-issue, 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., 2019, 2025).

Meanwhile, several other considerations prospectively have an offsetting effect in creating, rather than avoiding, gun deaths and crime. If the willingness of law enforcement to confront criminals is also deterred by the possibility of encountering armed resistance, criminals face lower expected costs of carrying firearms (Donohue et al., 2025). Moreover, increased gun carrying in public – including firearms being left unattended in vehicles and unlocked at home to allow for easier access – 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., 2025; Khalil, 2017).

It is further possible that the shift toward less restrictive 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, 2024; 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 elevate gun deaths and crime 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 the outcomes that we observe.

Our analysis uses annual state-level data from 1999-2023 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 may issue laws in states that had not yet transitioned to shall issue. This makes shall issue laws, which prevailed in every state that subsequently switched to permitless carry and are nearly universal in other states by the end of our sample period, the baseline of comparison for our estimates. 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 cubic time trends to account for smooth but nonlinear temporal variation in outcomes that could be correlated with adoption timing.

Following Cheng and Hoekstra (2013), we also include year-by-census region fixed effects, thus restricting comparisons to between states within the same 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 impor-

tant in our setting, given strong regional variation in attitudes and preferences surrounding firearms (Cheng & Hoekstra, 2013; Cook & Ludwig, 2006).

Using CDC National Vital Statistics System (NVSS) mortality data, we define handgun deaths as all gun deaths other than those involving long guns. Our main finding is that permitless carry adoption predicts an increase in handgun deaths of about 0.5 per 100,000 residents, or 3.2 percent of the mean across the 25 sample adopting states in the year before the law took effect. This estimate is robust to including time-varying controls for other gun policies, demographic characteristics, economic conditions, and other factors that might affect deaths and crime, as well as to changing the polynomial degree of the included nonlinear state trends, not restricting comparisons to states in the same census region or further restricting them to the same census division, and not weighting by population. The effect is smaller, but still significant at 10 percent, using a constant elasticity model.

Perhaps unsurprisingly, roughly 70 percent of the handgun death effect is attributable to an increase in handgun homicides, which are predicted to rise by 5.6 percent relative to their pre-adoption mean. Another nearly 10 percent of the overall effect is explained by a 23 percent increase in unintentional handgun deaths. The remainder results from a higher, albeit insignificantly so, rate of handgun suicides. In contrast, corresponding changes in deaths from each of these three injury types occurring by long gun or nongun means are relatively small and statistically insignificant.

Our crime results leverage data from the FBI Uniform Crime Reporting (UCR) Return A files ((Kaplan, 2025)). The UCR handgun homicide effects are quite similar in magnitude to those based on the CDC data and stem nearly entirely from murder and non-negligent manslaughter. While the impact of permitless carry on robberies using a gun is effectively zero, gun-involved aggravated assaults are predicted to rise by nearly nine percent, contributing to statistically significant increases in overall rates of aggravated assault and, at the 10 percent level, total violent crime. Again, analogous changes in nongun and, for UCR homicides, long gun forms of death are statistically insignificant.

Likewise, our results are inconsistent with the conjectured deterrent effect of less restrictive concealed carry regulations on property crime. Instead, we estimate that permitless carry legislation brings about statistically significant increases in burglary, mostly attributable to more burglaries with forced entry, and theft, ultimately leading to significant rises in overall property and index crime.

Finally, utilizing data on federal firearm licensees (FFLs) from the ATF and background checks for gun purchases and permits recorded through the National Instant Criminal Background Check System (NICS), we investigate whether handgun demand responded to permitless carry enactment. Statistically significant increases in both dealer FFLs and handgun transfer checks suggest that handgun sales did indeed rise in response. This finding is consistent with the premise that the perceived benefit derived from handgun ownership is enhanced by the ability to carry a concealed handgun in public, for which the full cost of obtaining legal status to do so is lowered considerably by the transition to permitless carry. This offers a potential mechanism for the observed increases in handgun death and gun crime rates upon removing the permit requirement for concealed carrying. However, the presence of non-dealer and non-firearm licensees and rate of long gun transfer checks are unaffected by permitless carry passage, while as expected NICS permit checks decline in response.

To assess the possibilities of endogenous policy adoption and effect heterogeneity over time, respectively, we estimate event study models and stacked regressions using only non- and later-adopting states as controls. While both of these exercises suggest that our difference-in-differences estimates might be overstated for property crimes, results for handgun deaths and gun assaults are similar in stacked models. Event study graphs for handgun outcomes show flat pre-trends followed by sharp increases that begin to emerge in the year before adoption, consistent with anticipatory behavior once it becomes apparent that permitless carry is being considered by state legislative bodies.

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 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 have emerged only in the last 15 years. Consequently, the existing research on CCW deregulation has focused on the move from no and 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., 2019, 2025; 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 homicide, suicide, accidental shooting deaths, and criminal victimization more generally (Balestra, 2018; Billings, 2023; Briggs & Tabarrok, 2014; Cook & Ludwig, 2006; David C. Vitt & Sawyer, 2018; Depetris-Chauvin, 2015; Duggan, 2001; Johnson & Robinson, 2024; 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 gun dealer presence and background checks for hand-

gun 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 sales.

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 prohibited private citizens from carrying concealed weapons in public for any reason or "may issue" laws that empowered local authorities to decide whether to issue concealed carry permits to legal gun owners. Under may-issue regimes, permit applicants were required to demonstrate "proper cause," such as specific safety concerns, to legally carry a weapon in public, and law enforcement agencies were allowed substantial discretion in evaluating applications. Subsequently, an extensive campaign by gun rights advocates led to a widespread shift toward shall-issue laws (Donohue et al., 2025), which eliminated the proper cause requirement and mandated that local authorities issue permits to nearly all applicants who were not legally prohibited from possessing a gun, conditional only on payment of the associated fee and completion of

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 adopting permitless carry legislation. Between 2003 and 2019, 14 states transitioned from shall-issue to permitless carry. After a brief lull in 2020 during which no state converted to permitless carry, the number of states to do so roughly doubled, from 14 to 26, just between 2021 and 2023. As of October 2025, 29 states no longer require a permit to carry a concealed firearm.[1] Permitless carry laws specifically eliminate the additional fees, federal background checks, and safety training requirements that shall issue laws mandated for carrying a concealed firearm. The remaining 21 states and D.C. continue to issue permits on a shall- or may-issue basis.

Further contributing to the national trend toward less restrictive concealed carrying regulation, in June 2022 the U.S. Supreme Court's 6–3 decision in *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 upon initial enactment (Depew & Swensen, 2022). In the wake of the *Bruen* decision, New York and five other states that until then had operated under may-issue laws – California, Hawaii, Maryland, Massachusetts, and New Jersey – effectively became shall-issue states, leaving Delaware and Rhode Island as the only remaining may-issue states as of October 2025. The fact that shall-issue has now become the default concealed carry regime for the minority of states that have not transitioned to permitless carry further underscores the relevance of our study, and directly informs the specification of our empirical model.

Table 1 lists the 26 states that transitioned from shall-issue to permitless carry during our sample period of 1999–2023, along with the 18 states that switched from may- or no-issue

[1]Vermont never had a law regulating concealed weapon carrying so has always been considered a permitless carry state. North Carolina is likely to imminently become the 30th permitless carry state, as the governor's veto of the corresponding bill was overriden by the state Senate in September and will be taken up by the House later in October.

to shall-issue in that time.[2] We note that our analysis codes law transitions as occurring in the adoption year for effective dates of July 1 or earlier and in the following year otherwise.

[Insert Table 1 here]

2.2 Data

2.2.1 Firearm Deaths

Our primary outcomes of interest are mortality rates derived from the National Vital Statistics System (NVSS), which compiles all U.S. death certificates and is widely regarded as the most comprehensive source of data on firearm deaths. Using the Centers for Disease Control and Prevention (CDC) Wonder retrieval mechanism (CDC, 2023), we collect annual data on deaths by injury intent and mechanism for the 50 states and D.C. from 1999 through 2023. We specifically examine three injury intents – homicides and suicides, which comprise most gun deaths, along with unintentional deaths – and consider deaths by firearm vs. all other mechanisms.[3]

For each injury intent, we further distinguish whether firearm deaths involve handguns or long guns. For each injury intent other than legal intervention, the NVSS specifies separate ICD-10 cause-of-death codes for three gun types: 1) handguns, 2) rifles, shotguns, and larger firearms, and 3) other and unspecified firearms. Following (Koenig & Schindler, 2023), we label deaths in category 2 above as occurring by long gun those in categories 1 and 3 as occurring by handgun. We use these category labels for ease of exposition. The main idea is that the long gun category includes only deaths that are definitively not by handgun, allowing us to conduct a set of placebo tests, while the handgun category thereby contains

[2]Kansas and Missouri each progressed from no-issue to shall-issue and ultimately permitless carry over the period: Kansas switched from no- to shall-issue in 2007 and then to permitless carry in 2015, while Missouri shifted analogously in 2003 and 2017, respectively.

[3]A small fraction of injury deaths are classified as by either legal intervention or undetermined intent. We do not separately study either of these categories. However, we include undetermined intent deaths of each gun type in our counts of total handgun and long gun deaths, as described below, and both undetermined intent and legal intervention deaths in our counts of total gun and nongun injury deaths.

a higher concentration of deaths by the gun type that conversely is directly affected by permitless carry policy.

This breakdown by intent, mechanism, and gun type yields deaths counts by handgun, long gun, and nongun means – the latter providing an opportunity for another set of falsification tests – overall and separately for homicide, suicide, and unintentional death. Figure 1 (a) plots the population-weighted average handgun death rate per 100,000 residents from 1999 to 2023, separately for treated states that adopted permitless carry during the study period and for control states that did not. Prior to widespread adoption, the trends in treated and control states move in parallel, consistent with the identifying assumptions of our difference-in-differences framework outlined in the next section. After more widespread adoption beginning in the mid-2010s and accelerating after 2020, handgun deaths in treated states diverge sharply upward relative to control states, providing preliminary visual evidence that permitless carry laws increase handgun-related mortality.

[Insert Figure 1 here]

2.2.2 FBI Crime Data

Our crime data for the 50 states and D.C. come from the FBI's Uniform Crime Reports (UCR). Specifically, we utilize the UCR Return A files and the Supplemental Homicide Reports (SHR) compiled by Jacob Kaplan (Kaplan, 2025). From these sources, we collect data on the seven FBI index crimes: homicide, rape, robbery, aggravated assault, burglary, theft (larceny), and motor vehicle theft.

The UCR homicide counts, in conjunction with SHR data on homicide weapon type, enable us to construct an alternative panel of handgun, long gun, and nongun homicides, similarly including unknown gun type in the handgun category, with which to corroborate our NVSS homicide results. In addition, the UCR data allow for negligent homicides to be extracted in order to specifically investigate murders and nonnegligent homicides, given the implied differences in intents between the two groupings. Whether a gun was used in crime

commission is also observed for robberies and aggravated assaults, though no information on gun type is available. We also separately examine burglaries by whether they involve forced entry or are considered merely attempted.

Although the UCR data are widely used in crime research, they are known to suffer from reporting issues (Koenig & Schindler, 2023; Targonski, 2011). Data are collected monthly by the FBI from local law enforcement agencies, some of which do not provide information for every or even any month in a given year. This results in undercounts, even aside from the issue that not all crime is observed by law enforcement.

We take several steps to address this. For all crime regressions, we include data only from agencies that report for all 12 months in the year, construct rates based on the population covered by these agencies rather than the overall state population, and exclude the four observations for which these agencies covered less than 20 percent of the state population (Alabama 2019, Alaska 2021, Florida 2018, and Kansas 1999). For gun and nongun homicides and murders, we further exclude another 43 observations for which the SHR homicide count was below 20 percent of the Return A homicide count (Alabama 1999, 2011–2018, and 2020, California 2021, D.C. 1999–2008 and 2012, and Florida 1999–2017 and 2019–2020). For the robbery, aggravated assault, and burglary subcategories, we instead exclude another 24 observations (DC 1999 and 2007 and Illinois 1999–2020) for which gun involvement and burglary type are reported for fewer than 7 percent of these crimes.[4]

Among the violent crimes beyond homicide, we pay particular attention to robbery and aggravated assault, which constitute the bulk of total violent crime and, as described above, for which firearm involvement is explicitly identified in the Return A files. This enables further studying how the transition to permitless carry may affect firearm-related crime specifically. For these reasons, robbery and aggravated assault have been the most widely studied crimes in tests of deterrent effects of permissive concealed carry laws (Barati, 2016; Donohue et al., 2019, 2025; Lott & Mustard, 1997).

[4]This is the case for the subcategories of all three crimes for all 24 of these observations; the next lowest reporting shares are 34 percent for assault, 25 percent for robbery, and 59 percent for burglary.

Figure 1 (b) displays time series of population-weighted average aggravated assaults with guns per 100,000 residents from 1999 to 2023. The pattern closely mirrors that of handgun deaths, with no discernible pre-trend between treated and control states followed by a clear divergence after the adoption of permitless carry laws, with increases in adopting states. This graphical evidence suggests that aggravated assaults with guns rise in states that adopt permitless carry laws.

[Insert Figure 1 here]

2.2.3 Federal Firearm Licensee (FFL) Data

To investigate the mechanisms underlying the observed increases in handgun deaths and violent crime following permitless carry enactments, we examine changes in gun prevalence as proxied by the presence of federal firearm licensees (FFLs). Our FFL data are drawn from Johnson and Robinson (2024), who compiled county-by-year counts (as of each January) of all federally licensed dealers, manufacturers, and importers of firearms and destructive devices, as recorded by the Bureau of Alcohol, Tobacco, Firearms, and Explosives (ATF), from 2003-2023. We aggregate annual county-level counts of each FFL type to the state level to form state-by-year panels of licensees in five groups: firearm dealers (type 1), firearm pawnbrokers (type 2), firearm/ammunition manufacturers (types 6 and 7), firearm importers (type 8), and destructive device dealers, manufacturers, or importers (types 9–11).

The Johnson and Robinson dataset provides a new way of measuring the prevalence of guns across states by capturing the legal infrastructure through which most firearms are sold. The number of active FFLs reflects both the scale of retail gun availability and the ease of access for potential buyers. Johnson and Robinson (2024) further show that higher FFL density is positively correlated with firearm homicide rates, supporting the interpretation of FFL counts as a meaningful proxy for gun prevalence. Importantly, this measure also offers a complementary perspective to background check data, since it captures the number of licensed points of sale rather than the flow of individual transactions.

In our analysis, we use annual counts of active FFLs to test whether permitless carry enactments induced expansions in the retail firearms market. We argue that gun dealers might also respond to the prospect of a larger future market by entering states that are on the verge of adopting permitless carry, positioning themselves to meet increased demand once the policy is in place. In this sense, increases in FFL counts observed around the time of adoption may partly reflect forward-looking behavior by dealers in anticipation of serving a bigger customer base, rather than solely responses to realized changes in handgun demand. Panel (c) of Figure 1 plots the population-weighted average number of licensed dealers (FFL type 1) per 100,000 residents in treated and control states from 1999 to 2023.

[Insert Figure 1 here]

2.2.4 FBI National Instant Criminal Background Check Data

To further explore whether gun prevalence rises in states adopting permitless carry laws, we study whether adoption increases 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 handgun death responses in states adopting permitless carry (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, long guns, and permits to estimate the impact of permitless carry laws on the demand for each category. Similar to our approach for gun deaths, 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. In addition, we study permit background checks to capture the "first-stage" impact of permitless carry laws, since eliminating permitting requirements should reduce the number of permit applications and associated checks.[5]

[5]Our NICS analyses exclude observations with anomalous BGC values. For transfer checks, we omit Colorado 1999-2001 because many checks were apparently double counted before the state switched from point-of-sale to NICS in 2002, Connecticut 1999-2001 because virtually no transfer checks were reported in 2000 and several months of 2001, D.C. 1999-2007 during which handgun sales were banned, Illinois 2016-2023 because of an unexplained spike in checks in 2016, and Maryland 2013-2023 because checks spiked in advance of implementing the Firearm Safety Act of 2013. For permit checks, we omit Illinois 2017-2023, Indiana 1999-2008 and 2020-2023, Kentucky 1999-2023, and Utah 1999-2011, because either permit regulations changed during the period (for Kentucky before 2006 and Utah) or permit rechecks were included in permit check counts.

Panel (d) of Figure 1 shows the population-weighted average number of handgun background checks per 100,000 residents in treated and control states from 1999 to 2023. The trend in handgun checks closely mirrors the patterns for handgun deaths and aggravated assaults with guns in panels (a) and (b), with treated and control states moving in parallel prior to adoption and diverging sharply once treated states begin implementing permitless carry laws.

[Insert Figure 1 here]

Though BGCs are the most widely-used 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 required for private transactions only in states with corresponding laws.[6] Despite these considerations, we believe that BGCs represent a reliable substitute measure of handgun sales, and as such their changes in response to permitless carry adoption reasonably reflect changes in handgun purchases.

2.2.5 Summary Statistics and Controls

We present summary statistics for our primary dependent variables in Table 2. Our time-varying controls include demographic characteristics (log state population and the proportions female, White, Black, Asian, multiple race, ages 13–20, ages 21–61, and ages 62+), economic measures (unemployment rate, employment-to-population ratio, log real income per capita, and poverty rate), and logged counts of non-injury deaths, the prison population, and police officers employed.

[6] 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.

[Insert Table 2 here]

To ensure that our results are not confounded by other gun legislation, we also control for Stand Your Ground (SYG), mandatory handgun purchase delay, permit-to-purchase, and Child Access Prevention (CAP) laws, each of which previous research has shown to influence firearm-related outcomes (Anderson et al., 2021; Cheng & Hoekstra, 2013; DeSimone et al., 2013; Edwards et al., 2018; Koenig & Schindler, 2023; McClellan & Tekin, 2017), along with private sales background check laws because of their deterministic relationship with transfer BGC counts. Our estimated permitless carry effects are therefore independent of any impacts these additional legislative measures might have on firearm mortality, crime, and sales over our sample period. , implying that our permitless carry coefficients reflect differences from the omitted category of shall-issue regulation.

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 25 states from shall issue to permitless carry laws between 1999 and 2023. We employ a difference-in-difference (DiD) research design with staggered event timing to test whether gun deaths and crime exhibit greater changes in states that adopt permitless carry laws compared with those that do not, focusing primarily on comparisons of states within the same Census region.

Formally, we estimate two-way fixed effects models using annual panel data on U.S. states, including D.C. but excluding Vermont as the one state that has always allowed permitless carry, over the 25-year period from 1999 to 2023. 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,[7]

[7]In the extremely rare case of zero homicide or suicide deaths of a specific type in a state during a

X is the vector of controls outlined in the Data Section, γ is a set of year fixed effects, τ is a set of state fixed effects, ρ is a set of region by year fixed effects,[8] 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[9]. 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 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 is adopted on or after July 1, we code 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 **??**.

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 ShallIssue_{it}. In states transitioning from shall issue to permitless carry, the value of ShallIssue_{it} changes from 1 to 0 concurrently with that of 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 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 β_1 relies on comparing changes in rates of mortality and crime in states that adopted permitless carry laws with contemporaneous analogous changes in non-

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.

[8]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.

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

adopting states. For β_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 **??**, **??**, **??**, and **??** 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 τ before (τ>0) or after (τ<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 **??**, Table **??**, and Table **??**, 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 **??** 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., 2019, 2025; 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 **??** 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 **??**, 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 **??** 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 **??** here]

Finally, in Table **??** 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 **??** and **??**, 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 **??** here]

4.2 Crime Deterrence

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

The upper panel Table **??** 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 **??**. 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 **??** here]

Results for aggravated assaults are presented in Table **??**. 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 **??** 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.[10] Figures **??**, **??** and **??** 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 **??** 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 **??** 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 in Figure **??**. 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 **??** here]

Figure **??** 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 **??** here]

[10] 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.

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 **??**. Our analysis reveals no statistically significant effects of permitless carry laws. Similarly, the results reported in Table **??** 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 **??** here]

[Insert Table **??** 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 (β_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.[11]

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.[12] 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" 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 **??**. 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 **??** 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

[11] 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.

[12] 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.

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., 2025). 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 **??**. 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 **??** 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 **??** 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 **??**, with point estimates reported in the appendix. We observe that the pattern shown in Figure **??** closely resembles the event study analysis plots for firearm-related homicides and total firearm-related deaths in Figures **??** and **??**. 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 **??** 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 **??** 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[13]. 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 for policymakers to consider the potential impacts on public safety when contempdlating the deregulation of concealed handgun carry laws.

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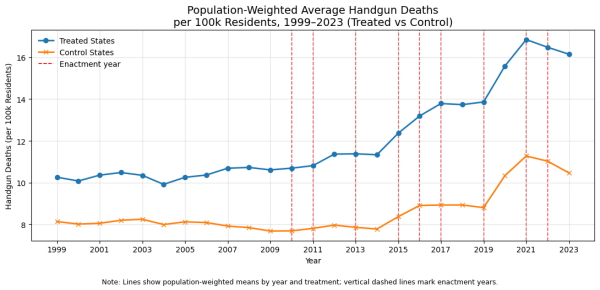
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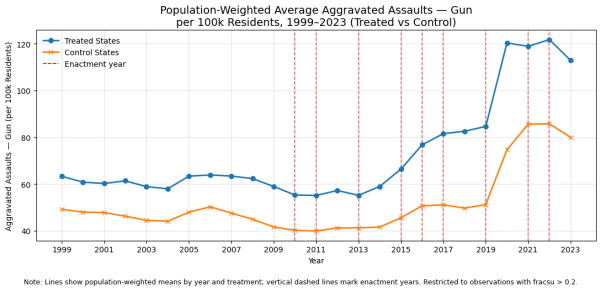
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Figures

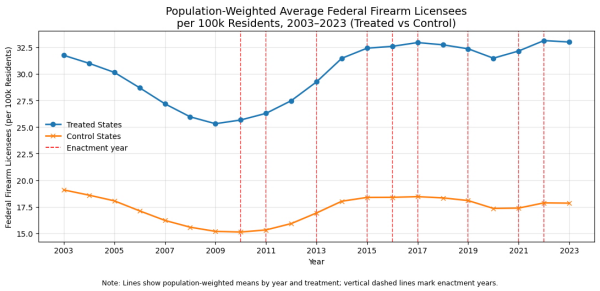
Figure 1: Trends in handgun deaths, aggravated assaults with gun, federally licensed gun dealers, and handgun background checks in treated vs. control states (1999–2023)



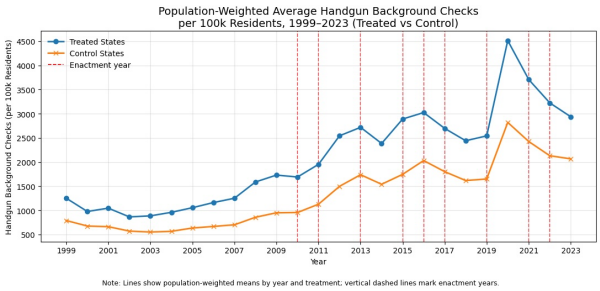
(a) Handgun deaths



(b) Aggravated assaults with gun



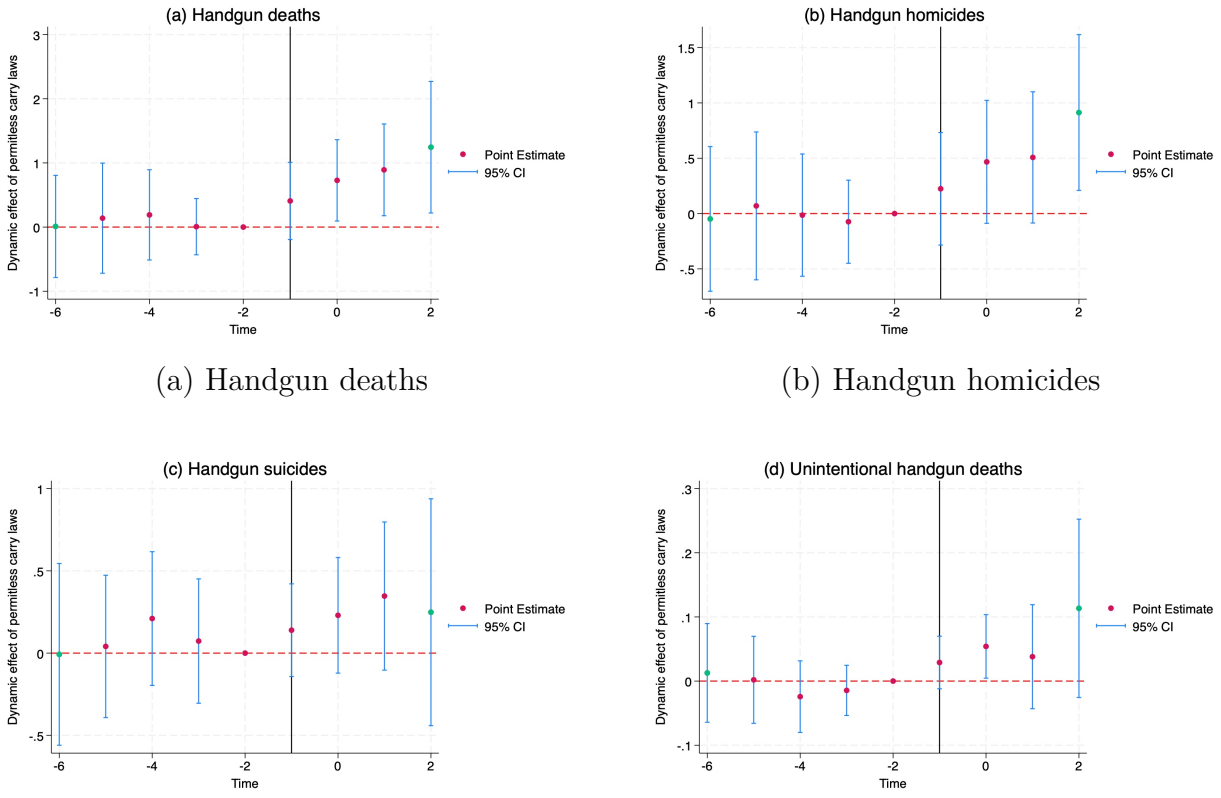
(c) Licensed gun dealers



(d) Handgun background checks

Notes: Handgun death data are from CDC Wonder; aggravated assault with gun data are from the FBI UCR, excluding state-year observations covering less than 20% of the population; federal firearm licensee (FFL) data are from Johnson and Robinson (2024); handgun background check data are from the FBI NICS.

Figure 2: Dynamic effect of permitless carry on deaths per 100,000 residents, by injury and gun type



(a) Handgun deaths

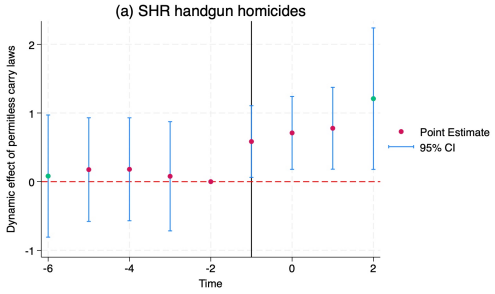
(b) Handgun homicides

(c) Handgun suicides

(d) Unintentional handgun deaths

Notes: Each panel presents an event-study regression for a different outcome, estimated using the baseline regression specification. Reported are point estimates with 95% confidence intervals.

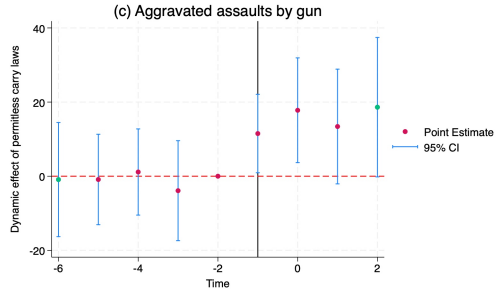
Figure 3: Dynamic effects of permitless carry on crime outcomes (per 100,000 residents)



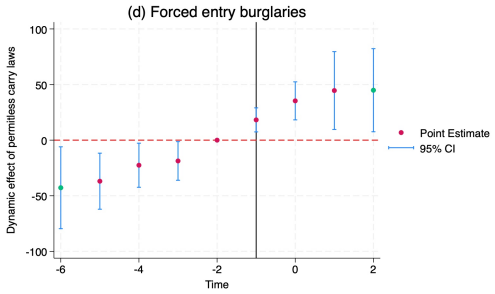
(a) SHR handgun homicides



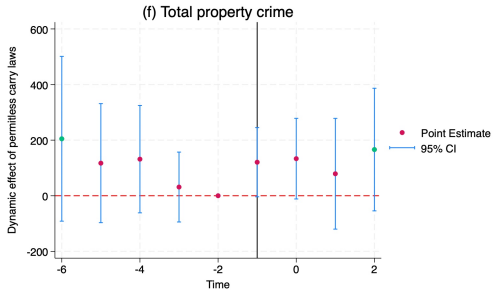
(b) SHR murder + nonnegligent homicides



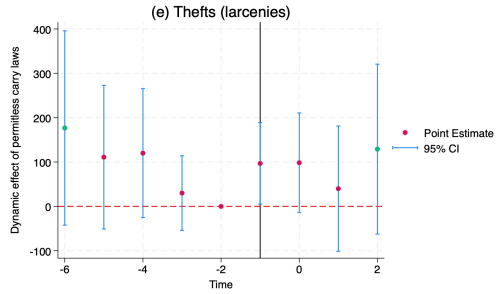
(c) Aggravated assaults by gun



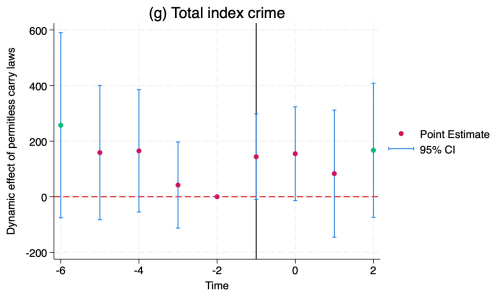
(d) Forced entry burglaries



(e) Total property crime



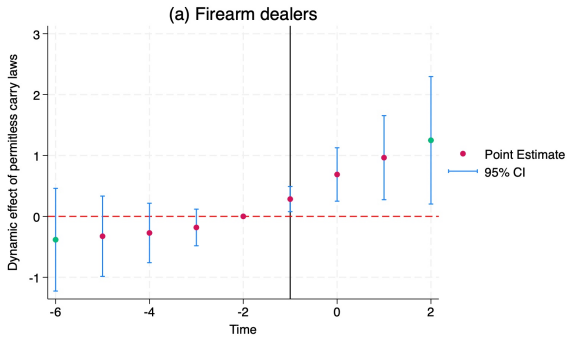
(f) Thefts (larcenies)



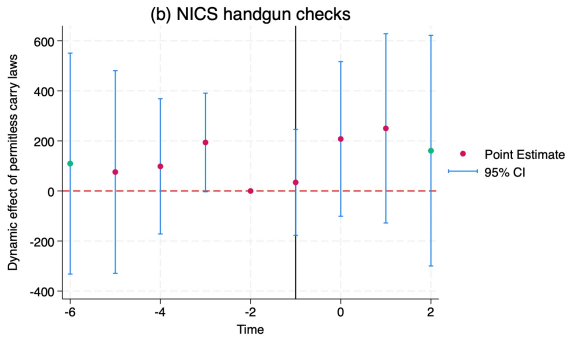
(g) Total index crime

Notes: Each panel presents an event-study regression for a different outcome,estimated using the baseline regression specification. Reported are point estimates with 95% confidence intervals.

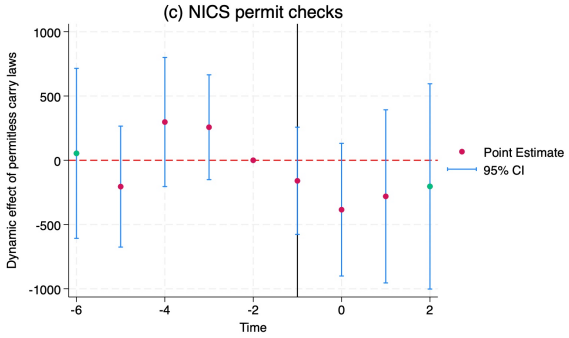
Figure 4: Dynamic effects of permitless carry on federal firearm licensee and background check counts (per 100,000 residents)



(a) Firearm dealers



(b) NICS handgun checks



(c) NICS permit checks

Notes: Each panel presents an event-study regression for a different outcome, estimated using the baseline regression specification. Reported are point estimates with 95% confidence intervals.

Tables

Table 1: Permitless carry and shall-issue law transitions, 1999–2023

Shall issue to Permitless			
State	**Effective Date**	**State**	**Effective Date**
Alabama	01/01/2023	Montana	02/18/2021
Alaska	09/09/2003	Nebraska	09/02/2023
Arizona	07/29/2010	New Hampshire	02/22/2017
Arkansas	10/17/2018	North Dakota	08/01/2017
Florida	07/01/2023	Ohio	06/13/2022
Georgia	04/12/2022	Oklahoma	11/01/2019
Idaho	07/01/2016	South Dakota	07/01/2019
Indiana	07/01/2022	Tennessee	07/01/2021
Iowa	07/01/2021	Texas	09/01/2021
Kansas	07/01/2015	Utah	05/05/2021
Kentucky	06/27/2019	West Virginia	05/26/2016
Maine	10/12/2015	Wyoming	07/01/2011
Mississippi	07/01/2015		
Missouri	01/01/2017		
May/No issue to Shall issue			
State	**Effective Date**	**State**	**Effective Date**
California	06/23/2022	Minnesota	04/29/2003
Colorado	05/17/2003	Missouri	10/11/2003
District of Columbia	07/28/2017	Nebraska	01/01/2007
Hawaii	06/23/2022	New Jersey	06/23/2022
Illinois	07/09/2013	New Mexico	01/01/2004
Iowa	01/01/2011	New York	06/23/2022
Kansas	01/01/2007	Ohio	04/01/2004
Maryland	06/23/2022	Wisconsin	11/01/2011
Massachusetts	06/23/2022		
Michigan	07/01/2001		

Note: Law characterizations and timing are based on https://www.rand.org/research/gun-policy/law-navigator.html#concealed-carry-laws and https://www.handgunlaw.us/documents/Permitless_Carry_States.pdf. California, Hawaii, Maryland, Massachusetts, New Jersey, and New York switched from may- to shall-issue on June 23, 2022 as a result of the U.S. Supreme Court's *Bruen* ruling, which sets a precedent for shall-issue laws to be universally applied across the U.S. other than in Delaware and Rhode Island, the only remaining may-issue states as of 2025. For the analysis, law transitions are coded as occurring in the adoption year for effective dates of July 1 or earlier and in the following year otherwise.

Table 2: Descriptive statistics

	Treated states	**Control states**
	Mean (weighted by population)	
CDC deaths *(per 100,000 residents)*		
Total gun deaths	13.50	9.72
	(3.59)	(4.06)
Total handgun deaths	12.07	8.58
	(3.64)	(3.88)
Total long gun deaths	1.296	1.005
	(0.706)	(0.600)
Gun homicides	4.59	4.07
	(2.12)	(2.31)
Handgun homicides	4.36	3.91
	(2.09)	(2.28)
Long gun homicides	0.234	0.160
	(0.150)	(0.110)
Nongun homicides	1.824	1.744
	(0.529)	(0.952)
Gun suicides	8.37	5.30
	(2.09)	(2.58)
Handgun suicides	7.36	4.48
	(2.04)	(2.34)
Long gun suicides	1.016	0.819
	(0.618)	(0.538)
Nongun suicides	5.93	6.15
	(1.46)	(1.43)
Unintentional gun deaths	0.263	0.145
	(0.193)	(0.141)
Unintentional handgun deaths	0.226	0.126
	(0.171)	(0.130)
Unintentional long gun deaths	0.037	0.019
	(0.048)	(0.024)
Unintentional nongun deaths	50.04	42.01
	(14.06)	(14.21)
FBI crime *(per 100,000 residents)*		
SHR handgun homicides	3.58	3.56
	(1.69)	(2.13)
SHR murders + nonnegligent homicides	3.52	3.53
	(1.66)	(2.10)

continued on next page

Table 2 (continued)

	Treated states	**Control states**
	Mean (weighted by population)	
Aggravated assaults by gun	74.26	51.37
	(43.09)	(39.02)
Forced entry burglaries	413.0	283.8
	(187.4)	(186.7)
Thefts (larcenies)	2,225	1,841
	(615)	(568)
Total property crime	3,225	2,694
	(970)	(914)
Total index crime	3,665	3,134
	(1,068)	(1,040)
Federal firearm licensees & background checks *(per 100,000 residents)*		
Dealers (FFLs)	21.18	13.61
	(13.31)	(8.03)
NICS handgun checks	2,081	1,310
	(1,258)	(1,156)
NICS long gun checks	2,035	1,405
	(943)	(823)
NICS multiple-gun checks	102.5	39.5
	(68.6)	(88.9)

Notes: Entries are sample means for dependent variables (deaths, crimes, gun dealers, and background checks per 100,000 residents) with standard deviations in parentheses. Handgun deaths represent all gun deaths other than those by long gun. Sample restrictions are described in the footnotes to the corresponding results tables and in the text.

Table 3: Effect of permitless carry on handgun deaths per 100,000 residents (N = 1,250)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Permitless carry	0.496***	0.454**	0.369**	0.334*	0.323*	0.365*	0.525**	0.021*
(*t-statistics*)	(2.61)	(2.11)	(1.78)	(1.69)	(1.56)	(1.64)	(2.00)	(1.62)
[*p-values*]	[0.006]	[0.019]	[0.048]	[0.069]	[0.097]	[0.078]	[0.022]	[0.063]
Mean handgun deaths in year prior to adoption				15.4				
Specification								
Baseline	✓							
Excl. time-varying controls (except may issue)		✓						
Quadratic state trends			✓					
Quartic state trends				✓				
Year fixed effects not by region					✓			
Year×division fixed effects						✓		
Not population-weighted							✓	
Log handgun deaths								✓

Notes: Each column represents the permitless carry law coefficient from the regression specification indicated in the checked row heading, with absolute values of (*t*-statistics) in parentheses and [*p*-values] in square brackets. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Estimates are relative to shall-issue laws and based on 100,000 replications of the corresponding wild cluster bootstrap model. Handgun deaths are from the CDC and represent all gun deaths other than those by long gun. The handgun death mean is population-weighted for the 25 sample permitless carry states in the year before adoption. The sample spans 1999–2023 and excludes Vermont, which has never required a permit for concealed carry. The baseline model is population-weighted and controls for gun law indicators (may issue, stand your ground, private sales background check, purchase delay, permit to purchase, and child access prevention), demographic variables (log population and the proportions female, White, Black, Asian, multiple race, ages 13–20, ages 21–61, and ages 62+), economic variables (unemployment rate, employment-to-population ratio, log real income per capita, and poverty rate), and logged counts of non-injury deaths, the prison population, and police officers employed, along with year-by-census region indicators and cubic state trends.

Table 4: Effects of permitless carry on deaths per 100,000 residents, by injury and gun type

Gun type	All injury deaths (1)	Homicides (2)	Suicides (3)	Unintentional deaths (4)
Handgun	0.496***	0.343**	0.120	0.045**
(*t-statistics*)	(2.61)	(2.16)	(1.12)	(1.79)
[*p-values*]	[0.006]	[0.023]	[0.224]	[0.033]
Mean	15.4	6.14	8.91	0.195
All gun	0.450**	0.338**	0.086	0.049**
(*t-statistics*)	(2.34)	(2.12)	(0.79)	(1.66)
[*p-values*]	[0.012]	[0.026]	[0.392]	[0.048]
Mean	16.7	6.34	9.80	0.212
Long gun	-0.032	-0.002	-0.034	0.004
(*t-statistics*)	(0.62)	(0.07)	(0.88)	(0.47)
[*p-values*]	[0.508]	[0.918]	[0.341]	[0.626]
Mean	1.12	0.201	0.888	0.017
Nongun	1.11	-0.063	0.058	0.754
(*t-statistics*)	(1.08)	(1.23)	(0.54)	(0.77)
[*p-values*]	[0.286]	[0.154]	[0.565]	[0.429]
Mean	73.1	1.51	6.59	63.7

Notes: Entries are permitless carry law coefficients from regressions using the baseline specification from Table 3 with deaths of the injury type in the column heading by the gun type in the row heading as the dependent variable, with absolute values of (t-statistics) in parentheses and [p-values] in square brackets. *, **, and *** correspond to significance levels of 0.10, 0.05, and 0.01, respectively. Means are population-weighted for the 25 permitless carry states in the year before adoption. Deaths are from the CDC, with the handgun category representing all gun deaths other than those by long gun.

Table 5: Effects of permitless carry on UCR violent crimes, by crime and gun type

Crime	Homicide (1)	Murder (2)	Agg. Assault (3)	Robbery (4)	Violent (5)
Total	0.336	0.218	18.1***	-1.23	18.8*
(*t-statistics*)	(1.40)	(0.91)	(2.38)	(0.34)	(1.70)
[*p-values*]	[0.168]	[0.363]	[0.005]	[0.748]	[0.060]
Mean	6.61	6.21	278.4	62.1	392.5
N	1,203	1,203	1,246	1,246	1,246
All gun			9.91***	0.094	
(*t-statistics*)			(2.68)	(0.04)	
[*p-values*]			[0.003]	[0.991]	
Mean			113.6	29.3	
N			1,222	1,222	
Handgun	0.371**	0.354**			
(*t-statistics*)	(2.15)	(2.06)			
[*p-values*]	[0.027]	[0.033]			
Mean	4.57	4.45			
N	1,203	1,203			
Long gun	-0.027	-0.036			
(*t-statistics*)	(0.57)	(0.76)			
[*p-values*]	[0.569]	[0.445]			
Mean	0.374	0.362			
N	1,203	1,203			
Nongun	-0.008	-0.100	5.57	-2.15	
(*t-statistics*)	(0.08)	(1.07)	(1.01)	(0.96)	
[*p-values*]	[0.924]	[0.266]	[0.275]	[0.371]	
Mean	1.67	1.40	164.8	32.8	
N	1,203	1,203	1,222	1,222	

Notes: Entries are permitless carry law coefficients from regressions using the baseline specification from Table 3 with deaths of the injury type in the column heading by the gun type in the row heading as the dependent variable, with absolute values of (*t*-statistics) in parentheses and [*p*-values] in square brackets. *, **, and *** correspond to significance levels of 0.10, 0.05, and 0.01, respectively. Means are population-weighted for the 25 permitless carry states in the year before adoption. Crimes are per 100,000 residents. Homicides and murders are from the Supplemental Homicide Reports while aggravated assaults, robberies, and total violent crimes are from the UCR Return A files. Crime rates are constructed using the population covered by agencies reporting Return A data. Samples span 1999–2023 and exclude Vermont along with four observations for which agencies reporting Return A data covered below 20 percent of the state population (Alabama 2019, Alaska 2021, Florida 2018, and Kansas 1999). The samples for columns 1 and 2 exclude another 43 observations for which the SHR homicide count was below 20 percent of the Return A homicide count (Alabama 1999, 2011–2018, and 2020, California 2021, DC 1999–2008 and 2012, and Florida 1999–2017 and 2019–2020), while the gun and nongun samples in columns 3 and 4 exclude another 24 observations for which gun involvement is reported for fewer than 7 percent of assaults and robberies (DC 1999 and 2007 and

Illinois 1999–2020); the next lowest shares are 34 percent for assault and 25 percent for robbery. Because of this, and because gun involvement is not always further categorized for many included observations, coefficients for gun and nongun categories do not sum to the total crime coefficients in columns 3 and 4.

Table 6: Effects of permitless carry on property and overall crimes per 100,000 residents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Permitless carry	21.9**	17.9***	3.12	-0.405	69.2**	9.44	100.5***	115.6***
(*t-statistics*)	(2.01)	(2.44)	(0.70)	(0.20)	(2.22)	(1.08)	(2.46)	(2.42)
[*p-values*]	[0.030]	[0.006]	[0.468]	[0.834]	[0.015]	[0.246]	[0.005]	[0.005]
Mean	360.5	198.8	141.9	19.8	1,511	232.4	2,104	2,496
N	1,246	1,222	1,222	1,222	1,246	1,246	1,246	1,246
Outcome								
Burglary – total	✓							
Burglary – forced entry		✓						
Burglary – non-forced entry			✓					
Burglary – attempted				✓				
Theft					✓			
Motor vehicle theft						✓		
All property							✓	
All index (property + violent)								✓

Notes: Each column represents the permitless carry law coefficient from the regression specification indicated in the checked row heading, with absolute values of (*t*-statistics) in parentheses and [*p*-values] in square brackets. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Means are population-weighted for the 25 permitless carry states in the year before adoption. Crime data are from the UCR Return A files, with rates constructed using the population covered by agencies reporting Return A data. Samples span 1999–2023 and exclude Vermont along with four observations for which agencies reporting Return A data covered below 20 percent of the state population (Alabama 2019, Alaska 2021, Florida 2018, and Kansas 1999). Burglary subcategory samples also exclude the 24 observations noted in Table 5 reporting these categorizations for fewer than 6 percent of burglaries (DC 1999 and 2007 and Illinois 1999–2020); the next lowest share is 59 percent. Because of this, and because burglaries are not always further categorized for many included observations, coefficients for burglary categories do not sum to the burglary total.

Table 7: Effects of permitless carry on federal firearm licensee and background check counts (per 100,000 residents)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Permitless Carry	0.454***	-0.121	0.005	0.003	0.007	176*	16.4	-325*
(*t-statistics*)	(3.10)	(1.19)	(0.04)	(0.26)	(0.57)	(1.73)	(0.32)	(1.65)
[*p-values*]	[0.000]	[0.212]	[0.995]	[0.773]	[0.590]	[0.076]	[0.741]	[0.070]
Mean	21.2	3.98	6.16	0.497	0.281	3,447	2,166	1,800
N	1,050	1,050	1,050	1,050	1,050	1,216	1,216	1,191
Outcome								
Firearm dealers (type 1)	✓							
Firearm pawnbrokers (type 2)		✓						
Firearm/ammunition manufacturers (type 6 or 7)			✓					
Firearm importers (type 8)				✓				
Destructive device dealers/ manufacturers/importers (type 9, 10, or 11)					✓			
Handgun background checks						✓		
Long gun background checks							✓	
Permit background checks								✓

Notes: Each column represents the permitless carry law coefficient from the regression specification indicated in the checked row heading, with absolute values of (*t*-statistics) in parentheses and [*p*-values] in square brackets. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. Means are population-weighted for the 25 permitless carry states in the year before adoption (23 for permit checks, with Indiana and Kentucky excluded in the years surrounding adoption as noted below). Federal firearm licensee (FFL) data are from Johnson & Robinson (2024) and span 2003–2023. Background check data are from NICS. As a result of data anomalies, 34 observations are excluded for handgun and long gun checks (Colorado 1999–2001, Connecticut 1999–2001, DC 1999–2007, Illinois 2016–2023, and Maryland 2013–2023), and 59 observations are excluded for permit checks (Illinois 2017–2023, Indiana 1999–2008 and 2020–2023, Kentucky 1999–2023, and Utah 1999–2011).

Table 8: Effects of permitless carry using a stacked regression model

Outcome (per 100,000 residents)	(1)	(2)
Handgun deaths	0.688**	0.418*
(*t-statistics*)	(2.35)	(1.78)
N	7,500	12,813
Handgun homicides (CDC)	0.461*	0.288
(*t-statistics*)	(1.90)	(1.58)
N	7,500	12,813
Handgun suicides	0.170	0.124
(*t-statistics*)	(1.61)	(1.02)
N	7,500	12,813
Unintentional handgun deaths	0.062**	0.034
(*t-statistics*)	(2.43)	(1.39)
N	7,500	12,813
Handgun homicides (SHR)	0.440	0.322*
(*t-statistics*)	(1.58)	(1.93)
N	7,333	12,287
Handgun murders	0.425	0.321*
(*t-statistics*)	(1.52)	(1.92)
N	7,333	12,287
Gun assaults	12.3**	6.45*
(*t-statistics*)	(2.20)	(1.75)
N	7,232	12,496
Forced entry burglaries	15.7	8.68
(*t-statistics*)	(1.35)	(1.03)
N	7,232	12,496
Theft	53.4	53.8
(*t-statistics*)	(1.12)	(1.19)
N	7,496	12,760
All property	81.2	78.3
(*t-statistics*)	(1.31)	(1.30)
N	7,496	12,760
All index	97.3	89.1
(*t-statistics*)	(1.34)	(1.31)
N	7,496	12,760

continued on next page

Table 8 (continued)

Outcome (per 100,000 residents)	(1)	(2)
Firearm dealers	0.378**	0.527***
(*t-statistics*)	(2.36)	(3.21)
N	6,300	10,613
Handgun background checks	275**	150
(*t-statistics*)	(2.13)	(1.58)
N	7,126	12,439
Permit background checks	-478**	-231
(*t-statistics*)	(2.05)	(1.22)
N	7,481	12,223
Not-yet-treated states in comparison group?	No	Yes

Notes: Each entry represents a stacked regression model, with the dependent variable indicated in the row heading, in which states that eventually adopt permitless carry laws are excluded from the comparison group altogether (column 1) or once they adopt the law (column 2). Absolute values of (*t*-statistics) are in parentheses and [*p*-values] are in square brackets. *, **, and *** represent significance levels of 0.10, 0.05, and 0.01, respectively. The specification is otherwise identical to the baseline model and sample as described in the footnote to Table 3.

Appendix

Table A1: Event-study Estimates of Permitless Carry on Handgun Outcomes

	Handgun deaths	**Handgun homicides**	**Handgun suicides**	**Unintentional handgun deaths**
lead6	0.010	-0.048	-0.008	0.013
	(0.396)	(0.325)	(0.275)	(0.038)
lead5	0.139	0.069	0.041	0.002
	(0.427)	(0.332)	(0.215)	(0.034)
lead4	0.190	-0.014	0.210	-0.024
	(0.350)	(0.275)	(0.202)	(0.028)
lead3	0.006	-0.074	0.073	-0.015
	(0.218)	(0.187)	(0.188)	(0.019)
lead1	0.408	0.224	0.139	0.029
	(0.299)	(0.253)	(0.140)	(0.020)
lag0	0.728**	0.467*	0.230	0.054**
	(0.315)	(0.276)	(0.175)	(0.025)
lag1	0.892**	0.507*	0.347	0.038
	(0.356)	(0.295)	(0.224)	(0.040)
lag2	1.245**	0.913***	0.248	0.113*
	(0.510)	(0.350)	(0.343)	(0.069)
Observations	1,250	1,250	1,250	1,250

Notes: Each column represents an event-study regression for a different handgun outcome, based on the baseline regression specification. Reported are coefficients with standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample size is 1,250.

Table A2: Event-study Estimates of Permitless Carry on FBI Crime Outcomes (per 100,000 residents)

	SHR handgun homicides	**SHR murder + nonneg. homicides**	**Aggravated assaults (gun)**	**Forced entry burglaries**	**Thefts (larcenies)**	**Total property crime**	**Total index crime**
lead6	0.081	0.055	-0.904	-42.813**	176.706	204.845	257.077
	(0.443)	(0.437)	(7.659)	(18.313)	(108.945)	(147.467)	(165.669)
lead5	0.176	0.170	-0.885	-36.924***	110.932	117.210	158.904
	(0.376)	(0.370)	(6.071)	(12.538)	(80.558)	(106.543)	(120.120)
lead4	0.180	0.172	1.133	-22.563**	119.964	131.613	164.976
	(0.373)	(0.372)	(5.784)	(9.868)	(72.343)	(96.116)	(109.516)
lead3	0.079	0.069	-3.905	-18.699**	29.998	31.271	42.016
	(0.396)	(0.395)	(6.713)	(8.656)	(41.845)	(62.544)	(77.047)
lead1	0.584**	0.561**	11.509**	18.216***	96.908**	120.651*	143.890*
	(0.260)	(0.261)	(5.264)	(5.377)	(45.682)	(61.914)	(76.593)
lag0	0.710***	0.692***	17.787**	35.326***	98.361*	133.377*	154.702*
	(0.264)	(0.259)	(7.032)	(8.510)	(55.849)	(72.166)	(83.992)
lag1	0.778**	0.743**	13.408*	44.589**	39.901	79.047	83.212
	(0.297)	(0.297)	(7.702)	(17.412)	(70.285)	(99.123)	(113.775)
lag2	1.209**	1.216**	18.620*	44.888**	129.133	166.114	167.146
	(0.512)	(0.514)	(9.357)	(18.587)	(95.303)	(109.728)	(120.024)
Observations	1,204	1,204	1,222	1,222	1,246	1,246	1,246

Notes: Each column represents an event-study regression for a different outcome, estimated using the baseline specification. Reported are coefficients with standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Event-study Estimates of Permitless Carry on Firearm Dealers and NICS Background Checks

	Firearm dealers	**NICS handgun checks**	**NICS permit checks**
lead6	-0.383	109.325	53.834
	(0.420)	(219.665)	(328.887)
lead5	-0.326	75.730	-204.970
	(0.328)	(201.499)	(234.077)
lead4	-0.272	98.286	297.594
	(0.243)	(134.469)	(249.881)
lead3	-0.182	193.792*	257.120
	(0.150)	(98.013)	(202.759)
lead1	0.284***	34.230	-159.926
	(0.103)	(105.272)	(207.443)
lag0	0.688***	207.849	-384.510
	(0.218)	(153.743)	(256.717)
lag1	0.965***	250.107	-280.854
	(0.344)	(188.148)	(334.993)
lag2	1.250**	160.963	-203.535
	(0.521)	(229.241)	(397.221)
Observations	1,050	1,216	1,191

Notes: Each column represents an event-study regression for a different outcome, based on the baseline regression specification. Reported are coefficients with standard errors in parentheses. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.