Permitless Carry and Police Killings*

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Abstract

Both media and researchers have grown increasingly interested in the racial differences in victimization from fatal use of force by law enforcement. Whether gun policy plays a role in this phenomenon, however, is incompletely understood. We investigate racial heterogeneity in the effects of permitless carry laws on deaths from police use of force in events from 2013 to 2024 compiled by Mapping Police Violence. Victims who are Black or Hispanic, defined mutually exclusively, together comprise 44% of the police killings in this sample. A difference-in-differences design applied to an annual state panel shows that adoption of permitless carry increases per capita killings by a statistically significant 24% and 19% for Black and Hispanic victims, respectively, but neither statistically nor substantively does so for victims who are White or of other races. The effects on Black and Hispanic victims primarily represent gunshot killings, with significant portions also reflecting incidents initiated by 911 calls, incidents where the victim showed signs of mental illness or drug use, and incidents where the victim was unarmed. The policy of permitless carry is therefore socially costly in terms of additional police killings, and that cost appears to fall entirely on Black and Hispanic victims.

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1 Introduction

Over the past decade, lethal police violence against people of color has become one of the most pressing social and political issues in the United States. The killing of Michael Brown in 2014 brought national attention to police use of force, and the killing of George Floyd in 2020 sparked one of the largest protest movements in US history, renewing calls for policing reforms and remedies for racial discrimination (Buchanan et al., 2020). Despite these urgent calls for reform, there is limited empirical evidence on the factors that drive officers' use of deadly force, including the policy environment in which police operate. In this paper, we study how police killings have responded to one of the most significant recent changes in US gun policy: the transition from shall-issue to permitless carry laws, which allow civilians to carry a concealed handgun with no permit, background check, or firearms training.

This transition may have affected police killings by changing civilian carrying behavior and the conditions under which police interact with the public. First, by making public carry easier, these laws may have increased the likelihood that officers encounter armed civilians, including individuals who might previously have been denied a permit or required to complete firearms training. [1] At the same time, the dismantling of the permitting system may have made it harder for police to identify and deter illegal handgun carrying, lowering criminals' expected cost of carrying guns unlawfully (Parker, 2026). Second, consistent with these changes, recent research on permitless carry laws shows that permitless carry increases violent crime, particularly crimes involving firearms (Parker, 2026; Li and DeSimone, 2025), potentially exposing officers to greater risks of gun violence in the course of their work. Together, such effects may heighten officers' perceived danger during encounters, raising the cost of hesitation and potentially lowering their threshold for using lethal force.

We use annual state-level data on police killings from Mapping Police Violence (MPV) for 2013–2024 to estimate the effects of permitless carry laws. Specifically, we exploit differences in the timing of adoption across 24 transitioning states in a difference-in-differences (DiD) design, comparing changes in police killings in the adopting states with changes over the same period in the states without permitless carry laws. We further construct police-killing rates by victim race, armed status, and incident characteristics such as signs of mental illness or substance use.

The main finding of the paper is that permitless carry laws increase police killings significantly both statistically and substantively, but only for victims who are Black or Hispanic. Following permitless carry adoption, rates of police killings rise by 24% and 19% among Black and Hispanic victims, respectively, but change negligibly for victims who are White or of other races. Given that these race categories are defined mutually exclusively and the effects on Black and Hispanic victims

[1] Recent research shows that personal safety is an important motive behind the demand for guns (Alsan et al., 2026). By lowering the cost of carrying, permitless carry may therefore lead more civilians to carry guns in public for self-protection purposes.

are each large and of similar proportion, we combine the two groups to explore robustness and effect heterogeneity across incident characteristics. Similarly, we combine victims who are White or of other races into one group and check whether the negligible overall effect masks meaningful impacts for certain types of incidents.

The predicted post-policy increase in police killings is 22.6% for Black and Hispanic victims together vs. 0.2% for victims who are White or of other race groups. The former finding is largely invariant to myriad specification permutations regarding sample composition, construction of the comparison group, covariates held constant, and functional form of the shootings outcome.

The effects of permitless carry on the two combined racial groups, but especially those on Black and Hispanic victims, vary substantially across several dimensions of incident characteristics. Most of the impact on the Black and Hispanic group arises from killings by gunshot, which is also how most killings occur. Perhaps more disturbingly, substantial portions of the effect also reflect incidents initiated by 911 calls rather than officers, incidents where the victims showed signs of mental illness or drug use, and incidents where the victim was unarmed. Permitless carry also raises police killings significantly among Black and Hispanic victims reported to have attacked an officer or brandished a weapon and Black and Hispanic victims reported to be armed, although both of these scenarios are likelier among victims who are White or of other races. Across none of these dimensions does permitless carry adoption increase police killings in the combined group of victims who are neither Black nor Hispanic.

Finally, preliminary analysis of FBI data on killings and assaults of police officers fails to show statistically significant responses to permitless carry adoption, regardless of whether the killings were felonies or accidents or whether the assaults were by gun or resulted in injury. This suggests that the effects of the laws on killings by police are unlikely explained as reactions by officers to avert expected consequences in terms of personal harm. Even significant impacts of permitless carry on injuries or deaths of officers, however, would not account for the observed racial heterogeneity in such reactions absent evidence of similar differences in the racial composition of the officers' assailants.

This paper contributes to three strands of literature. First, it relates to research on the social costs and determinants of police violence. Prior work shows that police violence imposes substantial costs on affected communities. For instance, police killings reduce the educational performance of affected students (Ang, 2021) and harm the health of Black Americans (Bor et al., 2018; Venkataramani et al., 2024), while high-profile incidents of police violence reduce civilian engagement with law enforcement (Ang et al., 2025). [2] A growing literature examines what drives officers' use of force, showing that it is shaped by their peers (Holz et al., 2023), field training officers (Adger et al., 2026), and access to military equipment (Masera, 2021). Most of this work focuses on factors internal to police agencies. We contribute by showing that the external legal environment also

[2]More broadly, institutional mistreatment of Black Americans can erode trust in public institutions, with lasting consequences for their well-being (Alsan and Wanamaker, 2018).

shapes officers' use of lethal force.

Second, the paper contributes to the literature on racial disparities in policing. This work documents substantial racial disparities in police use of force (Fryer, 2019; Knox et al., 2020; Edwards et al., 2019; Weisburst, 2019; Gonçalves et al., 2025) and shows that these disparities depend on officer race, gender, and workforce diversity (Ba et al., 2021; Hoekstra and Sloan, 2022; Cox et al., ming), police leadership (Wu, 2021), individual officers' bias (Goncalves and Mello, 2021), the political climate (Grosjean et al., 2023), and protests (Cunningham and Gillezeau, 2018; Skoy, 2021). We contribute to this literature by showing that a race-neutral gun policy change can widen racial disparities in police killings, consistent with officers relying more heavily on group-based priors about perceived danger during encounters.[3]

Third, the paper speaks to the literature on the effects of concealed carry laws on crime and violence. Early research on concealed carry laws largely focused on the transition from no-issue or may-issue regimes to shall-issue laws (Lott and Mustard, 1997; Ludwig, 1998; Bartley and Cohen, 1998; Benson and Mast, 2001; Rubin and Dezhbakhsh, 2003; Donohue, 2004; Barati, 2016; Siegel et al., 2017), and recent studies provide consistent evidence that shall-issue laws are associated with higher rates of homicide and other violent crime (Luca et al., 2017; Siegel et al., 2017; Donohue et al., 2019, 2025; Colmer and Doleac, 2026). A growing body of work has begun to examine the shift from shall-issue to permitless carry, finding increases in violent crime (Parker, 2026) and gun deaths (Li and DeSimone, 2025). [4] We extend this literature by studying the effects of permitless carry laws on police violence, an outcome so far largely overlooked, and thereby shed light on the broader social costs of concealed carry deregulation.

The remainder of the paper proceeds as follows. Section 2 provides background on concealed carry laws and describes our data. Section 3 presents our empirical strategy. Section 4 reports our main results and robustness checks, and Section 5 concludes.

2 Background and Data

2.1 Concealed Carry Laws in the United States

Concealed weapon carry regulation in the United States has become substantially less restrictive over the past several decades. Through the 1970s, most states either prohibited civilians from carrying concealed handguns or operated under may-issue regimes, in which local authorities had broad discretion to grant or deny permits and applicants typically had to demonstrate a specific need. Beginning in the 1980s, most states shifted to shall-issue laws, which require authorities to

[3]For a related example, Cunningham and Gillezeau (2026) show that another race-neutral policy, the introduction of collective bargaining rights for police, increased police killings primarily of non-White civilians.

[4]Using Gun Violence Archive data and synthetic control methods, Doucette et al. (2022) find that permitless carry increased officer-involved shootings. Their analysis, however, covers 2014–2020 and therefore focuses only on early adopters of permitless carry, and it does not examine the race of those shot by police.

grant a permit to any applicant not legally prohibited from owning a firearm, subject to a background check, a fee, and in many states, completion of firearms training (Donohue et al., 2025). More recently, states have moved further by adopting permitless carry, which allows firearm owners to carry a concealed handgun in public without first obtaining a license or undergoing a background check. After Alaska's adoption in 2003,[5] the pace accelerated sharply after 2015, and by 2024, 29 states allowed civilians to carry a concealed handgun without a permit. The remaining 21 states and Washington, DC, continue to issue permits on a shall- or may-issue basis. In our sample period, every adopting state transitioned from a shall-issue regime, so our estimates capture the effect of permitless carry relative to the outcomes under shall-issue laws. Figure 1 shows a map of states by permitless carry adoption status and year of adoption.

2.2 Police Killings Data

Our main outcome is the annual state-level rate of police killings per 1,000,000 residents by victim race and incident characteristics. We obtain data on police killings from Mapping Police Violence, a research collaborative that has compiled records of people killed by law enforcement in the United States since 2013 (Campaign Zero, 2026). MPV identifies incidents from crowdsourced databases, including FatalEncounters.org, and supplements them with police reports, news coverage, and other public records. MPV also records information on the circumstances of each incident, including the victim's race, age, and gender; whether the victim was armed; and, where available, whether the victim showed signs of mental illness or substance use and whether the victim was fleeing. The MPV data are widely used across economics (Moreno-Medina et al., 2025), political science (Morris and Shoub, 2024), and criminology (DeAngelis, 2024) to study police killings.

2.3 LEOKA Data

We also investigate the effect of permitless carry adoption on killings and nonfatal assaults of police officers as reported by the FBI's Law Enforcement Officers Killed and Assaulted (LEOKA) Program. Outcomes include counts of officers killed, separately for felonies and accidents, and nonfatally assaulted, separately by whether the assaults resulted in injury and for each injury category separately for those committed by gun and other means.

This analysis is preliminary. Thus far we have assumed that assaults on officers are always reported and therefore treat law enforcement agencies reporting incompletely or not at all in a given year as having zero counts. This deviates from the standard practice with FBI crime data of using counts only from agencies that report data for all 12 months of a year and dividing by the population covered by these agencies to construct crime rates. We will compare results of applying that approach to the LEOKA data in our next version. In addition, while most killings of

[5] Vermont has never regulated concealed weapon carrying so has always been considered a permitless carry state.

police officers are by gun, some are not, and the main LEOKA file does not report separate totals for gun and nongun killings. However, these data are reported in a separate FBI file and will be incorporated in our next version.

2.4 Controls and Summary Statistics

We control for a set of other state gun laws that research has shown to affect firearm-related outcomes (Cheng and Hoekstra, 2013; DeSimone et al., 2013; McClellan and Tekin, 2017; Edwards et al., 2018; Anderson et al., 2021; Koenig and Schindler, 2023). These include stand-your-ground (SYG) laws, child access prevention (CAP) laws, permit-to-purchase requirements, mandatory waiting periods, and universal background checks.

Our time-varying controls include demographic, economic, and crime and enforcement characteristics. Demographic controls include state population and population shares by gender (female), race and ethnicity (Black, White, Asian, multiracial, and Hispanic), and age (13–20, 21–61, and 62 and older). Economic controls include the unemployment rate, the employment-to-population ratio, real per capita income (in 2024 dollars), and the poverty rate. Crime and enforcement controls include gun homicides and one-year lags of the number of prisoners and law enforcement officers.

3 Empirical Strategy

To estimate the effect of permitless carry laws on police killings, we exploit within-state variation generated by the transition of 24 states from shall-issue to permitless carry regimes between 2013 and 2024. We use a DiD design with staggered adoption, comparing changes in police killings in states that adopted permitless carry with the changes in states that did not, with an emphasis on comparisons among states in the same census region.

We estimate two-way fixed effects (TWFE) models using an annual state-level panel covering 2013 to 2024. The sample includes the District of Columbia and excludes the states that adopted permitless carry before 2013 (Alaska, Arizona, Vermont, and Wyoming). Our main specification is

$$Y_{it} = \alpha + \beta_1 \text{Permitless}_{it} + \beta_2 \text{MayIssue}_{it} + \Theta X_{it} + \gamma_i + \rho_r \times \tau_t + \gamma_i \times f(t) + \epsilon_{it}, \tag{1}$$

where Y_{it} is the number of victims killed by police per 100,000,000 residents in state i and year t for a given victim group or incident type. Permitless_{it} indicates that a permitless carry law is in effect, and MayIssue_{it} indicates a may-issue regime. X_{it} is a vector of time-varying state controls described in Section 2, γ_i is state fixed effects, $\rho_r \times \tau_t$ is census region-by-year fixed effects, and $\gamma_i \times f(t)$ is state-specific time trends. All regressions are weighted by state population, and standard errors are clustered at the state level.

The identifying variation comes from Permitless_{it}, an indicator equal to one in year t if a per-

mitless carry law took effect in state i on or before July 1 of that year. For laws taking effect after July 1, the indicator switches on in year $t+1$. Under this coding, a state is treated as permitless in a given year if the law was in effect for at least half of that year.

Our models also include MayIssue_{it}, an indicator for states with a may-issue regime, so that the coefficient on Permitless_{it} can be interpreted relative to the outcomes in states with shall-issue laws. Within our sample period, this indicator primarily captures the states that operated under may-issue laws until the Supreme Court's 2022 decision in *New York State Rifle & Pistol Association v. Bruen*, after which they effectively became shall-issue. We code transitions from may-issue to shall-issue using the same July 1 rule that we apply for permitless carry.

In addition to conventional cluster-robust t-statistics, we report p-values from the wild cluster bootstrap (Cameron et al., 2008), which provides more reliable inference when the number of clusters is small or treatment is concentrated in a limited number of clusters. We use 100,000 bootstrap replications so that simulation noise in the bootstrapped p-values is negligible.

To address concerns about TWFE DiD estimates being biased when treatment timing is staggered and effects vary across cohorts or over time (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Baker et al., 2022), we estimate stacked DiD models as a robustness check (Cengiz et al., 2019; Baker et al., 2022). For each adoption cohort, we compare the adopting states with clean controls—either never-adopting states alone or never-adopting states combined with later adopters—and stack the cohort-specific datasets with cohort-by-state and cohort-by-year fixed effects.

Causal identification of our model relies on the parallel trends assumption: namely, that absent the adoption of permitless carry, police killings in the adopting states would have evolved similarly to those in nonadopting states within the same census region. To provide evidence supporting this assumption and to examine the dynamic effects of permitless carry on police killings, we estimate an event study version of Equation (1):

$$Y_{it} = \alpha + \sum_{\substack{k=-4 \\ k\neq -1}}^{2} \beta_k \, \text{Permitless}_{it}^{k} + \delta \, \text{MayIssue}_{it} + \Theta X_{it} + \gamma_i + \rho_r \times \tau_t + \gamma_i \times f(t) + \epsilon_{it}, \tag{2}$$

where $\text{Permitless}_{it}^{k}$ is an indicator equal to one if year t is k years relative to the year in which state i's permitless carry law took effect, defined using the same July 1 rule as above. The endpoint indicators are binned, so that $\text{Permitless}_{it}^{-4}$ equals one for all years four or more years before adoption and $\text{Permitless}_{it}^{2}$ equals one for all years two or more years after adoption. The year before adoption ($k=-1$) is the reference period. All other terms are defined as in Equation (1). The coefficients β_k for $k<-1$ test for differential pre-adoption trends between adopting and nonadopting states, and the coefficients for $k \geq 0$ trace out the evolution of the effect after adoption.

4 Results

Table 1 displays our main results. MPV codes victim race as a single category, so that White (46.4% of the police killings in the sample), Black (26.1%), and Hispanic (18.4%) are mutually exclusive. The "other race" category includes Asian (1.6%), American Indian and Alaska Native (1.5%), Native Hawaiian and Pacific Islander (0.6%), and unknown (5.4%). Permitless carry adoption significantly increases police killings—both statistically and substantively—for both Black and Hispanic victims. Relative to the rate in the year prior to law adoption in treated states, permitless carry is predicted to raise rates of police killings by 24% among Blacks and 19% among Hispanics. In contrast, the analogous effects are not statistically significant and are quantitatively negligible for both White victims and those of other races, even though prior to adoption, killings of White victims were nearly twice as likely as killings of Black and over four times as likely as killings of Hispanic ones.

Given the similarity in the coefficients' significance and relative magnitude for the Black and Hispanic groups, on the one hand, and the White and other race groups, on the other, we combine each pair in our subsequent analyses. Table 2 explores the robustness of the large and significant effects on Black and Hispanic victims. The top row reports the combined effect, the sum of the two coefficients in Table 1, which is significant at the 1% level and represents 22.6% of the pre-adoption year mean across treated states. The corresponding event study plot in Figure 3.a indicates an absence of a pretrend and a statistically significant impact that materializes immediately upon adoption.

Though we exclude from the sample the four states that had enacted permitless carry before the start of our analysis period, for the same reason, one might be concerned about the standard DiD estimator moving sample states into the comparison group in all periods following that in which they adopted the policy. Table 2 Panel A therefore reports estimates from two stacked DiD models: one that excludes from the comparison group all states that adopted the law during the sample period and another that excludes the adopting states for the years after the law's adoption. In both cases, the estimated policy coefficient is somewhat larger in magnitude than baseline and remains significant at 1%.

Table 2 Panel B shows estimates from two further alternative models. The first omits all covariates other than the fixed effects and state trend terms. The coefficient size decreases, albeit only slightly, and its significance holds at 1%, which implies that the natural experimental structure of the panel data model is largely responsible for effect identification. The second model specifies the outcome in inverse hyperbolic sine form, which replicates the natural log slope but accommodates cells with zero shootings, which correspond to 82 of the 564 state-by-year observations. In fact, the midpoint of the percentage increases in police killings implied by the calculus approximation and the exact calculation using this estimate almost exactly track the baseline estimate.

Table 2 Panel C indicates that the permitless carry coefficient is altered only minimally when state trends are excluded, when the comparisons are not restricted to states within the same census

region plus the South Atlantic division, when the South is taken as one region for the comparisons, and when the comparisons are restricted to the same census division (with the eight sample Northeast states combined because the three Middle Atlantic division states are never treated).

Table 2 Panel D investigates changes in sample composition. The top row indicates that a traditional DiD model with the four always-treated states included yields an almost identical estimate. The same is true when we exclude Louisiana, which the baseline sample includes as a never-treated state because it adopted on July 4, 2024 (that is, it missed being classed as a 2024 adopter by only three days). The remaining seven rows exclude single census regions or divisions from the sample. The coefficient pattern implies that the largest effects correspond to the eight South Central division states, whose exclusion reduces the impact by nearly one-third and significance to the 10% level. Both the East and the West South Central divisions apparently contribute to this difference, as excluding either the former (Alabama, Kentucky, Mississippi, and Tennessee) or the latter (Arkansas, Louisiana, Oklahoma, and Texas) lowers the coefficient magnitude by a lesser amount, with significance at 5% maintained.

Table 3 examines effect heterogeneity across various incident characteristics. We successively divide the sample into two along each of eight dimensions for both victims who are Black or Hispanic and victims who are White or of other races. Figure 2 displays this information graphically, and Figure 3 presents event studies corresponding to the significant coefficients.

Table 3 Panel A begins in columns 1 and 2 by stratifying the sample into killings by gunshot and killings by other means. Most of the impact on Black or Hispanic victims operates through killings by gun, but this also describes most police killings for all racial groups.

Columns 3 and 4 show that slightly over half the effect on Black or Hispanic victims stems from incidents initiated by 911 calls, with the rest representing officer-initiated incidents. Although the coefficient is significant only for the former, the proportionate effects are virtually identical. The concern that permitless carry can exacerbate the existing inflation of reports of the eventual victim being armed relative to actual rates of weapon carrying cannot be evaluated with the MPV data. However, the significant positive effect for 911-initiated incidents involving Black or Hispanic victims contrasts with the negative (though nonsignificant) coefficient for such incidents involving victims who are White or of other races.

Columns 5 and 6 focus on whether the officer reported being under attack or the victim having brandished or used a weapon. These all come from one MPV variable so are mutually exclusive choices, with threat levels for remaining incidents characterized as involving a sudden threatening movement, other, none, or unreported. Most of the overall impact on Black or Hispanic victims stems from the 60.7% of incidents in the former category, with a post-adoption increase in police killings of 30.2% which is significant at 1% vs. an insignificant 10.1% for encounters in which officers feel less threatened. In contrast, while officers report feeling more imminently threatened by victims who are White or another race slightly more often (64.3% of fatal encounters), those types of police killings increase minimally (by $< 2\%$) and insignificantly.

Columns 7 and 8 distinguish encounters by whether the victim showed signs of mental illness or drug use before the fatal shooting. While much of the estimated overall impact of permitless carry on Blacks and Hispanics represents victims displaying no evidence of either, about one-quarter of the effect arises from a nearly 29% rise in killings of those whose actions indicated potential mental illness or drug use. Again, no corresponding responses in police killings are observed among victims who are neither Black nor Hispanic.

Moving to Panel B of Table 3, while nearly two-thirds of the total permitless carry effect for Blacks and Hispanics comprises armed victims, the post-adoption increase for unarmed victims is also statistically significant, and in magnitude represents an increase of over 50% from the pre-adoption mean. Yet again, categorizing victims along this dimension does not reveal any previously hidden effects for either armed or unarmed victims who are not Black or Hispanic.

The remainder of Panel B looks at law effects in categories defined by income or demographic share of the census tract in which the fatal encounter occurred. For income, in columns 3 through 5, tracts are characterized by whether their median household income is in the bottom quartile, in the second quartile, or above the median level of household income nationally in the corresponding year. The preponderance of the impact for Black and Hispanic victims occurs in second income quartile tracts, corresponding to the preponderance of incidents before law adoption. However, for White and other race victims, among whom fatal police encounters congregate even more so in second income quartile tracts, the analogous estimate is negative and large though statistically insignificant. In contrast, a positive effect emerges in bottom income quartile tracts that while significant statistically only at 10% represents over half of the pre-adoption mean rate of killings.

Columns 6 and 7 consider the tract share that is Black or Hispanic, with most of the impact for victims either Black or Hispanic eminating from tracts with above median concentrations of Blacks and Hispanics combined, which is where nearly three-quarters of such fatal encounters in the pre-adoption year occur. Meanwhile, police killings of non-Black, non-Hispanic victims likewise increase substantially, with 10% significance, in tracts with above-median Black and Hispanic representation, even though in the pre-adoption year less than one-quarter of such incidents take place in these tracts.

Finally, Table 4 turns to estimating the effects of permitless carry adoption on killings of, rather than by, police officers along with assaults of officers by gun and other means. Any such effects would not necessarily justify our findings but nonetheless might suggest that permitless carry laws prompt violence against police that officers preempt with legal force against their would-be assailants. However, the estimates in Table 4 fail to indicate statistically significant post-adoption jumps in lethal or nonlethal assaults of police officers. The coefficients on killings of police in columns 1 and 2 are larger relative to their pre-adoption means but more so for accidental than for felonious killings, and both are statistically nonsignificant and, most importantly, negative. The gun assault coefficients in columns 3 and 4 are positive and quantitatively meaningful but are one-half to two-thirds smaller proportionally than the coefficient for killings of Black and Hispanic victims

by police and are statistically nonsignificant. The coefficients for nongun assaults in columns 5 and 6 are much smaller, again statistically nonsignificant, and negative in the case of injurious assaults. Of course, even large and significant coefficients in these regressions would not explain why we observe racial differences in how permitless carry laws increase fatal use of force by police.

5 Conclusion

Our analysis establishes that a statuatorily race-neutral policy, allowing legal gun owners to carry concealed handguns without a separate dedicated permit, in fact is not race neutral in at least one socially harmful respect. Permitless carry adoption predicts statistically and substantively significant subsequent increases in fatal police encounters for both Blacks and Hispanics but not for others. Meaningful portions of these increases reflect encounters after 911 calls, victims who displayed signs of mental illness or drug use, and unarmed victims.

These results naturally raise several questions. First and foremost, while Blacks and Hispanics are killed by police in proportions that exceed their population representation, it's nonetheless the case that in our 2013-2024 sample period, five in nine victims of police killings are neither Black nor Hispanic. This makes it hard to understand why a law that raises the likelihood that potential criminal suspects police encounter are armed with handguns would increase the fatality of such encounters only for Blacks and Hispanics.

Implications of the results across encounter characteristics are speculative without the data necessary for their direct investigation. Taken together, however, the findings suggest that when permitless carry policies go into effect, officers become more likely to act quickly, particularly by using their guns, before information from 911 callers or judgments they make based on characteristics of the person they encounter or the neighborhood of the encounter can be verified. Various proposed policing reforms regarding how certain types of 911 calls or encounters with those who might be suffering from mental illness are handled seemingly have the potential to mitigate resulting harm when inaccurate assessments turn such encounters deadly.

Two broad conclusions, however, are clear from our analysis. One is that permitless carry laws that were implemented by half of U.S. states over 2015-2024 are socially costly in terms of increased fatal encounters with police upon their adoption. The other is that those costs have been borne virtually entirely by Black and Hispanic victims of police killings.

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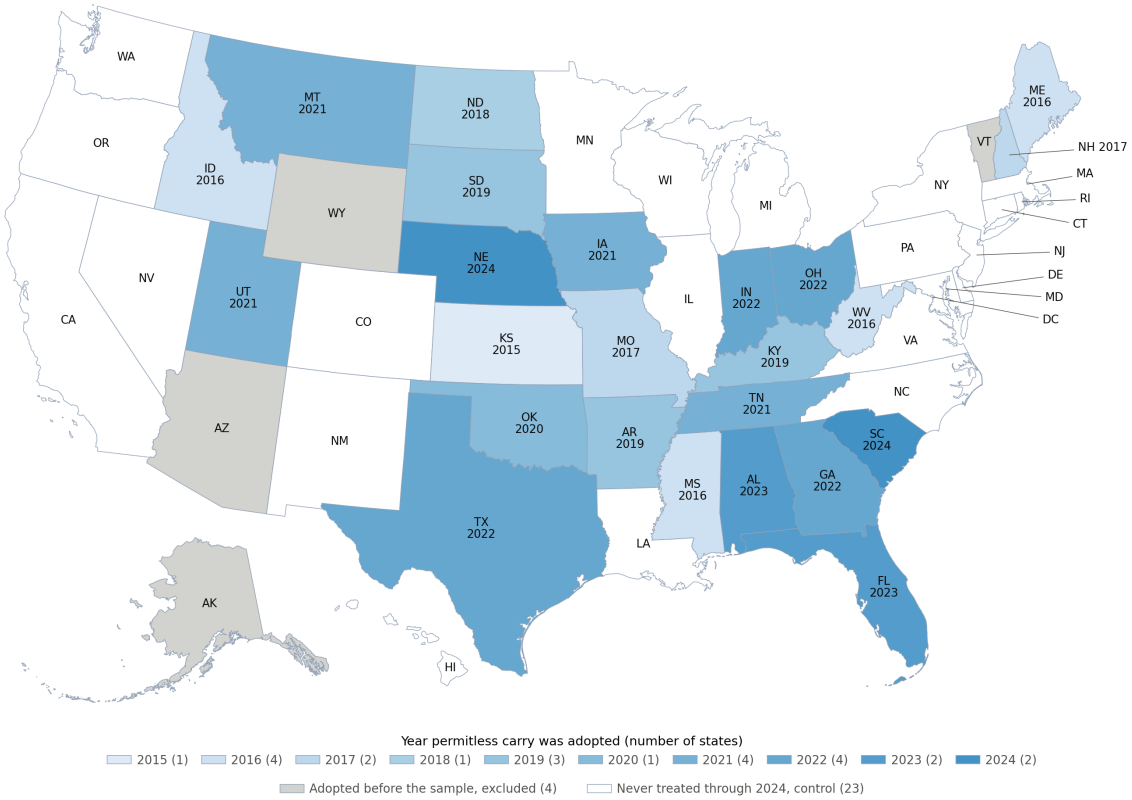
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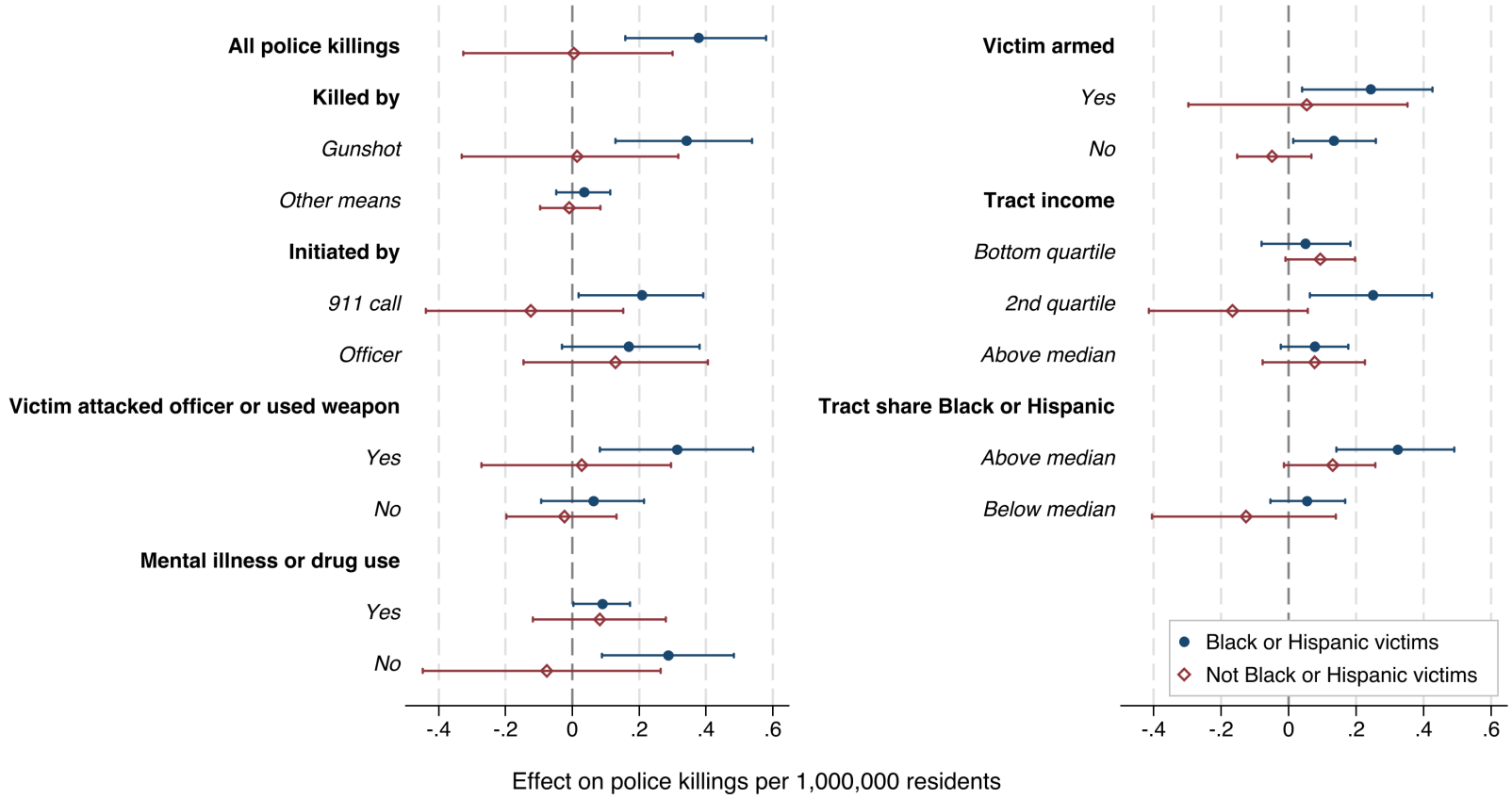
Figures

Figure 1: Permitless Carry Adoption by State and Year



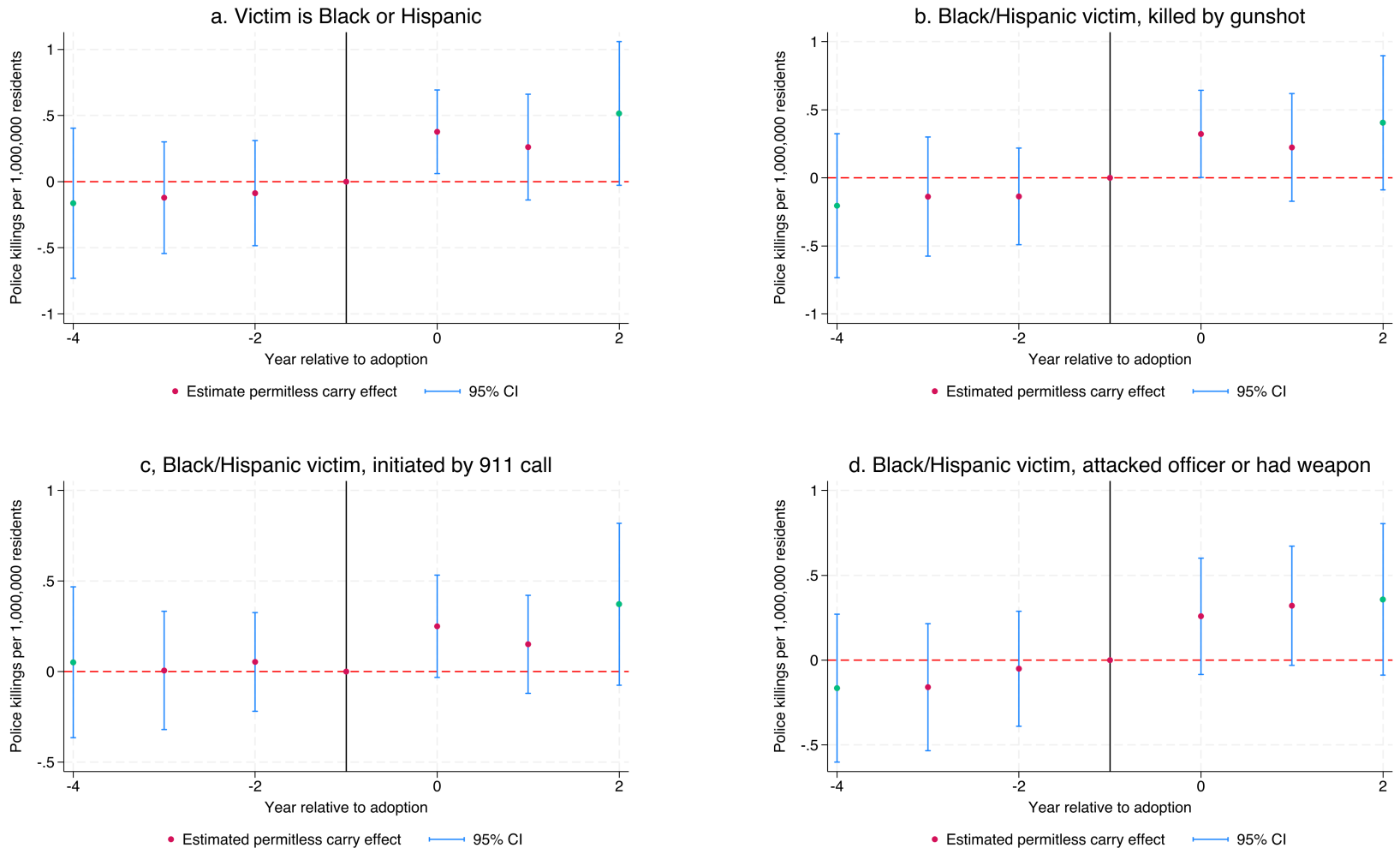
Notes: This figure maps states by their permitless carry adoption status and adoption year. Adoption timing is determined relative to July 1: For a law taking effect on or before July 1, adoption is assigned to that calendar year, and for a law taking effect after July 1, adoption is assigned to the following year. Numbers in parentheses in the legend give the number of states in each group. The four states in gray (Alaska, Arizona, Vermont, and Wyoming) already had permitless carry before the start of the sample period (2013) and are dropped from all analyses. Adoption timing is based on information from RAND (https://www.rand.org/research/gun-policy/law-navigator.html#concealed-carry-laws) and Handgunlaw.us (https://www.handgunlaw.us/documents/Permitless_Carry_States.pdf).

Figure 2: Effects of Permitless Carry by Racial Group and Characteristic



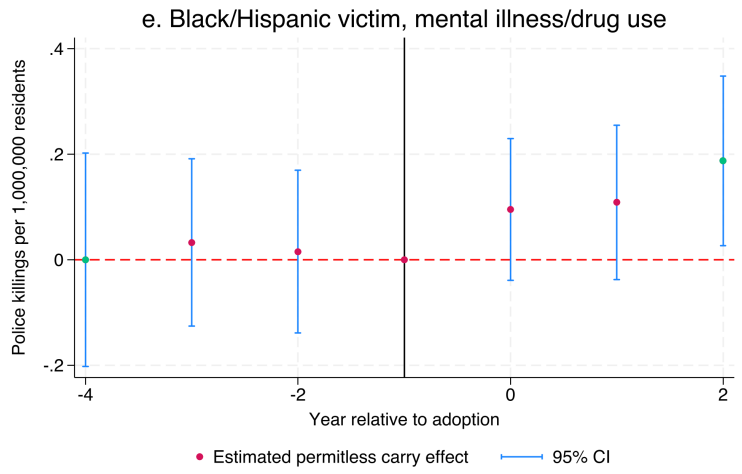
Notes: The figure plots the coefficients and 95% confidence intervals reported in Table 3. Each estimate is from a separate regression of police killings per 1,000,000 residents, for victims in the indicated group and incidents in the indicated category, on an indicator for permitless carry, using the baseline specification in Table 1. Confidence intervals are based on standard errors clustered by state. See Table 3 for variable definitions, wild cluster bootstrap p-values, and pre-adoption means.

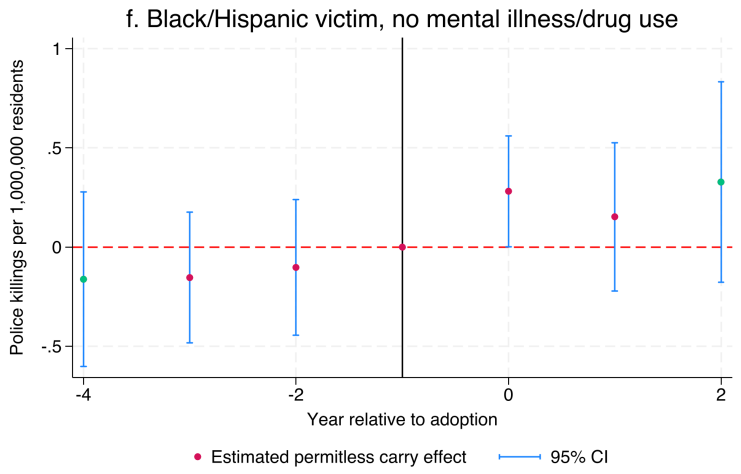
Figure 3: Event Study Estimates of the Effect of Permitless Carry on Police Killings

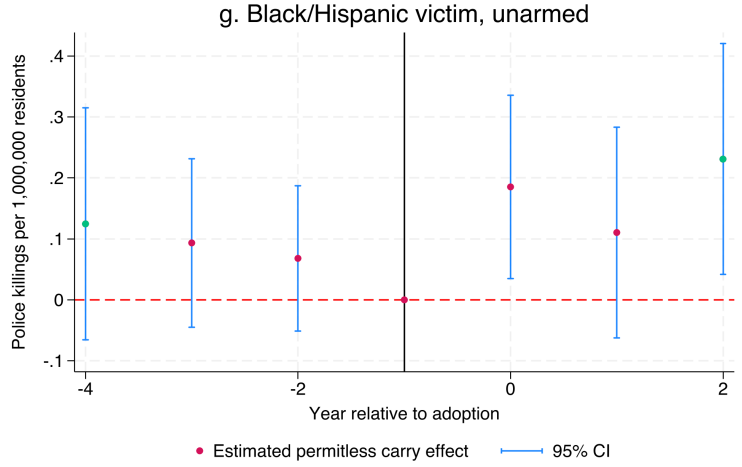


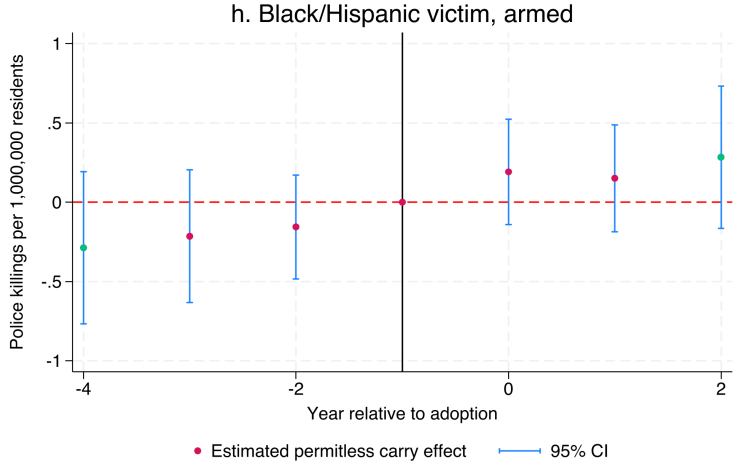
Notes: See the notes to Figure 3.

Figure 3: Event Study Estimates of the Effect of Permitless Carry on Police Killings (continued)



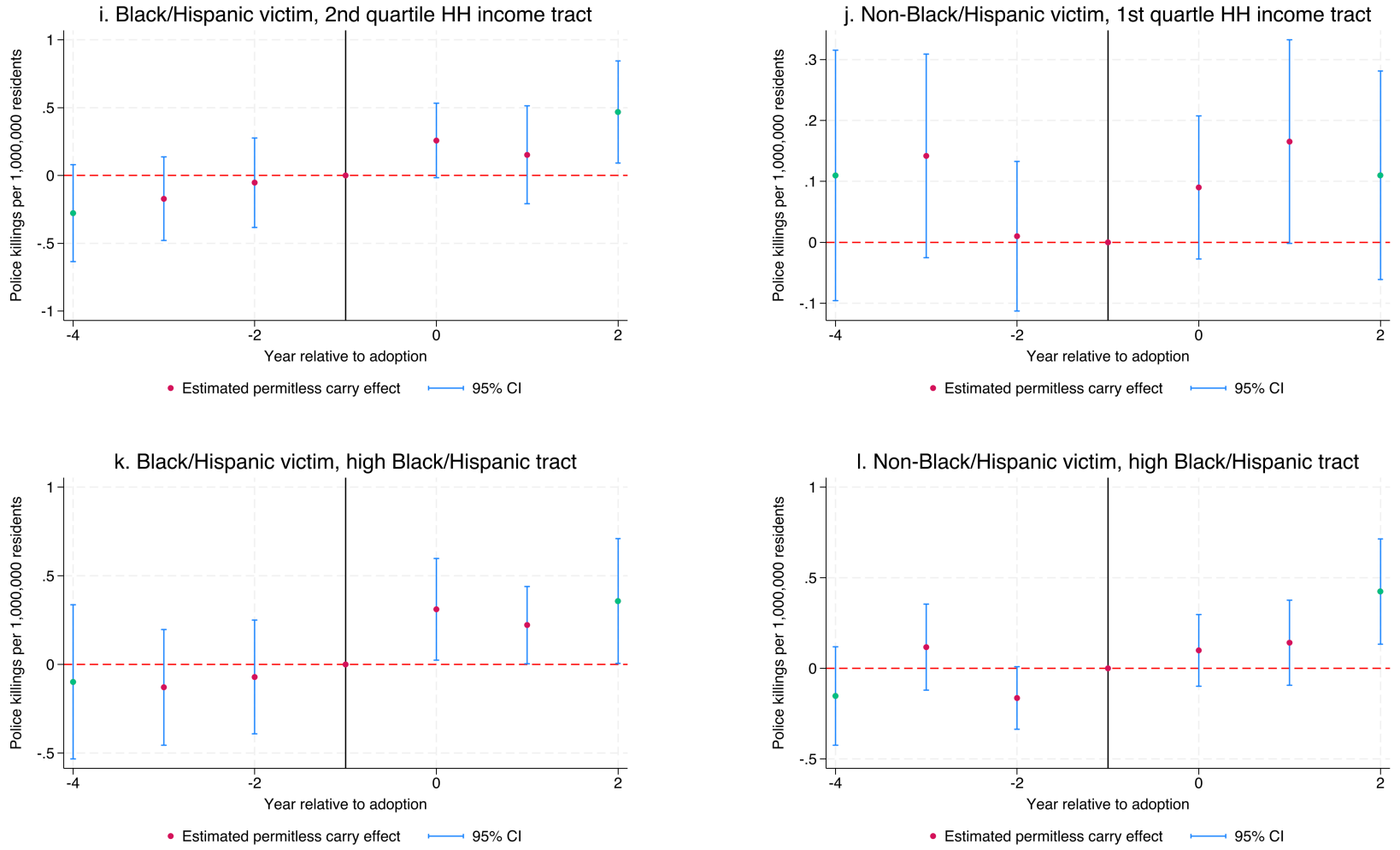






Notes: See the notes to Figure 3.

Figure 3: Event Study Estimates of the Effect of Permitless Carry on Police Killings (continued)



Notes: Each panel plots coefficients and 95% confidence intervals from the event study specification in Equation (2) for the outcome indicated in the panel title, measured as police killings per 1,000,000 residents. Year 0 is the first year a permitless carry law is in effect for at least half the year. The year before adoption (-1) is the reference period. The endpoints are binned: -4 includes all years four or more years before adoption, and 2 includes all years two or more years after adoption. Confidence intervals are based on standard errors clustered by state. The sample consists of 564 state–year observations covering 46 states and the District of Columbia from 2013 to 2024.

Tables

Table 1: Effects of Permitless Carry on Police Killings, by Victim Race

	(1) Black	(2) Hispanic	(3) White	(4) Other
Permitless carry	0.277**	0.101**	0.006	−0.001
(*t-statistic*)	(2.48)	(2.14)	(0.04)	(0.02)
[*Wild bootstrap p-value*]	[0.016]	[0.011]	[0.953]	[0.990]
Pre-adoption mean	1.153	0.520	2.197	0.279
Specification				
State fixed effects	✓	✓	✓	✓
Region × year fixed effects	✓	✓	✓	✓
State-specific linear trends	✓	✓	✓	✓
Population-weighted	✓	✓	✓	✓

Notes: Each column reports the permitless carry coefficient from a separate regression of police killings per 1,000,000 residents with victims of the race specified in the column heading. "Other" includes all victims not identified as Black, Hispanic, or White, including those of unknown race. All regressions include the may-issue indicator, controls for additional state firearm policies, demographic and economic characteristics, and crime and enforcement measures. Regressions are weighted by average state population. t-statistics in parentheses are based on standard errors clustered by state. p-values in brackets are from a wild cluster bootstrap with 100,000 replications. The pre-adoption mean is the population-weighted mean of the dependent variable in adopting states in the year before adoption. The sample consists of 564 state–year observations covering 46 states and the District of Columbia from 2013 to 2024; Alaska, Arizona, Vermont, and Wyoming adopted permitless carry before 2013 and are excluded. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, based on the wild cluster bootstrap p-values.

Table 2: Robustness of Effect of Permitless Carry on Police Killings of Black or Hispanic Victims

	Coefficient	*t*-statistic	*p*-value	Observations
Baseline	0.378***	3.43	0.001	564
Panel A. Estimator				
Stacked DiD, never-treated controls	0.482***	3.19	0.003	3,048
Stacked DiD, not-yet-treated controls	0.457***	2.91	0.005	4,506
Panel B. Controls and functional form				
No time-varying controls	0.338***	2.72	0.005	564
Inverse hyperbolic sine of killings	0.213***	2.81	0.004	564
Panel C. Fixed effects and time trends				
No state-specific trends	0.344***	2.85	0.009	564
Year FE, not interacted	0.322***	2.93	0.003	564
Year-by-Census-region FE (4 regions)	0.330***	2.98	0.001	564
Year-by-division FE (8 groups)	0.345***	3.21	0.001	564
Panel D. Sample				
Includes pre-2013 adopters	0.381***	3.48	0.001	612
Excludes Louisiana	0.372***	3.35	0.001	552
Excludes Northeast	0.371***	3.45	0.001	468
Excludes Midwest	0.372***	2.84	0.008	420
Excludes South Atlantic	0.409***	3.32	0.002	456
Excludes South Central	0.265*	1.72	0.078	468
Excludes East South Central	0.291**	2.35	0.016	516
Excludes West South Central	0.310**	2.25	0.020	516
Excludes West	0.362***	3.22	0.003	444

Notes: Each row reports the permitless carry coefficient from a separate regression of police killings of Black or Hispanic victims per 1,000,000 residents. The baseline is the specification in Table 1, which includes the may-issue indicator, controls for additional state firearm policies, demographic and economic characteristics, crime and enforcement measures, state fixed effects, region-by-year fixed effects, and state-specific linear trends and is weighted by average state population. Panel A replaces the estimator with stacked difference-in-differences, using never-treated or not-yet-treated states as controls; observations are stacked state–years, and *p*-values are from cluster-robust *t*-tests. Panel B drops all time-varying controls except the may-issue indicator or uses the inverse hyperbolic sine of the number of killings as the dependent variable. Panel C drops the state-specific trends or changes the time fixed effects. At baseline, the year is interacted with five regions: Northeast, Midwest, South Atlantic, South Central (East and West South Central combined), and West. The four-region row uses census regions, which combine South Atlantic and South Central into a single South; the eight-group row uses the nine census divisions with New England and Middle Atlantic combined. Panel D adds the four states that adopted permitless carry before 2013 (Alaska, Arizona, Vermont, and Wyoming), drops Louisiana, or drops one region or division at a time using the groupings above. *t*-statistics are based on standard errors clustered by state. Except in Panel A, *p*-values are from a wild cluster bootstrap with 100,000 replications. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3: Effects of Permitless Carry by Racial Group and Incident Characteristic

Panel A. Cause of death, encounter initiation, and victim behavior

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Killed by		Initiated by		Attacked officer or brandished weapon		Signs of mental illness or drug use	
	Gunshot	Others	911 call	Officer	Yes	No	Yes	No
Black or Hispanic victims	0.342***	0.036	0.209**	0.169	0.314***	0.064	0.090**	0.288***
(*t-statistic*)	(3.25)	(0.84)	(2.20)	(1.58)	(2.79)	(0.80)	(1.92)	(2.76)
[*Wild bootstrap p-value*]	[0.002]	[0.470]	[0.031]	[0.103]	[0.007]	[0.445]	[0.042]	[0.004]
Pre-adoption mean	1.52	0.156	0.929	0.744	1.04	0.636	0.314	1.36
Not Black or Hispanic victims	0.014	−0.010	−0.124	0.129	0.029	−0.024	0.082	−0.077
(*t-statistic*)	(0.09)	(0.20)	(0.81)	(0.90)	(0.19)	(0.27)	(0.77)	(0.43)
[*Wild bootstrap p-value*]	[0.919]	[0.846]	[0.412]	[0.376]	[0.848]	[0.774]	[0.443]	[0.689]
Pre-adoption mean	2.35	0.130	1.63	0.844	1.61	0.864	0.659	1.82

Panel B. Victim armed status, neighborhood characteristics, and all incidents

	Victim armed		Tract income relative to median household income			Tract share Black or Hispanic		Overall
	Yes	No	Bottom quartile	Second quartile	Above median	Above median	Below median	All incidents
Black or Hispanic victims	0.244**	0.134**	0.050	0.250***	0.078	0.323***	0.055	0.378***
(*t-statistic*)	(2.40)	(2.09)	(0.73)	(2.61)	(1.40)	(3.54)	(0.86)	(3.43)
[*Wild bootstrap p-value*]	[0.020]	[0.027]	[0.492]	[0.009]	[0.134]	[0.001]	[0.351]	[0.001]
Pre-adoption mean	1.42	0.249	0.437	0.909	0.327	1.22	0.451	1.67
Not Black or Hispanic victims	0.053	−0.049	0.094*	−0.167	0.077	0.131*	−0.126	0.005
(*t-statistic*)	(0.33)	(0.85)	(1.82)	(1.31)	(0.98)	(1.77)	(0.92)	(0.03)
[*Wild bootstrap p-value*]	[0.746]	[0.418]	[0.077]	[0.153]	[0.336]	[0.076]	[0.395]	[0.959]
Pre-adoption mean	2.27	0.208	0.174	1.67	0.631	0.606	1.87	2.48

Notes: Each coefficient is from a separate regression of police killings per 1,000,000 residents in the victim group (row) and incident category (column) on an indicator for permitless carry, using the baseline specification in Table 1. Absolute *t*-statistics in parentheses are based on standard errors clustered by state. *p*-values in brackets are from a wild cluster bootstrap with 100,000 replications. The pre-adoption mean is the population-weighted mean of the dependent variable in adopting states in the year before adoption. Each regression uses 564 state–year observations. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, based on the wild cluster bootstrap *p*-values.

Table 4: Effects of Permitless Carry on Killings and Assaults of Police Officers

	Killed		Assaulted with a gun		Assaulted by other means	
	Feloniously	By accident	Injured	Not injured	Injured	Not injured
	(1)	(2)	(3)	(4)	(5)	(6)
Permitless carry	−0.046	−0.051	0.143	0.619	−0.738	2.92
(*t-statistic*)	(0.54)	(1.33)	(0.99)	(0.69)	(0.18)	(0.27)
[*Wild bootstrap p-value*]	[0.619]	[0.129]	[0.305]	[0.526]	[0.886]	[0.751]
Pre-adoption mean	0.203	0.007	1.25	9.90	62.39	117.69
Specification						
State fixed effects	✓	✓	✓	✓	✓	✓
Region × year fixed effects	✓	✓	✓	✓	✓	✓
State-specific linear trends	✓	✓	✓	✓	✓	✓
Population-weighted	✓	✓	✓	✓	✓	✓

Notes: All regressions include the may-issue indicator, controls for additional state firearm policies, demographic and economic characteristics, and crime and enforcement measures. Regressions are weighted by average state population. t-statistics in parentheses are based on standard errors clustered by state. p-values in brackets are from a wild cluster bootstrap with 100,000 replications. The pre-adoption mean is the population-weighted mean of the dependent variable in adopting states in the year before adoption. The sample consists of 564 state–year observations covering 46 states and the District of Columbia. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$, based on the wild cluster bootstrap p-values.