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The Effects of Waiting Periods on Firearm Suicides in the U.S.

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Abstract

In this paper, we analyze the causal effect of mandatory firearm waiting periods on suicide rates using difference-in-differences methodology. We find waiting periods reduce overall firearm suicides by 12% (0.92 deaths per 100,000), with steeper declines among white individuals (37%) and adults over 55 (40%). We find no evidence of substitution toward non-firearm methods; conversely, repealing these laws increases firearm suicides. Back-of-the-envelope calculations suggest that waiting periods prevent approximately 3,000 deaths annually, generating $41 billion in social benefits. These findings demonstrate that "cooling-off periods" effectively disrupt the transition from suicidal ideation to action by delaying access to lethal means.

JEL classification

I18, I12, K32, J17, H75

Keywords

firearm waiting periods, suicide prevention, gun policy, public health, difference-in-differences, event-study design

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1 Introduction

Suicide claims nearly one million lives worldwide each year and is often an impulsive act (Lewiecki and Miller 2013). In the United States, firearms—particularly handguns—account for over half of all gun-related fatalities and more than half of all suicides, and the economic burden associated with suicide is substantial (Greenberg et al. 2015; Greenberg et al. 2021; National Center for Health Statistics 2007). Since gunshots are highly lethal and require little planning, policies that introduce a barrier between purchase and possession may be uniquely positioned to save lives. Waiting period laws create such a barrier, theoretically giving individuals experiencing suicidal ideation time for the crisis to pass or seek medical care before they can access a firearm. Therefore, it is important to causally identify whether waiting periods are effective in preventing firearms suicide.

Emerging research in economics and public health further reinforces that suicide is not solely a function of long-standing mental illness but is highly responsive to acute shocks and the availability of means. Economic hardship—such as job loss, income volatility, or relative status decline—has been shown to significantly increase suicide rates (Breuer 2014; Christian, Hensel, and Roth 2019; Daly, Wilson, and Johnson 2013). These results support the idea that the implementation of policy measures aimed at impulsive moments, such as waiting periods for firearms purchases, can significantly influence the outcomes by disrupting critical time frames that could otherwise lead to deadly actions. Moreover, impulsive-aggressive behavior has consistently been identified as a risk factor for suicide across the life course, with adolescents and young adults being particularly vulnerable (Anestis et al. 2014; McGirr et al. 2008). Many suicide attempts are driven by transient states of hopelessness and distress rather than long-term ideation. Restricting immediate access to lethal means could create a critical window in which intense suicidal urges can fade and lifesaving intervention can be provided.

We link six decades of county-level mortality data from the National Vital Statistics System with the longitudinal RAND State Firearm Law Database, which codes the exact timing of every waiting period statute enacted between 1813 and 2015 (Cherney et al. 2022; National Center for Health Statistics 2007). To enable full use of the county-level mortality data, we create a crosswalk that accounts for all county mergers and splits occurring after 1959, using information from Bailey and Goodman-Bacon (2015) and Forstall (1994). This produces a balanced panel of counties with harmonized Federal Information Processing Standards (FIPS) codes spanning 1959 to 2019. The staggered adoption of waiting-period laws across states enables us to use the recent development in the difference-in-differences literature to compare suicide trajectories in treated, not-yet-treated, and never-treated states. We find that adopting waiting periods reduces the firearms suicide rate by about 0.92 deaths per 100,000 people, a 12% decrease from baseline. The effects are larger for key demographic groups: among men, waiting periods reduce firearm suicides by 1.5 deaths per 100,000 (11% decrease); among adults aged 55 and older, we observe a reduction of 25 deaths per 100,000 (40% decrease); and among white individuals, we find a reduction of 14.4 deaths per 100,000 (37% decrease). These groups account for the majority of firearm suicides in the United States. Crucially, we do not find that individuals switch toward non-firearm suicide methods; in fact, among men, adults 55 and older, and white individuals, we find significant decreases in non-firearm suicides, consistent with waiting periods curbing fatalities by delaying access to a uniquely lethal method rather than merely redirecting individuals toward alternative methods.

We estimate the effect of repealing waiting periods on suicides. We find that states that repealed their waiting periods experienced statistically significant increases in firearm suicides: a 4.5% increase overall, 4.6% among men, and 2.8% among white individuals. Notably, we find significant increases in non-firearm suicides following repeal across all demographic groups—12.6% overall, 12.8% among men, 15.5% among adults 55 and older, and 13.3% among white individuals. The exception is adults aged 55 and older, who experienced a 5.1% decrease in firearm suicides following repeal. These asymmetric effects between adoption and repeal warrant further investigation. Several factors may contribute to this asymmetry. First, adoption and repeal occur in different states and different time periods, so the comparison groups and baseline conditions differ. Second, states that adopted waiting periods earlier (and thus contribute more to the adoption estimates) may have experienced larger absolute effects due to higher baseline suicide rates or fewer substitute policies. Third, if waiting periods interact with other concurrent policy changes (e.g., expanded background checks, mental health investments), the adoption effect may capture a broader policy environment rather than the waiting period alone. Fourth, cohort effects may play a role: the populations affected by early adoption may differ systematically from those affected by later repeal. While we cannot definitively adjudicate among these explanations, the asymmetry underscores the importance of interpreting adoption and repeal estimates as coming from distinct natural experiments with potentially different local average treatment effects.

We investigate whether the protective effects of waiting periods operate through a delay mechanism. We estimate models in which the independent variable is the number of days a purchaser must wait. We find evidence of a dose-response relationship: each additional day of mandatory waiting is associated with reductions in firearm suicides, with particularly large effects among older adults and white individuals. These findings suggest that waiting periods prevent deaths by allowing time for acute suicidal crises to subside, and states considering waiting period legislation should attend not only to whether a waiting period exists but also to its duration.

Adopting waiting periods is associated with approximately 3,000 fewer firearm suicide deaths per year nationwide. The reductions are especially pronounced among men, older adults, and white individuals—groups that account for most firearm suicides in the United States. Importantly, there is no evidence of a shift toward other suicide methods. Non-firearm suicides also decline in key groups, consistent with waiting periods saving lives rather than merely changing methods. Valuing these mortality reductions using a value of a statistical life (VSL) of $13.7 million, this implies a back-of-the-envelope annual social benefit of roughly $41 billion, even before accounting for potential spillovers to nonfatal injuries or broader welfare gains.[1]

A growing body of empirical work examines the relationship between waiting-period laws and firearm suicides. Luca, Malhotra, and Poliquin (2017) use state-year panel data and find that waiting periods reduce gun homicides by roughly 17%; Edwards et al. (2018) study mandatory handgun purchase delays and estimate 2–7% declines in firearm suicide mortality; Donohue, Cai, and Ravi (2023) exploit age-based variation in handgun purchase delay laws and find that delays reduce firearm suicides, with effects concentrated among younger individuals; Schell et al. (2024) employ Bayesian regression models of state firearms laws and find evidence that waiting periods reduce firearm suicides; and Arnold and Priestley (2025) show that the beneficial effects of waiting periods on firearm suicides are attenuated in counties close to states without such laws, presumably because residents can easily cross state borders to purchase firearms.[2] Single-state analyses include Oliphant (2022), who examines the repeal of Wisconsin's handgun waiting period and finds increased suicide rates, and Anestis, Anestis, and Butterworth (2017), who study changes in state handgun legislation and overall suicide rates. Woods (2026) provides a comprehensive review of this literature.

Our paper contributes to this body of work in several ways. First, we assemble the longest panel used in this literature, spanning six decades (1959–2019) of county-level mortality data, which captures five

[1] The value of a statistical life (VSL) is obtained from U.S. Department of Transportation (2024).

[2] Our paper, Donohue, Cai, and Ravi (2023), and Arnold and Priestley (2025) were developed independently and contemporaneously. Donohue, Cai, and Ravi (2023) focus on age-based heterogeneity using state-level data and a shorter panel, while Arnold and Priestley (2025) rely primarily on variation induced by the federal Brady Act rather than the staggered adoption and repeal of state-level waiting-period statutes. Our analysis differs from both in its use of six decades of county-level data, its exploitation of the full staggered adoption and repeal of state waiting-period laws, its application of the imputation estimator of Borusyak, Jaravel, and Spiess (2024) with robustness across multiple modern difference-in-differences estimators, and its examination of both demographic heterogeneity and the dose-response relationship between waiting-period duration and firearm suicides.

additional waiting-period adoptions relative to the 1970–2019 panels used in prior work and provides substantially more identifying variation than studies that rely on a single federal policy change. Second, we use county-level rather than state-level data, which substantially increases statistical power and allows us to exploit within-state variation—an important advantage over the state-level analyses in Donohue, Cai, and Ravi (2023) and Edwards et al. (2018). Third, we exploit the full history of staggered state-level adoption and repeal of waiting-period laws, rather than relying on a single source of variation such as the Brady Act (Arnold and Priestley 2025), providing a more complete and externally valid picture of policy effects. Fourth, we investigate heterogeneity across a broader set of demographic groups—age, race, and gender—than has been considered previously, and examine the dose-response relationship between waiting-period length and suicides, which has not been studied in the prior literature. Fifth, we employ the imputation estimator of Borusyak, Jaravel, and Spiess (2024), which addresses biases in conventional two-way fixed effects estimation under treatment effect heterogeneity, and demonstrate robustness across multiple recently developed staggered difference-in-differences estimators—a methodological rigor that goes beyond the standard TWFE or Goodman-Bacon decomposition approaches used in earlier work.

A substantial body of evidence links easy access to firearms with an increased risk of suicide. International comparisons find strong correlations between household gun ownership and suicide rates, with no signs that people simply switch to other means when guns are less available (Killias 1993). A systematic review and meta-analysis by Anglemyer, Horvath, and Rutherford (2014) confirms that access to firearms is associated with roughly tripled odds of suicide. At the state level, Siegel, Ross, and King III (2013) document strong correlations between gun ownership rates and firearm mortality. In the United States, Grossman et al. (2005) show that unloaded guns and separate ammunition storage are associated with markedly lower odds of suicide by youth. These findings align with clinical observations that many suicide attempts suddenly arise during moments of acute psychological distress (Lewiecki and Miller 2013).

Cross-national policy evaluations reinforce the value of restricting rapid access to firearms. Following the tightening of gun laws in 1992, New Zealand saw a 46% decrease in firearm suicides among the general population and a 66% decrease among individuals aged 15–24 (Beautrais, Fergusson, and Horwood 2006). The 1996 Australian National Firearms Agreement, which combined large gun buybacks with stricter licensing, has also been associated with subsequent declines in firearms suicide (Baker and McPhedran 2007).[3] In the United States, the laws on firearms removal based on risk ('red flag') enacted

[3] The Australian National Firearms Agreement's buyback provision was a mandatory government purchase

in Connecticut and Indiana were followed by measurable reductions in statewide suicide rates (Kivisto and Phalen 2018).

Firearm-related injuries and suicides among youth remain a significant concern in the United States. Chaudhary et al. (2024) find that mental health diagnoses often precede youth suicides, underscoring the need for earlier identification and intervention strategies within healthcare systems. The financial burden of firearms injuries, both fatal and nonfatal, has been substantial. Injuries are estimated to cost the healthcare system and the economy billions of dollars through lost productivity (Miller et al. 2024). These challenges are compounded by persistent trends of firearm-related harm in children and adolescents, emphasizing the urgent need for prevention and harm-reduction approaches (Kaufman et al. 2021; Lee et al. 2022). This urgency is magnified by psychological research that emphasizes the impulsive nature of many youth suicides, where access to firearms dramatically increases the risk of fatal outcomes (Anestis et al. 2014; McGirr et al. 2008).

Efforts to reduce firearm-related injuries must also focus on storage practices and perceptions of accessibility. Miller et al. (2025) highlight the critical role of secure firearms' storage in reducing the risk of suicide, particularly in households with adolescents. However, firearms storage practices vary widely, and older adults and parents often underestimate the extent to which firearms are accessible to youth (Carter et al. 2022; Hastings et al. 2025). Even when parents report using safe storage methods, teens may still perceive firearms as accessible, suggesting that education and behavioral interventions must account for both the parental and youth perspectives (Hastings et al. 2025). We contribute to this literature by evaluating the efficacy of waiting periods on firearm suicides.

2 Data

We use two main data sources. To measure the effect of waiting period on firearm suicides, we use mortality data from the National Vital Statistics System (NVSS) death files for county-level suicides. We also use RAND's state firearms law dataset for waiting periods.

program in 1996-1997 that collected approximately 650,000 prohibited firearms from civilians at market value, funded by a temporary Medicare levy. Participation was compulsory, with criminal penalties for non-compliance after the amnesty period.

Firearms suicide data

To measure the effect of waiting periods on firearm suicides in the United States, we use mortality data from the National Vital Statistics System (NVSS) covering the years 1959 to 2019 (National Center for Health Statistics 2007). Our outcome of interest is the firearms suicide rate, defined as the number of firearm suicides per 100,000 population in each county and year. We use the Multiple Cause of Death files, which include information on the underlying and contributing causes of death for each decedent, coded using ICD-10. Our identification of suicide and suicide method is based on ICD-10 codes for the underlying cause of death, which allows us to distinguish firearm suicides from other suicide methods. These codes permit the identification of suicide and its method, enabling a more detailed analysis of different types of suicide, including those involving firearms.[4] In addition, the data include a range of socioeconomic characteristics of the deceased, such as age, sex, race, marital status, and education level. For each death, we also observe the county of occurrence, county of residence, and county population size.

Using information on counties that merged and split from Bailey and Goodman-Bacon (2015) and Forstall (1994), we recombine all counties that split or merged after 1959 to produce a crosswalk for the Multiple Cause-of-Death files, creating a balanced panel of all counties from 1959 to 2019. We also harmonize Federal Information Processing Standards (FIPS) codes across counties. This crosswalk represents a methodological contribution, as it had not been previously available, which would allow for the full usage of the county-level death files.[5]

State firearms law

The second dataset we use is the RAND State Firearm Law Database, developed as part of the Gun Policy in America initiative launched in 2016. It is a longitudinal database that tracks all gun laws by state from 1813 to the present (Cherney et al. 2022). The database covers various categories of gun laws, including background check requirements for handguns and long guns, firearm sales restrictions, minimum age requirements, and the presence of waiting periods, defined as the time a seller must wait between the

[4] The ICD-10 codes used to define underlying causes of death due to suicide are X60–X84 (intentional self-harm) and Y87.0 (sequelae of intentional self-harm). Codes X60–X69 correspond to intentional self-poisoning, while codes X70–X84 correspond to intentional self-harm by other and unspecified means. Firearm suicides are identified using codes X72 (intentional self-harm by handgun discharge), X73 (intentional self-harm by rifle, shotgun, and larger firearm discharge), and X74 (intentional self-harm by other and unspecified firearm discharge).

[5] We provide more details on how we harmonize counties and codes in the Data Appendix C.

purchase and delivery of a firearms.

We construct the waiting period treatment variable based on the presence of a waiting period in a given state and year using the RAND database. Several states are considered "never-takers," meaning that they never implemented waiting periods for firearms purchases. These states include Colorado, Delaware, Iowa, Massachusetts, Michigan, Missouri, Nebraska, Nevada, New York, North Carolina, Ohio, South Carolina, Utah, and Virginia.[6] Among the states that implemented waiting periods, two scenarios arise: (1) states with a single policy transition, meaning they switched from no waiting period to having one only once, and (2) states with multiple transitions between implementation and non-implementation. States with a single policy transition include California, the District of Columbia (DC), Florida, Hawaii, Illinois, Maryland, Minnesota, Mississippi, New Jersey, Rhode Island, and Washington. The remaining states did not experience policy transitions. We present different cohorts of states in Table A.1 and a map of all states with different treatment sequences in Figure A.1. We note that some discrepancies in the number of treated states may arise depending on whether states treated prior to the start of our sample period (1959) are counted; we clarify the treatment status of each state in Table A.1 and Appendix Figure 1.

Coding considerations

Several coding decisions merit discussion. First, our treatment variable captures explicit statutory waiting periods as coded by the RAND database and does not capture de facto delays that arise from permit or licensing requirements. States such as Massachusetts and New York, which we classify as control states, impose licensing requirements that can create substantial delays between the decision to purchase a firearm and actual possession. To the extent that these de facto delays have similar effects to statutory waiting periods, our control group includes states with implicit "treatment," which would attenuate our estimates toward zero. Future work should attempt to code de facto waiting periods arising from licensing and permit requirements to obtain a more comprehensive measure of purchase delays.

Second, our treatment variable does not distinguish between waiting periods for handguns and those

[6] Although the RAND database classifies New York as never having enacted an explicit statutory waiting period, New York's licensing and permit requirements create a de facto waiting period that delays firearm acquisition. Because this de facto waiting period means New York is effectively always treated throughout our sample period, we include New York as an always-treated unit in our analysis, and it is therefore dropped from both the treatment and control groups in our main adoption sample. Similarly, other states listed here—such as Massachusetts—impose licensing requirements that function as implicit waiting periods. We discuss the broader implications of de facto waiting periods for our coding and identification in the Coding Considerations subsection below.

for long guns. Because the overwhelming majority of firearm suicides involve handguns, focusing on handgun-specific waiting periods would be more precise. However, most state waiting-period laws apply to handguns (and many extend to all firearms), so the distinction is less consequential in practice. Nonetheless, we note this as a limitation and encourage future analyses that exploit variation in the firearm types covered by waiting-period statutes.

Third, gun regulations are often enacted in clusters: legislation that imposes a waiting period may simultaneously introduce other requirements such as background checks, permit-to-purchase mandates, or safe storage provisions. Our empirical strategy attributes the full estimated effect to the waiting period, but if correlated policies were adopted simultaneously, our estimates may partially capture the joint effect of a policy bundle rather than the waiting period alone. We partially address this by controlling for county and year fixed effects, which absorb any time-invariant state characteristics and common time trends, and by noting that our robustness checks across multiple estimators yield consistent results. However, we cannot fully rule out confounding from co-adopted policies, and we acknowledge this as a limitation.

Sample construction

The sample is composed of states that experience either one policy transition from no-treatment to treatment, or none at all. This yields a final sample of 23 states: 10 treatment states that adopted a waiting period at some point before or during the sample period and 13 control states that never adopted a waiting period for firearm purchases. The final sample is composed of 1,306 counties: 932 'never-takers' counties, six counties that adopted a waiting period prior to 1959, and 374 counties that adopted a waiting period between 1959 and 2019. We present the states and counties included in our final sample, along with their adoption status of the waiting period policies, in Figures 1 and 2. We show the staggered adoption of waiting periods in Figure 1, while we present the corresponding county count in Figure 2. For the purpose of our analysis, the comparison group is composed of never-takers and yet-to-be-treated. For population and demographic information, we combined our final dataset with data from the Decennial US Census (U.S. Census Bureau 2020).

We also construct a second sample to estimate the effects of repealing waiting periods. This sample includes states that either transitioned from treatment to no-treatment or were always treated. This yields a final sample of 14 states: 8 treatment states that repealed their waiting period law during the sample period and 6 control states that maintained a waiting period throughout. We present the staggered repeal of waiting periods and the county counts in Figures A.2 and A.3 respectively.

Outcome variable

The main outcome variable is the firearms suicide rate per 100,000 people. We identify firearm-related suicides using ICD-10 codes for the underlying cause of death.[7]

We then calculated county-level suicide rates by aggregating individual-level mortality data for each year and county. Specifically, we sum the number of firearm suicides in each county-year and divide by the corresponding county population, multiplying by 100,000 to calculate the suicide rate by firearms in a given county and year. You can find trends in firearm and non-firearm suicide rates from 1959 to 2019 in Figure A.4. There is a substantial heterogeneity in firearm suicide rates by treatment status (Figure A.5), with consistently lower rates observed in states that implemented waiting period policies, as well as marked demographic disparities (Figure 3), with particularly elevated rates among men and people over 55+ years of age.

Summary statistics

We present the summary statistics in Table 1. Counties that adopted a mandatory waiting period tend to have lower total suicide rates per 100,000 people, overall firearm suicide rates, and men, older adults (age 55 and older) and white individual firearm suicide rates than counties that never adopted a mandatory waiting period. Furthermore, adopting counties share similar gender demographics with their counterparts but generally have higher education levels and a lower share of the population living below the poverty line. Demographic and socioeconomic covariates are drawn from the Decennial US Census and are measured at the county level; non-mortality measures in Table 2 reflect census-year values interpolated to annual observations. To assess the comparability of treated and control counties, we present covariate balance statistics in Table 2. The table shows that while treated and control counties are similar on several dimensions, there are statistically significant differences in the share of college-educated residents and the proportion living below the poverty line, with control counties having higher rates of both. Notably, treated counties also exhibit significantly lower firearm suicide rates among adults aged 55 and older prior to treatment. We present covariate balance over the full sample period; we note that comparing balance separately for pre-treatment and post-treatment periods would provide additional insight into whether treated and control counties diverge on observable characteristics around the time of policy adoption.

[7] We use the following ICD-10 codes from the NVSS for firearm suicides: X72 (intentional self-harm by handgun discharge), X73 (intentional self-harm by rifle, shotgun, and larger firearm discharge), and X74 (intentional self-harm by other and unspecified firearm discharge).

3 Empirical Strategy

In this paper, we estimate the dynamic effects of firearms purchase waiting periods on suicide rates using the imputation estimator developed by Borusyak, Jaravel, and Spiess (2024). This approach addresses the well-documented biases that arise when using conventional two-way fixed effects (TWFE) estimators in settings with staggered treatment adoption (De Chaisemartin and d'Haultfoeuille 2020, 2023; Goodman-Bacon 2021; Roth et al. 2023; Sun and Abraham 2021). We now discuss the model, identification assumptions, and estimation approach.

Let y_{ist} denote the firearms suicide rate per 100,000 people in county i in state s at time t. Following Borusyak, Jaravel, and Spiess (2024), we specify the following event study model that allows for unrestricted treatment effect heterogeneity:

$$y_{ist} = \theta_i + \lambda_t + D_{ist}\tau_{ist} + \varepsilon_{ist} \tag{1}$$

where θ_i represents county fixed effects and λ_t are year fixed effects. D_{st} is an indicator equal to one if a waiting period is active in state s at time t, and τ_{ist} represents the fully heterogeneous treatment effect for county i at time t. In equation (1), for each state, the data contains an adoption date, E_s, when D_{ist} switches from 0 to 1. This specification allows treatment effects to vary arbitrarily across counties and time periods without imposing parametric restrictions.

Our identification strategy leverages the staggered adoption of firearms purchase waiting periods across states. The model in equation (1) requires three main assumptions on potential outcomes and causal effects. First, the parallel trends assumption requires that in the absence of waiting periods, firearms suicide rates would have evolved similarly between treated and control counties. Second, we assume no anticipation effects—that waiting periods did not affect firearm suicide rates before the policy's actual implementation in each state. This assumption is plausible given that suicides are often impulsive and acute, making it unlikely that individuals would systematically time suicide attempts based on anticipated future gun policy changes. Finally, we impose a model of unrestricted causal effects, referred to as the "null model" in Borusyak, Jaravel, and Spiess (2024). In this case, the target estimand (parameter of interest) is the dynamic average treatment effect on the treated (ATT) l periods (horizons) since the treatment for a given $l \geq 0$:

$$\tau_l = \sum_{\{i,s,t\}:K_{st}=l} w_{ist}\tau_{ist} \tag{2}$$

where weight is given by $w_{ist} = \frac{\mathbb{I}(K_{st}=l)}{|\{i,s,t\}:K_{st}=l|}$ and sums to one within each event time l. Borusyak, Jaravel, and Spiess (2024) proposes an imputation estimator that uses untreated observations to predict what would have happened to treated units in the absence of treatment. The estimator proceeds in three steps:

1. Using untreated observations only (i.e., observations with $D_{st} = 0$) and ordinary least squares (OLS), we obtain $\hat{\theta}_i$ and $\hat{\lambda}_t$ from

$$y_{ist} = \theta_i + \lambda_t + \varepsilon_{ist}.$$

2. For each treated observation $\{i,s,t\}$ with $D_{st} = 1$, we construct the untreated potential outcome (counterfactual outcome) as $\hat{y}_{ist}(0) = \hat{\theta}_i + \hat{\lambda}_t$ and estimate the individual-specific treatment effect as $\hat{\tau}_{ist} = y_{ist} - \hat{y}_{ist}(0)$.

3. Estimate the event-time coefficients as weighted averages: $\hat{\tau}_l = \sum_{\{i,s,t\}:K_{st}=l} w_{ist}\hat{\tau}_{ist}$.

We cluster standard errors at the state level for two reasons: to address potential serial correlation within states across time and because the treatment is assigned at the state level. While the core assumptions underlying the difference-in-differences framework cannot be directly verified in the post-treatment period, we can test the identifying assumptions during the pre-treatment period through a pre-trends test. In contrast to conventional pre-trends testing approaches that rely on standard event studies, the imputation-based estimator allows us to assess both the parallel trends and no-anticipation assumptions using only untreated observations. Implementing this pre-trends test requires specifying an alternative model for the outcome y_{ist} among untreated units. In particular, given an observable vector W_{ist}, we specify the alternative model as $y_{ist} = \theta_i + \lambda_t + W_{ist}\phi + \varepsilon_{ist}$, where W_{ist} can consist of binary indicators corresponding to $1,\dots,k$ periods before treatment onset for some selected k. We then estimate ϕ via OLS using only untreated observations and test whether $\phi = 0$. Our main findings are displayed graphically, combining these pre-trend coefficient estimates with the horizon-specific ATTs derived from equation (2). Following Borusyak, Jaravel, and Spiess (2024), this OLS-based approach to pre-trends testing circumvents the pre-testing problems identified by Roth (2022). Specifically, regression-based tests that use the entire sample—including treated units—implicitly impose constraints on treatment effect heterogeneity. Furthermore, inference based on imputation-estimated ATTs remains valid conditional on passing the pre-trends test, thereby avoiding the inflated variances and overly conservative inference that characterize standard pre-trend testing procedures (Roth 2022).

An important institutional consideration is the Brady Handgun Violence Prevention Act, which became effective in February 1994. Before the National Instant Criminal Background Check System (NICS) was implemented in November 1998, Brady required each handgun buyer to undergo a background check conducted by the local Chief Law Enforcement Officer (CLEO), who had up to five business days to determine whether the transfer would be lawful—effectively introducing a waiting period of up to five business days. States that already had their own approved alternative background-check systems were exempt from this federal provision. As shown in Figure 1, most of our treated states had already adopted their own waiting-period laws well before 1994, and several control states lacked approved alternative systems and were therefore temporarily subject to the federal Brady waiting period during 1994–1998. However, this temporary federal exposure does not confound our identification for two key reasons. First, the Supreme Court's 1997 decision in *Printz v. United States* struck down the CLEO provision as unconstitutional, and most of the affected control states promptly ceased enforcing the federal waiting period, meaning their exposure was brief and often incomplete. Second, and more importantly, the control states that were temporarily exposed to Brady switched out of treatment after the CLEO provision was invalidated—they did not maintain waiting periods—and our identifying variation comes from the staggered adoption of *state-level* waiting-period statutes across a wide range of years (from before 1959 through 2015), not from the transient federal mandate. The Brady-induced waiting periods in control states were short-lived and are unlikely to have generated the sustained changes in suicide trajectories that would be needed to bias our estimates. Nonetheless, we note this institutional feature for completeness and as context for interpreting our results.

To evaluate whether waiting period laws simply shift methods of suicide rather than reducing overall suicide rates, we analyze their impact on non-firearm suicide rates. If waiting periods genuinely decrease total suicides, we would expect no significant change in non-firearm suicide methods. Using the same analytical approach with non-firearm suicide rates as our outcome variable, we test for method substitution. The absence of any significant increases on non-firearm suicides would strengthen the validity of our identifying assumptions.

For the analysis examining states that repeal their waiting period laws, we reverse the treatment design: the treatment group consists of states that transition from having a waiting period to not having one, while the control group consists of states that consistently maintain waiting periods throughout the study period. This approach allows us to examine whether the removal of waiting periods leads to increases in firearm suicide rates, providing additional evidence for the causal effect of these policies.

4 Results

The Effects of Waiting Period Adoption on Firearm Suicide Rates

Overall Effect on Firearm Suicides. We present the results of estimating equation 1 in Figure 4. We show the estimates 10 years before the adoption of waiting periods and 10 years after. For the 10 years before adoption, we present point estimates and their associated 95% confidence intervals, which correspond to pre-periods. We can use these pre-treatment estimates to assess the parallel trends assumption. These estimates are statistically insignificant, indicating that the parallel trends and no anticipation assumption probably hold. For 10 years after adoption, we present the point estimates and their associated 95% confidence intervals for the post-adoption periods. These estimates correspond to the treatment effects. We find statistically significant decreases in the suicide rate by firearms two years following the adoption of waiting periods. Specifically, the average treatment effect on the treated (ATT) of adopting waiting periods reduces the suicide rate by firearms by about 0.92 deaths per 100,000 people. This is equal to a 12% decrease from baseline.

Effect on men. We present the results of estimating equation 1 in Figure 5 when restricting the sample to firearm suicides by men only. The pre-treatment estimates are not statistically significant from zero, supporting the assumption of parallel trends. In the post-treatment period, we observe an average reduction in firearm suicides among men by 1.5 deaths per 100,000 after the adoption of waiting periods. That is equal to an 11% decrease in firearm suicides among men from baseline. This finding is particularly important given that men account for the vast majority of firearm suicides in the United States. The statistically significant and larger reduction among men suggests that waiting periods may be most effective for populations with higher baseline rates of firearm suicides. The mechanism likely operates through disrupting impulsive suicide attempts, which research suggests are more common among men who use firearms (Anestis et al. 2014; McGirr et al. 2008).

Effect on adults aged 55 and older. We present the results of estimating equation 1 on suicides among individuals 55 years and older in Figure 6. We find that the pre-treatment estimates are not statistically significant from zero, supporting the assumption of parallel trends. After the adoption of waiting periods, we observe a reduction in firearm suicides for adults 55 years and older by 25 deaths per 100,000 (p-value = 0.18)—a 40% decrease from baseline. We note that this estimate is not statistically significant at conventional levels, and the wide confidence interval reflects the relatively small number of treated state-year observations for this subgroup. This age group merits particular attention in suicide prevention efforts, as older adults who attempt suicide are more likely to die from their attempts, making the potential protective effect of waiting periods especially valuable for this population.

Effect on white individuals. We show the causal effect of waiting period laws on firearm suicides among white individuals in Figure 7. The pretreatment estimates support the assumption of parallel trends. We find a statistically significant reduction in firearm suicides among white individuals by 14.4 deaths per 100,000, which represents a substantial 37% decrease in the suicide rate by firearms among this population. This large and statistically significant effect is noteworthy because white individuals have historically had the highest firearm suicide rates in the United States. We acknowledge an apparent tension: under a pure "cooling off" mechanism, waiting periods should primarily affect first-time buyers, yet white individuals are more likely to already own firearms. Several explanations may reconcile this pattern. First, the relevant margin may not be first-time ownership but rather the acquisition of an additional firearm during a period of crisis; a waiting period delays access to the specific weapon being purchased, even if the buyer owns other firearms. Second, white individuals have higher baseline rates of impulsive firearm suicide, so even a modest per-purchaser effect translates into a larger aggregate reduction. Third, as noted above, the large magnitude for this subgroup should be interpreted with caution given wide confidence intervals, and may partly reflect limitations of the counterfactual imputation for this demographic group.

Effect on other causes of suicide. We present the causal effect of waiting period laws on non-firearm suicides (i.e., all causes of suicide excluding suicides by firearm) in Figures A.6–A.9. The pretreatment estimates support the assumption of parallel trends. For the overall population, we find a small and marginally significant increase of 0.03 deaths per 100,000, equal to a 0.6% increase in non-firearm suicides. Among men, adults 55 years of age and older, and white individuals, we find significant *decreases* in non-firearm suicides.

We acknowledge that declines in non-firearm suicides following the adoption of waiting-period laws are not predicted by the proposed delay mechanism alone and warrant careful interpretation. Because waiting periods directly restrict only firearm access, they should not, under a pure "cooling off" mechanism, reduce suicides by other methods. Three possible interpretations merit discussion. First, the declines in non-firearm suicides may reflect broader concurrent changes in the policy or social environment within treated states that coincidentally reduced suicide risk through all methods. If states that adopted waiting periods simultaneously invested in mental health services or enacted other protective policies, then our estimates of firearm suicide reductions may partly capture these co-occurring changes rather than the waiting period alone. We discuss this possibility further below in the context of policy clustering. Second, waiting-period laws may serve as a signal of a broader societal commitment to suicide prevention, potentially triggering increased awareness and help-seeking behavior. Third, to the extent that some individuals planning non-firearm suicide first attempt to acquire a firearm, a mandatory delay could interrupt the broader suicidal crisis, preventing deaths by any method. Nonetheless, we recognize that the non-firearm suicide results introduce some ambiguity about whether the imputation estimator is fully recovering an appropriate counterfactual, as Duggan (2001) and others have noted that minimal substitution between methods is expected. We discuss robustness checks and diagnostics below that help assess the credibility of the counterfactual imputation.

Interpreting the Magnitude of the Estimates

Our estimated 12% reduction in firearm suicides from waiting-period adoption is larger than the 2–7% range reported in prior studies (Edwards et al. 2018; Luca, Malhotra, and Poliquin 2017; Schell et al. 2024). Several factors may contribute to this difference. First, our longer panel (1959–2019) captures earlier waiting-period adoptions that occurred during periods of higher baseline firearm suicide rates, when the absolute effect of a waiting period may have been larger. Second, our county-level analysis exploits finer geographic variation than state-level studies, which may attenuate estimates through within-state heterogeneity. Third, we note that the repeal estimates (4.5%) are within the range of prior work, suggesting that the adoption estimates may partly reflect contemporaneous policy or social changes in early-adopting states. We acknowledge that the magnitude of our adoption estimates—particularly the 37–40% reductions among white individuals and adults aged 55 and older—is large and merits scrutiny. These demographic subgroups have the highest baseline firearm suicide rates and the highest rates of firearm ownership, so larger absolute reductions are expected. However, the implied per-capita effects among new firearm purchasers would be substantial, as noted by one of our referees, and could reflect in part the imputation of counterfactuals from a limited set of donor states with potentially different underlying trends. We view the point estimates for these subgroups as indicative of the direction and relative magnitude of effects across groups, while recognizing that confidence intervals are wide and the precise magnitudes should be interpreted with caution.

The Effects of Waiting Period Repeal on Firearm Suicide Rates

Overall effect of repeal on firearm suicides. We present the results in Figure 8, showing estimates for 5 years before and 5 years after the repeal of waiting periods. The pretreatment point estimates and their associated 95% confidence intervals allow us to assess the parallel trends and no anticipation assumptions; they suggest that these assumptions hold. For the post-repeal periods, we find statistically insignificant period-specific estimates but a statistically significant ATT increase in the firearms suicide rate following repeal, suggesting that firearm suicides increased, on average, by 4.5%.

Effect on men. We present the results for men in Figure 9, showing estimates for 5 years before and 5 years after the repeal of waiting periods. The pretreatment point estimates and their associated 95% confidence intervals suggest that the parallel trends and no anticipation assumptions hold. For the post-repeal periods, we find statistically insignificant but positive period-specific estimates and a statistically significant ATT increase in the firearms suicide rate of 0.78 deaths per 100,000, suggesting that firearm suicides among men increased, on average, by 4.6%.

Effect on adults aged 55 and older. We present the results for adults aged 55 and older in Figure 10, showing estimates for 5 years before and 5 years after the repeal of waiting periods. The pretreatment point estimates and their associated 95% confidence intervals suggest that the parallel trends and no anticipation assumptions hold. For the post-repeal periods, we find mostly statistically insignificant but negative period-specific estimates and a statistically significant ATT decrease in the firearms suicide rate of 0.71 deaths per 100,000, suggesting that firearm suicides among adults aged 55 and older decreased, on average, by 5.1%.

Effect on white individuals. We present the results for white individuals in Figure 11, showing estimates for 5 years before and 5 years after the repeal of waiting periods. The pretreatment point estimates and their associated 95% confidence intervals suggest that the parallel trends and no anticipation assumptions hold. Although the point estimate at $t = -1$ is statistically significant, it is unlikely that this represents a violation of the no anticipation assumption since the estimate is positive rather than negative. For the post-repeal periods, we find statistically insignificant but positive period-specific estimates and a statistically significant ATT increase in the firearms suicide rate of 0.28 deaths per 100,000, suggesting that firearm suicides among white individuals increased, on average, by 2.8%.

Effect on other causes of suicide. We present the causal effect of repealing waiting periods on non-firearm suicides (i.e., all causes of suicide excluding suicides by firearm) in Figures A.10–A.13. The pretreatment estimates support the assumption of parallel trends. For the overall population, we find mostly statistically insignificant but positive period-specific estimates and a statistically significant ATT increase in non-firearm suicides of 0.5 deaths per 100,000, representing a 12.6% increase. Among men, we find a significant ATT increase of 0.8 deaths per 100,000 (12.8%). Among adults aged 55 and older, we find a significant ATT increase of 0.62 deaths per 100,000 (15.5%). Among white individuals, we find a significant ATT increase of 0.58 deaths per 100,000 (13.3%). These results suggest that the repeal of waiting periods is associated with increases in non-firearm suicides across all groups.

Placebo and Other Estimators

One potential concern is that the timing of waiting period adoption could coincide with other unobserved factors that differentially affect suicide rates. To address this issue, we conduct two sets of placebo tests. First, we implement a lead placebo test by shifting the treatment date 5 years earlier and estimating effects during the placebo treatment window (event time 0–4 in Figure A.14), which corresponds to 5–1 years before the actual policy change. If the parallel trends assumption holds, we should observe no significant effects during this pre-policy period. Across all demographic subgroups—the full population, men, adults aged 55 and older, and white individuals—the placebo estimates are statistically indistinguishable from zero, providing evidence that our results are not driven by pre-existing differential trends. Second, we estimate effects on negative control outcomes that should be unaffected by waiting period laws: non-suicide mortality rates (Figure A.15). These specifications include pre-treatment coefficients (event time -10 to -1) to test for parallel trends and post-treatment coefficients (event time 0 to 10) to detect spurious effects. If waiting period laws were spuriously correlated with other determinants of mortality, we would expect to see effects on these unrelated outcomes. The results show no systematic post-treatment effects on non-suicide mortality across demographic groups, further supporting the validity of our identification strategy.

We note several methodological considerations raised by the recent literature. First, because some states in our repeal sample experience treatment that switches on and off, the imputation estimator of Borusyak, Jaravel, and Spiess (2024) may not be ideally suited for this setting. De Chaisemartin and d'Haultfoeuille (2024) develop difference-in-differences estimators specifically designed for intertemporal treatment effects that accommodate treatments turning on and off. Applying their estimator to our repeal sample would provide a valuable robustness check, and we flag this as an important direction for future work. Second, our analysis of waiting-period length in Table 3 uses a continuous treatment variable within a TWFE framework, which raises additional concerns about the interpretation of regression coefficients when treatment intensity varies across units (De Chaisemartin and d'Haultfoeuille 2023). We interpret these results as suggestive evidence of a dose-response relationship, but acknowledge that they do not benefit from the same identification guarantees as our binary treatment estimates and should be viewed as complementary rather than definitive. Third, as noted by Roth (2022), pre-trends tests may have limited power to detect violations of parallel trends when confidence intervals are wide relative to the estimated treatment effects. In several of our event-study figures, the pre-treatment confidence intervals are indeed wide, meaning that we cannot rule out economically meaningful pre-existing trends. We supplement the visual evidence with formal joint pre-trends tests and placebo tests that shift the treatment date, which provide additional reassurance, but we acknowledge this limitation.

Furthermore, recent methodological advances in difference-in-differences estimation have underscored potential biases in traditional two-way fixed effects (TWFE) models when treatment timing is staggered across units. To evaluate the robustness of our findings, we compare treatment effect estimates from multiple estimators specifically designed for staggered adoption settings in Figure A.16. Alongside TWFE, we report estimates from Two-way fixed effects (TWFE), Callaway and Sant'Anna (2021) (CS), Borusyak, Jaravel, and Spiess (2024) (BJS), De Chaisemartin and d'Haultfoeuille (2023) (dCDH), Sun and Abraham (2021) (SA), Cengiz et al. (2019) (CDLZ), Gardner (2022), and Wooldridge (2025). Each of these estimators addresses potential bias from staggered treatment timing through different methodological approaches, yet all are designed to avoid the "forbidden comparisons" that can contaminate conventional TWFE estimates. Across these approaches, we find that the results consistently indicate that waiting periods reduce firearm suicide rates. The point estimates are similar in magnitude, and the confidence intervals substantially overlap across methods. This convergence of evidence from methodologically distinct estimators provides strong support for the robustness of the estimated treatment effect, indicating that our findings are not driven by the choice of identification strategy. We note several additional diagnostics that would further strengthen the analysis. First, augmenting the imputation estimator with time-varying covariates (e.g., population, demographics, income, and other state-level policy variables) could improve the comparability of treatment and control units and yield more precise counterfactual predictions. Second, visualizing the imputed counterfactual suicide rates alongside observed data for each treated state would allow readers to assess whether the imputation is reasonable on a state-by-state basis. Third, alternative approaches such as synthetic difference-in-differences (Arkhangelsky et al. 2021) may provide a useful complement by constructing data-driven counterfactuals that better match the pre-treatment trajectory of each treated unit. We flag these extensions as important directions for future revisions. The consistency across estimators is particularly important given recent methodological debates about the validity of TWFE models in staggered adoption settings. Our results demonstrate that the protective effects of waiting periods are not artifacts of potentially biased TWFE estimation but rather represent genuine causal effects that persist across alternative, more robust estimation approaches. This robustness check substantially strengthens the credibility of our core findings and their policy implications.[8]

Mechanism: The Role of Waiting Period Length

A key question for policy design is whether the protective effects of waiting periods operate through a delay mechanism—that is, whether longer waiting periods provide greater protection against impulsive firearm suicides. To investigate this mechanism, we estimate a two-way fixed effects model in which the treatment variable is the number of days a purchaser must wait between buying and receiving a firearm, rather than a binary indicator for any waiting period.

Table 3 presents the results. We find that each additional day of mandatory waiting is associated with a reduction in firearm suicides across all demographic groups examined. For the overall population, an additional waiting day reduces firearm suicides by 0.063 deaths per 100,000, representing a 0.74 percent decline relative to the baseline mean of 8.45 deaths per 100,000. The effect is statistically significant at the 10 percent level.

The protective effects of longer waiting periods are substantially larger for older adults and white individuals—two groups with elevated baseline firearm suicide rates. Among adults aged 55 and older, each additional waiting day is associated with a reduction of 5.63 deaths per 100,000, a 22.5 percent decline relative to the baseline mean of 24.99. For white individuals, the corresponding reduction is 4.12 deaths per 100,000, or 25.6 percent of the baseline rate. Both estimates are statistically significant at the 5 percent level. Among men, we observe a reduction of 0.096 deaths per 100,000 per waiting day, though this estimate is imprecisely estimated.

These findings provide direct evidence that the delay mechanism is central to the protective effects of waiting period laws. The dose-response relationship—whereby longer waiting periods yield larger reductions in firearm suicides—is consistent with the hypothesis that waiting periods prevent deaths by

[8] The differences between the estimators, especially compared with Callaway and Sant'Anna (2021), could be due to multiple factors, including the choice of weights in aggregating group-time effects into over ATT estimates; see for e.g., Deb et al. (2025).

allowing time for acute suicidal crises to subside. The particularly large effects among older adults and white individuals suggest that these populations may be especially responsive to delays in firearms access, potentially because their suicide attempts are more likely to be impulsive or because they have higher baseline access to firearms.

From a policy perspective, these results suggest that states considering waiting period legislation should attend not only to whether a waiting period exists but also to its duration. A three-day waiting period, for example, may yield meaningfully different public health outcomes than a seven-day or fourteen-day requirement. The substantial per-day effects we observe indicate that even modest extensions to existing waiting periods could generate additional reductions in firearm suicides.

5 Conclusion

This study contributes to a growing body of evidence that waiting periods for firearms purchases are an effective population-level suicide prevention tool. Leveraging six decades of county-by-year mortality data and the full historical record of state gun laws, we find that waiting-period laws reduce firearm-suicide rates by 0.92 deaths per 100,000 population, or about 12 percent relative to baseline. The effects are strongest for men (1.5 deaths per 100,000, 11% reduction), adults aged 55 and older (25 deaths per 100,000, 40% reduction), and white individuals (14.4 deaths per 100,000, 37% reduction)—the groups that account for the majority of firearm suicides. Crucially, we do not observe a significant shift toward non-firearm methods of suicide; rather, among men, older adults, and white individuals, we find significant decreases in non-firearm suicides as well, consistent with the interpretation that waiting periods save lives by limiting access to a uniquely lethal method. Rough estimates indicate that mandatory waiting periods averted about 3,000 firearm suicides each year, generating social benefits on the order of $41 billion annually.

Our analysis of waiting period repeals provides additional insight into the dynamics of these policies. States that repealed their waiting periods experienced statistically significant increases in firearm suicides—4.5% overall, 4.6% among men, and 2.8% among white individuals. Moreover, non-firearm suicides also increased significantly following repeal across all demographic groups, with increases ranging from 12.6% to 15.5%. These repeal effects reinforce the protective value of waiting period policies. The one exception is adults aged 55 and older, who experienced a 5.1% decrease in firearm suicides following repeal; this counterintuitive finding may reflect cohort effects or other confounding factors specific to this age group during the repeal period and warrants further investigation.

Our placebo analyses further support this causal interpretation. Women, who are substantially less likely to use firearms in suicide attempts, serve as a natural placebo group. Their firearm suicide rates did not decline after the adoption of waiting periods, strengthening the case that the reductions observed among men and other high-risk groups reflect the true effect of waiting period laws rather than contemporaneous unobserved changes or spurious correlations. Additionally, our lead placebo tests, which shift the treatment date 5 years earlier, show no significant effects during the pre-policy period across all demographic subgroups, confirming that our results are not driven by pre-existing differential trends. We also find no effects on non-suicide mortality rates—a negative control outcome that should be unaffected by waiting period laws—providing further evidence against the possibility that our estimates capture spurious correlations with other determinants of mortality. Moreover, robustness checks using recently developed difference-in-differences estimators for staggered adoption consistently indicate negative treatment effects, reinforcing the credibility of our findings.

We also provide direct evidence on the mechanism through which waiting periods reduce firearm suicides. By estimating models in which the treatment variable is the number of mandatory waiting days rather than a binary indicator, we find a clear dose-response relationship: each additional day of mandatory waiting is associated with further reductions in firearm suicides. The effects are particularly pronounced among older adults and white individuals, with each additional waiting day reducing firearm suicides by over 20 percent relative to baseline for these groups. These findings support the hypothesis that waiting periods save lives by allowing time for acute suicidal crises to subside and suggest that policymakers should consider not only whether to implement a waiting period but also its optimal duration.

From a policy perspective, these results highlight the preventive potential of waiting periods as a low-cost intervention. Unlike broader restrictions on firearms ownership, waiting periods impose only a temporary delay on purchases while providing a crucial buffer against impulsive, high-lethality acts of self-harm. The findings suggest that relatively modest regulatory measures can yield substantial public health benefits. Policymakers debating gun violence interventions often face a trade-off between political feasibility and measurable impact. Waiting periods appear to offer an unusually favorable balance: they impose minimal costs on lawful purchasers while generating sizable reductions in mortality.

We note that while our demographic heterogeneity analysis is informative for understanding the mechanism through which waiting periods operate, it does not directly translate into demographically targeted policy recommendations, as firearms regulations must be applied uniformly across demographic groups. The heterogeneity results are nonetheless valuable for informing complementary, targeted suicide prevention strategies—such as outreach to high-risk populations and clinical interventions—that can operate alongside universal waiting-period policies. Future research should extend this work by examining heterogeneity in effects across urban and rural contexts (Dunton et al. 2022), by considering potential interactions with complementary interventions such as extreme risk protection orders and safe storage laws, and by incorporating de facto waiting periods arising from permit and licensing requirements. In addition, comparative analyses with other "cooling-off" regulations—such as waiting periods for prescription opioid refills or other lethal means—may provide further insight into the broader applicability of time-based barriers in suicide prevention.

Overall, this study demonstrates that waiting periods are a powerful tool for reducing firearm suicides, particularly among the groups most at risk. By creating a critical pause between purchase and possession, waiting-period laws save lives in contexts where minutes and hours can make the difference between a temporary crisis and a permanent tragedy.

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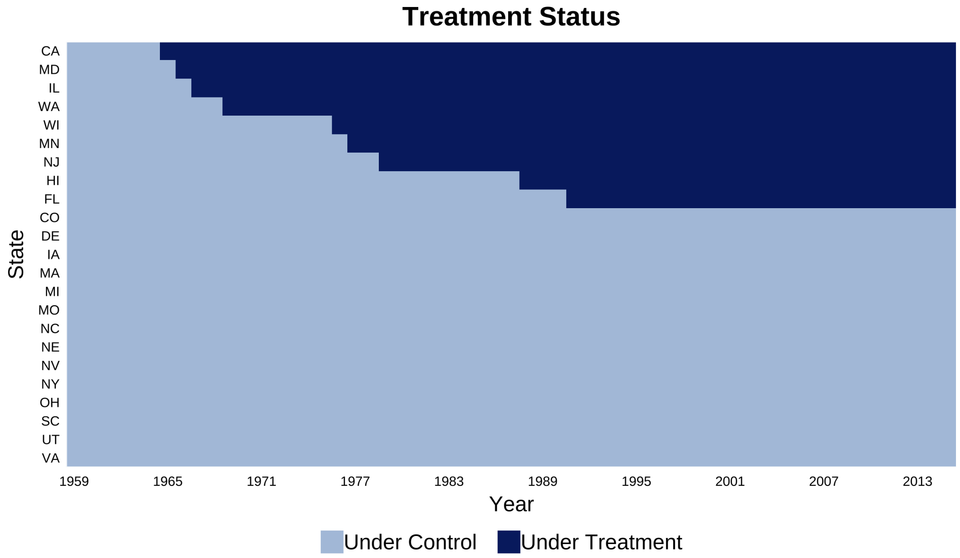
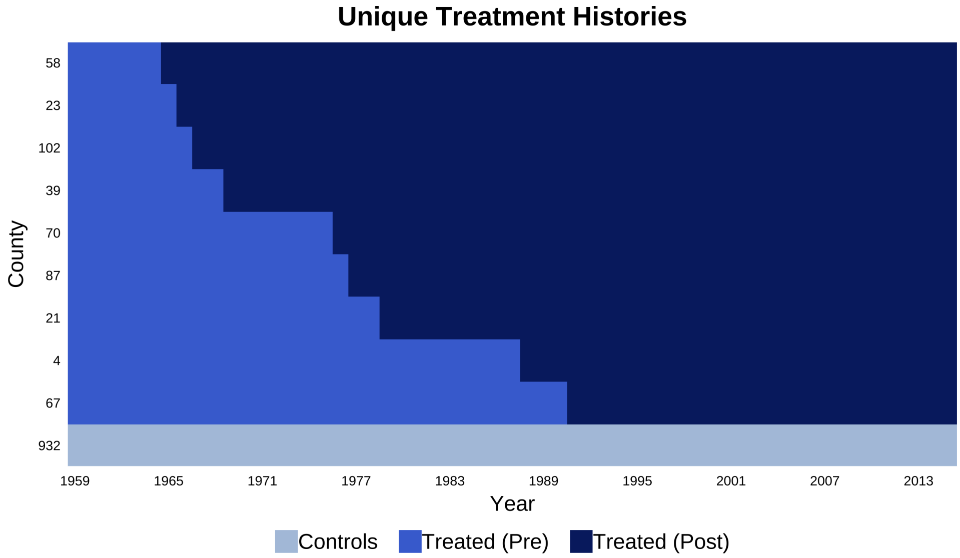
Fig. 1. Timing of Waiting Period Policy Adoption Across States



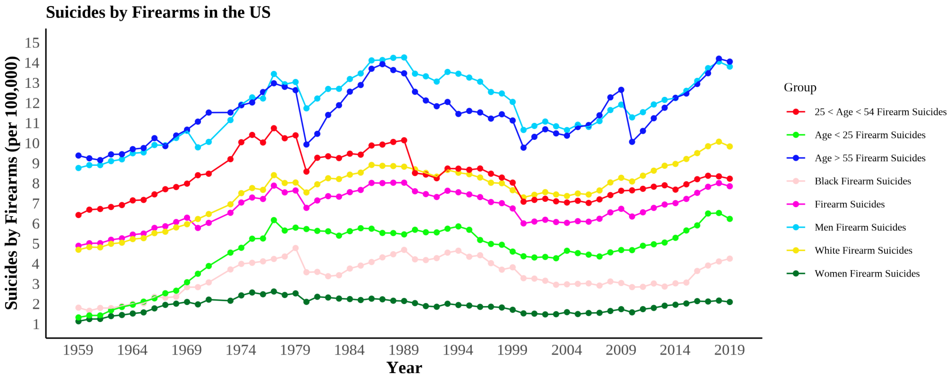
Note: This staggered adoption panel view illustrates the year of waiting period policy implementation for each state included in the study. It provides visual clarity on treatment timing across states, which is crucial for interpreting the event study estimates and understanding the source of identifying variation.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. 2. Timing of Waiting Period Policy Adoption Across States: Number of Counties



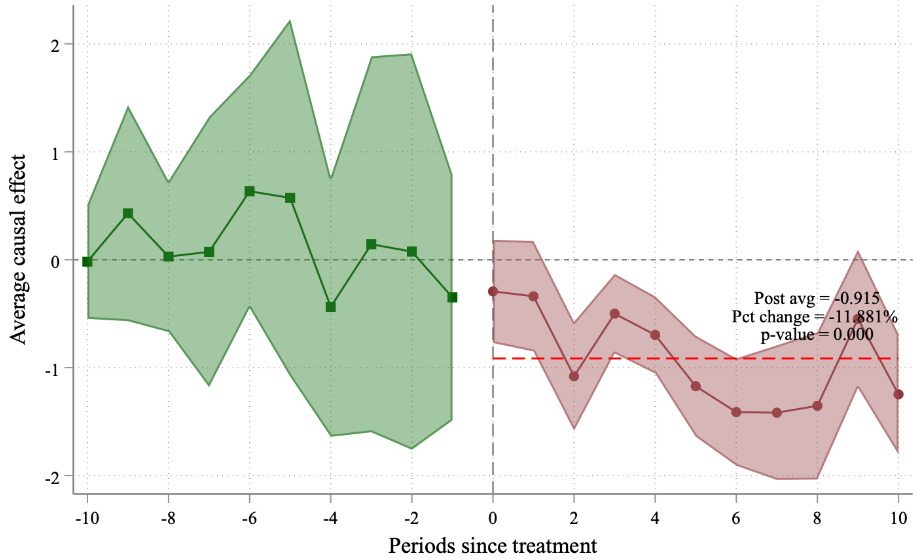
Note: This alternative view of policy adoption timing complements Figure 1. It emphasizes the distribution of treated versus control counties over time, helping to validate the use of staggered treatment timing in the empirical strategy.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. 3. Firearm Suicide Rates by Demographic Group in the US, 1959–2019



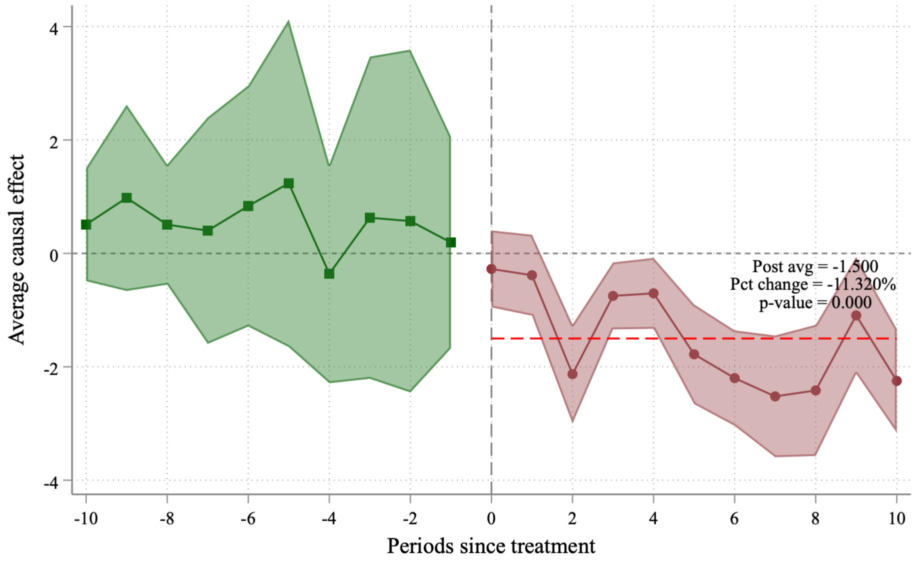
Note: This figure illustrates firearm suicide rates (per 100,000 population) across demographic categories from 1959 to 2019. The substantial differences between men and women, age groups, and racial categories highlight the importance of demographic-specific approaches to suicide prevention. The recent increases across multiple groups after 2010 suggest concerning trends that may warrant targeted intervention strategies.
Source: National Vital Statistics System (NVSS), Multiple Cause of Death Files, 1959–2019.

Fig. 4. Estimated Effect of Waiting Periods on Overall Firearm Suicide Rates



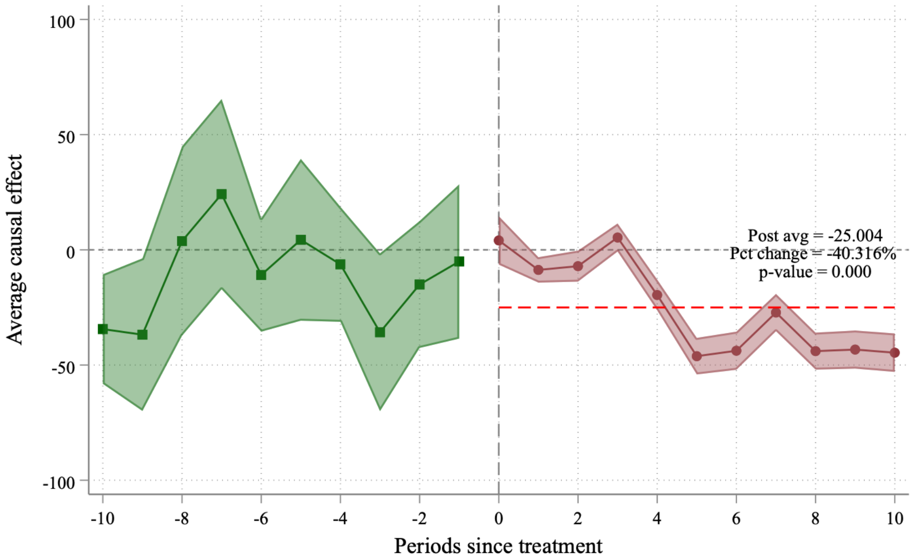
Note: This figure shows the dynamic effects of waiting period laws on firearm suicide rates across US counties. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. Standard errors are bootstrapped and clustered at the state level.

Fig. 5. Effect of Waiting Periods on Firearm Suicide Rates Among Men



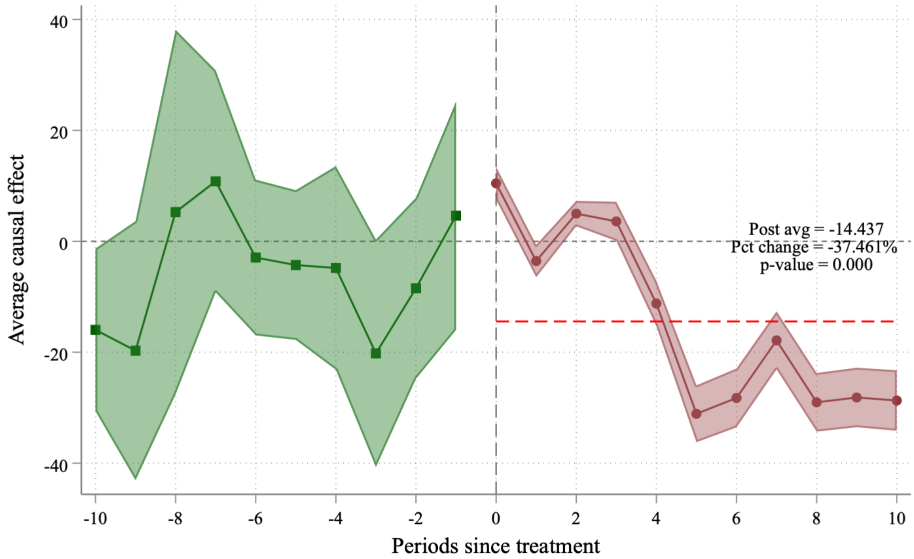
Note: This figure focuses on the male population, showing how waiting period laws affect firearm suicide rates for men specifically. Standard errors are bootstrapped and clustered at the state level.

Fig. 6. Effect of Waiting Periods on Firearm Suicide Rates Among Adults Aged 55+



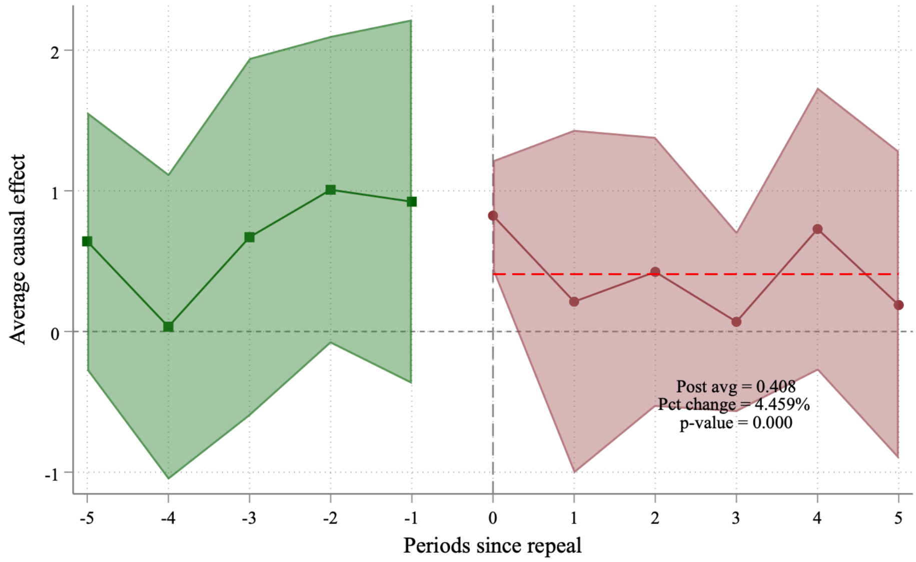
Note: This event study estimates the policy effect on older adults, a group at elevated suicide risk. Standard errors are bootstrapped and clustered at the state level.

Fig. 7. Effect of Waiting Periods on Firearm Suicide Rates Among White Individuals



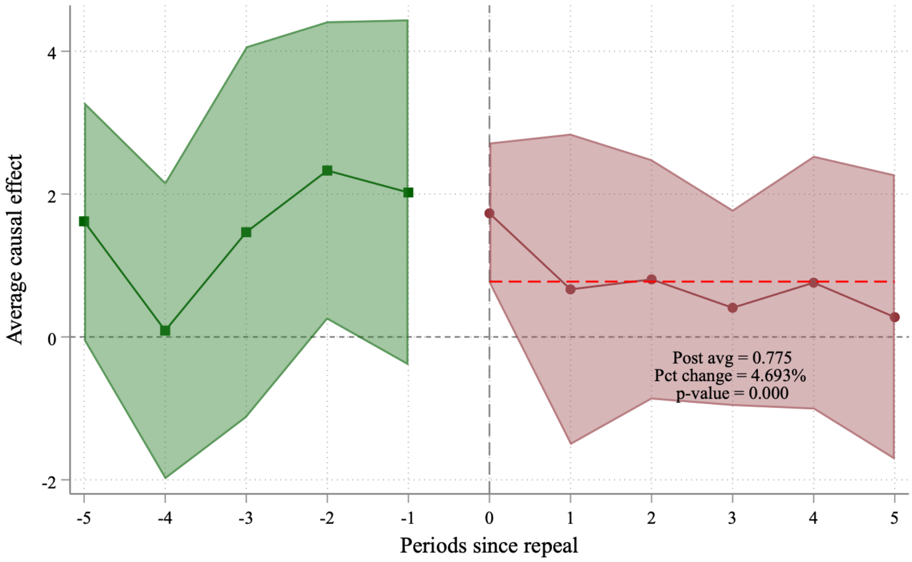
Note: This figure examines firearm suicide trends for white individuals. Standard errors are bootstrapped and clustered at the state level.

Fig. 8. Effect of Waiting Period Repeal on Overall Firearm Suicide Rates



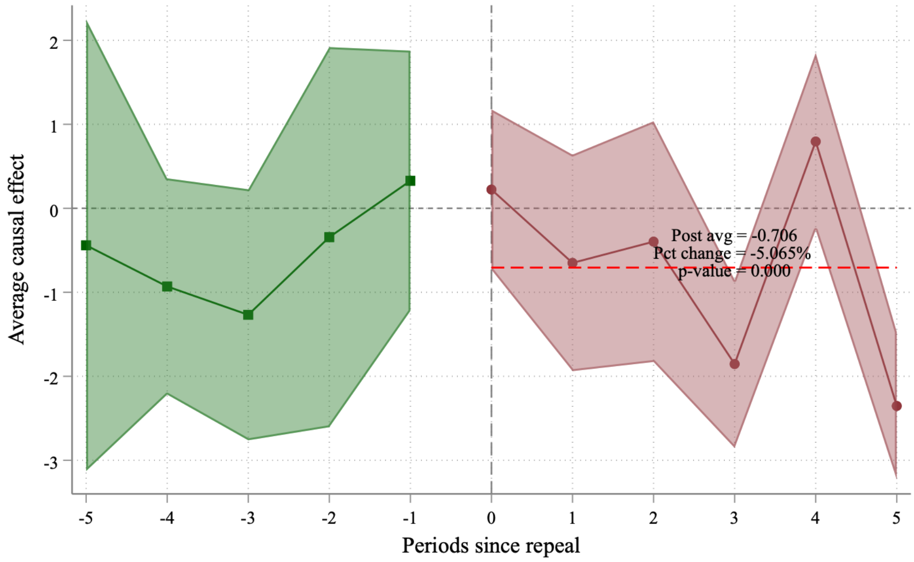
Note: This figure shows the dynamic effects of repealing waiting period laws on firearm suicide rates across US counties using a sample of states that repealed waiting periods. The sample is composed of states that experience either one policy transition from treatment to no-treatment, or always treated, yielding 14 states (8 treatment, 6 control). Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. Standard errors are bootstrapped and clustered at the state level.

Fig. 9. Effect of Waiting Period Repeal on Firearm Suicide Rates Among Men



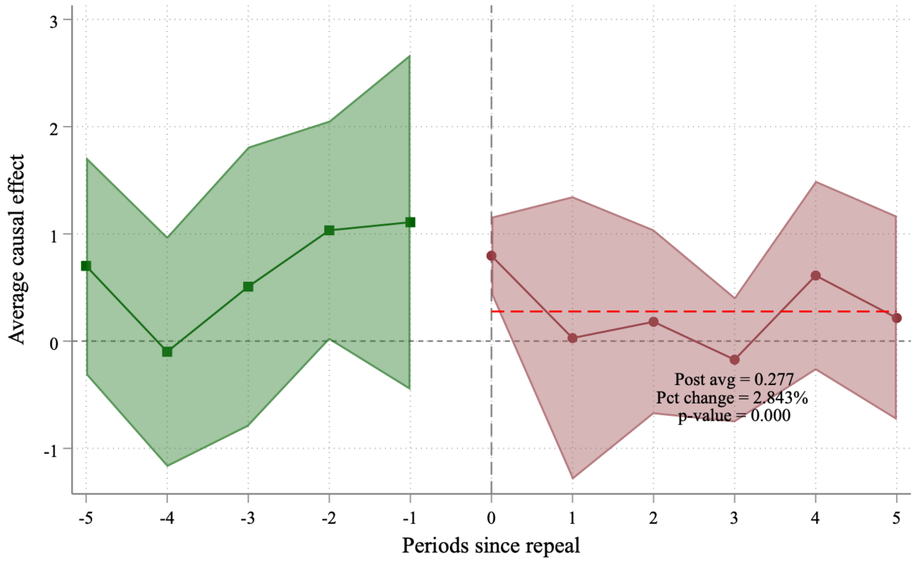
Note: This figure focuses on the male population using a sample of states that repealed waiting periods, showing how waiting period laws affect firearm suicide rates for men specifically. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

Fig. 10. Effect of Waiting Period Repeal on Firearm Suicide Rates Among Adults Aged 55+



Note: This event study estimates the policy effect on older adults using a sample of states that repealed waiting periods, focusing on a group at elevated suicide risk. The sample comprises 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

Fig. 11. Effect of Waiting Period Repeal on Firearm Suicide Rates Among White Individuals



Note: This figure examines firearm suicide trends for white individuals using a sample of states that repealed waiting periods. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

Table 1: Summary Statistics for County-Year Data

	Mean	SD	N
Total Suicide Rate (per 100k)	13.64	14.14	80,313
Firearm Suicide Rate (per 100k)	8.45	11.00	80,313
Men's Firearm Suicide Rate (per 100k)	14.86	19.92	80,313
Firearm Suicide Rate Aged 55+ (per 100k)	24.99	221.42	80,313
White Individuals' Firearm Suicide Rate (per 100k)	16.11	97.85	80,313
Non-Firearm Suicide Rate (per 100k)	5.19	7.55	80,313
Female Population Share	0.50	0.02	80,313

Notes: Mortality data come from the National Vital Statistics System (NVSS), 1959–2019; non-mortality measures are drawn from the Decennial US Census. Suicide rates are expressed per 100,000 population. Female Population Share" is a proportion. SD denotes standard deviation. N is the number of county–year observations (80,313).

Table 2: Covariate Balance: Treated vs. Control Groups

Variable	Control (N = 60,300)	Treated (N = 20,013)	Difference
Total Suicide Rate (per 100k)	13.752	13.314	-0.437 (1.453)
Firearm Suicide Rate (per 100k)	8.720	7.639	-1.081 (0.990)
Men's Firearm Suicide Rate (per 100k)	15.320	13.461	-1.859 (1.582)
Firearm Suicide Rate Aged 55+ (per 100k)	27.112	18.612	-8.500* (5.145)
White Individuals' Firearm Suicide Rate (per 100k)	17.282	12.561	-4.721 (3.517)
Non-Firearm Suicide Rate (per 100k)	5.032	5.675	0.644 (0.566)
Female Population Share	0.505	0.503	-0.002 (0.003)
College Educated Share	0.133	0.090	-0.043** (0.020)
Below Poverty Line Share	0.282	0.124	-0.157*** (0.040)

Note: Mortality data come from the National Vital Statistics System (NVSS), 1959–2019; non-mortality measures are drawn from the Decennial US Census. Suicide rates are expressed per 100,000 population. Female Population Share", College Educated", and "Below Poverty Line" are proportions. Standard errors clustered at the state level. Significance: * p<0.1, ** p<0.05, *** p<0.01.

Table 3: Delay Mechanism: Waiting Days and Firearm Suicides (TWFE)

	(1) All	(2) Men	(3) Age 55+	(4) White
Waiting days	-0.063^{*}	-0.096	-5.630^{**}	-4.123^{**}
	(0.036)	(0.060)	(2.617)	(1.879)
Baseline mean	8.45	14.86	24.99	16.11
Pct change per day (%)	-0.74	-0.64	-22.53	-25.60
N	80,313	80,313	80,313	80,313

County and year fixed effects; SEs clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ONLINE APPENDIX

The Effects of Waiting Periods on Firearm Suicides in the U.S.

Hussain Hadah, Gael Compta, and Ali Saffouri

A Tables

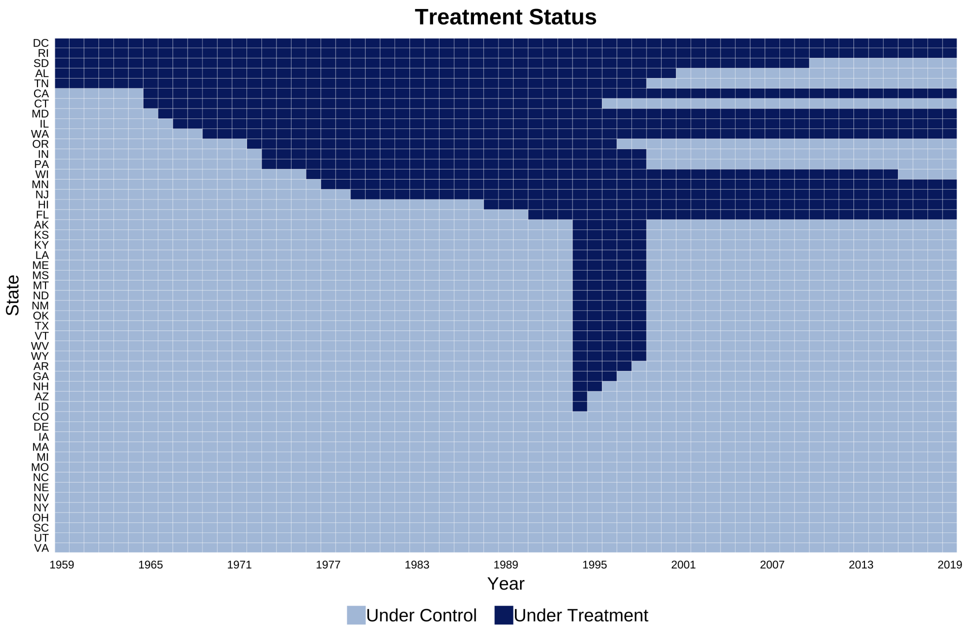
Table A.1: Treatment Cohorts by State

Treatment Cohort	State Count	Percent of States	State Abbreviations
1965 Cohort	2	3.92%	CA, CT
1966 Cohort	1	1.96%	MD
1967 Cohort	1	1.96%	IL
1969 Cohort	1	1.96%	WA
1973 Cohort	3	5.88%	IN, OR, PA
1976 Cohort	1	1.96%	WI
1977 Cohort	1	1.96%	MN
1979 Cohort	1	1.96%	NJ
1988 Cohort	1	1.96%	HI
1991 Cohort	1	1.96%	FL
1994 Cohort	19	37.25%	AK, AZ, AR, GA, ID, KS, KY, LA, ME, MS, MT, NH, NM, ND, OK, TX, VT, WV, WY
Always Treated	5	9.8%	AL, DC, RI, SD, TN
Never Treated	14	27.45%	CO, DE, IA, MA, MI, MO, NE, NV, NY, NC, OH, SC, UT, VA
Total	51	100%	

Notes: Data from RAND State Firearm Law Database. Always treated are the states that had a waiting period law in place before at 1959, the first year of vital statistics data that is available. They include states that might have moved to a less restrictive waiting periods or abolished them.

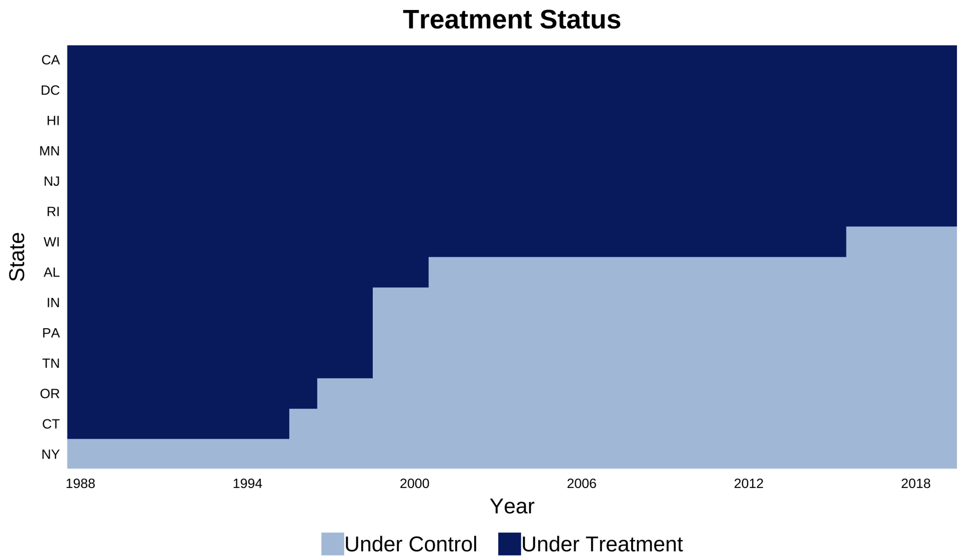
B Figures

Fig. A.1. Timing of Waiting Period Policy Adoption Across All States



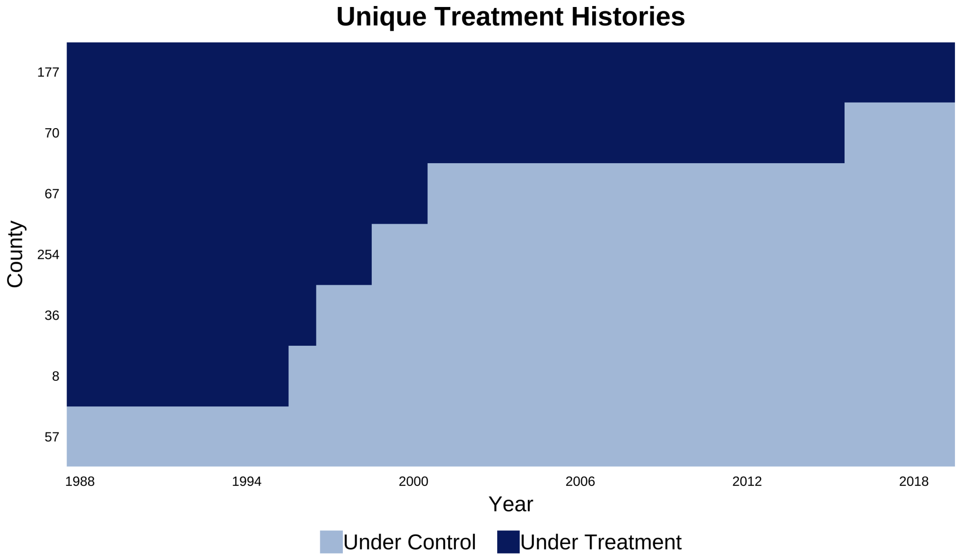
Note: This staggered adoption panel view illustrates the year of waiting period policy implementation or exit for each state. It provides visual clarity on treatment timing across states, which is crucial for interpreting the event study estimates and understanding the source of identifying variation.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. A.2. Timing of Waiting Period Policy Adoption Across States That Moved Out of Treatment



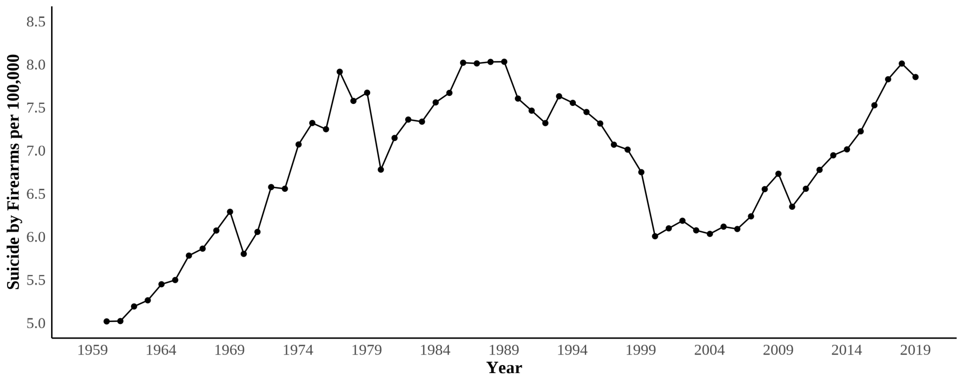
Note: This staggered adoption panel view illustrates the year of waiting period policy implementation for each state included in the study. It provides visual clarity on treatment timing across states, which is crucial for interpreting the event study estimates and understanding the source of identifying variation.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. A.3. Timing of Waiting Period Policy Adoption Across States: Number of Counties

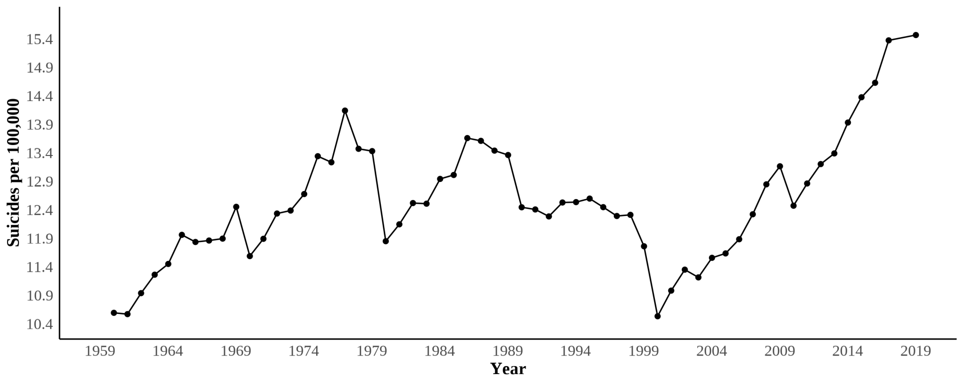


Note: This alternative view of policy adoption timing complements Figure A.2. It emphasizes the distribution of treated versus control counties over time, helping to validate the use of staggered treatment timing in the empirical strategy.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. A.4. Trends in Suicide Rates Across Counties, 1959–2019



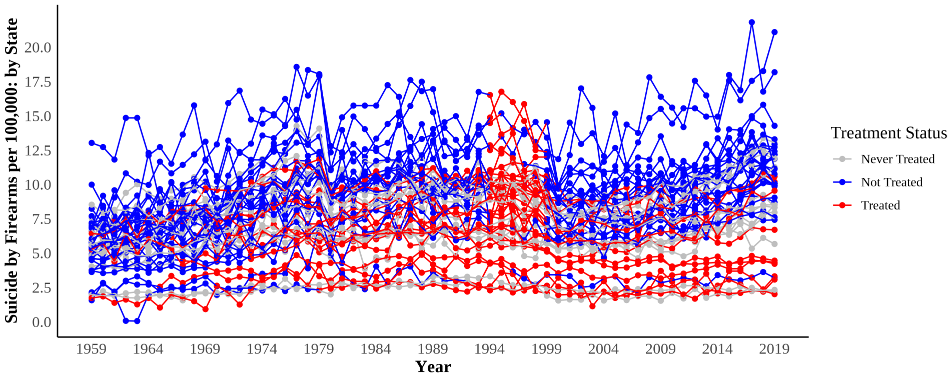
(a) Firearm Suicide Rates



(b) Overall Suicide Rates

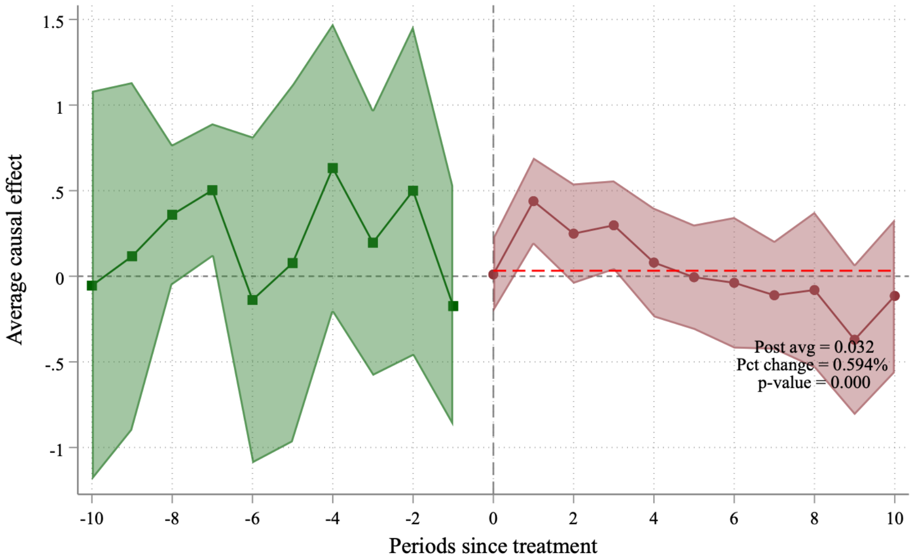
Note: These figures show annual trends in suicide rates (per 100,000 population) across US counties from 1959 to 2019. Panel (a) highlights firearm-specific suicides, while Panel (b) offers a comparison benchmark of overall suicide rates to evaluate potential confounding trends.
Source: National Vital Statistics System (NVSS), Multiple Cause of Death Files, 1959–2019.

Fig. A.5. Firearm Suicide Rates by State Treatment Status, 1959–2019



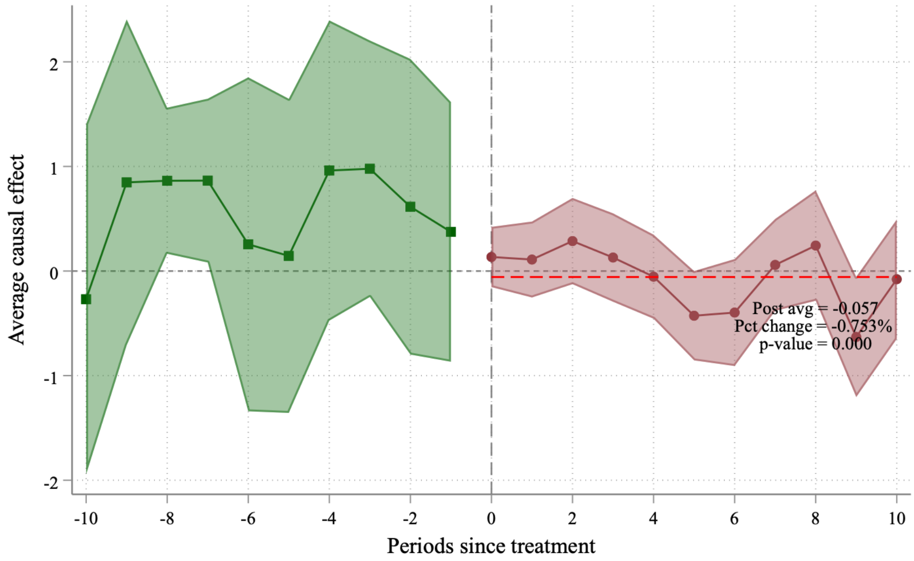
Note: This figure displays firearm suicide rates (per 100,000 population) from 1959 to 2019 across states, categorized by treatment status. "Treated" states implemented waiting period policies, "Not Treated" states never adopted such policies during the study period, and "Never Treated" states represent the control group. The consistently lower rates in treated states suggest a potential protective effect of waiting period legislation on firearm suicide mortality.
Source: National Vital Statistics System (NVSS), Multiple Cause of Death Files, 1959–2019.

Fig. A.6. Effect of Waiting Periods on Non-Firearm Suicide Rates



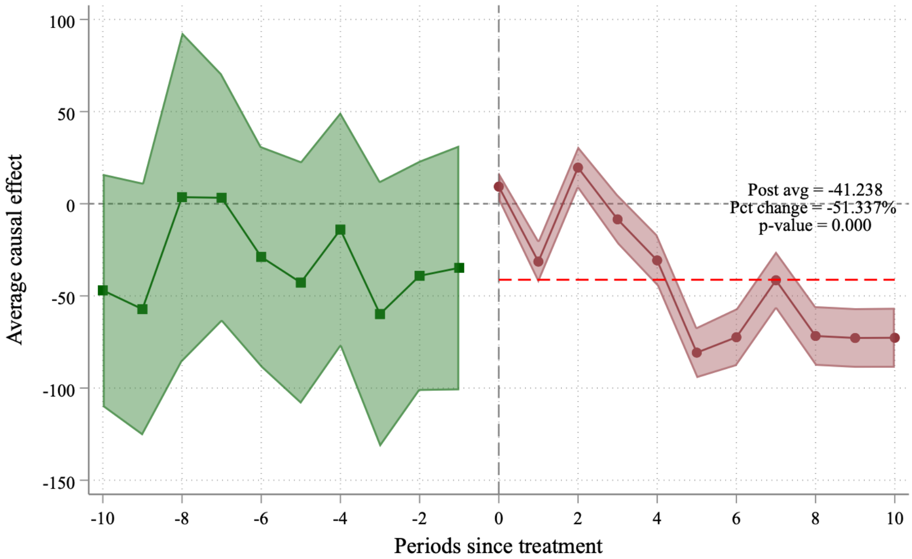
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The absence of significant differences in pre-treatment periods supports the parallel trends assumption. Standard errors are bootstrapped and clustered at the state level.

Fig. A.7. Effect of Waiting Periods on Non-Firearm Suicide Rates Among Men



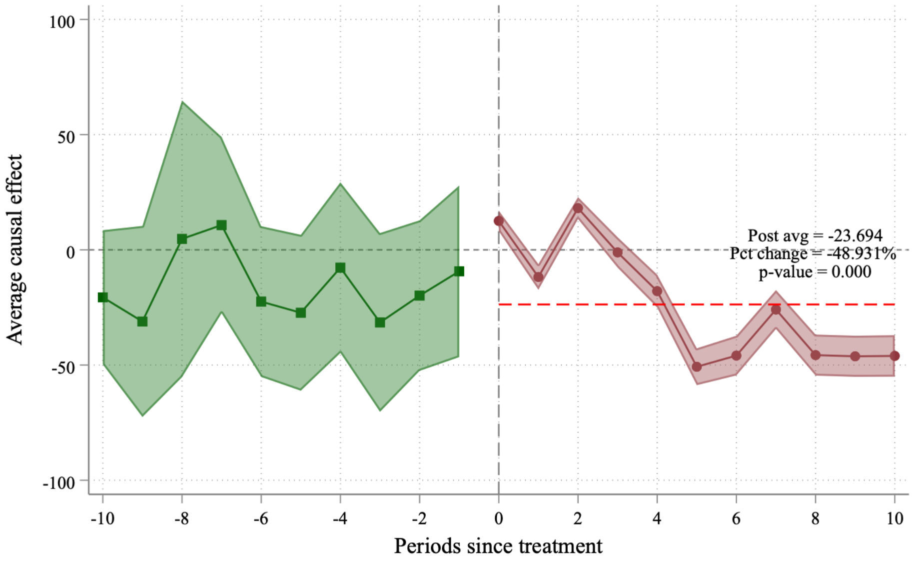
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among men. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The absence of significant differences in pre-treatment periods supports the parallel trends assumption. Standard errors are bootstrapped and clustered at the state level.

Fig. A.8. Effect of Waiting Periods on Non-Firearm Suicide Rates Among Adults Aged 55+



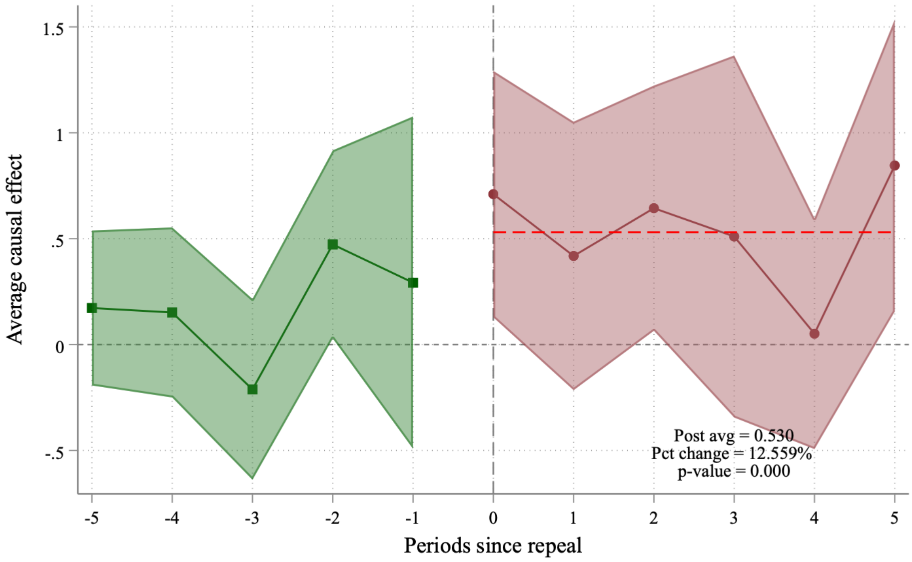
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among adults aged 55+. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The absence of significant differences in pre-treatment periods supports the parallel trends assumption. Standard errors are bootstrapped and clustered at the state level.

Fig. A.9. Effect of Waiting Periods on Non-Firearm Suicide Rates Among White Individuals



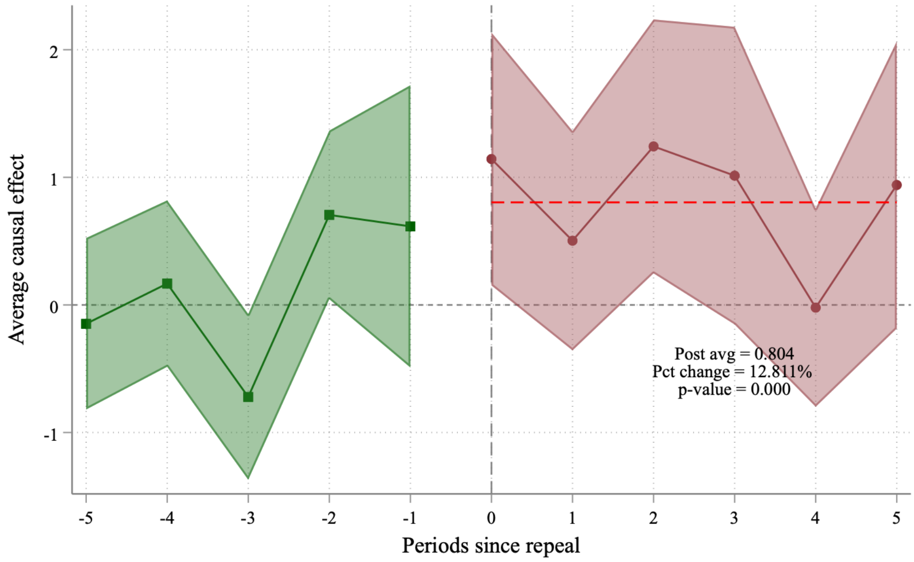
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among White individuals. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The absence of significant differences in pre-treatment periods supports the parallel trends assumption. Standard errors are bootstrapped and clustered at the state level.

Fig. A.10. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates



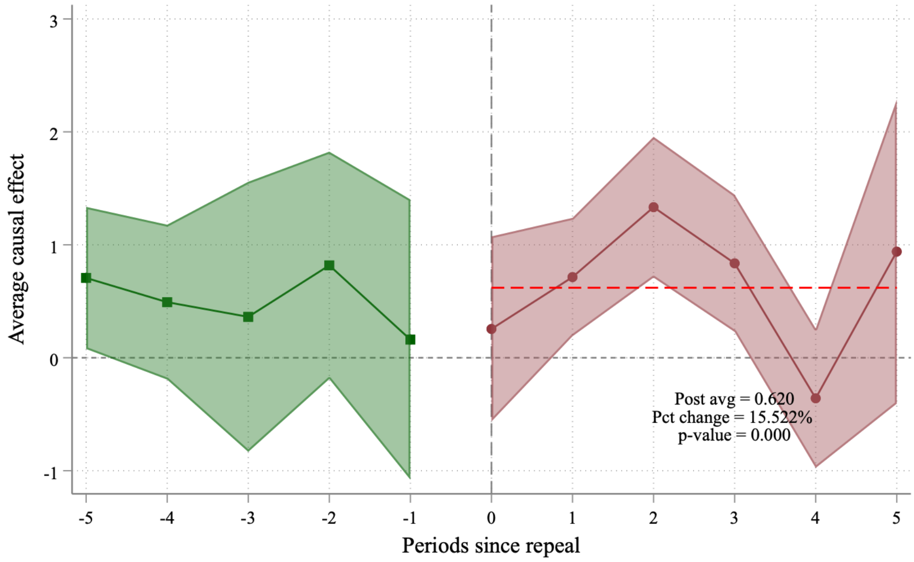
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties using a sample of states that repealed waiting periods. The sample comprises 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

Fig. A.11. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates Among Men



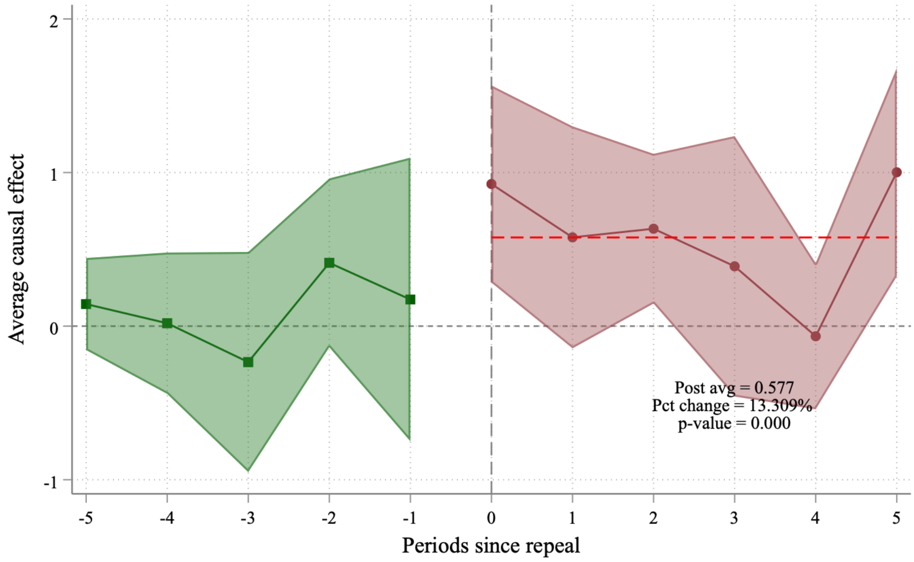
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among men using a sample of states that repealed waiting periods. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

Fig. A.12. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates Among Adults Aged 55+



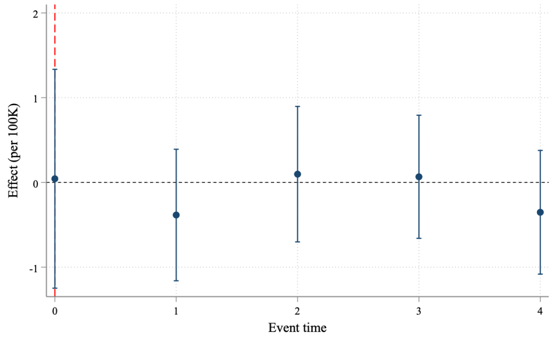
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among adults aged 55+ using a sample of states that repealed waiting periods. The sample comprises 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

Fig. A.13. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates Among White Individuals

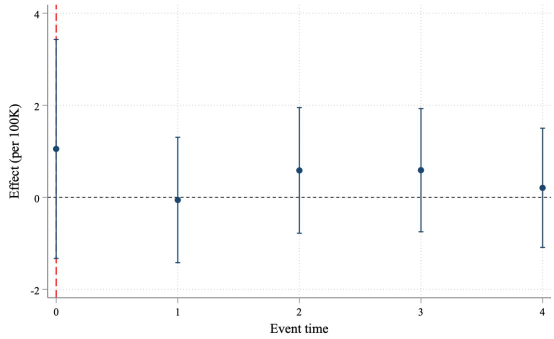


This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among White individuals using a sample of states that repealed waiting periods. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped and clustered at the state level.

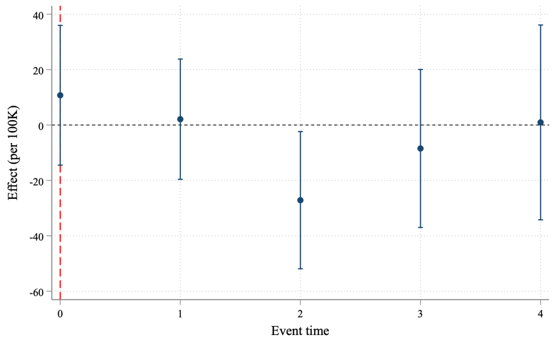
Fig. A.14. Placebo Tests: Effect of Waiting Period Repeal on Firearm Suicide Rates (5-Year Lead)



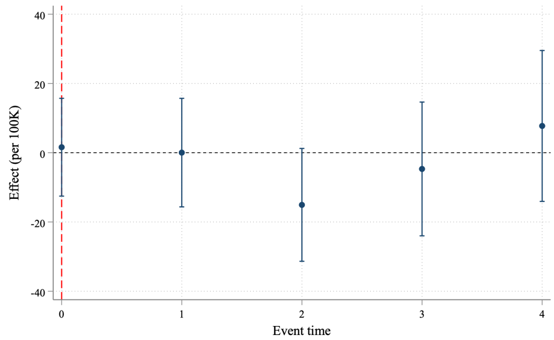
(a) All Individuals



(b) Men



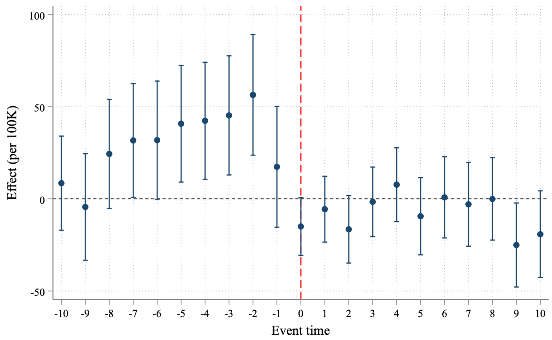
(c) Adults Aged 55+



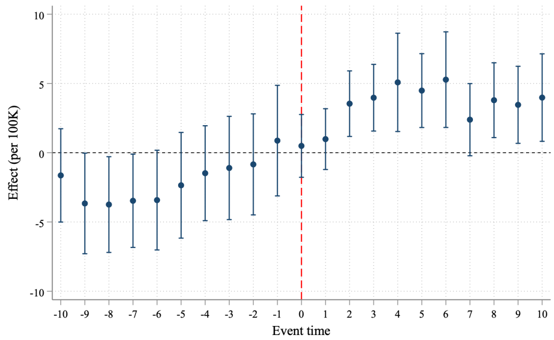
(d) White Individuals

Note: These figures present placebo tests by shifting the treatment date 5 years earlier. If the parallel trends assumption holds, we should observe no significant effects during this placebo treatment window (event time 0–4), which corresponds to 5–1 years before the actual policy change. Panel (a) shows results for all individuals, panel (b) for men, panel (c) for adults aged 55 and older, and panel (d) for white individuals. Standard errors are clustered at the county level.

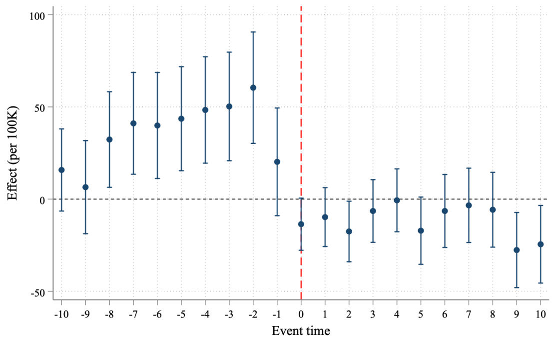
Fig. A.15. Effect of Waiting Period Repeal on Non-Suicide Mortality Rates



(a) All Individuals



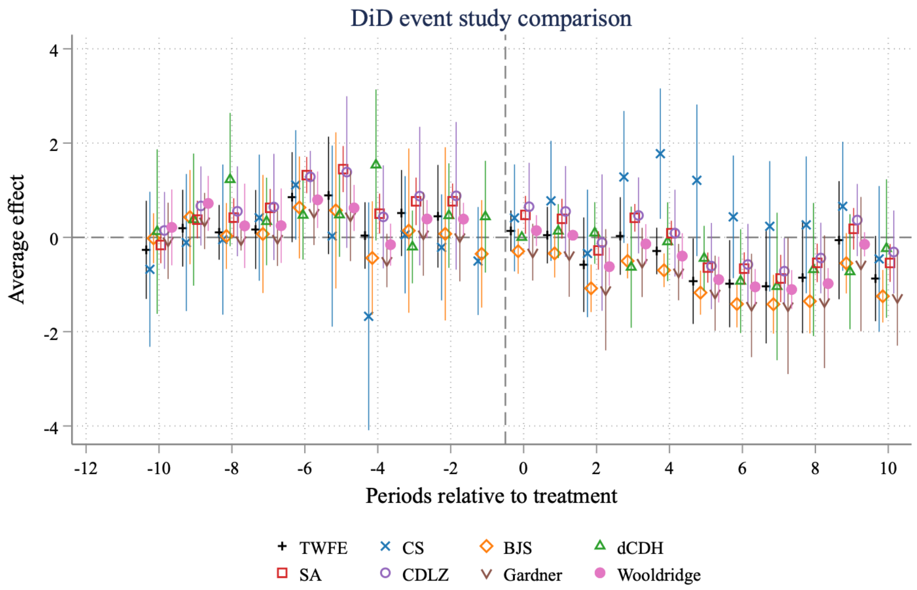
(b) Adults Aged 25–54



(c) Adults Aged 55+

Note: These figures present negative control tests using non-suicide mortality rates per 100,000 population as the outcome. Waiting period laws should not affect deaths unrelated to suicide. The pre-treatment coefficients (event time -10 to -1) test for parallel trends, while the post-treatment coefficients (event time 0 to 10) test for spurious effects. Panel (a) shows results for all individuals, panel (b) for adults aged 25–54, and panel (c) for adults aged 55 and older. Standard errors are clustered at the county level.

Fig. A.16. Effect of Waiting Periods on Firearm Suicide Rates: Different Types of Estimators



This figure compares treatment effect estimates from multiple difference-in-differences estimators for the effect of waiting periods on firearm suicides. Different estimators include Two-way fixed effects (TWFE), Callaway and Sant'Anna (2021) (CS), Borusyak, Jaravel, and Spiess (2024) (BJS), De Chaisemartin and d'Haultfoeuille (2023) (dCDH), Sun and Abraham (2021) (SA), Cengiz et al. (2019) (CDLZ), Gardner (2022), and Wooldridge (2025) to assess sensitivity of results to methodological choices in staggered adoption designs.

C Data

C.1 Harmonizing County Identifiers Across Decades

The NCHS Multiple Cause-of-Death (MCOD) micro-data span 1959–2019 and record the county of occurrence under several coding conventions. Prior to 1982, the files use one of three distinct non-FIPS coding regimes, none of which is directly comparable to the Federal Information Processing Standard (FIPS) codes used in the Census Bureau population files. From 1982 onward, FIPS codes are provided in the data. A separate issue affects the entire 1959–2019 span: the underlying county geography itself is not static. Counties have split, merged, been renamed, or reclassified as independent cities, so a given county identifier, legacy code or FIPS, does not necessarily refer to the same territorial unit from one year to the next. Simply concatenating the annual files would therefore mix observations from different geographies.

To produce a consistent panel of county $\times$ year $\times$ demographic cells, we therefore construct a single master crosswalk that performs two harmonizations simultaneously: (i) a translation of the pre-1982 coding schemes into FIPS, and (ii) a boundary harmonization that maps each historical county—whether originally identified by a legacy code or by a contemporaneous FIPS code—onto a time-invariant set of 2019 geographies.

C.1.1 Harmonizing territorial change

Because the target geography is, by construction, the successor of every historical county, the same dictionary that translates codes also performs boundary harmonization. Counties that no longer exist in the 2019 geography are redirected to the FIPS code of the unit that absorbed them; counties that did not yet exist at the start of the sample are redirected to their antecedent. The most quantitatively important cases are:

- **Virginia independent-city consolidations.** Virginia independent cities that either split from a county and later rejoined it, or that were too small to support stable population denominators, are collapsed back into their parent county. This affects: Bedford City → Bedford County (51019) after its 1968 split; Clifton Forge City → Alleghany County (51005) in 2001; Emporia City → Greenville County (51081) in 1967; Fairfax City → Fairfax County (51059) in 1961; Franklin City → Southampton County (51175) in 1961; Lexington City → Rockbridge County (51163) in 1966; Manassas and Manassas Park Cities → Prince William County (51153) in 1975; Nansemond County → Suffolk City (51800) in 1974; Norfolk County and South Norfolk City → Chesapeake City (51550) in 1963; Poquoson City → York County (51199) in 1975; Princess Anne County → Virginia Beach City (51810) in 1963; Salem City → Roanoke County (51161) in 1968; and South Boston City → Halifax County (51083) after its 1995 reconsolidation.
- **Arizona.** La Paz County (04012), which split off from Yuma County in 1983, is merged back into Yuma (04027) for the entire sample.
- **Florida.** Dade County (12025) is recoded to its post-1997 identifier Miami-Dade (12086).
- **Maryland.** Because Baltimore City (24510) and Baltimore County (24005) are incompletely disaggregated in the 1966 and 1967 mortality files, we combine them throughout.
- **Nevada.** Ormsby County (32025) is redirected to Carson City (32510) for the pre-1969 period.
- **New Mexico.** Cibola County (35006), which was carved out of Valencia County in 1981, is folded back into Valencia (35061).

- **South Dakota.** Washabaugh County (46131), annexed to Jackson County in 1979, is redirected to Jackson (46071) for the full sample.
- **Wisconsin.** Menominee County (55078), created in 1961 out of Shawano (55115) and Oconto (55083), is present throughout by mapping its parent counties onto 55078 for the pre-1961 years.
- **Yellowstone National Park.** The pre-1997 portions of Yellowstone NP that appear as standalone counties in Idaho, Montana, and Wyoming carry no Census population and are dropped; their deaths are negligible.

C.2 Harmonizing Cause-of-Death Codes Across ICD Revisions

The NCHS underlying cause of death is coded under four successive revisions of the International Classification of Diseases during our sample window:

Table A.2: ICD revisions used in the NCHS mortality files.

Years	Revision
1959–1967	ICD-7
1968–1978	ICDA-8 (Eighth Revision, Adapted)
1979–1998	ICD-9
1999–2019	ICD-10

The external-cause E-codes that identify suicide are renumbered and repartitioned at each transition, so we construct a single set of mutually exclusive categories—firearm suicide, other suicide, and non-suicide—that is stable across all four regimes. Every death record is assigned exactly one of these three categories, and we verify that the classification is non-missing for every record.

C.2.1 Firearm-suicide definition

We define a firearm suicide as any underlying cause that identifies an intentional self-inflicted injury by firearm or explosive. The code ranges are:

In ICD-9 and ICD-10, the sub-codes distinguish the type of firearm or explosive: handgun (E955.0 / X72), rifle-shotgun-larger firearm (E955.1–E955.3 / X73), other and unspecified firearm (E955.4 / X74), explosive material (E955.5 / X75), and other or unspecified firearms and explosives (E955.9 / X76–X77). This sub-classification is unavailable in ICD-7 and ICDA-8, which record only "firearms and explosives" as a single category. To preserve a consistent aggregate across the full sample, our baseline firearm-suicide

Table A.3: Firearm-suicide codes by ICD revision.

Revision	Years	Firearm-suicide codes
ICD-7	1959–1967	E976
ICDA-8	1968–1978	E955
ICD-9	1979–1998	E955.0–E955.9
ICD-10	1999–2019	X72–X77

category pools all firearm and explosive sub-codes within each revision; a finer classification that retains the handgun, long gun, other firearm, and explosive sub-categories is available for the post-1979 period only.

We follow the NCHS convention of ignoring the fourth digit of the ICD-7 E-code: for 1959–1967, the sub-divisions below E976 are not documented in a way that separates firearm from explosive, and so the full E976 block is treated as firearm suicide.

C.2.2 Other-suicide and non-suicide definitions

The remaining intentional self-inflicted causes are grouped into an "other suicide" category, and all deaths outside the suicide block are classified as "non-suicide." The mapping is:

Table A.4: Crosswalk of suicide cause-of-death codes across ICD revisions.

Mechanism	ICD-7 (1959–1967)	ICDA-8 (1968–1978)	ICD-9 (1979–1998)	ICD-10 (1999–2019)
Poisoning — solid/liquid substances	E970–E971	E950	E950	X60–X65
Poisoning — gases	E972–E973	E951–E952	E951–E952	X66–X69
Hanging, strangulation, suffocation	E974	E953	E953	X70
Submersion (drowning)	E975	E954	E954	X71
Firearms and explosives	**E976**	**E955**	**E955**	**X72–X77**
Cutting and piercing instruments	E977	E956	E956	X78–X79
Jumping from high place	E978	E957	E957	X80
Other and unspecified means	E979	E958–E959	E958–E959	X81–X84, Y87.0

The inclusion of Y87.0 ("sequelae of intentional self-harm") in the ICD-10 range follows NCHS practice for maintaining comparability with earlier revisions, which did not distinguish acute from sequelae deaths. Any record whose underlying cause falls outside the suicide block is assigned to non-suicide, so that the three categories sum to total deaths by construction.