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

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Abstract

Suicide is often an impulsive act, and in the United States, nearly one-half of suicides involve a firearm, the most lethal and readily available method. In this paper, we use recent developments in difference-in-differences methodology to study the causal effect of waiting-period laws on firearm suicides. We find that waiting periods reduce firearm suicides among men by 1.5 deaths per 100,000 population, an 11% decrease from baseline. For white individuals, we observe a statistically significant reduction of 37.5 deaths per 100,000, a 37% decrease. For adults aged 55 and older, we find a reduction of 25 deaths per 100,000, representing a 40% decrease. For the overall population, we find a statistically significant reduction of 0.92 deaths per 100,000, a 12% decrease from baseline. Crucially, we find no evidence of substitution toward non-firearm suicide methods following waiting period adoption; among men, adults 55 and older, and white individuals, we find significant decreases in non-firearm suicides. We also examine the effects of waiting period repeal and find statistically significant increases in firearm suicides. Back-of-the-envelope calculations suggest waiting periods prevented approximately 3,000 firearm suicide deaths annually, yielding social benefits of roughly $41 billion. Our findings show that even brief delays in firearm access can disrupt the pathway from suicidal ideation to death, suggesting that cooling-off periods may be an important policy tool for suicide prevention. **JEL**: 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, giving individuals experiencing suicidal ideation time for the crisis to pass before they can access a firearm. Therefore, it is important to causally identify whether waiting periods are effective in preventing firearm 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). The results support the idea that the implementation of policy measures aimed at impulsive moments, such as waiting periods for firearm 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 firearm 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 37.5 deaths per 100,000 (37% decrease). These groups account for the majority of firearm suicides in the United States. Crucially, we detect no significant substitution 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, though they may reflect cohort-specific differences in the populations affected by waiting period policies over time.

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 that 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 substitution toward other suicide methods; instead, non-firearm suicides also decline in key groups, consistent with waiting periods saving lives rather than merely shifting 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]

While many studies examine gun control policies and suicide rates, few identify causal effects. The empirical literature notably lacks causal analyses of US waiting-period laws on firearm suicides. The existing literature is generally limited to individual states, short periods following legislative actions, or relies on broad national metrics, thereby complicating efforts to disentangle causal estimates. To our knowledge, this will consequently be the first paper aiming to causally estimate the effect of waiting periods on suicides.

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). 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 firearm suicide (Baker and McPhedran 2007).[2] In the United States, the laws on firearm removal based on risk ('red flag') enacted 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 firearm injuries, both fatal and nonfatal, has been substantial. Injuries are estimated to cost the healthcare system and the economy, through lost productivity, billions of dollars (Miller et al. 2024).

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

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

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 firearm storage in reducing the risk of suicide, particularly in households with adolescents. However, firearm 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 firearm law dataset for waiting periods.

Firearm 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 firearm suicide rate, defined as the number of firearm suicides per 100,000 population in each county and year. We specifically use the Multiple Cause of Death files, which provide detailed causes of death for each death recorded in the US at the county level using ICD-10 codes. The ICD-10 codes allow for the identification of specific causes of death, including suicide. Suicides are further broken down into several categories, allowing for a more detailed analysis of different types of suicide, including those involving firearms.[3] In addition, the data

[3] The ICD-10 codes used to defined underlying causes of death due to suicide were X60-X84 (intentional self-harm), and Y87.0 (Sequelae of intentional self-harm). X60 to X69 correspond to intentional self-poisoning, while X70 to X84 correspond to intentional self-harm by other and unspecified means, including drowning,

include a range of socioeconomic characteristics of the deceased, such as age, sex, race, marital status, and education level. For each death, we also have information on 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.

State firearm 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 purchase and delivery of a firearm.

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 firearm purchases. These states include Colorado, Delaware, Iowa, Massachusetts, Michigan, Missouri, Nebraska, Nevada, New York, North Carolina, Ohio, South Carolina, Utah, and Virginia. 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 the 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 3 and a map of all states with different treatment sequences in Figure 1.

hanging, strangulation, and suffocation, smoke, sharp object, etc. Suicide by firearms was categorized using three specific 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).

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. In terms of counties, the final sample is composed of 1,306 counties. A total of 932 counties as part of the 'never-takers', six counties adopted a waiting period prior to 1959, and 374 counties 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 2 and 3. We show the staggered adoption of waiting periods in Figure 2, while we present the corresponding county count in Figure 3. 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 4 and 5 respectively.

Outcome variable

The main outcome variable is the suicide rate by firearm per 100,000 people. We identify suicides related to firearms using ICD-10 codes for the underlying causes of death.[4]

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 firearm in a given county and year. You can find trends in firearm and non-firearm suicide rates from 1959 to 2019 in Figures 6 and 7. There is a substantial heterogeneity in firearm suicide rates by treatment status (Figure 8), with consistently lower rates observed in states that implemented waiting period policies, as well as marked demographic disparities (Figure 9), with particularly elevated rates among men and people over

[4] 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).

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, men, older adults (age 55 and older) and white individual firearm suicide than counties that never adopted a mandatory waiting period. In addition, counties that adopted a mandatory waiting period tend to have a similar share of the female population, college educated, and tend to have a lower proportion of their population living below the poverty line. 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.

3 Empirical Strategy

In this paper, we estimate the dynamic effects of firearm 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 firearm 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 firearm 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, firearm 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 $1,\ldots,k$ 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).

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 10. 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 firearm two years following the adoption of waiting periods. Specifically, the average treatment effect (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 11 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 suicide. The mechanism likely operates through disrupting impulsive suicide attempts, which research suggests are more common among men who use firearms.

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 12. 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 statistically significant 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. 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 13. The pretreatment estimates support the assumption of parallel trends. We find a statistically significant reduction in firearm suicides among white individuals by 37.5 deaths per 100,000, which represents a substantial 37% decrease in the suicide rate by firearm 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. The magnitude of the reduction suggests that waiting periods may be particularly effective in communities with higher baseline firearm ownership rates and greater cultural acceptance of firearm use. This finding provides strong evidence that mandatory waiting periods can significantly reduce firearm suicides in high-risk populations.

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 14–17. Once again, the pretreatment estimates support the assumption of parallel trends. We find that adopting waiting periods is associated with an increase of 0.03 deaths per 100,000, equal to a 0.6% increase in non-firearm suicides, suggesting limited substitution towards other suicide methods. Among men, adults 55 years of age and older, and white individuals, we find significant *decreases* in non-firearm suicides. The absence of substantial substitution effects is crucial for interpreting the policy impact of waiting periods. If individuals simply switched to other methods, the net public health benefit would be diminished. Our results suggest that waiting periods reduce overall suicide mortality rather than merely redirecting individuals toward alternative methods, supporting the hypothesis that many firearm suicide attempts are impulsive and that introducing a delay can prevent deaths.

The Effects of Waiting Period Repeal on Firearm Suicide Rates

Overall effect of repeal on firearm suicides. We present the results in Figure 18, 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 firearm 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 19, 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 firearm 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 20, 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 firearm 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 21, 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 firearm 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 22–25. 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 26), 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 27). 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.

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 28. 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. 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.[5]

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 4 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 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 firearm access,

[5] 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).

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 provides the most extensive evidence to date that waiting periods for firearm 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 (37.5 deaths per 100,000, 37% reduction)—the groups that account for the majority of firearm suicides. Crucially, we detect no significant substitution toward non-firearm suicides; 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 firearm 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.

Future research should extend this work by examining heterogeneity in effects across urban and rural contexts, racial and ethnic groups, and by considering potential interactions with complementary interventions such as extreme risk protection orders and safe storage laws. 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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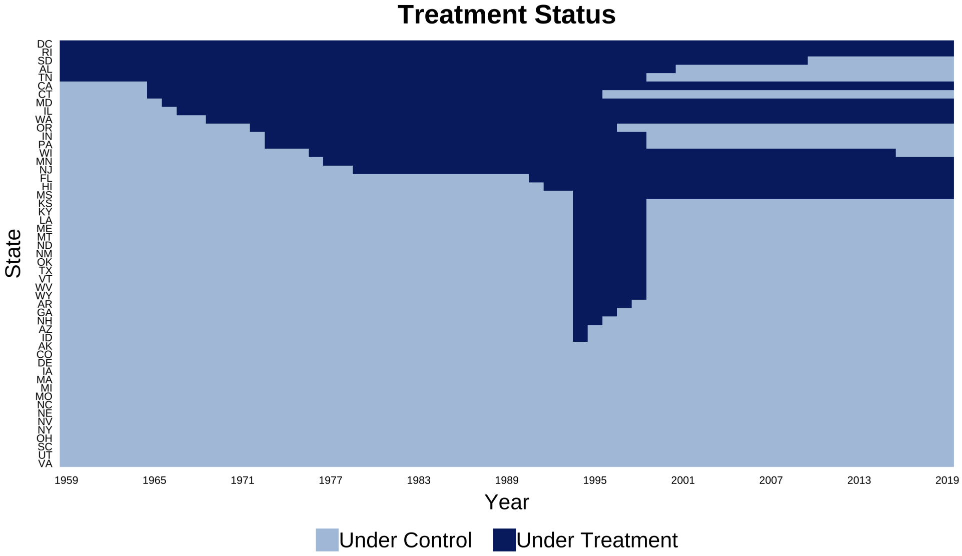
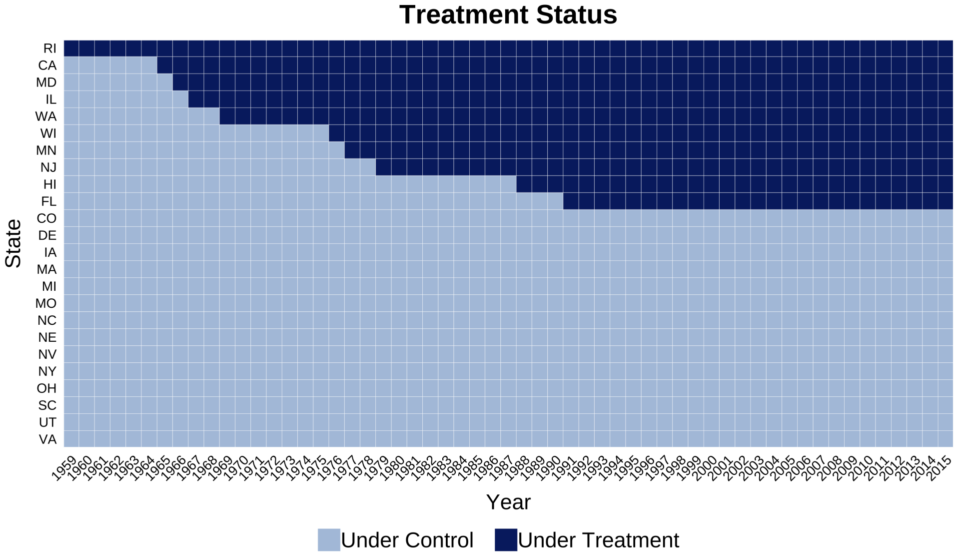
Fig. 1. Timing of Waiting Period Policy Adoption Across All States



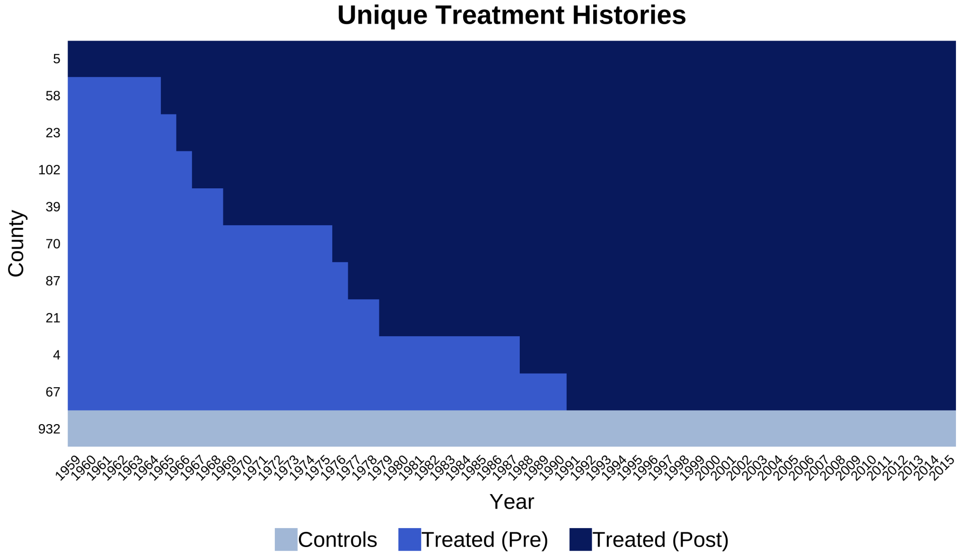
Note: This staggered adoption panel view illustrates the year of waiting period policy implementation or exit for each state. It provides visual clarity on treatment timing across states, which is crucial for interpreting the event study estimates and understanding the source of identifying variation.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

Fig. 2. Timing of Waiting Period Policy Adoption Across States



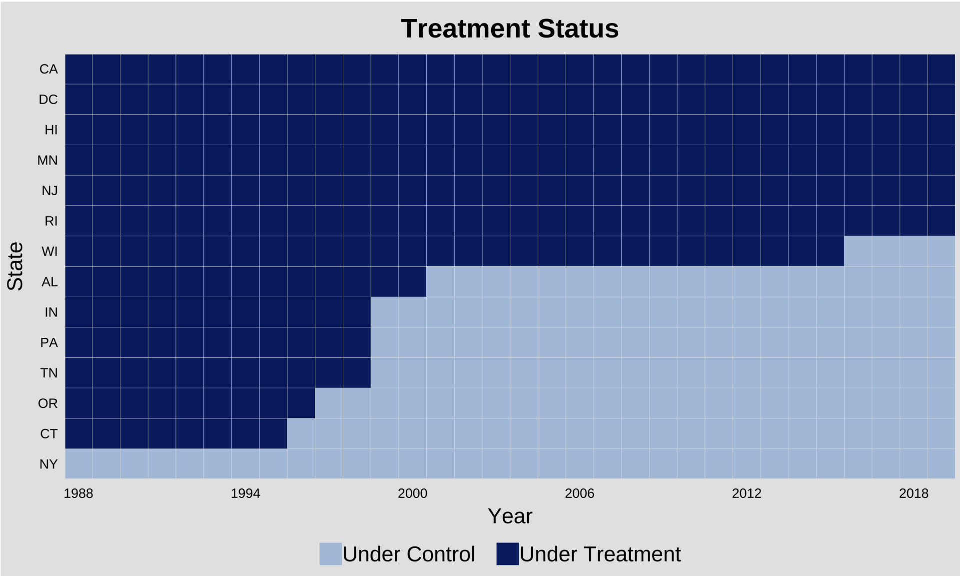
Note: This staggered adoption panel view illustrates the year of waiting period policy implementation for each state included in the study. It provides visual clarity on treatment timing across states, which is crucial for interpreting the event study estimates and understanding the source of identifying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

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



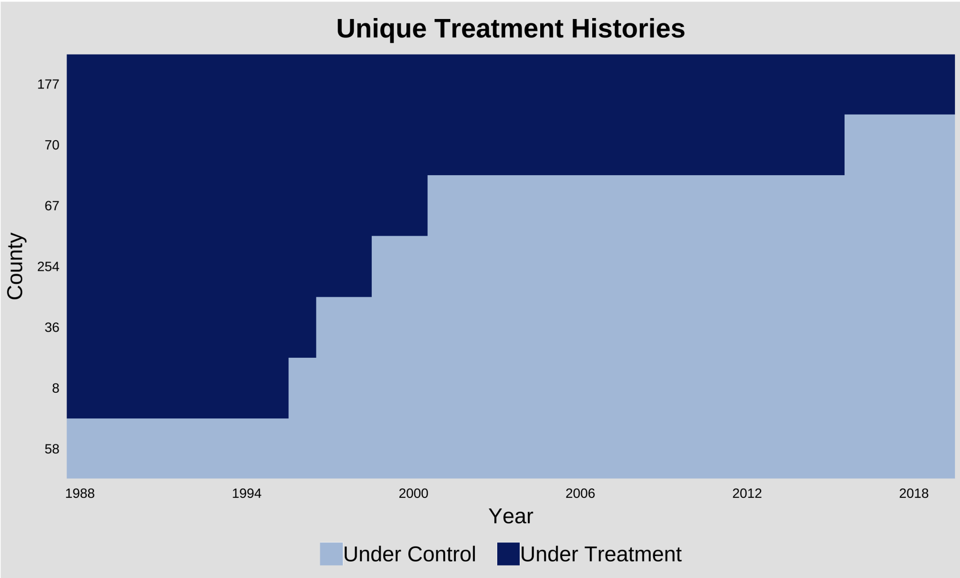
Note: This alternative view of policy adoption timing complements Figure 2. It emphasizes the distribution of treated versus control counties over time, helping to validate the use of staggered treatment timing in the empirical strategy.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. 4. Timing of Waiting Period Policy Adoption Across States That Moved Out of Treatment



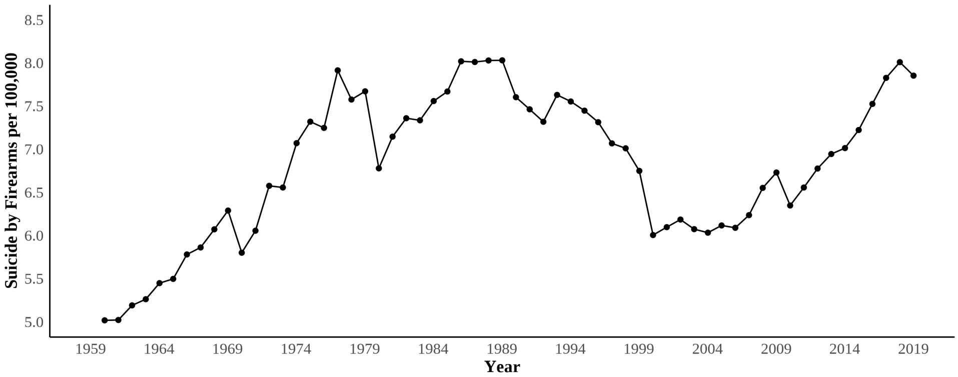
Note: This staggered adoption panel view illustrates the year of waiting period policy implementation for each state included in the study. It provides visual clarity on treatment timing across states, which is crucial for interpreting the event study estimates and understanding the source of identifying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

Fig. 5. Timing of Waiting Period Policy Adoption Across States: Number of Counties



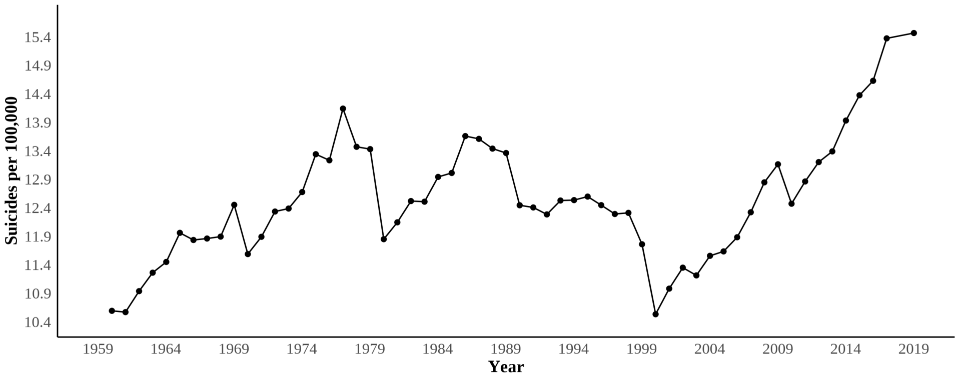
Note: This alternative view of policy adoption timing complements Figure 4. It emphasizes the distribution of treated versus control counties over time, helping to validate the use of staggered treatment timing in the empirical strategy.
Source: RAND State Firearm Law Database, 1813–2015.

Fig. 6. Trends in Firearm Suicide Rates Across Counties, 1959–2019



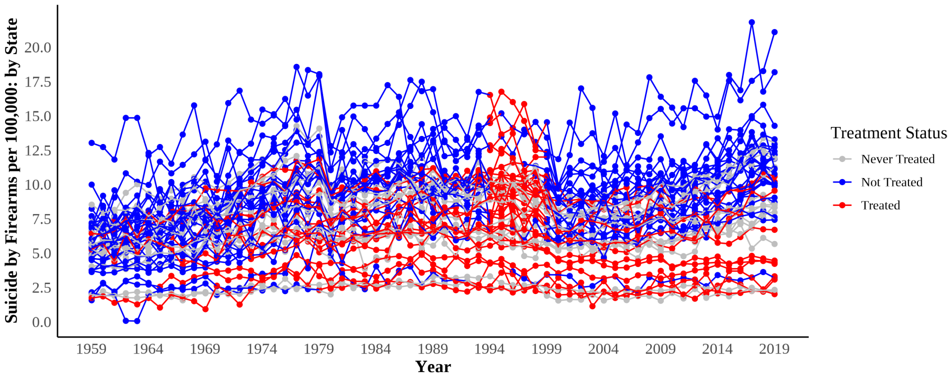
Note: This figure shows the annual trend in suicide rates by firearm (per 100,000 population) across US counties from 1959 to 2019. The figure highlights the long-term trajectory of firearm suicides, providing historical context for analyzing the impact of waiting period laws introduced in different years and states.
Source: National Vital Statistics System (NVSS), Multiple Cause of Death Files, 1959–2019.

Fig. 7. Trends in Overall Suicide Rates Across Counties, 1959–2019



Note: This figure displays the overall suicide rate (all causes, per 100,000 population) from 1959 to 2019. It offers a comparison benchmark for firearm-specific suicides and helps evaluate whether general suicide trends might confound the estimated effects of waiting period policies.
Source: National Vital Statistics System (NVSS), Multiple Cause of Death Files, 1959–2019.

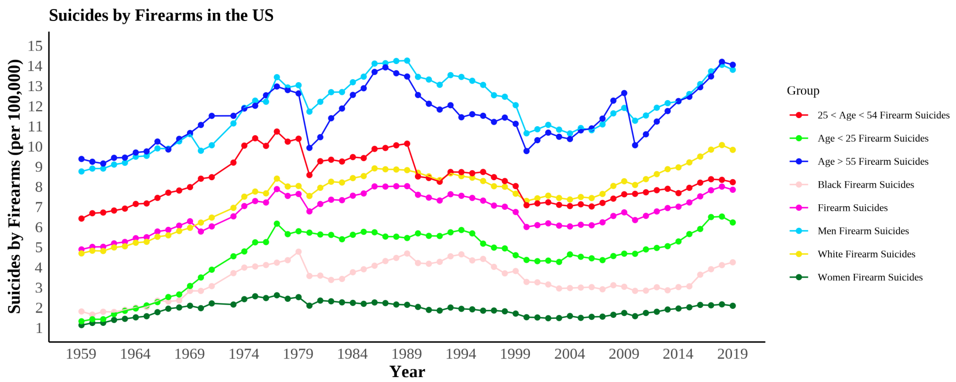
Fig. 8. Firearm Suicide Rates by State Treatment Status, 1959–2019



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.

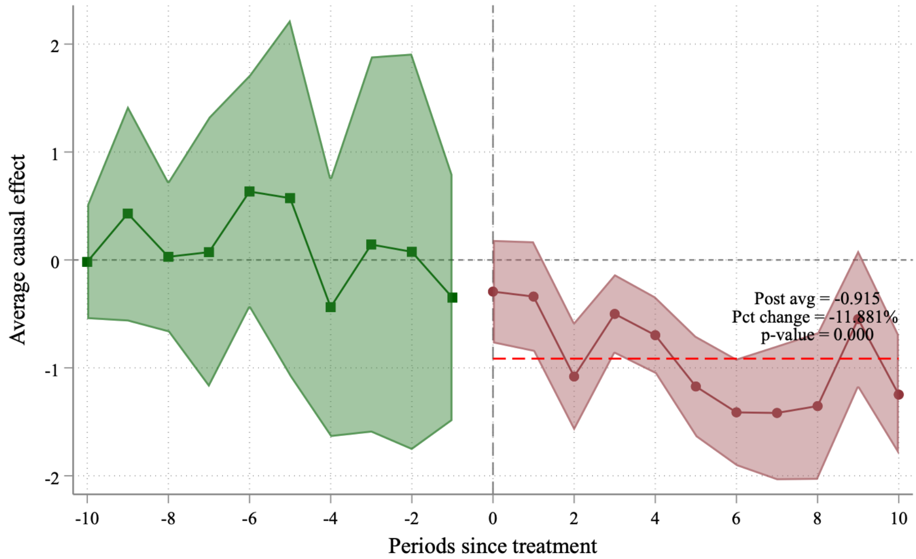
Source: National Vital Statistics System (NVSS), Multiple Cause of Death Files, 1959–2019.

Fig. 9. Firearm Suicide Rates by Demographic Group in the US, 1959–2019



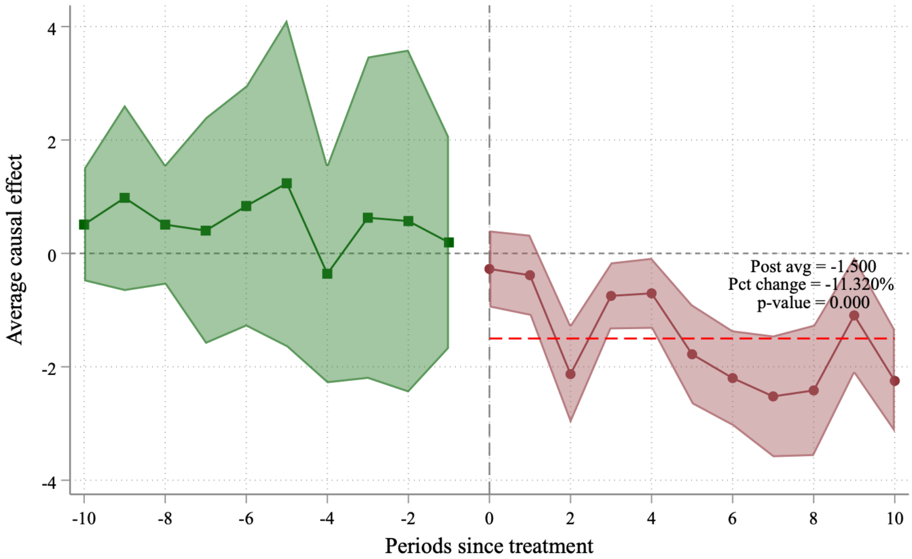
Note: This figure illustrates firearm suicide rates (per 100,000 population) across demographic categories from 1959 to 2019. The substantial differences between men and women, age groups, and racial categories highlight the importance of demographic-specific approaches to suicide prevention. The recent 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. 10. Estimated Effect of Waiting Periods on Overall Firearm Suicide Rates



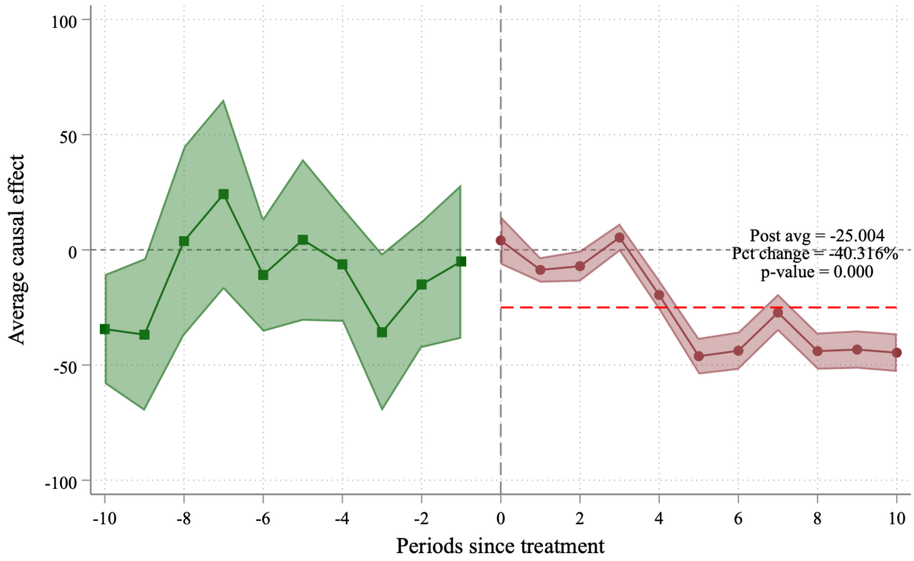
Note: This figure shows the dynamic effects of waiting period laws on firearm suicide rates across US counties. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. Standard errors are bootstrapped and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

Fig. 11. Effect of Waiting Periods on Firearm Suicide Rates Among Men



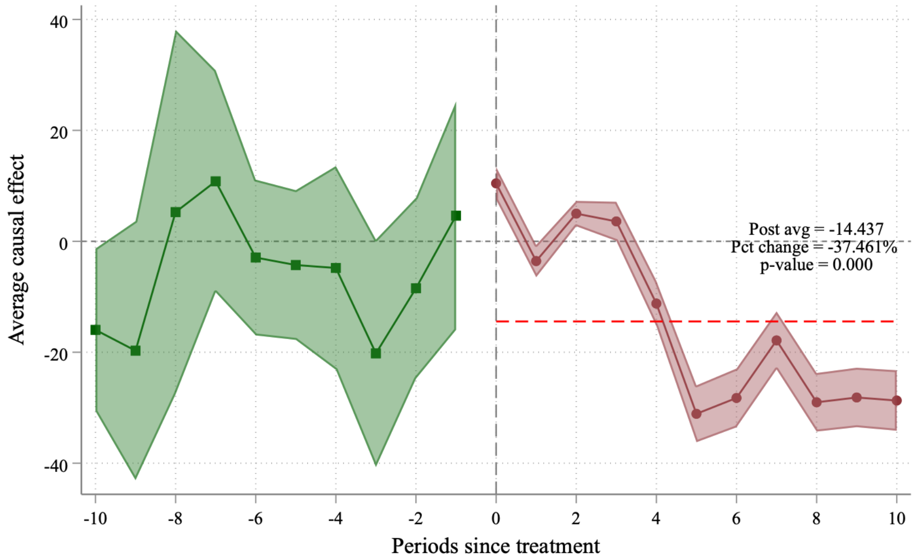
Note: This figure focuses on the male population, showing how waiting period laws affect firearm suicide rates for men specifically. Standard errors are bootstrapped and clustered at the state level.

Fig. 12. Effect of Waiting Periods on Firearm Suicide Rates Among Adults Aged 55+



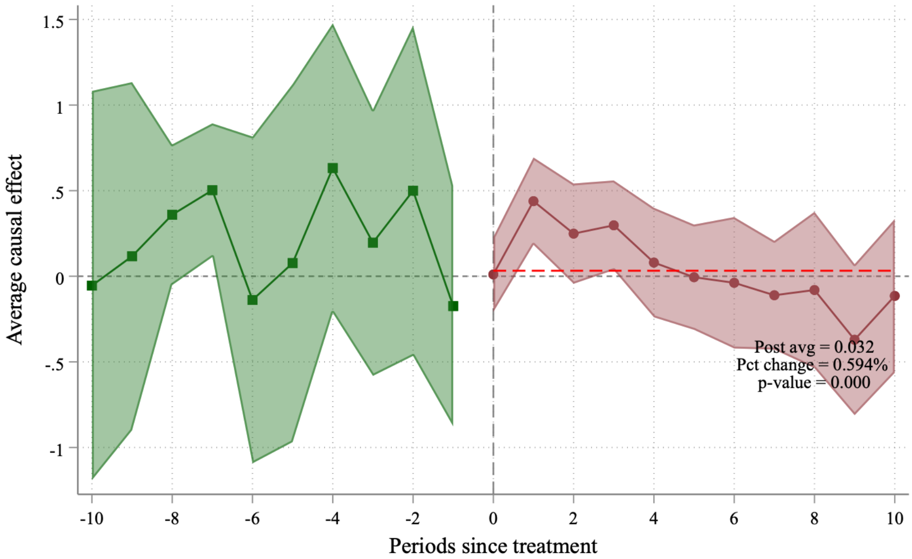
Note: This event study estimates the policy effect on older adults, a group at elevated suicide risk. Standard errors are bootstrapped and clustered at the state level.

Fig. 13. Effect of Waiting Periods on Firearm Suicide Rates Among White Individuals



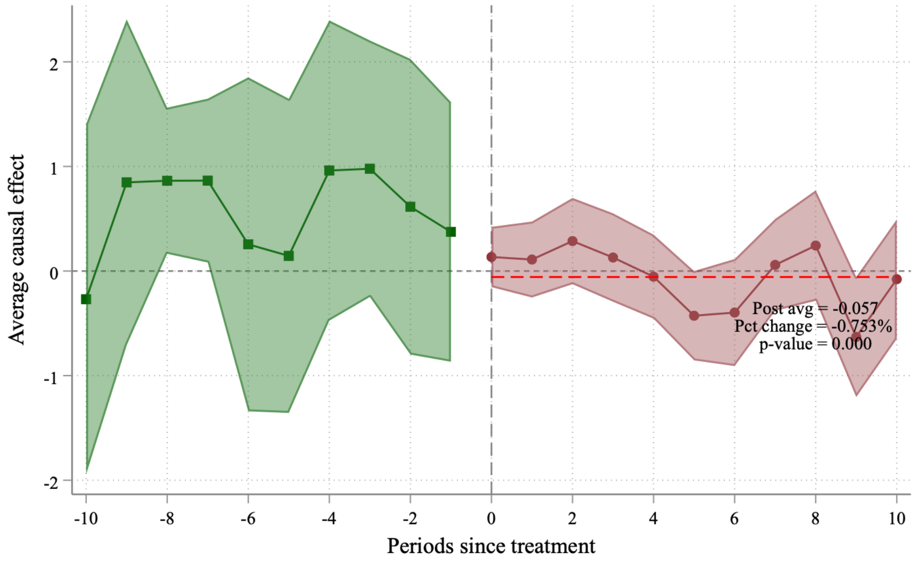
Note: This figure examines firearm suicide trends for white individuals. Standard errors are bootstrapped and clustered at the state level.

Fig. 14. Effect of Waiting Periods on Non-Firearm Suicide Rates



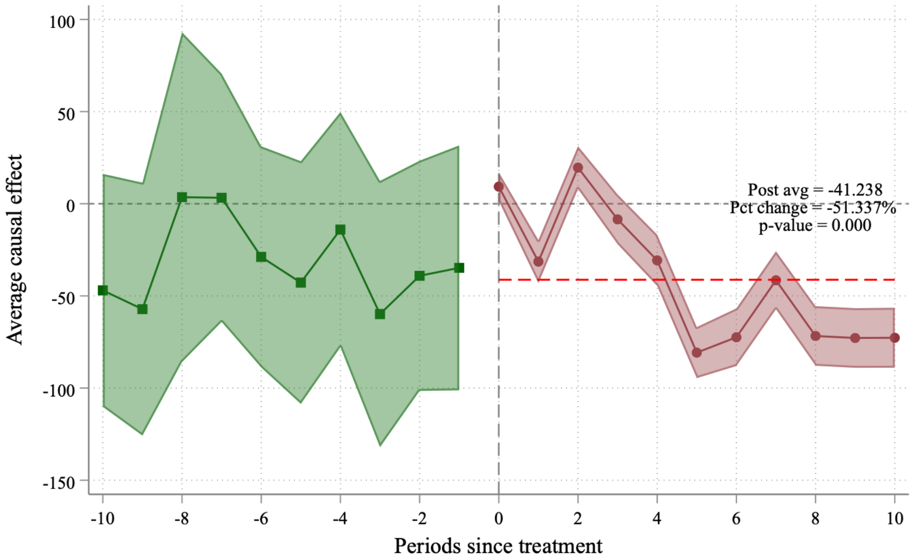
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The absence of significant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

Fig. 15. Effect of Waiting Periods on Non-Firearm Suicide Rates Among Men



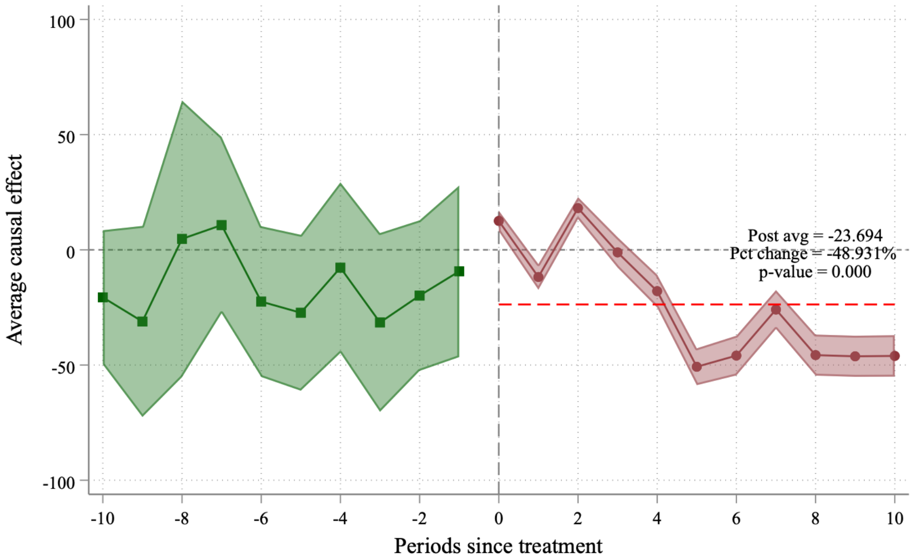
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among men. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The absence of significant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

Fig. 16. Effect of Waiting Periods on Non-Firearm Suicide Rates Among Adults Aged 55+



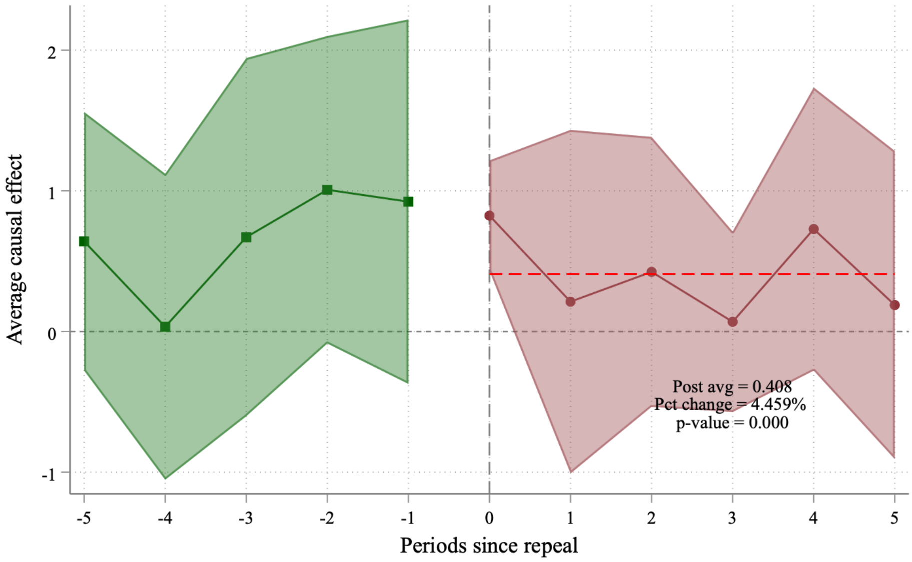
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among adults aged 55+. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The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

Fig. 17. Effect of Waiting Periods on Non-Firearm Suicide Rates Among White Individuals



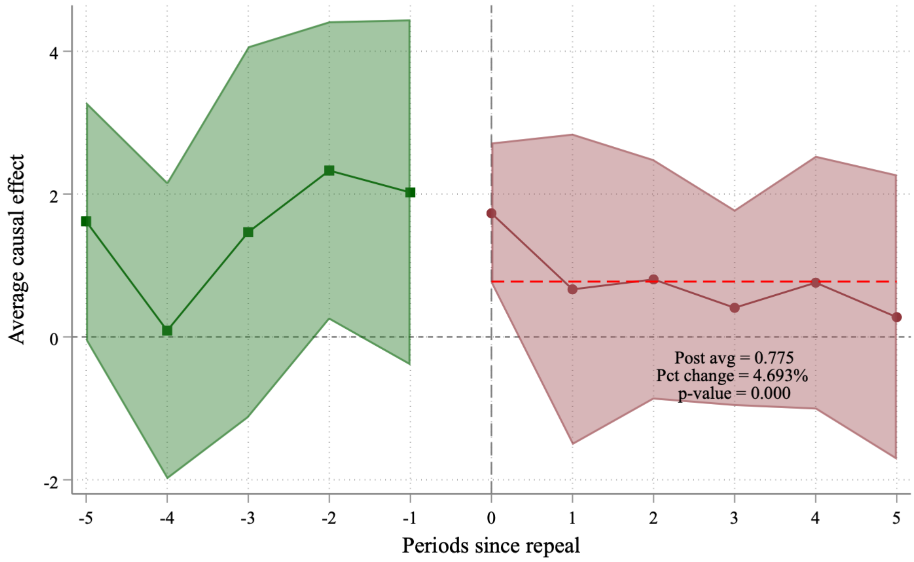
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among White individuals. Each point represents the estimated difference in firearm suicide rates relative to the year of policy adoption (year 0), with 95% confidence intervals. The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

Fig. 18. Effect of Waiting Period Repeal on Overall Firearm Suicide Rates



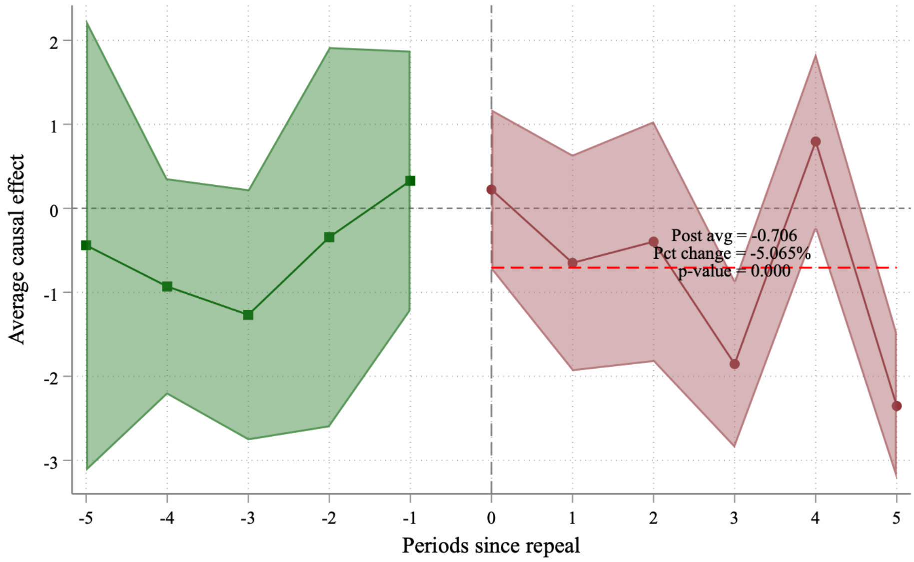
Note: This figure shows the dynamic effects of repealing waiting period laws on firearm suicide rates across US counties using a sample of states that repealed waiting periods. The sample is composed of states that experience either one policy transition from treatment to no-treatment, or always treated, 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. 19. Effect of Waiting Period Repeal on Firearm Suicide Rates Among Men



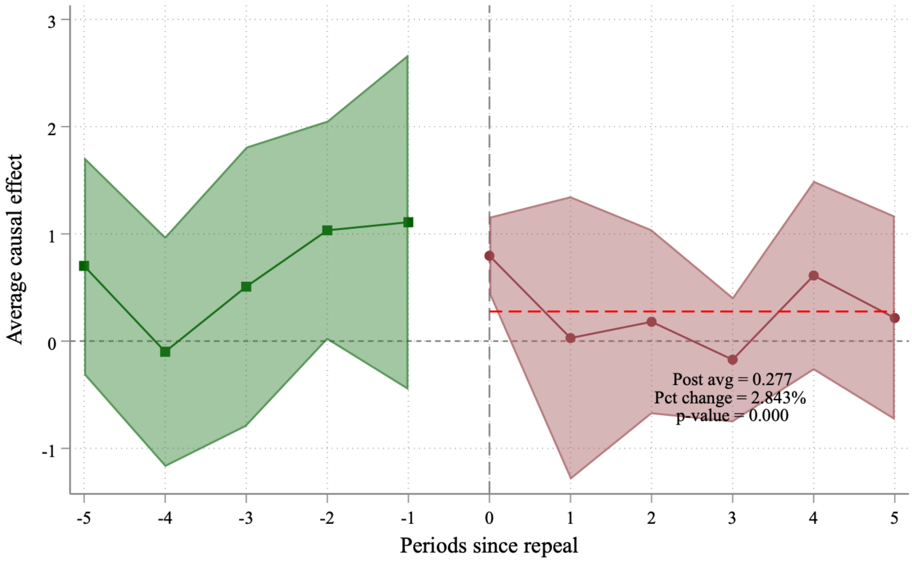
Note: This figure focuses on the male population using a sample of states that repealed waiting periods, showing how waiting period laws affect firearm suicide rates for men specifically. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment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

Fig. 20. Effect of Waiting Period Repeal on Firearm Suicide Rates Among Adults Aged 55+



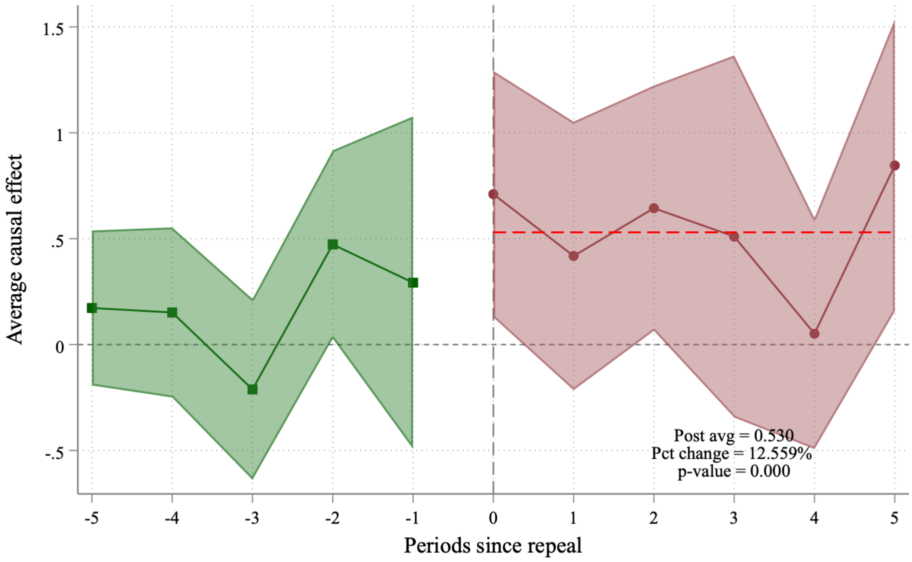
Note: This event study estimates the policy effect on older adults using a sample of states that repealed waiting periods, focusing on a group at elevated suicide risk. The sample comprises 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment,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

Fig. 21. Effect of Waiting Period Repeal on Firearm Suicide Rates Among White Individuals



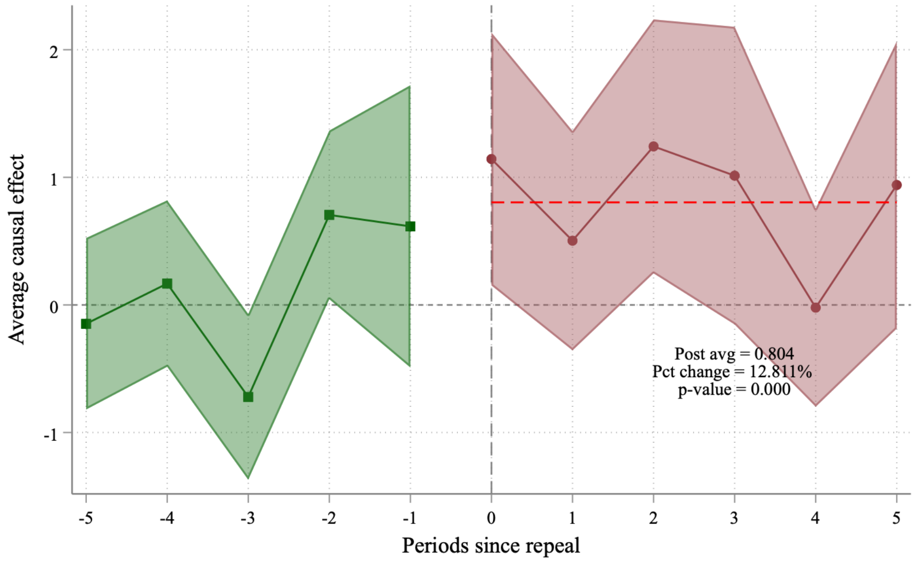
Note: This figure examines firearm suicide trends for white individuals using a sample of states that repealed waiting periods. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment, or always treated. Standard errors are bootstrapped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

Fig. 22. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates



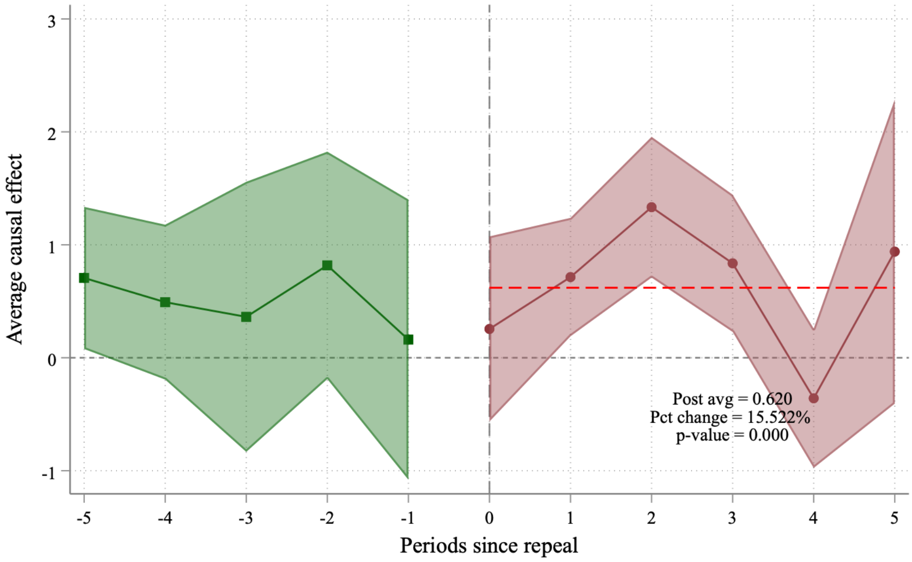
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties using a sample of states that repealed waiting periods. The sample comprises 14 states (8 treatment, 6 control) that experience either one policy transition from treatment to no-treatment,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

Fig. 23. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates Among Men



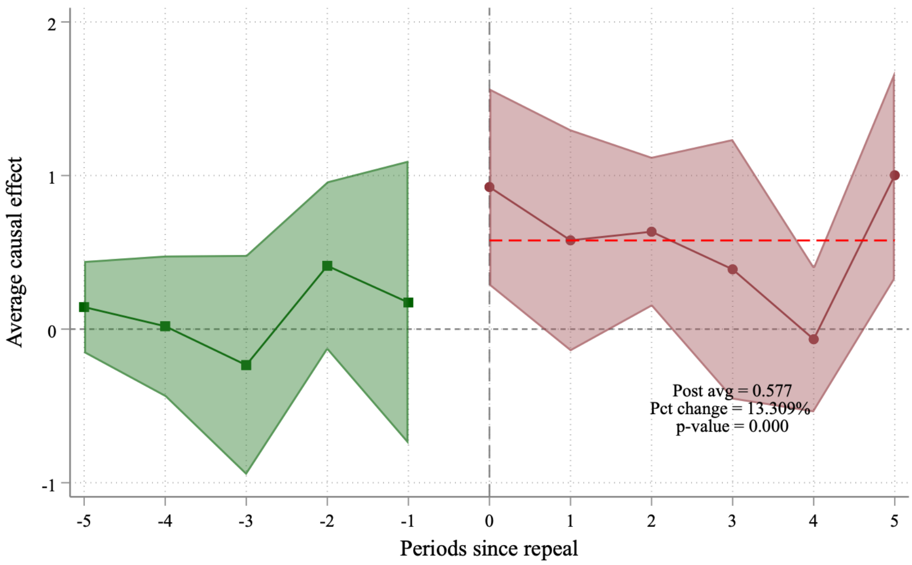
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among men using a sample of states that repealed waiting periods. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition from treatment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

Fig. 24. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates Among Adults Aged 55+



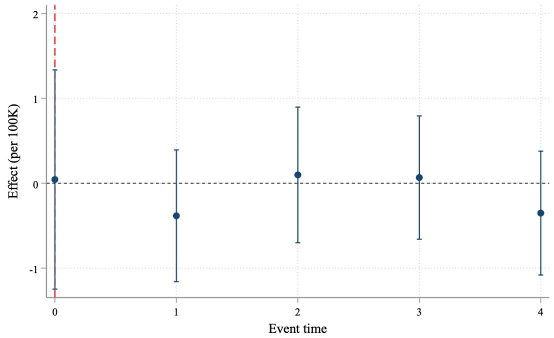
This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among adults aged 55+ using a sample of states that repealed waiting periods. The sample comprises 14 states (8 treatment, 6 control) that experience either one policy transition from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

Fig. 25. Effect of Waiting Period Repeal on Non-Firearm Suicide Rates Among White Individuals

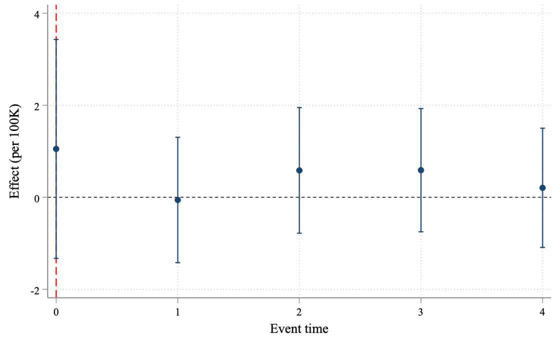


This figure shows the dynamic effects of waiting period laws on all other suicide rates (non-firearm) across US counties among White individuals using a sample of states that repealed waiting periods. The sample includes 14 states (8 treatment, 6 control) that experience either one policy transition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

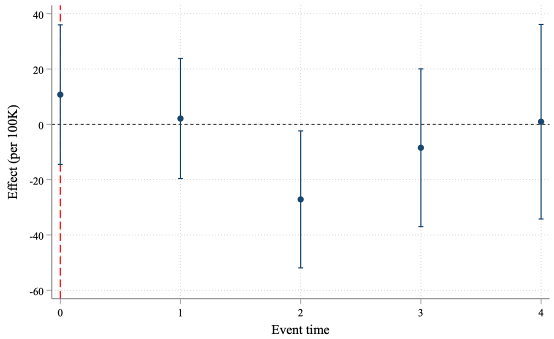
Fig. 26. Placebo Tests: Effect of Waiting Period Repeal on Firearm Suicide Rates (5-Year Lead)



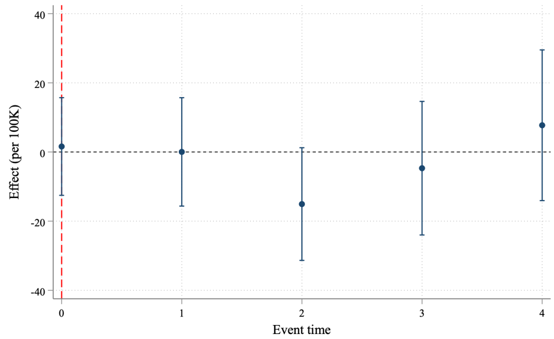
(a) All Individuals



(b) Men



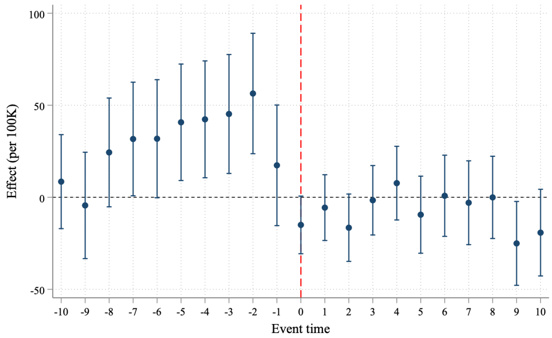
(c) Adults Aged 55+



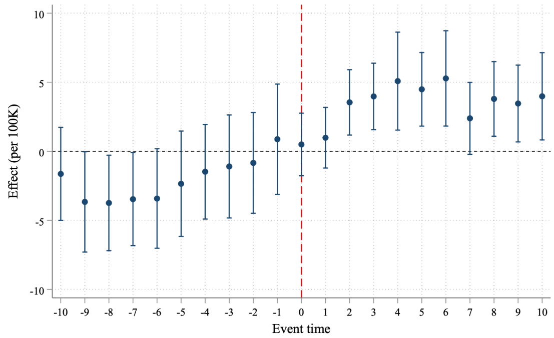
(d) White Individuals

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.

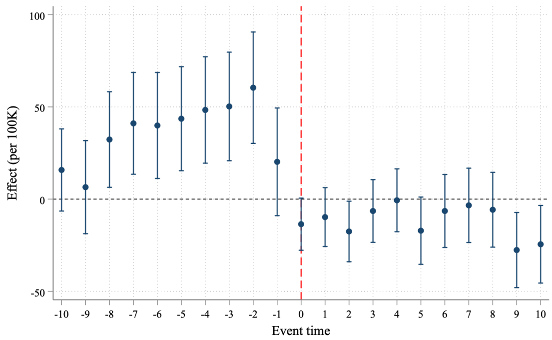
Fig. 27. Effect of Waiting Period Repeal on Non-Suicide Mortality Rates



(a) All Individuals



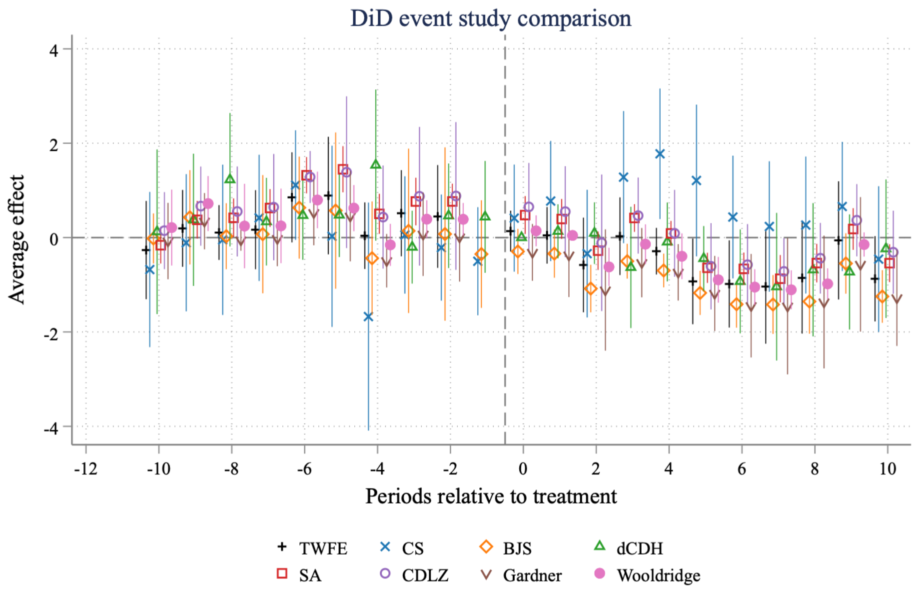
(b) Adults Aged 25–54



(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. 28. 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.

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: Data come from the National Vital Statistics System (NVSS), 1959–2019. Suicide rates are expressed per 100,000 population. "Female Population Share", "College Educated", and "Below Poverty Line" are proportions. 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: Data come from the National Vital Statistics System (NVSS), 1959–2019. 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: 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.

Table 4: 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.

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