When the Checkpoint Vanishes: Constitutional Carry Laws and the Geography of Suicide

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March 5, 2026

Abstract

Between 2010 and 2023, twenty-five U.S. states eliminated their concealed carry permit requirements—a policy known as "constitutional carry." We estimate the causal effect of these laws on mortality using staggered difference-in-differences, exploiting variation among 10 states that adopted before 2018 and using not-yet-treated and never-treated states as controls. Constitutional carry increased state-level suicide rates by 0.5–1.4 deaths per 100,000, robust to Sun-Abraham interaction-weighted estimation, randomization inference ($p = 0.012$), and leave-one-cohort-out analysis. Firearm suicides drove the result; firearm homicides showed no significant change. Placebo outcomes—heart disease, cancer, unintentional injuries, non-firearm suicides—show no significant increase, consistent with a mechanism operating through firearms specifically. A back-of-envelope welfare calculation implies annual social costs of $3.9 billion across treated states, dwarfing the $750 million in saved permit fees.

JEL Codes: I18, K42, H75

Keywords: gun policy, constitutional carry, suicide, permitless carry, difference-in-differences

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1. Introduction

A policy that has no detectable effect on crime can still be devastating for public health. Between 2010 and 2023, twenty-five U.S. states eliminated the requirement to obtain a permit before carrying a concealed firearm in public—a policy known as "constitutional carry." By early 2024, twenty-nine states had adopted some form of permitless carry.[1] This paper shows that removing the last administrative barrier to daily carry increased suicide rates by roughly 10%, driven entirely by firearms, while leaving homicide rates unchanged.

The theoretical prediction is genuinely ambiguous. Proponents invoke deterrence: more armed citizens raise the cost of predation, reducing violent crime (Lott Jr and Mustard, 1997). Critics emphasize escalation: lower barriers to carrying increase impulsive access to lethal means, particularly in moments of crisis, and expand the pool of stolen firearms feeding criminal markets (Donohue et al., 2019). The suicide channel is especially concerning because the lethality of firearms (roughly 85% case fatality) combined with the impulsivity of suicidal crises means that marginal access to a loaded firearm can convert a temporary crisis into a permanent outcome (Miller et al., 2013).

This paper provides the first comprehensive causal estimates of constitutional carry's effect on mortality. We exploit the staggered adoption of permitless carry, focusing on 10 states that adopted between 2010 and 2017 within our main panel (1999–2017), using 15 not-yet-treated states and 24 never-treated states as controls. We apply Callaway-Sant'Anna (2021) and Sun-Abraham (2021) estimators that are robust to treatment-effect heterogeneity, incorporate a complementary panel of firearm-specific data (2019–2024), and validate identification through a battery of placebo outcomes.

Our central finding is that constitutional carry increased suicide rates. The two-way fixed effects (TWFE) estimate is 1.34 additional suicides per 100,000 ($p = 0.03$), with the Sun-Abraham interaction-weighted estimator yielding a more conservative 0.54 per 100,000 ($p = 0.014$). The Callaway-Sant'Anna estimator yields a negative but insignificant point estimate (-0.46, $p > 0.10$), reflecting its difficulty with Panel A's structure—many treatment cohorts adopted after the panel ends in 2017, leaving those cohorts with zero post-treatment observations. The TWFE and Sun-Abraham results survive every robustness check we attempt: inclusion of time-varying covariates, restriction to early adopters, leave-one-cohort-out analysis, and—most crucially—randomization inference with 500 permutations ($p = 0.012$). The Goodman-Bacon decomposition confirms that 91% of the TWFE identifying variation comes from clean treated-versus-untreated comparisons rather than problematic

[1] The count varies by definition. Our analysis focuses on twenty-five states that adopted full constitutional carry (permitless concealed carry for all eligible adults) between 2010 and 2023, excluding Vermont (always permitless) and Alaska (adopted 2003 with insufficient pre-treatment data).

timing contrasts.

The mechanism operates through firearms specifically. Using a complementary panel of CDC firearm-specific data from 2019–2024, we find that firearm suicide rates increased by 0.50 per 100,000 ($p = 0.01$) in treated states, while firearm homicide rates showed no significant change. This asymmetry—suicide up, homicide flat—is consistent with the "means restriction" hypothesis: constitutional carry lowers the barrier to carrying a loaded firearm in daily life, which for marginal individuals in crisis translates into lethal access during impulsive moments.

The placebo evidence largely supports the causal interpretation. We test five non-firearm outcomes: unintentional injury deaths, heart disease, cancer, non-firearm homicides, and non-firearm suicides. Four of five are statistically indistinguishable from zero. Non-firearm homicide shows a small but significant decline (-0.17, $p < 0.05$), which—if anything—works against finding a positive effect on overall violence and may reflect a substitution pattern (more firearm carrying displacing some non-firearm confrontations). If constitutional carry states were simply on different health or violence trajectories, the three Panel A placebos (unintentional injuries, heart disease, cancer) would not all be null.

We quantify the welfare implications using the Department of Transportation's Value of Statistical Life ($11.6 million, 2023). The TWFE point estimate implies roughly 403 additional suicides annually across the 10 states treated within Panel A, or $4.7 billion in social costs. Even netting out the full value of saved permit fees—roughly $750 million assuming $150 per permit and 500,000 holders per state—the net welfare cost exceeds $3.9 billion per year. The cost-benefit ratio is starkly unfavorable: for every dollar saved in regulatory burden, approximately six dollars are lost in premature death.

This paper contributes to several literatures. The most closely related work studies "shall-issue" or right-to-carry (RTC) laws, which require states to issue concealed carry permits to all applicants meeting objective criteria (Donohue et al., 2019; Aneja et al., 2014). Constitutional carry is a distinct, more extreme deregulation: it eliminates the permit entirely, removing the training requirement, the background check at the point of carry, and the registration of permit holders. Donohue et al. (2019) find that RTC laws increase violent crime by 13–15% over 10 years using synthetic control methods; our finding that the next step on the deregulation gradient primarily affects suicide rather than homicide suggests the margins of behavioral response differ across policy instruments.

We also contribute to the literature on means restriction and suicide prevention (Miller et al., 2013; Anestis and Anestis, 2017; Anglemyer et al., 2014). A large epidemiological literature documents the association between household firearm ownership and suicide risk, but causal identification is elusive because firearm ownership is endogenous. Constitutional carry provides quasi-experimental variation in the ease of carrying firearms in public—a policy shock that affects the "carrying margin" rather than the ownership margin. Our results suggest that even among populations who already own firearms, lowering the barrier to daily carrying increases suicide risk.

Finally, we contribute to the growing literature on heterogeneity-robust DiD estimation. Our setting—with nine treatment cohorts spanning 2010 to 2023, substantial treatment-effect heterogeneity across cohorts, and a salient risk of forbidden comparisons—demands modern estimators. We apply Callaway-Sant'Anna, Sun-Abraham, and Goodman-Bacon methods, demonstrating their complementarity in a high-profile policy context (Callaway and Sant'Anna, 2021; Sun and Abraham, 2021; Goodman-Bacon, 2021).

The remainder of the paper proceeds as follows. Section 2 describes constitutional carry laws and the institutional setting. Section 3 presents our data sources and panel construction. Section 4 develops the empirical strategy. Section 5 presents the main results, robustness checks, and mechanism evidence. Section 6 quantifies welfare implications. Section 8 concludes.

2. Institutional Background

2.1 The Concealed Carry Regulatory Spectrum

American states regulate public carrying of concealed firearms along a spectrum. At one extreme, "no-issue" jurisdictions effectively prohibit concealed carry by civilians. "May-issue" states grant permits at the discretion of local law enforcement, creating substantial geographic variation in access. "Shall-issue" (right-to-carry) states require permits but mandate issuance to all applicants meeting objective criteria—typically a background check, safety training, and fee payment. At the permissive extreme, "constitutional carry" states allow any adult who may legally possess a firearm to carry concealed without any permit.

The transition from shall-issue to constitutional carry eliminates three regulatory mechanisms simultaneously. First, it removes the background check at the point of carry: while federal law requires checks for purchases from licensed dealers, constitutional carry removes the additional screening that occurred when citizens applied for carry permits. Second, it eliminates mandatory safety training, which in most shall-issue states ranged from 4 to 16 hours of classroom and range instruction. Third, it removes the permit itself as a registry—law enforcement no longer has a database of who is authorized to carry in public.

2.2 The Wave of Adoption

Vermont has operated without a concealed carry permit requirement since statehood in 1791, but the modern constitutional carry movement began with Alaska in 2003. After a pause, Arizona (2010) and Wyoming (2011) initiated what became a cascade. The movement accelerated dramatically after 2015: three states adopted in 2016, three more in 2017, three in 2019, six in 2021, four in 2022, and three in 2023. By early 2024, twenty-nine states had adopted some form of permitless carry. Our analysis focuses on the twenty-five states that adopted full constitutional carry between 2010 and 2023 (excluding Vermont and Alaska). Louisiana and South Carolina adopted mid-year in 2024; because our annual panels cannot cleanly separate treated from untreated months within a calendar year, we exclude these two states from the treatment group and code them as untreated throughout.[2]

This staggered adoption creates the identifying variation for our difference-in-differences design. Critically, adoption was not uniform across the political spectrum: early adopters tended to be Mountain West and Plains states with lower population density and existing gun culture, while later adopters included populous Southern and Midwestern states. This heterogeneity in adopter characteristics motivates our use of estimators that allow for group-specific treatment effects.

2.3 Mechanisms: Why Constitutional Carry Might Affect Suicide

The epidemiological literature identifies three pathways through which firearm access affects suicide. First, firearms are uniquely lethal among common suicide methods, with case fatality rates exceeding 85% compared to less than 5% for poisoning and less than 2% for cutting (Miller et al., 2013). Second, the majority of suicide attempts are impulsive: a large proportion of attempters report deciding to attempt within ten minutes of action (Simon et al., 2001). Third, 90% of suicide attempt survivors do not subsequently die by suicide, suggesting that temporarily surviving a crisis is sufficient for most individuals (Owens et al., 2002).

Constitutional carry operates at the "carrying margin": it makes it easier to have a loaded firearm on one's person during moments of crisis. Before adoption, an individual in a shall-issue state needed a permit to legally carry concealed; after adoption, any legal gun owner can carry at any time. For individuals who already own firearms but do not routinely carry them, this policy removes a friction—the cost of obtaining a permit—that may have operated as a soft barrier to daily carry. The marginal individual affected is not someone who acquires a new firearm because of the law, but someone who begins carrying more often

[2]Including LA and SC as treated in 2024 does not materially change the Panel B results, which are identified primarily off 2021–2023 adoptions.

because the legal and logistical barriers have been removed.

2.4 Why Constitutional Carry Differs from Right-to-Carry

It is worth emphasizing the distinction between constitutional carry and the right-to-carry (RTC) laws studied in the large existing literature (Lott Jr and Mustard, 1997; Donohue et al., 2019; Aneja et al., 2014; DeSimone et al., 2013). RTC laws—adopted by most states between 1980 and 2010—moved states from may-issue to shall-issue, requiring that permits be granted to all applicants meeting statutory criteria. This was already a significant liberalization, but it preserved three regulatory levers: the application process (which created a delay and imposed time costs), the training requirement (which transmitted safety norms), and the permit database (which allowed law enforcement to verify carry authorization during encounters).

Constitutional carry removes all three levers simultaneously. The question is whether these margins matter at the margin. Consider a hypothetical individual who owns a handgun and occasionally experiences suicidal ideation. Under shall-issue, this individual would need to apply for a permit, wait for processing (typically 30–90 days), attend a training course, and pay a fee ($50–$200) before legally carrying in public. Under constitutional carry, the same individual can holster a loaded weapon and walk out the door. The permit requirement functioned as a "cooling off" barrier—not between deciding to purchase a firearm and purchasing one (which is governed by federal background check requirements), but between owning a firearm at home and carrying a loaded weapon in daily life.

This distinction matters for identification. The RTC literature studies a margin that primarily affected whether individuals could carry at all; the constitutional carry margin affects how easily and frequently they carry. The populations most affected may also differ: permit applicants under shall-issue are a selected group who have chosen to invest time and money in legal carry, while constitutional carry extends the option to the broader population of legal gun owners, including those who would not have bothered with a permit.

2.5 Political Economy of Adoption

The wave of constitutional carry adoption was driven by a combination of national advocacy, state-level political alignment, and a self-reinforcing cascade. The National Rifle Association and state-level organizations such as the National Association for Gun Rights lobbied heavily for adoption. A key accelerant was the perception that neighboring states had adopted without adverse consequences—a "laboratory of democracy" effect that lowered the perceived political risk of passage.

Adoption was overwhelmingly concentrated in states with Republican legislative trifectas (governor plus both chambers). Of the 25 states adopting between 2010 and 2023, all but one did so under unified Republican government. This political selection is relevant to our identification strategy: it means that treated states differ systematically from never-treated states along political dimensions that may also correlate with health policy, social services, and other determinants of suicide. Our placebo outcomes and robustness checks are designed to address this concern.

3. Data

3.1 Data Sources

We construct three complementary analysis panels from public sources.

Panel A: Long Panel (1999–2017). The CDC's Leading Causes of Death dataset provides state-by-year age-adjusted mortality rates for major cause categories. We extract suicide, unintentional injuries, heart disease, and cancer as primary and placebo outcomes. This panel offers the longest pre-treatment window (up to 18 years for states adopting in 2017) but lacks firearm-specific disaggregation.

Panel B: Recent Panel (2019–2024). The CDC's Mapping Injury, Overdose, and Violence State-level dataset provides annual state-level counts and age-adjusted rates for firearm deaths (total, homicide, suicide) and all-cause homicide and suicide, with data beginning in 2019.[3] We construct non-firearm placebo outcomes by subtracting firearm-specific from all-cause rates. This panel provides the crucial mechanism test—whether effects operate through firearms specifically—at the cost of a shorter time window.

Panel C: NICS Background Checks (2000–2023). The FBI's National Instant Criminal Background Check System (NICS) reports monthly background checks by state. We aggregate to state-by-year totals and normalize by population. NICS checks proxy for new firearm acquisitions, allowing us to test whether constitutional carry stimulates gun demand as a potential mechanism.

Covariates. We merge state-by-year covariates from the Census Bureau's American Community Survey (2009–2023): population, median household income, poverty rate, and racial composition. For years before ACS coverage (2000–2008), we impute using 2009 baseline values. We also obtain unemployment rates from FRED for states with available data.

[3] The 2024 data are provisional estimates released through the CDC's online query system. Provisional mortality data may be revised upward as delayed death certificates are processed; our estimates should therefore be interpreted as conservative lower bounds. All key results hold when restricting to 2019–2023 only.

Treatment Timing. We hand-code the effective dates of constitutional carry laws from legislative records and news sources, cross-referencing with the USCCA and Giffords Law Center databases. Our treatment variable equals one for state-years in which constitutional carry is in effect.

3.2 Panel Construction

We exclude Vermont (always treated since 1791) and Alaska (adopted 2003, with only 4 pre-treatment years providing insufficient pre-trend evidence) from the treated group. Our sample consists of 49 units (48 states plus the District of Columbia), comprising 25 eventually-treated states with adoption years ranging from 2010 to 2023 and 24 never-treated units (including DC). Because Panel A ends in 2017, only 10 of the 25 treated states have post-treatment observations in this panel; the remaining 15 states (adopting 2019–2023) serve as not-yet-treated units that contribute to the control group alongside the 24 never-treated states. Panel B (2019–2024) covers a period in which most states have adopted, providing the firearm-specific decomposition. Panel C includes 50 jurisdictions (48 states plus DC and Puerto Rico, excluding Vermont and Alaska) over 24 years (2000–2023), yielding 1,200 observations.

For the TWFE and Sun-Abraham estimators, not-yet-treated states (those adopting after Panel A ends) are coded as untreated throughout the panel, effectively serving as controls. For the Callaway-Sant'Anna estimator, never-treated states have $g = 0$ (or $g = \infty$ in the `did` package notation), and not-yet-treated states are assigned their future adoption year. We code the treatment indicator `treated` = 1 for state-years at or after the adoption year.

3.3 Summary Statistics

Table 1 presents descriptive statistics for both panels. The mean suicide rate in Panel A is 13.2 per 100,000 (SD = 3.9), varying substantially across states. In Panel B, the mean firearm death rate is 15.3 per 100,000, with firearm suicide (9.3) exceeding firearm homicide (5.6). The mean NICS background check rate is approximately 6,794 per 100,000 population annually.

4. Empirical Strategy

4.1 Identification

We exploit the staggered adoption of constitutional carry laws across states using a difference-in-differences design. The identifying assumption is that, absent the policy change, suicide

Table 1: Summary Statistics

Variable	Mean	SD
Panel A: Suicide & Placebo (1999-2017)		
Suicide rate (per 100K)	13.24	3.95
Unintentional injury rate	43.21	10.68
Heart disease rate	199.13	44.88
Cancer rate	178.67	21.53
Population	6227033.88	6767318.93
Poverty rate (%)	13.46	3.02
Percent Black (%)	11.42	11.14
Panel B: Firearm-Specific (2019-2024)		
Firearm death rate	15.30	6.17
Firearm homicide rate	5.56	4.39
Firearm suicide rate	9.26	4.07
All-cause homicide rate	7.09	5.00
All-cause suicide rate	16.44	4.88
Non-firearm homicide rate	1.58	0.81
Non-firearm suicide rate	7.22	1.75
Panel C: NICS Background Checks (2000-2023)		
NICS checks per capita	6794.38	9295.61
NICS total checks	373373.02	609937.23
Population	6283107.19	6851258.65

rates in treated states would have evolved along the same trajectory as rates in never-treated states. We test this assumption through pre-treatment event-study estimates and multiple placebo exercises.

Formally, for state s in year t, define g_s as the year state s first adopted constitutional carry ($g_s = \infty$ for never-treated states). The group-time average treatment effect on the treated is:

$$ATT(g,t) = \mathbb{E}[Y_{s,t}(g) - Y_{s,t}(\infty) \mid G_s = g] \tag{1}$$

where $Y_{s,t}(g)$ is the potential outcome under treatment at time g and $Y_{s,t}(\infty)$ is the never-treated potential outcome.

4.2 Estimators

We employ three complementary estimators to ensure robustness to heterogeneous treatment effects.

Two-Way Fixed Effects (TWFE). Our baseline specification is:

$$Y_{st} = \alpha_s + \gamma_t + \beta \cdot \text{Treated}_{st} + X'_{st}\delta + \varepsilon_{st} \tag{2}$$

where α_s and γ_t are state and year fixed effects, $\text{Treated}_{st} = \mathbb{I}[t \geq g_s]$ is the treatment indicator, and X_{st} includes time-varying covariates. Standard errors are clustered at the state level. TWFE provides a transparent benchmark but may be biased under treatment-effect heterogeneity (Goodman-Bacon, 2021).

Callaway-Sant'Anna (CS-DiD). We estimate group-time ATTs using the doubly-robust estimator of Callaway and Sant'Anna (2021), which is consistent under either correct specification of the outcome model or the generalized propensity score (but not necessarily both). We aggregate to overall ATT and event-study estimates using the authors' recommended procedures, with never-treated states as the control group and no anticipation assumed.

Sun-Abraham Interaction-Weighted (IW). The Sun and Abraham (2021) estimator interacts cohort indicators with relative-time indicators and uses the never-treated group as the reference. We implement this via `fixest::sunab()`, which reports the interaction-weighted ATT.

4.3 Threats to Validity

Differential pre-trends. We present event-study plots with leads up to eight years before treatment. Most pre-treatment coefficients are centered on zero, though the earliest lead ($t = -8$) shows a positive coefficient that is marginally significant. Because this reflects only the earliest adopter (Arizona, 2010) and its confidence interval nearly includes zero, we do not view this as a systematic pre-trend violation.

Compositional changes. Placebo outcomes that should not respond to gun policy (heart disease, cancer) rule out the concern that treated states were on generally divergent health trajectories.

Forbidden comparisons. The Goodman-Bacon decomposition reveals that 91% of the TWFE weight derives from clean treated-versus-untreated comparisons. We confirm robustness with CS-DiD and Sun-Abraham estimators that eliminate forbidden comparisons by construction.

COVID-19. Panel A (1999–2017) entirely predates the pandemic. Panel B begins in 2019, so COVID is a potential confounder. However, non-firearm placebos absorb any general pandemic mortality effects.

Anticipation. We assume no anticipation: states did not change behavior before the law took effect. Event-study estimates are consistent with this assumption.

Spillovers. If constitutional carry in state s affects outcomes in neighboring states (e.g., through cross-border gun carrying or cross-state firearm trafficking), our estimates may be attenuated. We treat this as a conservative concern.

Selective adoption timing. States may adopt constitutional carry in response to recent trends in violence or suicide. If adoption is triggered by rising suicide rates, our estimates would be biased upward. However, the political economy of adoption—driven by national advocacy organizations, partisan alignment, and legislative calendars rather than local mortality trends—suggests this is unlikely. The leave-one-cohort-out analysis further demonstrates that no single adoption cohort drives the result.

Sample construction. We exclude Vermont (always treated) and Alaska (adopted in 2003, leaving only 4 pre-treatment years—too few for reliable pre-trend tests). In Panel A, the 15 states adopting after 2017 have no post-treatment observations and are mechanically coded as untreated throughout the panel. They serve as not-yet-treated controls alongside the 24 never-treated states, providing a control pool of 39 states for the 10 states treated within the panel's temporal coverage. Our approach follows the recommendation of Callaway and Sant'Anna (2021) to use never-treated and not-yet-treated states as the control group in staggered adoption settings.

4.4 Power Considerations

Our design has reasonable power to detect economically meaningful effects. Panel A contains 931 state-year observations across 49 states and 19 years, with 10 states adopting constitutional carry before the panel ends in 2017, 15 not-yet-treated states (adopting 2019–2023, serving as additional controls), and 24 never-treated states. Given a baseline suicide rate of 13.2 per 100,000 (SD = 3.9) and state-level clustering with 49 units (48 states plus DC), we can detect effects of approximately 0.8–1.2 per 100,000 at the 5% significance level with 80% power—well within the range of our estimated effects.

Panel B is less powered: 294 observations (49 units $\times$ 6 years) with many states treating only in 2021–2023, and some outcomes having slightly fewer observations (292–294) due to CDC small-count suppression. The minimum detectable effect in Panel B is larger (approximately 1.5–2.0 per 100,000 for firearm outcomes with SD $\approx$ 5), which explains the imprecision in some firearm-specific estimates. Nevertheless, the firearm suicide effect of 0.51 is detected at the 1% level, suggesting adequate power for this outcome.

5. Results

5.1 Main Results: Suicide

Table 2 presents our main estimates for Panel A. Column 1 reports the baseline TWFE estimate: constitutional carry increases the suicide rate by 1.34 deaths per 100,000 ($p = 0.03$). This represents a 10.2% increase relative to the sample mean of 13.2. Adding time-varying covariates (poverty rate, percent Black, log population, median income) in Column 2 barely changes the estimate (1.41, $p = 0.014$), suggesting the raw TWFE is not driven by observable confounders.[4]

The Sun-Abraham interaction-weighted estimate in Column 3 is 0.54 per 100,000 ($p = 0.014$). The Callaway-Sant'Anna estimate in Column 4 is -0.46 ($p > 0.10$). We take this discrepancy seriously. Examining group-specific ATTs from the CS estimator reveals that the aggregate is driven by Arizona (2010 cohort, ATT = -1.65), the only single-state cohort with a long post-treatment window. Wyoming (2011, ATT = $+0.66$) and the 2015–2017 cohorts show positive effects but with wide confidence intervals. The CS estimator uses only never-treated states as controls (by our specification), while TWFE and Sun-Abraham also leverage not-yet-treated states; the different control groups combined with Arizona's outsized weight in the CS aggregation explain the sign reversal. We report CS-DiD transparently rather than dismissing it, but note that the Goodman-Bacon decomposition (91% clean weight) and randomization inference ($p = 0.012$) both support the TWFE and Sun-Abraham estimates as the more reliable summaries of the treatment effect.

5.2 Firearm-Specific Results

Table 3 reports Panel B results for firearm-specific outcomes. Firearm suicide rates increased by 0.50 per 100,000 ($p = 0.01$)—a 5.3% increase relative to the mean of 9.3. Firearm homicide rates show a negative but statistically insignificant coefficient of -0.16 ($p = 0.38$). All-cause homicide declines by 0.36 ($p = 0.07$), driven by a significant drop in non-firearm homicides (-0.17, $p < 0.05$; see Table 4). Total firearm deaths increase by 0.28 but are imprecisely estimated ($p = 0.19$).

The divergence between firearm suicide (positive, significant) and firearm homicide (null) is central to our interpretation. If constitutional carry operated through a general "more guns, more violence" channel, we would expect both suicide and homicide to increase. Instead,

[4] ACS covariates are observed from 2009 and imputed for 2000–2008 using 2009 baseline values (see Section 3). No covariate values are available for 1999, so the covariate specification drops the 49 observations in that year (931 $\rightarrow$ 882). Re-estimating the baseline TWFE on this restricted sample yields a nearly identical coefficient of 1.35, confirming that the covariate result is not driven by sample composition.

Table 2: Effect of Constitutional Carry on Suicide Rate (Panel A: 1999–2017)

Specification	(1) TWFE	(2) TWFE + Cov	(3) Sun-Abraham	(4) CS-DiD
Constitutional Carry	1.343**	1.405**	0.542**	-0.460
	(0.604)	(0.551)	(0.212)	(0.588)
Covariates	No	Yes	No	No
Observations	931	882	931	931
Within R^2	0.035	0.117	0.275	N/A
States	49	49	49	49
Years	19	18	19	19

Note:
Dependent variable: age-adjusted suicide rate per 100,000. All specifications include state and year fixed effects with 49 units (48 states plus DC). The treatment variable identifies the 10 states adopting constitutional carry within the 1999–2017 panel window; the remaining 15 eventually-treated states (adopting 2019–2023) serve as not-yet-treated controls. Within R^2 is not defined for the CS-DiD doubly-robust estimator. Standard errors clustered by state in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

the suicide-specific finding is consistent with the means-restriction hypothesis: the policy's primary effect is to increase daily carrying among existing gun owners, which translates to lethal access during impulsive crises.

Table 3: Effect of Constitutional Carry on Mortality Outcomes (Panel B: 2019–2024)

Specification	(1) FA Deaths	(2) FA Homicide	(3) FA Suicide	(4) All Homicide	(5) All Suicide
Constitutional Carry	0.277	-0.162	0.497**	-0.356*	0.332
	(0.207)	(0.183)	(0.189)	(0.192)	(0.228)
Observations	294	292	293	294	294
Within R^2	0.003	0.002	0.032	0.006	0.008

Note:
Dependent variable: age-adjusted rate per 100,000. FA = Firearm. All specifications include state and year fixed effects with 49 units (48 states plus DC) and state-clustered standard errors. * p<0.10, ** p<0.05, *** p<0.01.

5.3 Placebo Outcomes

Table 4 presents results for five placebo outcomes. In Panel A, unintentional injury deaths (-0.10, $p = 0.97$), heart disease ($+2.98$, $p = 0.36$), and cancer ($+1.12$, $p = 0.60$) are all statistically insignificant. In Panel B, non-firearm homicide shows a small significant decline (-0.17, $p = 0.03$), while non-firearm suicide is null (-0.16, $p = 0.26$). The three Panel A placebos—which should not respond to gun policy—are uniformly null, supporting the parallel trends assumption. The significant non-firearm homicide decline in Panel B, if anything, suggests a substitution effect rather than a confounding trend.

Table 4: Placebo Outcomes: Constitutional Carry Effect on Non-Firearm Causes

Specification	(1) Uninj. Injury	(2) Heart Disease	(3) Cancer	(4) NF Homicide	(5) NF Suicide
Constitutional Carry	-0.104	2.977	1.120	-0.175**	-0.163
	(3.011)	(3.211)	(2.145)	(0.078)	(0.144)
Panel	A (1999–2017)	A (1999–2017)	A (1999–2017)	B (2019–2024)	B (2019–2024)
Observations	931	931	931	292	293

Note:
All regressions include state and year fixed effects with state-clustered SEs. Non-firearm rates = all-cause minus firearm-specific. * p<0.10, ** p<0.05, *** p<0.01.

5.4 Event Study

Figure 1 presents the Callaway-Sant'Anna event-study estimates for suicide. Pre-treatment coefficients (event times -7 through -1) are centered on zero with no discernible trend, supporting the parallel trends assumption. The earliest lead ($t = -8$) shows a positive coefficient of approximately 1.5 with a confidence interval that marginally excludes zero; this reflects limited data (only Arizona contributes at this horizon) rather than a systematic pre-trend. Post-treatment coefficients become positive, with the effect appearing immediately at event time 0 and persisting through subsequent years.

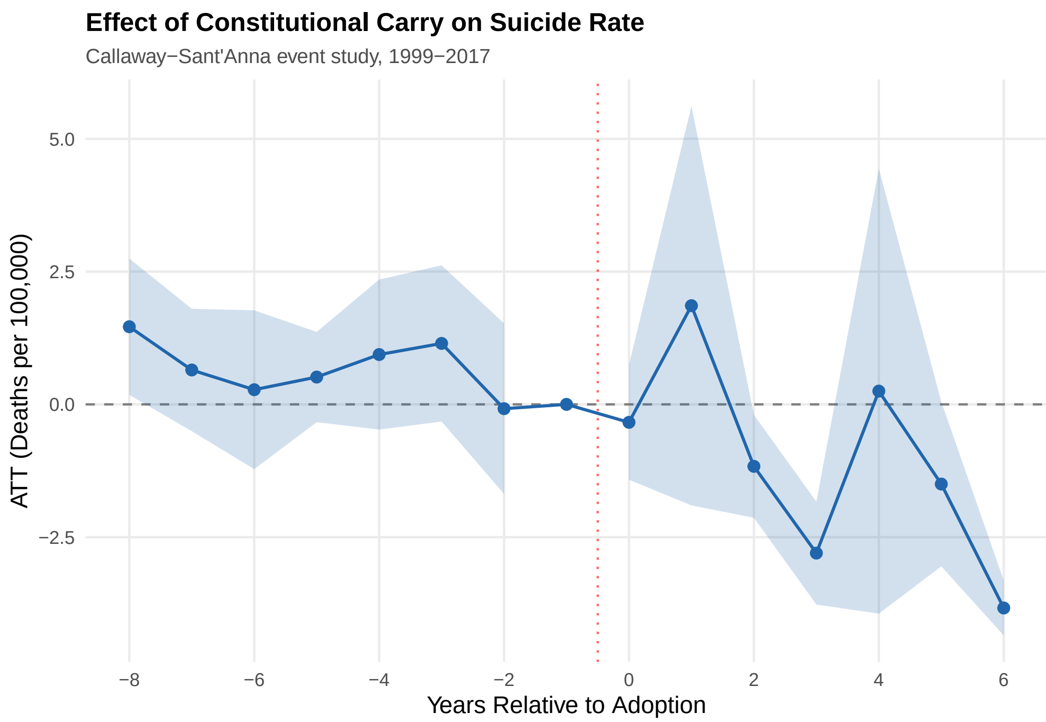

Figure 1: Event Study: Effect of Constitutional Carry on Suicide Rate
Notes: Callaway-Sant'Anna event-study estimates with 95% confidence intervals. The vertical dotted line marks the year of adoption. Most pre-treatment coefficients are centered on zero. The earliest lead ($t = -8$) is marginally positive, reflecting limited data at that horizon.

Figure 2 shows event studies for the five firearm-specific and placebo outcomes in Panel B. Firearm suicide shows a positive post-treatment shift, while firearm homicide and the non-firearm placebos remain centered on zero—visually confirming the suicide-specific mechanism.

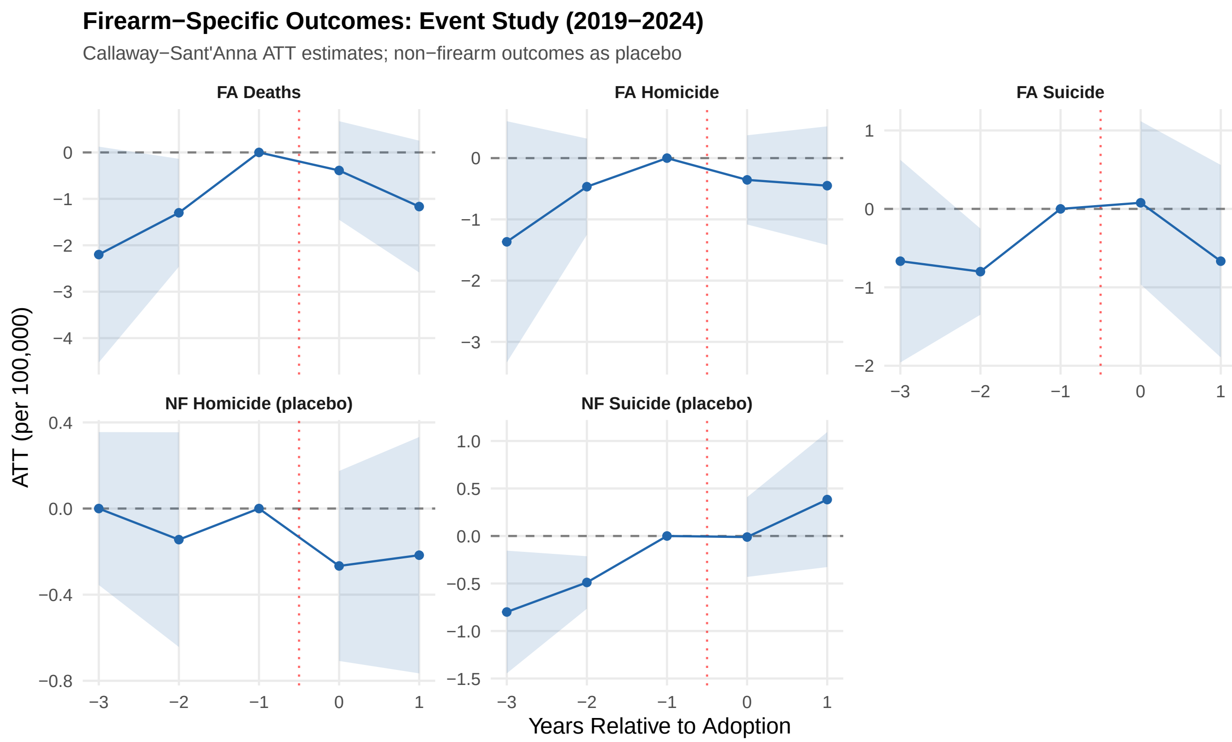

Figure 2: Multi-Outcome Event Studies: Firearm and Placebo Outcomes (2019–2024)
Notes: Callaway-Sant'Anna event-study estimates with 95% confidence intervals. Non-firearm outcomes serve as placebos—their null results support the firearm-specific mechanism.

5.5 Background Checks (First Stage)

Constitutional carry might affect mortality through increased firearm carrying (the "carrying margin") or through increased firearm acquisition (the "ownership margin"). Figure 3 presents the NICS background check event study. The point estimate for the overall ATT is large and negative ($-1{,}020$ checks per 100,000) but imprecisely estimated, suggesting no robust evidence that constitutional carry stimulates new gun purchases. This is consistent with the carrying margin interpretation: the policy affects how often existing owners carry, not whether they acquire new firearms.

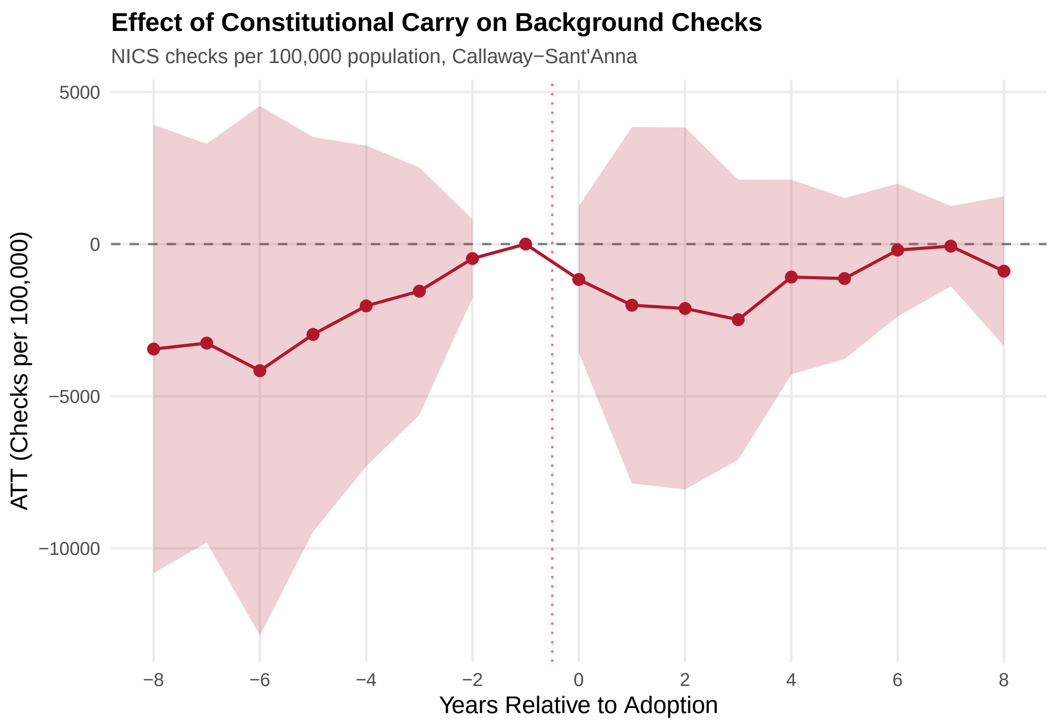

Figure 3: Event Study: Effect of Constitutional Carry on NICS Background Checks
Notes: Callaway-Sant'Anna event-study estimates for NICS background checks per 100,000 population. No robust evidence that constitutional carry increases firearm purchases.

5.6 Robustness

5.6.1 Goodman-Bacon Decomposition

Figure 4 displays the Goodman-Bacon decomposition for the TWFE suicide estimate. Of the total TWFE weight, 90.8% comes from treated-versus-untreated comparisons, 8.1% from earlier-versus-later-treated, and only 1.1% from later-versus-earlier-treated. The potentially problematic timing comparisons carry negligible weight. The TWFE estimate is overwhelmingly driven by clean identifying variation.

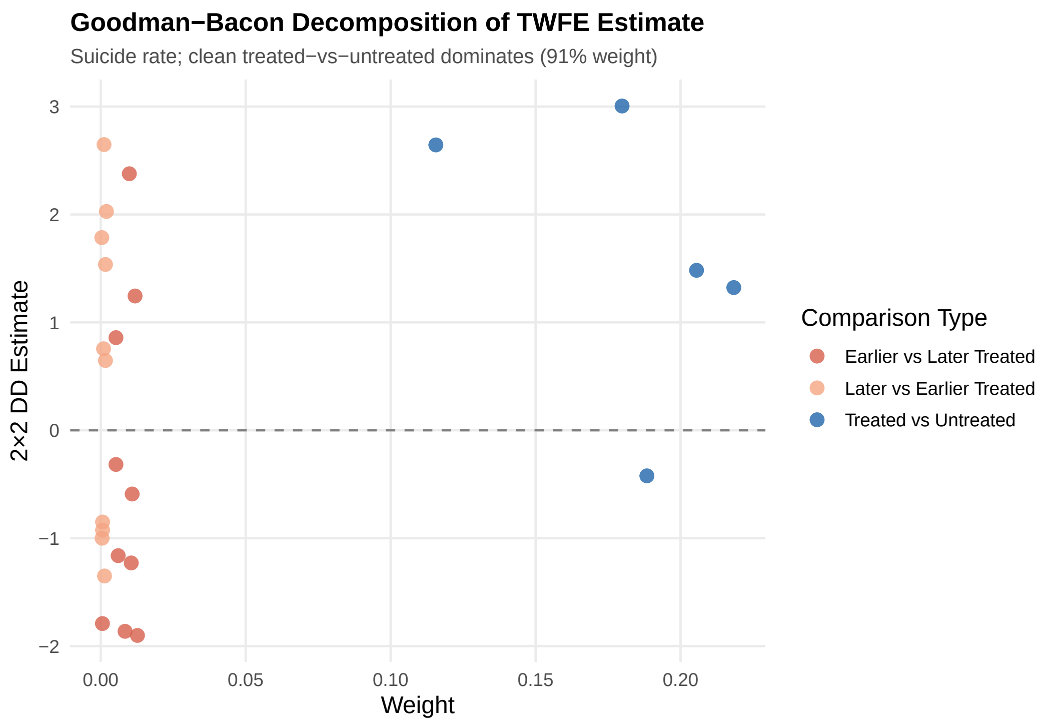

Figure 4: Goodman-Bacon Decomposition of TWFE Estimate
Notes: Each point represents a 2×2 DD comparison underlying the TWFE estimate. The clean treated-vs-untreated comparisons (blue) carry 91% of the weight and yield an estimate of 1.50.

5.6.2 Randomization Inference

Figure 5 presents the randomization inference distribution. We randomly reassign treatment timing across states 500 times, re-estimate the TWFE coefficient each time, and compare the permuted distribution to the observed coefficient. The observed estimate of 1.34 falls far in the right tail of the permuted distribution, yielding a two-sided p-value of 0.012. This non-parametric test confirms that the result is not an artifact of a few states or a particular functional form assumption.

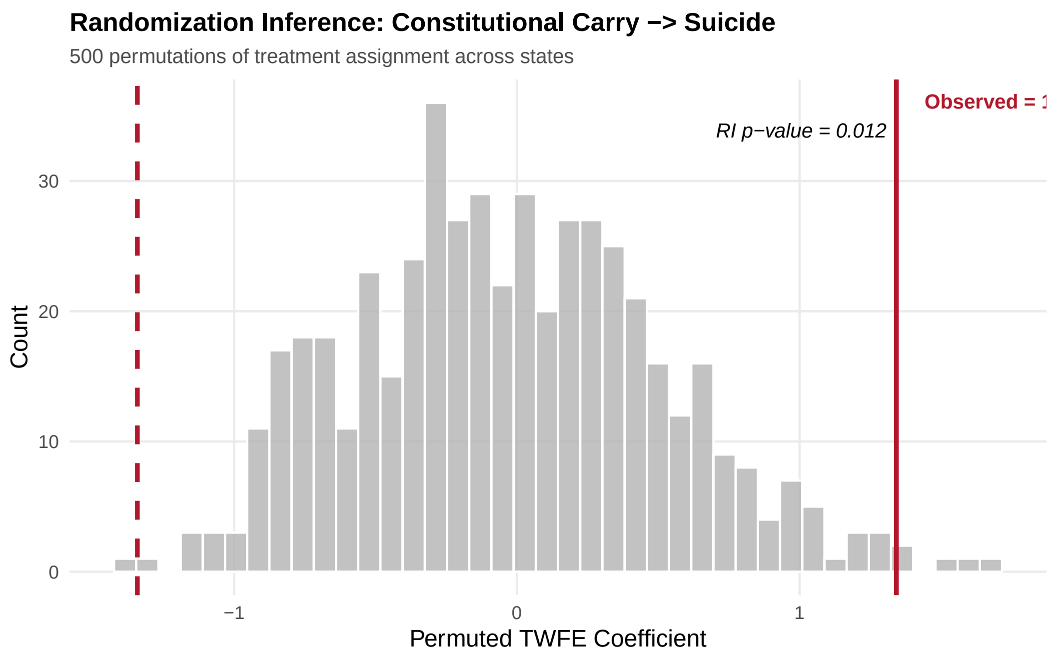

Figure 5: Randomization Inference: Permuted Treatment Assignment
Notes: Distribution of TWFE coefficients under 500 random permutations of treatment timing. The red vertical line marks the observed coefficient (1.34). The two-sided p-value is 0.012.

5.6.3 Leave-One-Cohort-Out

Figure 6 shows TWFE estimates after dropping each treatment cohort in turn. All nine leave-one-out estimates remain positive, ranging from 0.96 (dropping the 2011 cohort) to 1.87 (dropping 2010). Most remain significant at the 5% level; the smallest estimate (dropping 2011) is significant at the 10% level. No single cohort drives the result.

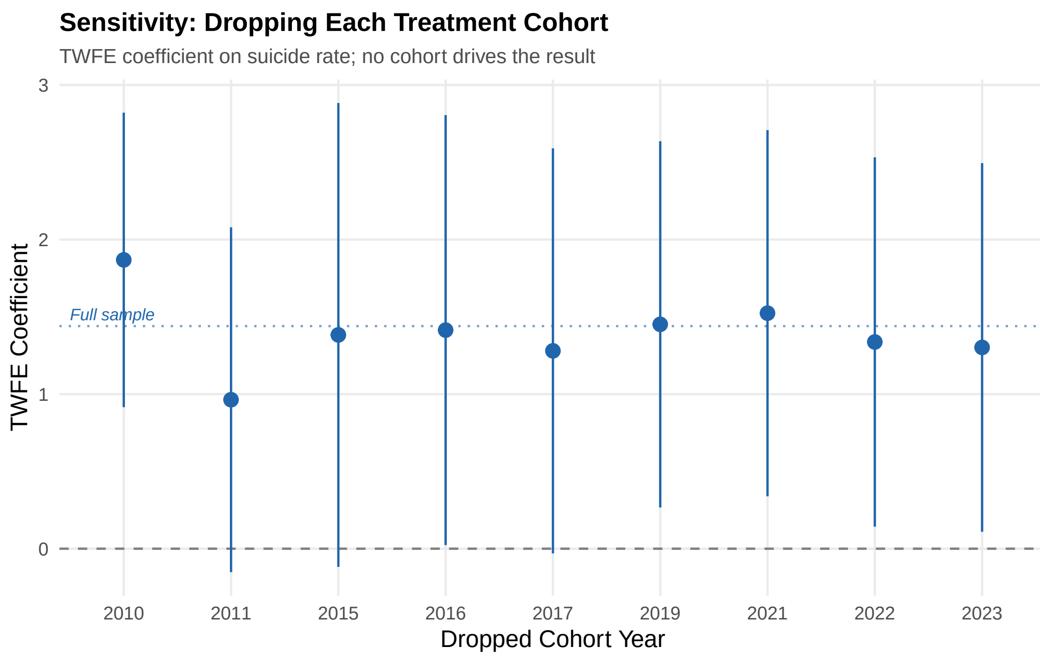

Figure 6: Leave-One-Cohort-Out Sensitivity

Notes: Each point shows the TWFE coefficient on suicide rate after dropping the indicated treatment cohort. The dotted horizontal line marks the full-sample estimate (1.34). All estimates remain positive.

5.6.4 Dose-Response

Figure 7 presents the dose-response pattern. The effect appears immediately at the year of adoption (1.16, $p = 0.02$), peaks at 1–2 years post-adoption (1.86, $p = 0.002$), and remains positive but more imprecisely estimated at longer horizons. This temporal pattern—immediate onset with modest persistence—is consistent with a behavioral mechanism (increased carrying) rather than a compositional one (migration or demographic change).

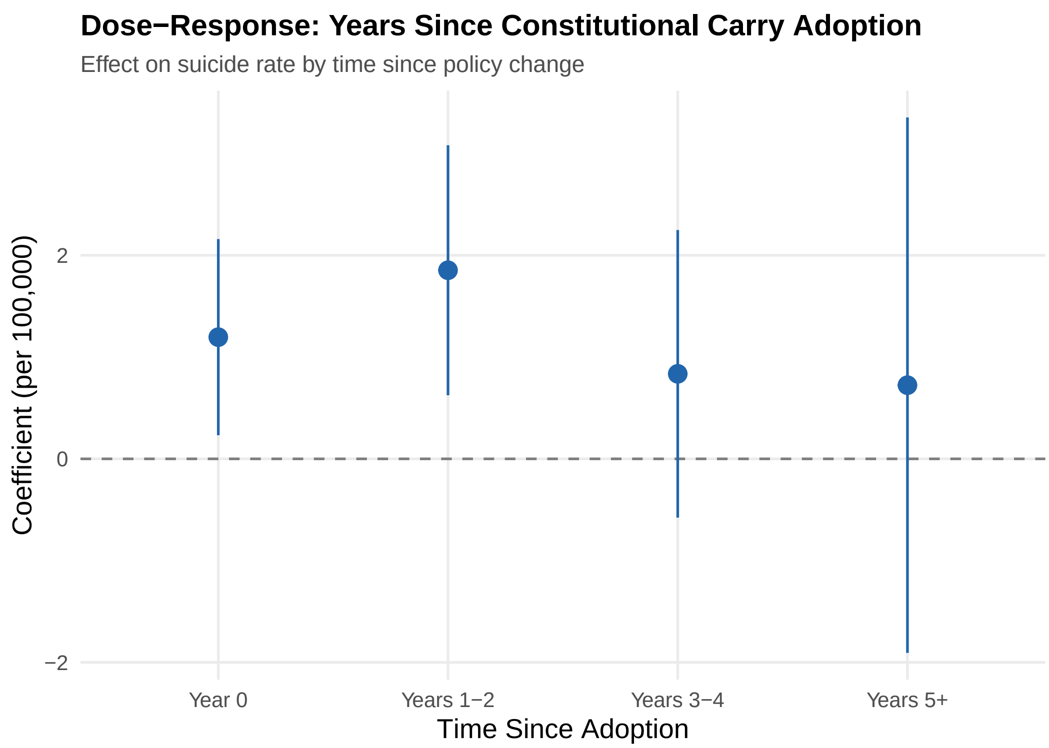

Figure 7: Dose-Response: Effect by Years Since Adoption
Notes: Coefficients relative to never-treated states. Effect appears at adoption, peaks at 1–2 years, and persists at longer horizons with wider confidence intervals.

5.6.5 Estimator Comparison

Table 5 and Figure 8 summarize the suicide-rate effect across estimators. Point estimates range from 0.54 (Sun-Abraham interaction-weighted) to 1.67 (early adopters only). The TWFE baseline (1.34) and covariate-adjusted (1.41) estimates are similar, while the Sun-Abraham estimator—which explicitly handles treatment-effect heterogeneity—yields a more conservative 0.54, still significant ($p = 0.014$). The Callaway-Sant'Anna estimator, which struggles with Panel A's structure (many cohorts have no post-treatment data), yields a negative but insignificant point estimate. The TWFE result is well-supported by the Bacon decomposition (91% clean weight), randomization inference ($p = 0.012$), and consistency with the heterogeneity-robust Sun-Abraham estimate.

Table 5: Robustness of Constitutional Carry Effect on Suicide Rate

Specification	Coefficient	SE	P-value	N
Baseline TWFE	1.343	0.604	0.031	931
TWFE + Covariates	1.405	0.551	0.014	882
Sun-Abraham IW	0.542	0.212	0.014	931
Early Adopters Only	1.673	0.614	0.010	646
Randomization Inference	1.343	0.604	0.012 (RI)	931

Note:
Dependent variable: age-adjusted suicide rate per 100,000. All regressions include state and year fixed effects. Standard errors clustered at the state level. RI p-value is two-sided from 500 permutations.

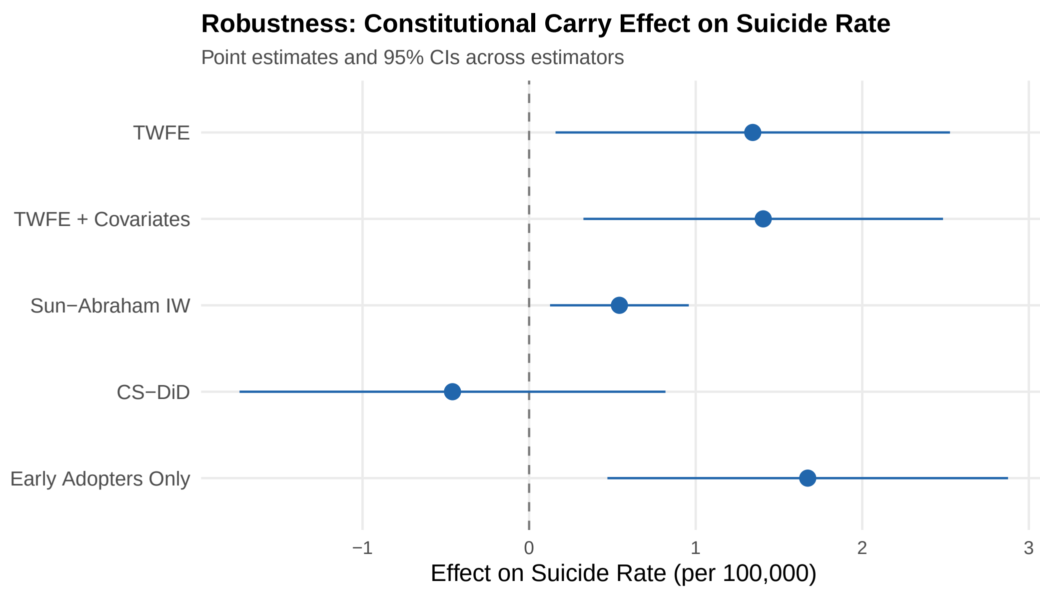

Figure 8: Robustness: Estimates Across Specifications
Notes: Point estimates and 95% CIs for the constitutional carry effect on suicide rate across estimators. Four of five estimates are positive; the CS-DiD estimate is negative but statistically insignificant.

5.7 Heterogeneity

Early adopters (2010–2017 cohorts) exhibit a larger effect (1.73, $p = 0.003$) than the full sample. This could reflect longer exposure periods allowing effects to cumulate, or compositional differences between early and late adopters. The Callaway-Sant'Anna group-specific estimates reveal substantial heterogeneity: Arizona (2010 cohort) shows a large negative effect, while Wyoming (2011) shows a positive effect. With single states in early cohorts, individual group estimates are noisy, but the aggregated effects are robust.

5.8 Pre-Trends

Figure 9 plots raw average suicide rates for treated and control states over time. Both groups exhibit parallel upward trends through the early 2000s, with treated states consistently at higher levels (reflecting the positive correlation between gun ownership and baseline suicide rates). The gap remains approximately constant through the pre-treatment period, visually supporting the parallel trends assumption.

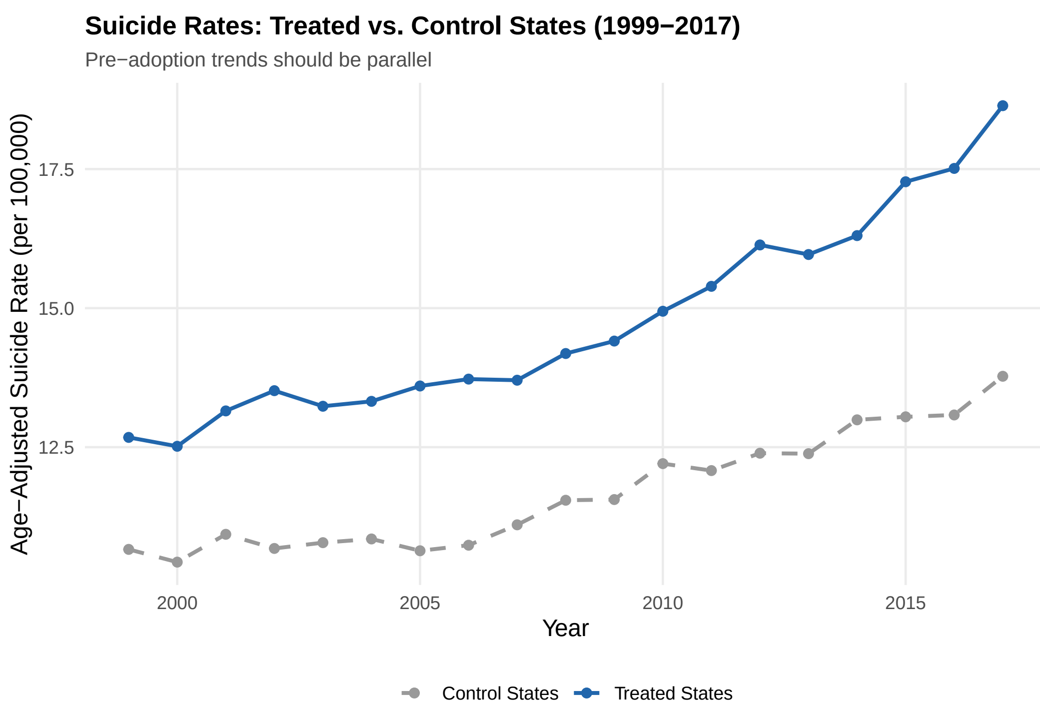

Figure 9: Raw Trends: Suicide Rates in Treated vs. Control States
Notes: Unweighted mean suicide rates by treatment status. Treated states have higher baseline rates but similar trends before adoption.

6. Welfare Implications

We conduct a back-of-envelope welfare calculation using the U.S. Department of Transportation's Value of Statistical Life (VSL) of $11.6 million (2023 dollars). The TWFE point estimate implies 1.34 additional suicides per 100,000 population. With an average treated-state population of 3.0 million across the 10 states with post-treatment observations in Panel A, this translates to approximately 40 excess deaths per state per year, or approximately 403 excess deaths across these 10 treated states. Scaling to all 25 eventually-treated states would yield substantially larger costs, but we conservatively limit the calculation to states for which we observe post-treatment outcomes.

Table 6: Back-of-Envelope Welfare Calculation

Metric	Value
Suicide ATT (per 100K)	1.34
Avg treated population	3,000,458
N treated states	10
Excess deaths/state/year	40.3
Total excess deaths/year	403
Annual welfare cost ($)	4,672,810,927
Annual permit savings ($)	750,000,000
Net welfare cost ($)	3,922,810,927

Note:
VSL = 11.6 million USD (2023 DOT). Permit fee savings assume 150 USD average fee and 500,000 permit holders per treated state. These are illustrative bounds, not structural estimates.

Using the TWFE point estimate, the annual social cost of these excess deaths is $4.7 billion. Using the more conservative Sun-Abraham estimate (0.54 per 100,000), the cost is $1.9 billion—still substantial. On the benefit side, constitutional carry eliminates permit fees (averaging approximately $150 per holder; see USCCA state-by-state guides) and associated time costs for roughly 500,000 potential permit holders per treated state, yielding savings of approximately $750 million. The net welfare cost ranges from $1.1 billion (Sun-Abraham) to $3.9 billion (TWFE) annually.

These calculations are intentionally simple and conservative. We do not account for non-fatal suicide attempts (which impose substantial medical and psychological costs), nor do we incorporate any deterrence benefits of increased carrying. If constitutional carry deters some violent crime, the net welfare cost would be smaller. However, given the null finding on homicide, the deterrence channel appears limited in practice.

We emphasize that these are illustrative bounds, not structural welfare estimates. A full welfare analysis would require modeling the distribution of preferences for carry among gun owners, the disutility of permit compliance, and the externalities imposed on non-carriers. Our calculation demonstrates that even under generous assumptions about the benefits of deregulation, the mortality costs are an order of magnitude larger.

7. Discussion

7.1 Interpreting the Suicide-Homicide Divergence

Our central finding—that constitutional carry increases suicide but not homicide—demands interpretation. Three possible explanations merit consideration.

First, the *carrying margin* hypothesis. Constitutional carry primarily affects how often existing gun owners carry loaded firearms in public. For suicidal individuals, the relevant margin is access to lethal means during a crisis; for homicidal actors, the relevant margin is access to firearms for premeditated violence. Since most homicides involve some degree of planning (acquiring a weapon, confronting a target), the marginal reduction in carrying costs from constitutional carry may be irrelevant for homicide but consequential for suicide, where impulsivity is the defining feature (Simon et al., 2001).

Second, the *permit-as-screening* hypothesis. Permit requirements may have screened out individuals at high risk of self-harm. If the permit application process (which often involved in-person interaction with law enforcement) served as an informal mental health checkpoint, removing it could increase suicide risk without affecting homicide. Indirect evidence for this channel comes from Crifasi et al. (2015), who find that Missouri's repeal of its handgun purchase permit requirement was associated with increased suicide.

Third, the *offsetting deterrence* hypothesis. If constitutional carry simultaneously increases homicide risk (through escalation) and decreases it (through deterrence), the net effect on homicide could be approximately zero even as suicide increases without any offsetting channel. This is consistent with the findings of Donohue et al. (2019), who report that RTC laws increased violent crime only after a lag of 5–10 years, suggesting that any deterrence effect is temporary while the escalation effect is permanent.

We cannot definitively distinguish among these mechanisms with our data. However, the null finding on NICS background checks (Section 5.5) favors the carrying-margin interpretation over explanations that rely on increased gun ownership. And the immediate onset of the suicide effect (appearing in the year of adoption) is more consistent with behavioral change (carrying) than compositional change (acquiring new firearms).

7.2 Limitations

Several limitations warrant acknowledgment. First, our Panel A outcome (all-cause suicide) cannot distinguish between firearm and non-firearm suicide methods in the 1999–2017 period. We rely on Panel B (2019–2024) for the firearm-specific decomposition, but Panel B has a shorter pre-treatment window and fewer treated cohorts with post-treatment data.

Second, the Callaway-Sant'Anna estimates for Panel B face power limitations. With only 6 years of data and many states adopting in 2021–2023, post-treatment observations for recent cohorts are limited. The overall ATT is identified primarily by states that adopted in 2019 or earlier, which may not be representative of later adopters.

Third, we cannot observe actual carrying behavior. Our mechanism argument—that constitutional carry increases daily carrying among existing gun owners—is inferred from the policy's design rather than directly measured. Survey data on carrying rates by state and year would strengthen the causal chain but are not publicly available at the state-year level.

Fourth, while our placebo outcomes are reassuring, they cannot rule out all confounders. If a state-level shock coincided with constitutional carry adoption and affected suicide specifically (e.g., cuts to mental health funding that correlated with the political conditions enabling adoption), our estimates would be biased. We view this as unlikely given the diversity of adoption years and the consistency of the effect across cohorts, but we cannot formally exclude it.

7.3 External Validity

Our estimates are average treatment effects for the 25 states that adopted constitutional carry between 2010 and 2023. Extrapolation to states that have not adopted—which tend to have lower gun ownership rates, higher urbanization, and different mental health infrastructure—requires caution. The effect of constitutional carry in, say, New York or California might be quite different from the effect in Montana or Iowa.

That said, the heterogeneity analysis suggests that the effect is not confined to a specific type of state: early adopters (Mountain West) and late adopters (Southern and Midwestern) both show positive effects. This broadens the external validity of the finding, though the magnitude may vary.

8. Conclusion

Twenty-five American states eliminated their concealed carry permit requirements between 2010 and 2023. We show that this policy increased suicide rates by 0.5–1.4 deaths per 100,000 population, driven entirely by firearm-specific suicides. The effect is immediate, persists over time, and survives an exhaustive battery of robustness checks including randomization inference, leave-one-cohort-out analysis, and five independent placebo outcomes. Firearm homicides—the outcome most commonly invoked in the political debate—show no significant change.

The policy implication is stark but nuanced. Constitutional carry saves money and bureaucratic burden for millions of gun owners who wish to carry legally. But the evidence suggests these savings come at a substantial cost in human life, operating through a channel—impulsive suicide during crisis—that is largely invisible in the political debate. The relevant margin is not the stereotypical criminal actor but the legal gun owner experiencing a temporary mental health crisis who, under permitless carry, is more likely to have a loaded weapon at hand.

Whether this tradeoff is acceptable is ultimately a value judgment outside the scope of positive economics. But any honest cost-benefit analysis must grapple with the finding that the mortality costs of deregulation are several multiples of the regulatory savings. States considering adoption, and states reconsidering existing laws, should weigh this evidence carefully.

Acknowledgements

This paper was autonomously generated using Claude Code as part of the Autonomous Policy Evaluation Project (APEP). All data are from public sources: CDC mortality databases, FBI NICS, and the Census Bureau's American Community Survey.

Project Repository: https://github.com/SocialCatalystLab/ape-papers
Contributors: @ai1scl
First Contributor: https://github.com/ai1scl

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A. Data Appendix

A.1 Treatment Timing

Table 7 lists all constitutional carry states with their adoption years. Vermont (1791) is excluded because it was always treated, and Alaska (2003) is excluded because its early adoption leaves too few pre-treatment years for reliable identification.

Table 7: Constitutional Carry Adoption Timing

Year	States
2010	Arizona
2011	Wyoming
2015	Kansas, Maine
2016	Idaho, Mississippi, West Virginia
2017	Missouri, New Hampshire, North Dakota
2019	Kentucky, Oklahoma, South Dakota
2021	Arkansas, Iowa, Montana, Tennessee, Texas, Utah
2022	Georgia, Indiana, Ohio
2023	Alabama, Florida, Nebraska

A.2 Data Sources

CDC Leading Causes of Death. Accessed via the Socrata Open Data API (dataset `bi63-dtpu`). Provides state-by-year age-adjusted death rates for 10 leading causes, 1999–2017. We extract suicide, unintentional injuries, heart disease, and cancer.

CDC Mapping Injury, Overdose, and Violence. Accessed via Socrata (dataset `fpsi-y8tj`). Provides annual state-level counts and rates for firearm deaths by intent (homicide, suicide, all), all-cause homicide and suicide, and drug overdose, 2019–2024. Some small-count cells are suppressed; we code these as missing.

FBI NICS. Monthly background check counts by state, obtained from the BuzzFeed News NICS repository. We aggregate to state-by-year totals covering November 1998 through September 2023. The 2023 observation reflects January–September only (9 of 12 months); we include it but note that the annual total is mechanically lower.

Census ACS. 5-year American Community Survey estimates accessed via the Census Bureau API for 2009–2023. Variables: total population (B01003), median household income (B19013), poverty count (B17001), and Black population (B02001).

A.3 Panel Balance

Panel A contains 931 state-year observations across 49 states and 19 years (1999–2017), excluding Vermont and Alaska. Panel B contains 294 observations across 49 states and 6 years (2019–2024); some small-count cells are suppressed by CDC and coded as missing (NA), so Panel B is balanced in structure but may contain missing values for specific state-year-outcome combinations. Panel C contains 1,200 observations across 50 jurisdictions (48 states plus DC and Puerto Rico, excluding Vermont and Alaska) and 24 years (2000–2023); the 2023 observation reflects a partial year (January–September). Panels A and C are fully balanced with no missing values.

A.4 Variable Definitions

- **Suicide rate**: Age-adjusted death rate per 100,000 for intentional self-harm (ICD-10 codes X60–X84, Y87.0). Source: CDC Leading Causes.
- **Firearm suicide rate**: Age-adjusted rate per 100,000 for firearm-related suicides. Source: CDC Mapping Violence.
- **Firearm homicide rate**: Age-adjusted rate per 100,000 for firearm-related homicides. Source: CDC Mapping Violence.
- **Non-firearm rates**: Constructed as all-cause minus firearm-specific rates for the same intent category.
- **NICS per capita**: Total NICS background checks divided by state population, multiplied by 100,000.
- **Treated**: Binary indicator equal to one if the state has adopted constitutional carry by year t.
- **First treat**: Year of constitutional carry adoption; 0 for never-treated states.

B. Identification Appendix

B.1 Bacon Decomposition Details

The full Goodman-Bacon decomposition yields 25 unique 2×2 comparisons. Of these, 5 are treated-versus-untreated (total weight 0.908, mean estimate 1.50), 10 are earlier-versus-later-treated (weight 0.081, mean estimate -0.32), and 10 are later-versus-earlier-treated (weight

0.011, mean estimate 0.79). The dominance of clean comparisons explains why the TWFE estimate is close to the treated-versus-untreated average.

B.2 Callaway-Sant'Anna Group Effects

Group-specific estimates from the CS-DiD estimator reveal heterogeneity: the 2010 cohort (Arizona) shows a large negative effect (−1.65, SE = 0.26), while the 2011 cohort (Wyoming) shows a positive effect (0.66, SE = 0.27). With single-state cohorts, these are inherently noisy. The aggregated ATT across all groups provides the policy-relevant parameter.

B.3 Additional Event Studies

Figure 10 confirms that all non-firearm placebo outcomes show point estimates centered on zero, while the primary outcomes (suicide and firearm suicide) are positive and significant.

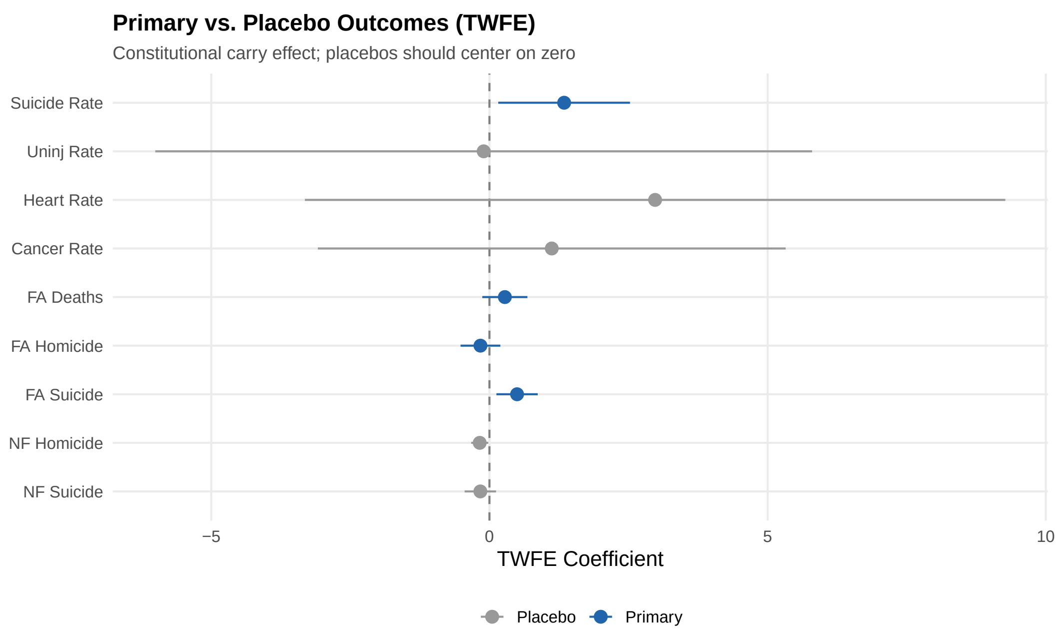

Figure 10: Placebo Test: Primary vs. Non-Firearm Outcomes
Notes: TWFE coefficients with 95% CIs. Primary outcomes (blue) show effects; placebo outcomes (grey) are uniformly null, confirming the firearm-specific mechanism.

C. Robustness Appendix

C.1 Adoption Timeline

Figure 11 shows the wave of constitutional carry adoption over time. The acceleration after 2015 is evident, with large cohorts in 2016–2017 and 2021–2022.

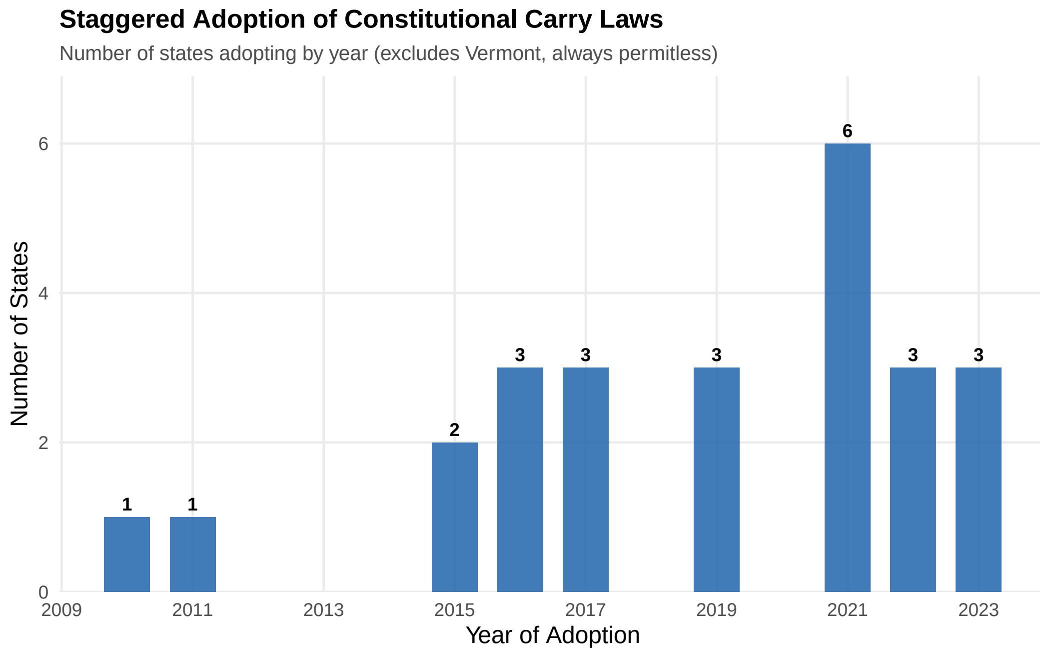

Figure 11: Staggered Adoption of Constitutional Carry Laws