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 important 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 handgun purchases complements the literature on the determinants of the demand for firearms. In particular, research by (Depetris-Chauvin, 2015), (Depew & Swensen, 2018), (Koenig & Schindler, 2023), (Levine & McKnight, 2017), and (Moshary et al., 2023) identify signifi-

cantly 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 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.

[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.

[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

[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.[4]

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 a higher concentration of deaths by the gun type that conversely is directly affected by

[3] We thus treat 25 states out of 26 that passed permitless carry laws from 1999 to 2023 as our treated states. We did not include Nebraska as a permitless carry state because its effective date was September 2, 2023.

[4] 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.

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.[5]

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).

Figure 1 (b) displays time series of population-weighted average aggravated assaults with

[5]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.

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.

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.

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.[6]

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

[6]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.

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.

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.[7] 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.

[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

[7] 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.

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. We further control for an indicator that no or may-issue laws are in place for states before converting or that have never switched to shall-issue laws. Our permitless carry coefficients are therefore independent of any impacts these additional legislative measures might have on firearm outcomes and reflect differences from the omitted category of shall-issue regimes, which are now the de facto default alternative to permitless carry.

3 Empirical Strategy

To study the impact of permitless carry laws on firearm-related mortality, crime, and demand, 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

$$\text{Outcome}_{it} = \alpha + \beta_1 \text{Permitless}_{it} + \beta_2 \text{MayIssue}_{it} + \Theta X_{it} + \gamma_i + \tau_t \times \rho_i + \gamma_i \times t + \epsilon_{it} \quad (1)$$

where Outcome_{it} is per 100,000 residents, X is the vector of controls outlined in the Data Section, γ is a set of state fixed effects, τ is a set of year fixed effects, ρ is a set of region by year fixed effects, and $\gamma_i \times f(t)$ are state-specific time trends, with $f(t)$ representing polynomials of varying degrees to capture potential changes in the outcome variables over the period that vary across states and are smooth but nonlinear. All regressions are weighted

by state population.

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 by July 1 of year t. If the law is adopted 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 1.

So that the effects of permitless carry laws can be interpreted in relation to shall issue laws, which as described now prevail in all but two states that have not adopted permitless carry, all models also control for the presence of a no or may issue law through the binary indicator MayIssue_{it}. In states transitioning from no/may to shall issue, the value of MayIssue_{it} changes from 1 to 0 in adoption year t or $t+1$ depending on whether or not the date during t is July 1 or earlier, mimicking our coding of permitless carry laws.

All regressions specify standard errors that are robust to heteroskedasticity and clustered at the state level. While we report asymptotic t-statistics using the conventional clustering approach, we also show p-values derived from the wild cluster bootstrapping technique typically reserved for models with fewer than 30 entities, based on 100,000 replications to essentially eliminate variation from randomly sampling observations in constructing bootstrapped estimates.

Our identification of β_1 relies on comparing outcome changes in states that adopted permitless carry laws with contemporaneous analogous changes in non-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, outcome changes in adopting states would have been identical to those in nonadopting states from the same Census region. The graphical evidence already presented illustrates that the trends in handgun deaths, aggravated assaults by gun, firearm dealers, and handgun transfer background checks are substantively parallel in adopting and non-adopting states before the former transitioned to permitless carry. We later provide more formal evidence by estimating an event-study specification that includes 6 years of

leading treatment variable indicators along with 2 years of lagging indicators to examine dynamic law effects. This framework tests whether pre-adoption outcome trends confound our difference-in-difference results while also confirming that effects are not transient. The event study model is a modification of (1):

$$\begin{aligned} \text{Outcome}_{it} = \alpha + \sum_{\substack{\tau=-6 \\ \tau\neq -1}}^{2} \xi_{\tau}\text{PostPermitless}_{i,t-\tau} + \beta_2 \text{ShallIssue}_{it} + \Theta X_{it} \\ + \ \gamma_i + \tau_t + \rho_{rt} + \gamma_i \times t + \epsilon_{it} \end{aligned} \tag{2}$$

where 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, with years -6 and +2 including all prior and subsequent years, respectively. The year prior to permitless carry adoption is the omitted category. We present our event study analyses in the robustness checks section.

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 estimate stacked regression models in which the set of control states is restricted to non-adopting (never-treated) states either alone or combined with later-adopting (not-yet-treated) states (Baker et al., 2022; Cengiz et al., 2019).

4 Results

4.1 Mortality

We begin with our estimated effects on total handgun deaths in Table 3. Column 1 contains the main result of our study: permitless carry adoption predicts that handgun deaths will

rise by 0.496 per 100,000 residents. This effect is statistically significant at the 1% level using either the traditional clustering adjustment or wild cluster bootstrapping, and represents a 3.2% increase over the average of 15.4 handgun deaths per 100,000 residents in the year before adoption across the 25 sample adopting states.

[Insert Table 3 here]

The column 1 estimate is from the baseline model described in the previous section. Remaining columns indicate robustness to excluding time-varying controls (other than the may-issue law indicator, column 2) and the cubic state trend term (column 3), adding a quartic state trend term (column 4), not interacting year fixed effects with Census region so that all states not adopting in the year are counterfactuals for states adopting that year (column 5), restricting comparisons to within each of the nine Census divisions (column 6), not weighting by population so that the effect is for the average state rather than U.S. resident (column 7), and using logged handgun deaths as the dependent variable (column 8). While coefficients are generally smaller in magnitude, using bootstrapped p-values they are significant at the 10% level always and at the 5% level in several cases. The smallest effect is 2.1% using logged handgun deaths, while not population-weighting produces the largest effect at 3.6%. Other than that from the log model, the smallest effects result from including quartic state trends, which is likely excessive in our 25-year panel, and not restricting comparisons to the same Census region, which may well reduce the similarity between the comparison groups and adopting states.

Returning to the baseline model, the first row of estimates in Table 4 disaggregates the coefficient from column 1 of Table 3, reproduced in column 1 of Table 4, into effects on homicides (column 2), suicides (column 3), and unintentional deaths (column 4).[8] 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,

[8]Because we do not show analogous estimates for unknown injury intent and, for all gun and nongun deaths, legal intervention, the coefficients in columns 2–4 do not sum to the column 1 coefficient.

while unintentional deaths have seldom been the focus of study even with respect to firearms more generally.[9]

Roughly 70% of the increase in handgun deaths are attributable to homicides, which we estimate to rise by 5.6% upon transition to permitless carry. Though the handgun suicide increase is not statistically significant, it accounts for over 20% of the handgun death effect. Conversely, while the unintentional handgun death increase comprises less than 10% of the overall effect, it is statistically significant and represents over 23% of the pre-adoption death rate.

[Insert Table 4 here]

The second row indicates that effects on total gun deaths, including long gun, are similar overall and by injury intent. As this similarity implies, effects on long gun deaths are relatively small and statistically insignificant. The same is true for the impact on nongun forms of deaths as shown in the lower two rows, with both of these sets of null results suggesting that the permitless carry coefficients from the handgun death regressions are not merely picking up a spurious relationship with injury deaths through unobserved common influences.

That implication is consistent with the evidence conveyed by the event study graphs in Figure 2. These show year-by-year differences in handgun deaths, overall (panel a) and by injury intent (panels b through d), between adopting an non-adopting states, from five years prior to one year after adoption, with the adoption year centered at zero and all previous and future years aggregated into years -6 and +2, respectively.

[Insert Figure 2 here]

[9]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. Meanwhile, the only economic research on unintentional gun injuries is Levine and McKnight (2017) and DeSimone et al. (2013), who respectively examined effects of Sandy Hook on accidental gun deaths and CAP laws on nonfatal accidental shootings.

The vertical line at year -1 serves as a reminder that our DiD estimates, as is typical, compare average differences in the adoption year and beyond to those in all pre-adoption years. However, the effects in our event study graphs are normalized to year -2 in order to emphasize that there appears to be an anticipatory effect in year -1. This is consistent with previous research (Bollman et al., 2025; Depetris-Chauvin, 2015; Koenig & Schindler, 2023), lags between the passage and adoption of the laws, and Google Trends data searches showing spikes in searches several months prior to adoption dates (not shown).[10]

Beyond the associated effects indicated at year -1, the event study graphs show pre-adoption differences that hover around zero and are highly insignificant, followed by positive differences that emerge in the adoption year and are significant, or nearly so, for outcomes other than suicides in panel c. These results are consistent with our DiD estimates and the interpretation of those estimates as causal effects of permitless carry adoption. The corresponding point estimates and standard errors are displayed in Appendix Table A1.

4.2 Crime

Table 5 turns attention to gun-related crime as measured by UCR data. The first two columns confirm the NVSS homicide results from Table 4. Specifically, the middle row indicates that while the UCR picks up fewer handgun homicides – in part because Florida is largely excluded due to missing SHR data – the associated permitless carry effects are quite similar in magnitude and significance, and are largely derived from murder and nonnegligent homicide. In the lower two rows, long gun and nongun homicides are again unaffected.

[Insert Table 5 here]

Columns 3 and 4 examine the two other UCR crimes for which gun involvement is reported, aggravated assault and robbery. Though gun type is not observed, overall gun-related aggravated assaults are estimated to rise by a highly statistically significant 8.7% relative to

[10]The next version of our paper will incorporate this possibility into our regression models by adding an indicator for the immediate pre-adoption period.

their average pre-adoption rate upon transition to permitless carry. The corresponding effect on nongun aggravated assaults is much smaller proportionally and statistically insignificant. Similar effects, however, are not observed for gun-related robberies. The associated effects on total aggravated assaults and, in column 5, violent crimes are likewise significant, though this is in part attributable to the positive though insignificant response of nongun aggravated assaults.

The estimates discussed thus far in Table 4 for homicide and Table 5 for violent crime are inconsistent with the premise that less restrictive concealed carry regulation deters violence, gun-involved or otherwise. The implications of those in Table 6 are similar. We find that permitless carry adoption significantly increases burglaries overall, by 6.1% (column 1), mainly because of a 9.0% rise in forced entry burglaries (column 2). Effects on other and attempted burglaries are small and insignificant (columns 3 and 4). Consistent with evidence reported by (Donohue et al., 2025) for shall issue laws and (Billings, 2023) for concealed carry permits, thefts (i.e., larcenies) are increased by 4.6% (column 5). Though motor vehicle theft is unaffected (column 6), the burglary and theft responses produce significant increases in property crimes (column 7), which coupled with the overall violent crime effects observed in 5 result in significant higher overall index crime rates (column 8).

[Insert Table 6 here]

Figure 3 shows the event studies for the crime outcomes. Panels a through c again reflect minimal differences between adopting and non-adopting states through year -2, an anticipator jump in handgun murders and aggravated assaults by gun in year -1, and large statistically significant differences starting in year 0. In d., post-adoption differences are likewise significant for forced-entry burglaries, though a positive pre-adoption gradient is apparent.

[Insert Figure 3 here]

Panels e. through g. indicate that for theft – and correspondingly for total property and index crimes, the vast majority of which are thefts and property crimes, respectively – any pre-adoption trends are in the opposite direction, reflecting declining differences between adopting and non-adopting states, until that trend is reversed staring in the pre-adoption year. Appendix Table A2 contains the corresponding point estimates and standard errors.

4.3 Firearm Demand

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). One simple but pivotal factor that might explain the effect of permitless carry on firearm-related deaths is an increase in handgun ownership upon law adoption. 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 could 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 carrying a handgun.

Formally, we test this mechanism by employing the same DiD framework from our main analysis, but using federal firearm licensees (FFLs) and FBI background checks (BGCs) per 100,000 residents as dependent variables. Similar to the previous research design, gun dealer FFLs and handgun transfer BGCs serve as proxies for handgun demand to examine whether permitless carry laws increase handgun purchases in adopting states, while non-dealer and non-firearm FFLs along with long gun transfer BGCs are employed as falsification test outcomes.

We report the result of this analysis in Table 7. Permitless carry adoption predicts

significant increases of 2.1% in licensed gun dealers (at the 1% level in column 1) and 5.1% in handgun BGCs (at the 10% level in column 6).[11] In contrast, estimated effects on non-dealer/non-firearm FFLs (columns 2-5) and long gun BGCs (column 6) are statistically insignificant and typically much smaller proportionally. In column 8, the first-stage effect on permit BGCs is negative and, while significant only at 10%, implies a reduction of over 18% upon eliminating the permit requirement for concealed handgun carrying.

[Insert Table 7 here]

Figure 4 contains the associated event study graphs. A pre-trend is evident for gun dealers in a., but this becomes statistically insignificant when considering the year -1 deviation as an anticipatory effect. While the noisiness of the BGC data is evident in b. and c., any pre-trend is negative for handgun checks and insignificant for permit checks. Appendix Table A3 lists the corresponding point estimates and standard errors.

[Insert Figure 4 here]

Both sets of evidence are consistent with the argument that at least part of the explanation for the increase in handgun deaths, gun assaults, and property crime in the wake of transitioning to permitless carry is an increase in the demand for handguns to carry, making guns more prevalent in the use of violence and more available for burglarizing and theft. New gun dealers appear to enter the market and the demand for permits starts to decline the year prior to adoption, with increased purchases reflected by BGCs beginning the adoption year. However, no corresponding shifts in the presence of other FFL types or long gun demand are apparent.

4.4 Stacked Difference-in-Differences

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

[11] We note that the same specification using log handgun BGCs yields a coefficient that predicts a 14.9% increase in handgun BGCs and is significant at the 1% level.

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, because the standard DiD estimator makes comparisons between states that adopted the law earlier as controls versus later-adopting treated states, it is potentially biased when the treatment effect varies over time or across states.[12]

To accommodate the possibility of differential treatment effects across time and states, we estimate the model in (1) using stacked DiD (Cengiz et al., 2019), restricting control states to those that had not adopted permitless carry by the end of our sample period and, alternatively, also those that had not yet adopted by the date of earlier adoptions.[13]

Table 8 has the corresponding estimates, focusing on outcomes that are significantly affected in the standard DiD model. When the comparison group contains only states that had not enacted permitless carry laws by the end of 2023 (column 1), coefficients are larger for deaths and assaults, though significance levels fall because standard errors are even more so. However, effects remain significant at 10% for CDC homicides and 5% for overall and unintentional handgun deaths and for aggravated assault by gun. Coefficient magnitudes decline once future permitless carry adopting states are included in the comparison group (column 2), though the accompanying added precision allows SHR handgun homicides and murders to regain significance at 10%. Moreover, effects remain significant at 5% in both specifications for licensed gun dealers and become so using the never-treated control group for handgun and permit background checks.

[Insert Table 8 here]

All told, we view the full set of estimates across Tables 3 through 8 as providing strong evidence that permitless carry laws causally increase handgun deaths, overall and by homicide and accident and aggravated assaults by gun, along with in support of one mechanism

[12] A similar potential source of bias is from the use of always treated states within the control group, but we avoid this by excluding Vermont, the only state with permitless carry before our sample period begins, from all samples.

[13] Our stacked DiD models continue to weight by average sample period population and cluster standard errors by state.

by which this occurs being a corresponding rise in the demand for handguns. Based on Table 8 in conjunction with the event study plots, the traditional DiD model finding of significant increases in forced-entry burglaries and thefts appears more tenuous.

5 Conclusion

This paper presents the first detailed analysis in the economic literature on the effects of permitless carry laws on firearm deaths and demand, accompanied by an investigation of their effects on crime. Using an annual state panel from 1999 to 2023, we find that upon transition from shall issue to permitless carry, deaths by handgun – i.e., excluding known long gun deaths – increase significantly by around 3%, with the response beginning just prior to adoption. This is attributable largely to additional handgun homicides, though unintentional handgun deaths also significantly rise. Aggravated assaults by gun likewise rise significantly, though robberies by gun are unaffected. Some evidence suggests substantive increases in forced-entry burglaries and thefts, though these might be an artifact of DiD comparisons with already-treated states. Long gun deaths and nongun mechanisms of death and crime are unaffected by permitless carry adoption.

Corresponding rises in the presence of licensed gun dealers, who appear to begin entering markets in advance of law adoption, and federal background checks for handgun transfers are consistent with the possibility that the effects on deaths and assaults are triggered in part by an upward shift in handgun demand accompanying the newfound ability to engage in concealed carrying without needing to acquire a permit. This is consistent with previous research demonstrating 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). A first-stage decline in federal background checks for permits is also apparent.

The results of our study have significant policy implications, especially with half of all adopting states, including high-population states such as Texas, Florida, Ohio, and Georgia, having done so since 2021. Our estimates underscore the importance for policymakers to consider the potential impacts on public safety when contemplating the deregulation of concealed handgun carry laws.

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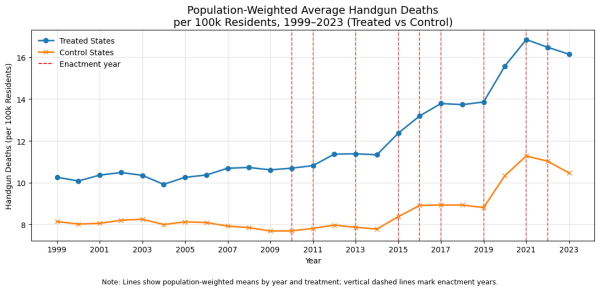
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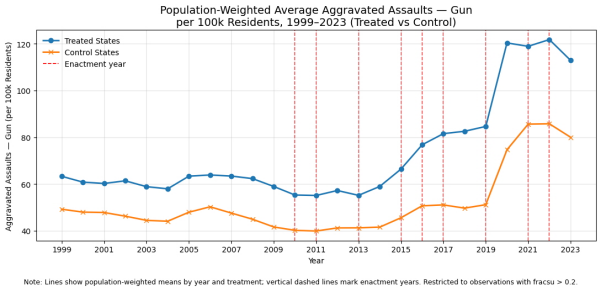
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Figures

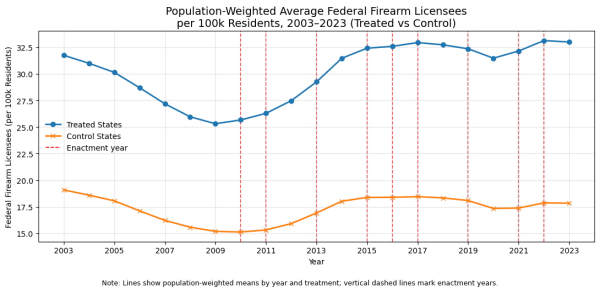
Figure 1: Trends in handgun deaths, aggravated assaults with gun, federally licensed gun dealers, and handgun background checks in treated vs. control states (1999–2023)



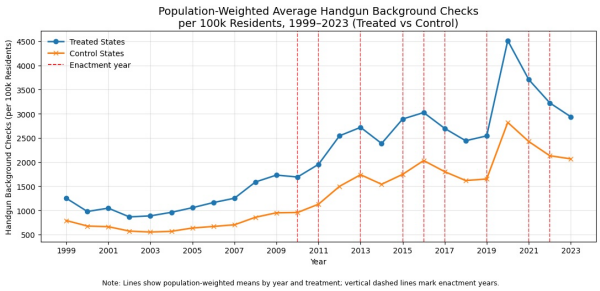
(a) Handgun deaths



(b) Aggravated assaults with gun



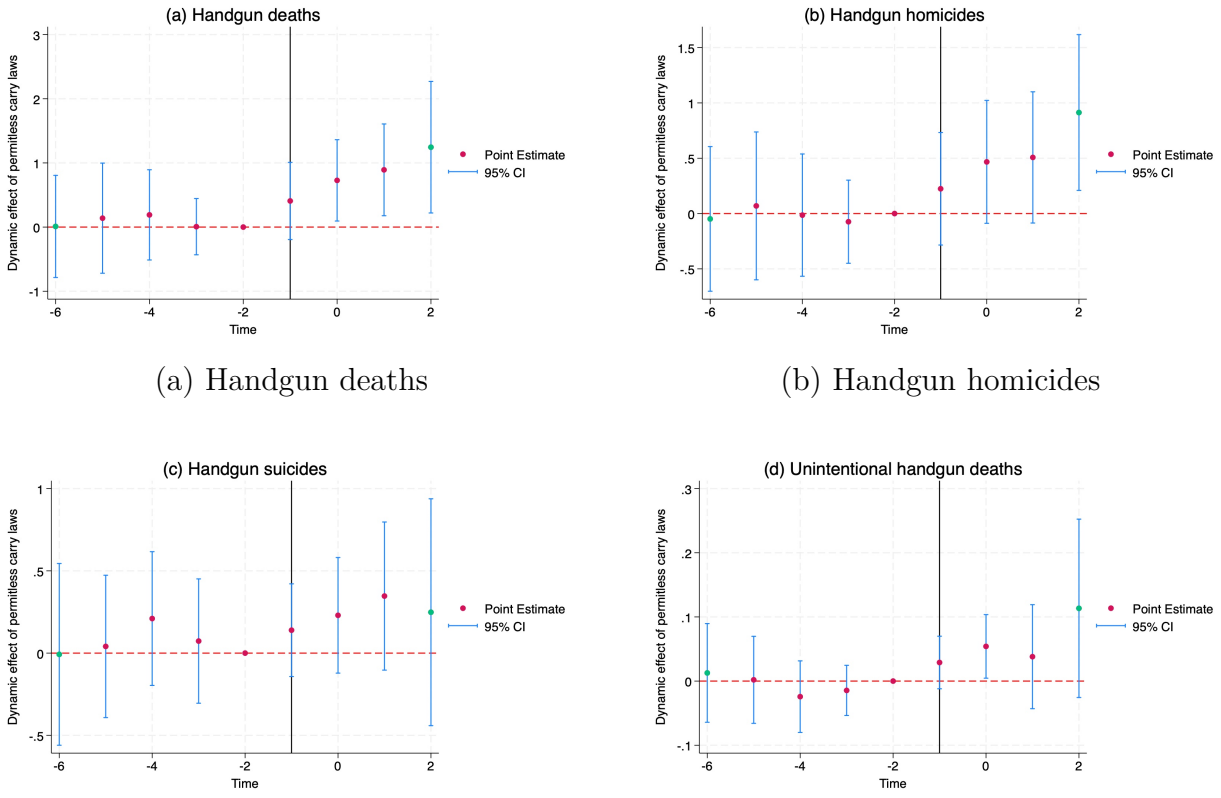
(c) Licensed gun dealers



(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 effects of permitless carry on deaths per 100,000 residents, by injury and gun type



(a) Handgun deaths

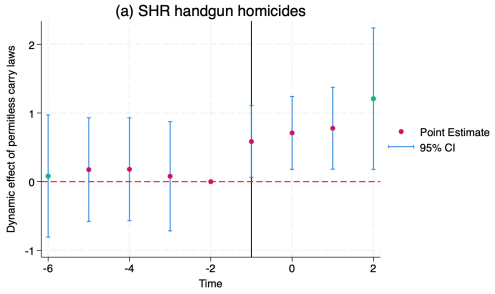
(b) Handgun homicides

(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.

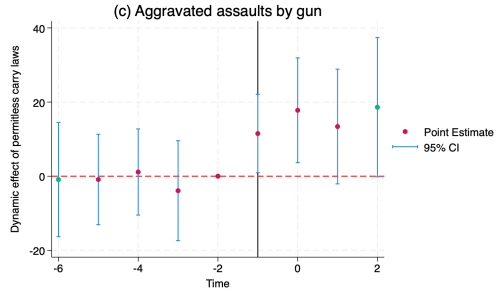
Figure 3: Dynamic effects of permitless carry on crime outcomes (per 100,000 residents)



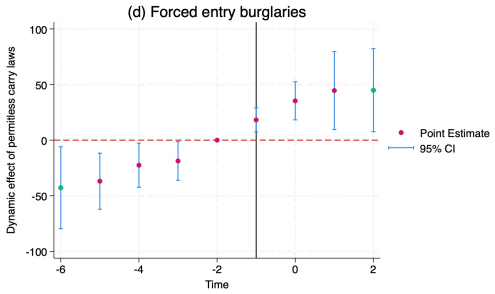
(a) SHR handgun homicides



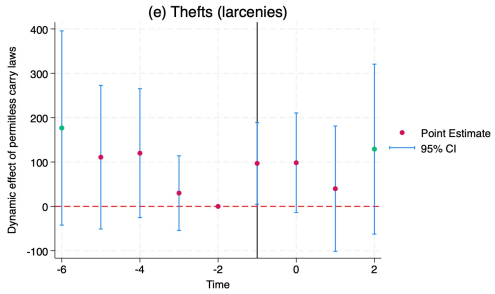
(b) SHR murder + nonnegligent homicides



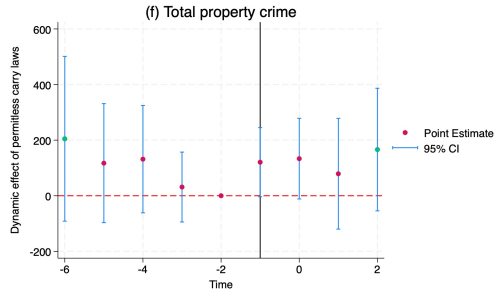
(c) Aggravated assaults by gun



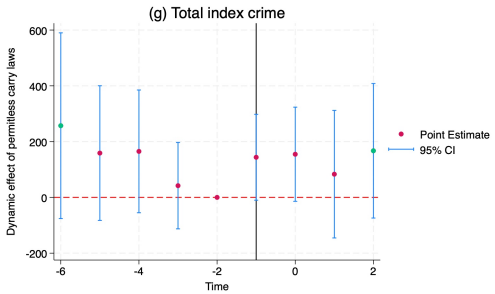
(d) Forced entry burglaries



(e) Thefts (larcenies)



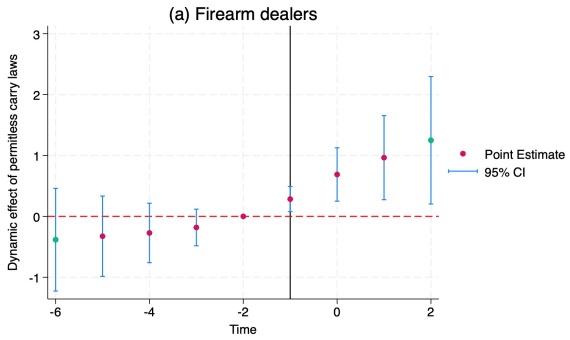
(f) Total property crime



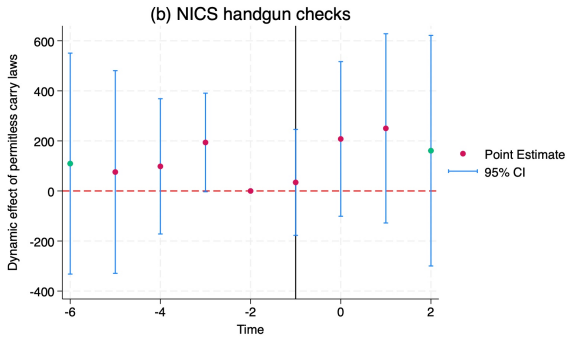
(g) Total index crime

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.

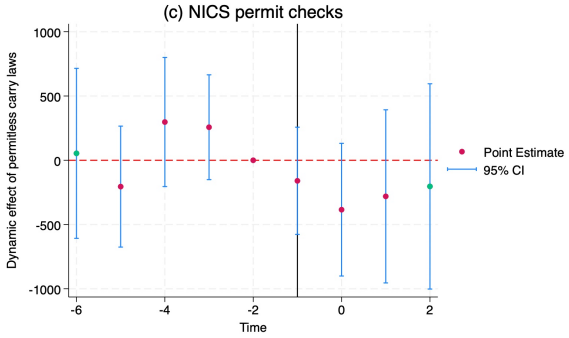
Figure 4: Dynamic effects of permitless carry on federal firearm licensee and background check counts (per 100,000 residents)



(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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.