The Effect of Concealed-Carry and Handgun Restrictions on Gun-Related Deaths: Evidence from the Sullivan Act of 1911

Briggs Depew* and Isaac D. Swensen**

Abstract:

In the wake of two public shootings, the state of New York passed the Sullivan Act in 1911. The first of its kind and a model for subsequent "may-issue" concealed-carry laws, the act outlawed carrying concealable firearms without a police-issued license, established a stringent set of rules for obtaining a license, and introduced regulations governing the sale and possession of handguns. The Sullivan Act influenced the evolution of gun control in the United States and was regarded as a model for national regulation by gun control advocates, yet little is known of its efficacy in curbing gun violence in New York. To analyze the effects of the Sullivan Act, we collected unique historical data including state mortality records, pistol permit and license data, and information on citations for carrying without a license. Our empirical approach employs both synthetic control and difference-in-differences methodologies to estimate the effects of the Sullivan Act. Our descriptive analysis of gun licenses, permits, and citations for illegal carrying reveal clear initial effects of the Sullivan Act on gun-related behaviors. Our main analyses show no evidence of the Sullivan Act having an effect on overall homicide rates, suggestive evidence of a reduction in overall suicide rates, and clear evidence that the Sullivan Act led to a large and sustained decrease in gun-related suicide rates.

JEL Codes: K42, N42

*__Correspondence address:__ briggs.depew@usu.edu
**isaac.swensen@montana.edu

1 Introduction

Early state laws targeting concealable weapons began in the late 19th century. Unlike modern concealed-carry laws that typically establish legal pathways to carry concealed firearms, these laws were heavily restrictive and clearly indicative of concerns that concealed weapons may have adverse effects on public safety. These early statutes established highly discretionary processes for obtaining concealed-carry licenses and severely penalized concealed carrying without a license. This first wave of concealed carry laws has shaped the evolution of gun legislation in the United States, yet data limitations have limited rigorous evaluations of their effects on public safety. Again, this stands in stark contrast to the intense scrutiny, divisive public debate, and decades of academic research focusing on modern concealed-carry laws that often move away from the discretionary approach established by these earlier laws toward policies that expand the lawfulness of carrying a concealed firearm.

In this paper, we conduct the first rigorous analysis evaluating the effects of early concealed-carry legislation, focusing on one prominent legislative act—New York (NY) state's 1911 Sullivan Act. The objective of the Sullivan Act was to curb gun violence in the state of NY primarily through the establishment of a felony penalty for carrying a concealed weapon without a license. The Sullivan Act also stipulated any ownership of a concealable handgun without a permit as a misdemeanor offense.[1] Backed by strong support from legislators and the public, the bill's author, Timothy Sullivan, stated "I think so much of this measure that if you pass it I believe it will save more souls than all the preachers in the city talking for the next ten years (NY Times, May 11, 1911)." The Sullivan Act was a model predecessor for many subsequent state concealed weapons laws and has been characterized as "the most important point of reference in national debates over firearms regulation" in the interwar era and a law that many advocates of gun control "looked to as a model for a national gun control law" (Mohun, 2013).[2] Though amended over time, this law is still on the books in the state of NY. Given this rich history, and that the Sullivan Act was the most rigid anti-firearm legislation at the time in the United States (Brabner-Smith, 1933), the Sullivan Act provides a unique opportunity to gain insight on the degree to which gun control affects public safety.

To explore the effects of the Sullivan Act we bring together historical data from multiple sources to examine the effect of restrictive gun control on illegal gun ownership, gun carrying, and measures of public safety. In particular, our data include measures for licenses for concealed carry, permits for handgun ownership, misdemeanor and felony citations for carrying a dangerous weapon, and gun-related mortality outcomes. We

[1] See 1911 NY Laws ch. 195, sec. 1, Âğ 1897 and N.Y. Penal L. Âğ 1897 (1909). Though often used interchangeably, we use *permits* to describe legal handgun ownership and *license* to describe the legal right to concealed carry a handgun.

[2] Though four states passed statutes restricting concealed carrying prior to 1911, these laws became more common after the passage of the Sullivan Act, and by 1938 all but two states restricted or required a license for concealed carrying (Warner, 1938).

use both a synthetic control design and difference-in-differences models to compare homicides and suicides in NY to other states before and after the implementation of the Sullivan Act.

Our analysis reveals that the the Sullivan Act led to stark increases in permitted handguns and citations for illegal gun carrying. While our estimates show that these changes had no clear effects on overall homicides, we find large and sustained decreases in gun suicide rates. We find that gun suicide rates fell by 32 to 48 percent following the Sullivan Act. That we find some evidence of a reduction in overall suicide and no clear effects on non-gun suicide rates suggests imperfect substitution away from guns toward alternative methods for suicide. Using legislation in states that passed similar gun control laws in the years following the Sullivan Act, we find evidence that supports our main analysis. Finally, our robustness checks highlight that the effects are apparent following, but not prior to, the passage of the Sullivan Act which speaks to concerns regarding potential endogeneity, and demonstrates that the estimates are similar under alternative modeling decisions.

From a historical perspective, our analysis of the Sullivan Act offers insight into the likely effects of similar laws passed over subsequent decades. In terms of typical delineations of modern concealed carry legislation, handgun laws in this early era would be considered may-issue laws as state officials were given significant discretion on the granting of licenses and permits.[3] Such laws were the guiding influence for the regulation of concealed weapons in public spaces until the later decades of the 20th century when states began moving away from a discretionary licensing process.[4] As such, these results offer an important point of reference to understand the historical evolution of gun laws and their effects on public safety in the U.S.

Our results are also informative to ongoing debates surrounding the implications of concealed-carry legislation on public safety. The focus on the reduced-form effect of modern shall-issue laws has yielded little consensus on the effect of concealed carrying on public safety.[5] Notably, key identification challenges arise in this literature because of the potential for lagged or limited take-up following expansions in concealed-carry legality. In contrast, the Sullivan Act allows us to credibly identify effects using a discrete change in legality that places sudden restrictions on all concealed-carrying in NY. Moreover, as one of the more restrictive gun regulations in U.S. history, the Sullivan Act provides a unique opportunity to credibly identify effects

[3]In contrast, shall-issue statutes grant concealed-carry licenses to qualified applicants without stated justification for a license as long as an individual has met the age, training, and background requirements.

[4]The pattern of moving toward shall-issue laws continued into the 21st century, with many states recently expanding to unrestricted carry provisions that eliminate the need for a license to carry a concealed firearm.

[5]Since Lott and Mustard (1997), a large number of studies have explored the reduced-form effects of concealed-carry laws on crime. Lott and Mustard (1997), Bronars and Lott (1998), Lott (1998), Moody (2001), Plassmann and Tideman (2001), Olson and Maltz (2001), Mustard (2001), and Moody et al. (2014) have found supporting evidence for a deterrent effect on crime. Others, including Black and Nagin (1998), Ludwig (1998), Dezhbakhsh and Rubin (1998), Duggan (2001), Ayres and Donohue III (2003), Rubin and Dezhbakhsh (2003), Aneja et al. (2011), Durlauf et al. (2016), and Donohue et al. (2017) have found no significant effect on crime or slight increases in certain types of crime.

on gun-carrying and public safety. In other words, if we expect to see drastic changes from a single state's adoption of gun reform, such changes plausibly occur when enacted regulation brings about significant change.

Our findings also contribute to a growing literature exploring the link between gun ownership, gun policies, and suicide. Broadly considered, ownership and availability of firearms are positively correlated with an increased risk of suicide (Kellermann et al., 1992), and similar evidence has linked increases in firearm background checks to increases in suicide rates (Lang, 2013). Duggan (2003) argues that selection into gun ownership of individuals with above average suicidal tendencies drives a significant part of this relationship. That said, quasi-experimental studies have demonstrated a reduction in suicide rates following mandatory handgun purchase delays (Edwards et al., 2018), reforms that reduced the prevalence of military-issued guns (Balestra, 2018), gun buy-back policies (Leigh and Neill, 2010), and child access prevention laws (Webster et al., 2004). Where effects are found, the existing evidence most often supports the notion that reductions in gun-related suicide also lead to reductions in overall suicide, which is suggestive of imperfect substitution between suicide methods.

In other contexts, researchers have found no change in suicide rates in the months surrounding gun shows in California and Texas (Duggan et al., 2011); after a spike in gun sales subsequent to the Sandy Hook school shooting (Levine and McKnight, 2017); or following the implementation of minimum age laws for purchase and possession of firearms (Webster et al., 2004). Our study contributes to this literature by highlighting a clear reduction in gun-related suicides in the context of concealed carry regulation. Moreover, despite historical differences in suicide rates and methods, our results similarly suggest imperfect substitution between gun and non-gun suicides.

2 Historical Context

In the wake of two tragic NY City events, State Senator Timothy D. Sullivan's bill to establish concealed carrying without a license as a felony offense had little opposition. The first tragedy occurred on August 9, 1910, as NY City mayor, William J. Gaynor, was posing for a photograph while waiting to board the German steamship at the Hoboken Pier. J. J. Gallager stepped out from the crowd and shot the mayor, hitting him in the neck. Gallager had been dismissed from the NY City Dock Department on July 19th, and had been "haunting the Mayor's office in a vain attempt to get his job back" (NY Times, Aug 10, 2010). The mayor recovered, but the entire event was vividly displayed across newspapers worldwide.

Adding to the momentum and public out-cry for swift gun reform, a well-known NY City novelist, David Graham Phillips, was shot and killed on January 23, 1911. As described by George Le Brun of the city's coroner office, who played a key role in advocating for the Sullivan Act, "[t]he increase of deaths by shooting

in murder and suicide cases in this city, ..., the shooting down of Mayor Gaynor, and the recent murder of David Graham Phillips, should arouse the public to the immediate necessity of a law governing the sale of revolvers." (NY Times, January 30, 1911). In response, Senator Timothy D. Sullivan introduced his bill to the state legislature in early 1911 (NY Times, Jan 30, 1911).[6] Only a few voices in the state legislature opposed the bill (NY Times, May 11, 1911).[7] While Senator Sullivan and others touted the law as a remedy to commonplace shootings and lawlessness prior to its passage, our analysis exploits the timing of the law's implementation that was driven by these high-profile shootings.[8]

The Sullivan Act was signed on May 30, 1911 and went in effect on September 1, 1911 (NY Times, Aug 29, 1911). Under the act, citizens were required to obtain a permit to possess and license to carry a concealable weapon.[9] Possession of a firearm without a permit was a misdemeanor offense and carrying without a license was a felony offense. Furthermore, the law required that gun dealers maintain detailed sales records and only sell handguns to individuals with a valid license. Lastly, lawful possessors of a concealable firearm were required to notify the police prior to any transfer, sale, or giving of their firearm to another.[10]

Although the Sullivan Act is often heralded as NY's first effort to regulate concealed carrying, the prior state penal code classified carrying a concealed weapon without a license as a misdemeanor offense and noted that license issuance practices were left to local magistrates and ordinances.[11] Our main analysis relies on the sudden change in statewide issuance and enforcement policies following the Sullivan Act which is suggested by the historical narrative and our subsequent analysis of permits and citations in NY City following the Sullivan Act.

Obtaining a concealed carry license or handgun permit under the Sullivan Act required police interviews, an application fee, fingerprints, and four photographs of the applicant. The application fee was originally set at $.50 (Hansen, 1976). For an individual to obtain a permit, they were required to convince the police in an interview that they needed a permit or license in "good reason." The police force held considerable authority in determining the issuance of permits. According to Kopel (2016), NY City's police commissioners held the view that residents should not have handguns. Kopel (2016) argues that "[N]o matter the reason a NY City applicant might give for wanting a handgun (e.g., target shooting, self-defense), the applicant

[6]Historical accounts also highlight the controversial nature of Tim Sullivan's ties to mob activities, corruption, and potential motives to enact the Sullivan Act in support of criminal activities. Sullivan was also known as a progressive political reformer who supported women's suffrage and right to fair pay (Welch, 2009).
[7]For instance, NY State Senator Ferris "Your bill won't stop murders. You can't force a burglar to get a license to use a gun" (NY Times, May 11, 1911).
[8]Also see Duffy (2011) who highlights the role of these shootings in the passage of the Sullivan Act. Moreover, suicide–an important outcome in our study–was not a primary focal point in the historical narrative leading up to the passage of the Sullivan Act.
[9]Concealable firearms were defined in the statute as a "pistol, revolver or other firearm of a size which may be concealed upon the person" (See N.Y. Penal L. Âğ 1897 (1914)
[10]See Hansen (1976) and Kopel (2016) for a detailed historical account of the passage of the Sullivan Act.
[11]See 1905 Laws of NY Ch. 92, S 2.

would be told that the reason was not good enough." On November 27th, 1911 the NY Times reported that a medical doctor who had previously been held up, was refused a permit to purchase a handgun. As suggested by Hansen (1976), "[t]his was and is a common occurrence under the Sullivan Act, as there is no uniform standard for granting or refusing a permit and perfectly reputable citizens can be denied a permit if the officials do not feel he has 'good cause' to own a handgun." Despite these prevailing narratives and anecdotes, it remains plausible that the induced demand from a potential felony penalty resulting from unlawful carry would significantly increase the number of permits issued even in the face of such obstacles to securing a permit. Indeed, our subsequent descriptive evidence shows dramatic increases in the number of issued permits and licenses after the passage of the Sullivan Act.

Historical reports of the Sullivan Act demonstrate varying accounts of its implementation and enforcement, and many have since speculated on its supposed effects. For instance, one historical source stated "[i]t cannot be denied, however, that the percentage of homicides and suicides by firearms is considerably less where rigid firearm laws are enforced, as in ... NY State, than where there are few or no regulatory provisions" (Brabner-Smith, 1933). On the other hand, a 1912 NY Times column titled "*Sullivan Pistol Law Has Failed*" asserts just the opposite. Our analysis provides a systematic approach to offer needed evidence of the effects of the Sullivan Act on gun-related behaviors, law enforcement practices, and gun-related mortality outcomes.

3 Data

Our main analysis uses mortality records from the U.S. Census Bureau's Mortality Statistics (CDC, 2015). The U.S. Census Bureau began publishing annual state-level mortality statistics in 1900.[12] Initially, only ten states participated in the program including CT, IN, MA, ME, MI, NH, NJ, NY, RI, and VT. However, by 1933 the lower 48 states and the District of Columbia were submitting vital statistics to the Census[13]

These data allow us to compare homicide and suicide rates in NY to other states over the same years. Particularly important for our context, they also provide a breakdown of suicide by cause of death, allowing us to measure potential effects on gun suicide.[14] For our main analysis, we restrict the data to a balanced panel of states for the years 1900 through 1920, which provides a sufficient post-treatment period to analyze any persistent effects of the Sullivan Act and it is prior to other states passing related laws in the 1920s. [15]

[12]Prior to 1900, the U.S. Census collected national mortality statistics based on the 1850 decennial census and each decennial census up to 1900. These reports were deemed inaccurate and incomplete (Stoke, 1939).
[13]The gradual increase in participation between 1900 and 1933 was due to the time needed to i) educate state boards of health regarding a national vital statistics program, ii) enact and enforce state laws requiring registration of deaths with the U.S. Census, and iii) to establish and organize the infrastructure and personnel responsible for collecting and submitting vital records (Stoke, 1939; Hetzel, 1997).
[14]Unfortunately, the homicide data collected by the U.S. Census do not provide a similar breakdown until 1910.
[15]While our sample size is notably smaller than most conventional panels, our evidence supporting the exgogeneity of the Sullivan Act along with our robustness exercises suggest sufficient statistical power to identify the effect of the Sullivan Act.

In ancillary analyses, we also consider an unbalanced panel and expand the sample through 1929 to explore the effects of related laws in other states. We calculate mortality rates per 100,000 state residents using population estimates obtained from the St. Louis Federal Reserve Bank (FRED, 2020).

Table 1 presents descriptive statistics for our main sample. Columns (3) and (6) compare outcomes and state characteristics in NY to all other states, and the remaining columns provide a similar comparison for time periods prior to and following the Sullivan Act. The comparison highlights high homicide and suicide rates in NY relative to the other states in our sample. It also shows increases in all mortality outcomes when comparing the pre and post periods with the exception of a decrease in gun suicide rates in NY, which foreshadows the results of our main analysis.

The state-year characteristics in Table 1 were compiled from the U.S. Census, the Census of Religious Bodies (Haines et al., 2010), Executions in the United States (Espy and Smykla, 2016), and Electoral Data for Congressional Races (Clubb et al., 2006). For each state, the U.S. Census data provide illiteracy rates for those 10 years or older and demographic measures for foreign born, non-white, and less than 10 years-old. We calculate the fraction of each state's population that identifies as Catholic from the Census of Religious Bodies. To control for state-specific criminal justice policies, we also measure the number of executions per-capita for each state. Finally, we calculate the fraction of the votes for Democrat in the most recent United States congressional election.[16]

Comparisons of state characteristics show many similarities in levels and trends over our time period. Balance tests confirm no significant differences in means when comparing NY to other states in the pre-Sullivan time period with the exception of the fraction of the state population that is foreign born. A higher number of immigrants is not surprising since NY was a gateway to the United States for many immigrant populations.

4 Effects on Gun Behaviors in NY City

The Sullivan Act formalized the permit requirement to possess a handgun and elevated the penalty for carrying a concealable weapon without a license from a misdemeanor to a felony classification. While state law allowed local magistrates to issue concealed-carry licenses prior to 1911, we anticipate that the Sullivan Act led to increases in the number of citizens seeking permits and changes in policing as officers were tasked with enforcing the new statutes.

We begin by exploring visual patterns of these gun-related behaviors using data on pistol permits/licenses and citations for carrying a dangerous weapon using data from NY City. The degree to which they are responsive to the Sullivan Act can offer proof of concept that the legislation led to changes in underlying

[16]To obtain annualized data, we calculated linearly interpolations for each of the state characteristics except for executions, which is available at the state-year level.

gun-related behaviors. While similar data are not available statewide or for other states used in our main analysis, much of the historical dialogue surrounding the Sullivan Act focuses on happenings in and around NY City. As such, we view this as initial evidence in the location where Sullivan Act policies may be most salient.

Figure 1 plots the number of issued pistol permits/licenses, and the number of felony and misdemeanor citations for carrying a concealed or dangerous weapon in NY City (NY, 1909). The figure highlights a sharp increase in the number of permits/licenses following the enactment of the Sullivan Act in September of 1911. We view this as suggestive evidence that the Sullivan Act contributed to a more-than-doubling of the annual number of pistol permits issued in NY City.

Figure 1 also shows a stark increase in felony citations for carrying a dangerous weapon starting in 1911 and continuing through 1914.[17] Misdemeanor citations show a slight increase in 1911, followed by a decrease in 1912 before continuing a modest upward trend.[18] The initial decrease in misdemeanors and increase in felonies is consistent with substitution between these classifications following the Sullivan Act, which upgraded the penalty for such violations from a misdemeanor to a felony. And the subsequent slight increases in misdemeanors is consistent with elevated misdemeanor citations for handgun possession without a permit in the years following the Sullivan Act. Notably, the issuance and enforcement prior to the Sullivan Act is likely related to NY law that allowed local magistrates to regulate concealed carrying.[19]

Taken together, the plots in Figure 1 provide visual evidence that the Sullivan Act increased both civilian compliance with the Act's permit requirement and the law enforcement of the concealed carry statues in NY City.[20] Our subsequent empirical analyses consider whether the changes in these behaviors affected suicides and homicides across the state of NY.

5 Empirical Strategy

Our empirical approach compares mortality rates in NY to other states before and after the implementation of the Sullivan Act. We show estimates using both a Synthetic Control Method (SCM) and traditional difference-in-differences models. SCM is intuitively appealing in that it highlights the pre-treatment similarities between

[17]Note that the police reports do not differentiate between handguns and other dangerous weapons. That said, handgun use and carrying were the clear focal point of historical accounts of the implementation and enforcement of the Act.
[18]Citations by the detective division were not included in the annual reports until 1911. Thus, the increase in felony citations for carrying a concealed or dangerous weapon may be partially spurious due to changes in enumeration. However, when contrasted with the reduction in misdemeanor citations, the data still present a compelling argument for changes in policing behavior as a result of the Sullivan Act.
[19]NY City required a pistol license to carry a pistol in the city as early as 1897 and the and the annual fee contributed to the police department's pension fund (NY City Charter 1897).
[20]We also explored plots specific to NY City including monthly data that considers short-run dynamics and annual data that separates homicides into gun-related and non-gun related incidents–sub-categories of homicide that are not available in our main analysis using U.S. Census data. These plots, available on request, show no visual evidence for changes in homicides, homicide by sub-category, or suicide around the enactment of the Sullivan Act.

a synthetic NY and actual NY, and visually identifies potential dynamic effects of the Sullivan Act. SCM is also well suited for our context where we have one treatment group, few control groups, and few observational units (Cameron and Miller, 2015). That said, given our small sample size and the balanced panel requirement of the SCM, statistical inference is limited using SCM. As such, we also employ a difference in differences model that allows a more flexible approach to inference and the option to explore the stability of the estimates across balanced and unbalanced panels.

5.1 Synthetic Control Method

SCM is a data-driven process to generate a synthetic control group for causal inference in comparative case studies as formulated by Abadie et al. (2010). In a recent review, Athey and Imbens (2017) characterize SCM as "arguably the most important innovation in the policy evaluation literature in the last 15 years." SCM constructs a synthetic control group for the treatment group using a weighted average of control groups that are most similar to the treatment group prior to treatment. Formally, SCM constructs a weighting vector W that minimizes the distance between pre-treatment characteristics in treated and control states as follows:

$$||X_1 - X_0 W||_V = \sqrt{(X_1 - X_0 W)' V (X_1 - X_0 W)}, \tag{1}$$

where X_0 and X_1 are vectors of pre-treatment characteristics for treated and control states, and V is a positive definite and diagonal matrix selected such that the root mean squared prediction error (RMSPE) of the outcome variable is minimized for the pre-treatment period.

Our donor pool of control states consists of the nine available states that report mortality outcomes consistently from 1900 through 1920. This sample restriction is due to the balanced panel requirement in the SCM as well as our desire to utilize the longest feasible pre-treatment window to construct a credible synthetic control.[21] Following Ferman et al. (2020) who cite risks of p-hacking due to the lack of a consensus on which variables should be chosen as predictors, our main estimates exclude all covariates and simply match on the outcomes in each year prior to treatment. We also explore an alternative approach in the appendix that uses a range of predictors to construct the synthetic control including measures for the fraction of the population that are less than age 25, black, catholic, literate, and foreign born.[22]

[21] Though we have a relatively small sample size, having the synthetic control comprised of few control units is common. Moreover, models that extend the sample to the 15 states that report from 1906-1920 yield similar results.

[22] These plots, shown in Appendix Figure A1, are consistent with our main SCM results. Moreover, to confirm that there are no deviations from synthetic NY in the pre-treatment period, the appendix figures do not match on the two years prior to the Sullivan Act.

5.1.1 SCM Results

Figure 2 shows the main results from our SCM analysis. In each graph in Figure 2, the solid line represents the outcome for NY and the dashed line measures the synthetic counterfactual, which is constructed using the process outlined in Equation 1.[23] The dotted line further shows the raw average outcome across all states excluding NY. Across all four panels, synthetic NY nearly reflects actual NY prior to treatment. Panels A and B reveal no distinctive difference in the post-treatment evolution of NY homicide or suicide rates relative to their respective synthetic controls. In contrast, there is a clear decline in gun suicide rates in Panel C relative to the synthetic control and perhaps a slight relative increase in non-gun suicide rates in Panel D. These differences persist through 1920 and suggest an average decrease in gun suicide rates of 1.05 or 32 percent each year and, to the degree that non-gun suicide rates are affected, the difference suggests an average increase of 1.33 or 12 percent each year. We also note that the pattern of the estimates implied by the SCM results, particularly for gun-related suicides, is evident when comparing the raw NY outcomes (solid line) to the average outcomes of other states (dotted line). These comparisons also confirm that the effects occur following the enactment of the law, which speaks to concerns regarding potential endogeneity of the Sullivan Act.

To provide support for our SCM results we explore a series of placebo exercises and an approach to inference following Abadie et al. (2010). We first show a graphical comparison of the SCM results after reassigning the same treatment period (1911) to each of our nine control states. That is, we construct a synthetic control that minimizes the pre-treatment RMSPE for each control state and plot the difference in the actual and synthetic outcomes for each state. This approach allows us to compare the results we see in NY (Figure 2) to potential effects in other placebo states that were not subject to the restrictive gun regulations of the Sullivan Act. Following Abadie et al. (2010), we also calculate the ratio of the post RMSPE to the pre RMSPE for each state in order to determine the likelihood that the effects we see in NY are a matter of chance. Intuitively, a credible match in pre-treatment NY should lead to a low pre-treatment RMSPE and large deviations from the synthetic control after treatment should lead to a large post-treatment RMSPE. For this reason, we expect the post RMSPE to pre RMSPE ratio to be high in NY relative to other states for outcomes with an observed effect in Figure 2 (gun-related suicide and non-gun suicide). After calculating the post RMSPE to pre RMSPE ratio for each state, we rank the ratios from highest to lowest and calculate a p-value for inference. Notably, with our limited sample of 10 states, the lower bound for this calculation is 1/10.

[23] SCM optimization constructs weighted control group for each of our outcomes — Homicides: IN (.764), RI (.236); Suicides: CT (.405), ME (.009), MI (.081), NJ (.335), RI (.148), VT (.022); Non-Gun Suicides: CT (.202), ME (.072), MI (.206), NJ (.520); Gun Suicides: ME (.126), NJ (.766), VT (.108).

We report the results for this exercise in Figure 3. The dark solid lines in each graph represent the difference between actual NY and synthetic NY, and the gray lines represent the difference between a control state and its synthetic state. Figure 3 panels A and B confirm that any differences seen in the post-treatment periods for homicide and suicide outcomes are not apparent outliers. P-value calculations for these outcomes are 0.9 and 0.4, respectively. For the gun suicide rate in Panel C, the pattern reinforces our findings in Figure 2 as the plotted difference in NY is a lower envelope to the differences seen in the placebo states, and the constructed p-value is 0.1. The visual evidence is less clear for gun-related suicide rates in Panel D, though the constructed p-value is also 0.1. The low constructed p-value is, in large part, due to the relatively low RMSPE in the pre-treatment period—i.e. it is evident in the plots that there is a better match between the raw data and the synthetic control for that outcome than for other outcomes in the pre period. As such, we view the results in Figure 3 together with those in Figure 2 as initial support that the Sullivan Act led to a large decrease in gun suicide rates and tenuous evidence for potential increases in the gun suicide rate.

5.2 Difference-in-Differences Model

We next explore the effects of the Sullivan Act in a difference-in-differences framework employing the following baseline regression:

$$y_{sy} = \beta_1 \mathbb{1}[NY_s = 1 \ \& \ PostSA_y = 1] + X_{sy}\Gamma + \theta_s + \delta_y + \varepsilon_{sy}, \tag{2}$$

where s indexes state and y indexes year. Similar to our SCM model, y_{sy} measures our mortality outcomes of interest including the homicide rate, suicide rate, gun suicide rate, and non-gun suicide rate. θ_s are state fixed effects, δ_y are year fixed effects and ε_{sy} represent unobserved factors. We also include state-year demographic and policy control variables. Demographic measures include the fractions of the state population that are non-white, illiterate, foreign born, Catholic, Democrat, and under the age of 10. Policy controls include indicators for state-level suffrage and alcohol prohibition laws, and the number of state executions. Finally, we weight the regressions by the average population of each state. Our parameter of interest is β_1, which measures the effect of the NY Sullivan Act on mortality outcomes.

While we show standard errors corrected for clustering at the state level, we acknowledge that our main estimation sample, which includes a balanced panel of 10 states over 21 years, provides relatively few state clusters.[24] Few clusters may lead to downwards-biased standard errors (Bertrand et al., 2004). We also

[24]For ease of comparison, we settled on showing estimates using the same balanced panel as our SCM approach. Estimates using all possible states (15 states over 30 years) yield similar results.

calculate p-values for our difference-in-differences estimates using the Wild bootstrap procedure suggested by Cameron et al. (2008).[25]

5.3 Results: Difference-In-Differences

We next show the results of from our difference-in-differences model in Table 2. Column 1 shows baseline estimates from Equation 2 that only includes state and year fixed effects. We then add demographic controls described in our data section in Column 2 that account for state-by-year changes in literacy, immigration, race, age, religiosity, and political affiliation. In Column 3, we include state-specific policy controls including indicators for suffrage laws, alcohol prohibition laws, and the number of state-sanctioned executions. We show standard errors corrected for clustering at the state level in parentheses and the associated Wild Bootstrap p-values in brackets.

Consistent with the SCM results, the estimates in Table 2 suggest no clear effects on the homicide rate following the enactment of the Sullivan Act. The coefficient estimates for total suicide rates show a consistent negative effect with varying precision across columns that appears to be driven by a significant decrease in the gun suicide rate. The estimate in Column 3 suggests a 1.61 decrease in the gun suicide rate, which is a 48 percent decrease relative to the mean gun suicide rate in NY. This is slightly larger than the decrease shown in our SCM analysis. Moreover, the estimated effects on gun suicide rates are precisely measured across all specifications in Table 2. Finally, the estimates on non-gun suicides are either positive or negative and are relatively noisy.

5.3.1 Robustness Checks

To explore the stability of our estimates, we conduct several robustness exercises. First, we show alternatives to modeling the outcome variables as rates.[26] Next, we show a series of charts that explore the results using alternative samples and combinations of our state-year controls to further probe the stability of the estimates. Table 3 shows results using the following alternative transformations of the dependent variable (still measured as rates): inverse hyperbolic sine (IHS), log, and quartic root.[27] We present results using the IHS and quartic root because they closely follow the natural log function for positive values and allow for the value of zero.[28] Columns 1, 2, and 3 present the results with the full set of demographic and policy controlsâĂŤconsistent

[25]We also explored alternative strategies that correct for clustering. We compared our results to results clustered at the state-by-pre/post Sullivan and at the state-by-year and found that the Wild bootstrap procedure that corrects for clustering at the state level yields the most conservative (largest) standard errors.
[26]Box-Cox tests of functional form support our choice to use rates as our outcome, rather than log rates.
[27]Unconditional histograms of the four dependent variables show right skewness, suggesting that a transformation may increase efficiency in the estimation. Since homicides per 100,000 takes the value of zero for six observations, the log is calculated as $ln(y+1)$.
[28]Several recent examples that employ a similar transformation to deal with zeros include Anderson et al. (2016) and Tarozzi et al. (2014).

with Table 2 Column 3. The estimates in Table 3 largely mirror the findings in Table 2 in terms of magnitude and precision.

Finally, in figures 4 and 5 we plot 64 estimated effects on overall and gun suicide rates for the combinations of sample restrictions and state-year control variables listed in each figure.[29] We sort the coefficient estimates by size from negative to positive to highlight the general pattern of estimates across specifications and sample restrictions. We also place blue markers to indicate the specification corresponding to the estimates in Table 2 Column 3. Figure 4 shows the pattern of estimates for overall suicide rates, which are always negative but not precisely estimated in many cases. Figure 5 shows the same for gun suicide rates, which are always negative, always significant at the 90 percent level, and significant at the 95 percent level in all but two of the 64 estimates. We view this as supporting our main results that show suggestive evidence of a decline in overall suicide rates driven by a clear decline in gun related suicide rates.

6 Ancillary Analysis: Post-Sullivan Laws in Other States

Statutes regulating the concealed carrying and ownership of handguns became more common following the passage of the Sullivan Act. In this section, we expand our analysis to determine whether the effects of such subsequent laws in other states are consistent with our main findings on the effects of the Sullivan Act. To do so, we extend our data to 1929 and analyze gun legislation passed in ten additional states: CA, CT, IN, MI, MO, MT, NH, NJ, NC, and OR.[30] While these laws differ to varying degrees from the Sullivan Act, they are similar in that they intend to regulate concealed carrying, handgun ownership, or closely related gun behaviors. We analyze the combined set of post-Sullivan laws in addition to separating the laws into three categories: *may issue* laws that establish a subjective process to obtain a concealed carry license; *ownership* laws that establish a licensing process to own a handgun, and *other* laws that require waiting periods or background checks and are described as "close and longstanding cousins" of the Sullivan Act.[31] In doing so, we can consider whether these breakdowns provide suggestive evidence on the independent effects of the concealed-carry regulation and the handgun license requirement, which are both present in the Sullivan Act but cannot be separately identified.

Table 4 shows the results of this exercise. We first show the effects of passing a gun law, broadly considered, on each of our mortality outcomes using the same model presented in our main analysis (Equation 2). In columns 1-4 in Panel A we do not include NY in the sample, and the estimates reveal meaningful reductions

[29] Similar figures for homicide and non-gun suicide rates are shown in appendix figures A2 and A3.
[30] See Appendix Table A1, for the dates of laws in each state. Law dates were obtained from https://everytownlaw.org and the Duke Center for Firearms Law (https://firearmslaw.duke.edu). This set of laws was chosen by limiting gun legislation to the states that passed legislation prior to the great depression in 1929 and actively reported mortality data to the Census Bureau.
[31] See https://everytownlaw.org/wp-content/uploads/sites/5/2020/10/Final-Brief.pdf.

in overall suicide rates and gun related suicide rates, though the estimated effect on the former is relatively imprecise. These estimates are notably smaller than our main results when focusing solely on the Sullivan Act. Not surprisingly, including NY in the sample leads to slightly larger and more precise estimates on the same outcomes. In Panel B, we consider the effects by the three categories of laws. These results show estimates that are largely similar in sign, but more imprecisely estimated. Perhaps noteworthy, the largest effects are associated with *may issue* laws and there is some slight evidence of an increase in non-gun suicides in the sample that excludes NY.[32] Taken together, these additional results provide supporting evidence that post-Sullivan gun laws led to effects similar to those of the Sullivan Act, albeit smaller in magnitude and less precisely estimated. This is perhaps expected given the strict enforcement of both the ownership and concealed carry components of the Sullivan Act.

7 Conclusion

Passed in 1911, NY State's Sullivan Act established stringent restrictions on concealed carrying and pistol ownership. The law was the first of its kind, an important point of reference for nation-wide debates over firearm regulation, and a model law for those advocating for tighter gun control. Indeed, the Sullivan Act was considered a catalyst that led to a wave of subsequent concealed carry laws in many states in the following decades. The influence of this law has persisted and it is still enforced in the state of NY.

Using unique historical data from multiple sources, we analyze the effect of the 1911 Sullivan Act on gun-related behaviors and mortality outcomes. We show results from both synthetic control methods (SCM) and difference in differences models, as well as an assortment of robustness exercises. Our main results show clear evidence of a 32-48 percent decrease in gun suicide rates following the enactment of the Sullivan Act. These results are stable across modeling and specification choices, are apparent following and not prior to the Sullivan act, and they persist in the observed decade following the law.

Results from our most flexible difference in differences model also show a decrease in overall suicide rates, and a variety of specification checks further highlight consistent negative estimates. In contrast, our SCM plots provide no visual evidence of an effect on overall suicide rates, though inference is limited by our relatively small sample of states. The patterns of our various estimation strategies also provide no consistent evidence for an effect on non-gun suicide rates.

Taken together, we view our results on suicide as suggestive that gun suicide and suicide by other means are imperfect substitutes. In other words, despite possible substitution between methods of suicide, the evidence is supportive of overall declines in suicide. Notwithstanding the stark differences in suicide rates

[32]Note that we categorize NY as having a *may issue* law in columns 5-8 of Panel B.

and methods, these results are largely consistent with existing evidence in current contexts and alternative policy settings.[33] Imperfect substitution between suicide methods is also consistent with evidence that suicide ideation is impulsive and a "cooling off" effect may follow such ideation (Miller et al., 2012). Restrictive gun policies may reduce the tendency to act on such impulses. Our results are also consistent with compositional effects that lead to substitution toward less effective methods, and evidence that most individuals that experience failed suicide attempts end up dying of other causes of death (Owens et al., 2002; Fischer et al., 1993).

In ancillary analyses, we expand our analysis to include states that passed similar laws subsequent to the Sullivan Act. These results support our main findings. As such, our study documents the effects of requiring permits for handgun ownership and licenses for concealed carrying in NY, and highlights the likely effects of the nationwide movement toward similar policies that followed.

Currently in the U.S., roughly two-thirds of all gun deaths are suicides and guns are the method for approximately half of all suicides. This clear link between firearms and suicides stresses the ongoing relevance of this area of inquiry and the need to better understand the extent to which gun regulations can reduce suicide. How to design effective gun policy remains a divisive but critical question and a point of emphasis in the economics literature on guns and suicide (Marcotte and Zejcirovic, 2020). In the context of recent dramatic deregulation of concealed carrying and the rise in suicide in the U.S., our study adds historical insight and an important data point to ongoing debates.

[33]For instance, imperfect substitution is apparent in the context of changes in gun prevalence (Balestra, 2018; Leigh and Neill, 2010), child access prevention laws (Webster et al., 2004), and gun purchase delays (Edwards et al., 2018).

Affiliations

[1]Utah State University
[2]Montana State University

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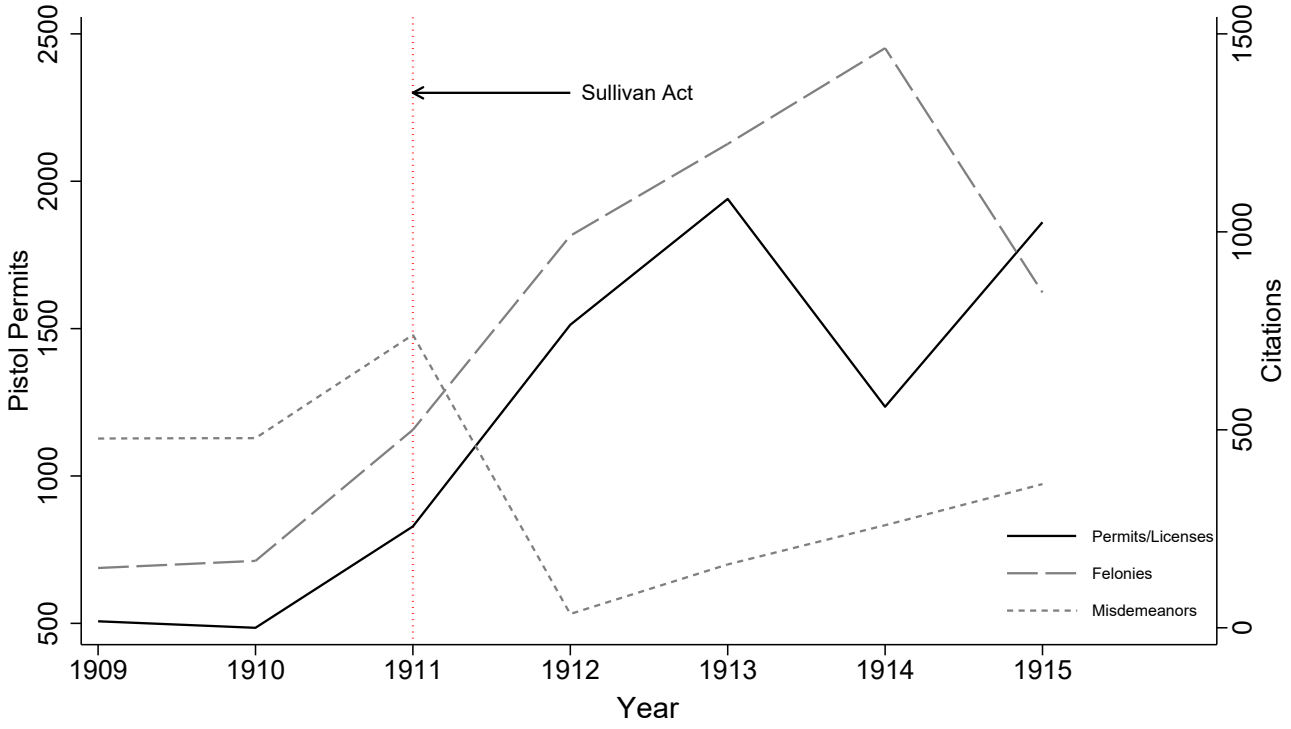
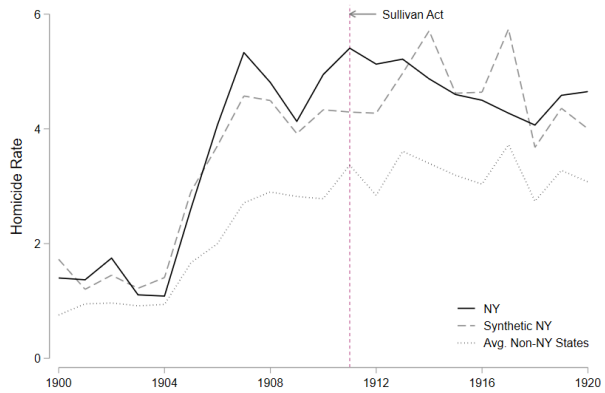
Figure 1
NYC Pistol Permits/Licenses and Citations for Carrying a Dangerous Weapon



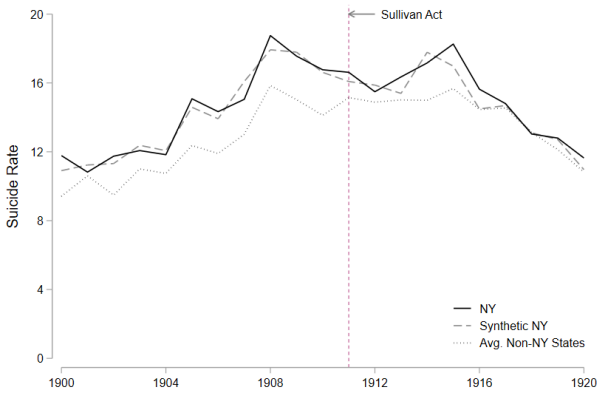
Notes: This graph plots the annual number of felony and misdemeanor citations for carrying a dangerous weapon, and the combined number of pistol permits and concealed carry licenses. The data were obtained from the Annual Report of the Police Department of the City of NY.

Figure 2
Synthetic Control Results

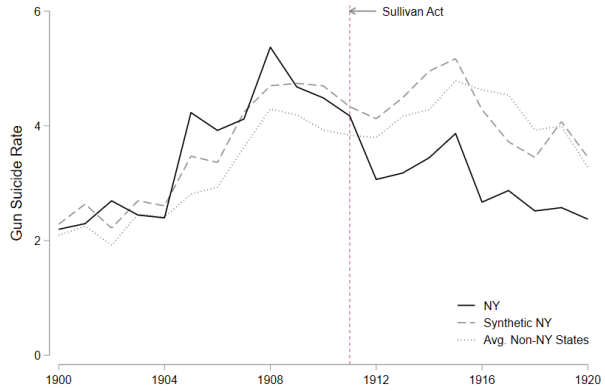
Panel A: Homicide Rate



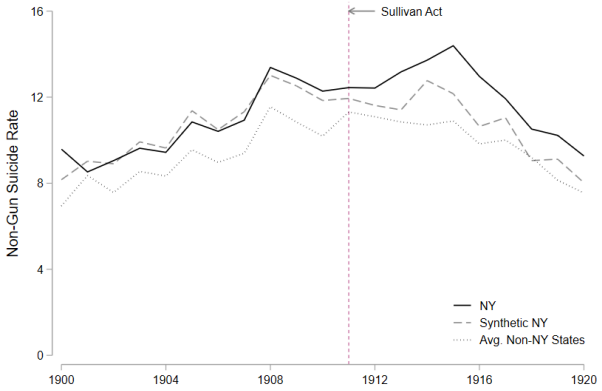
Panel B: Suicide Rate



Panel C: Gun Suicide Rate

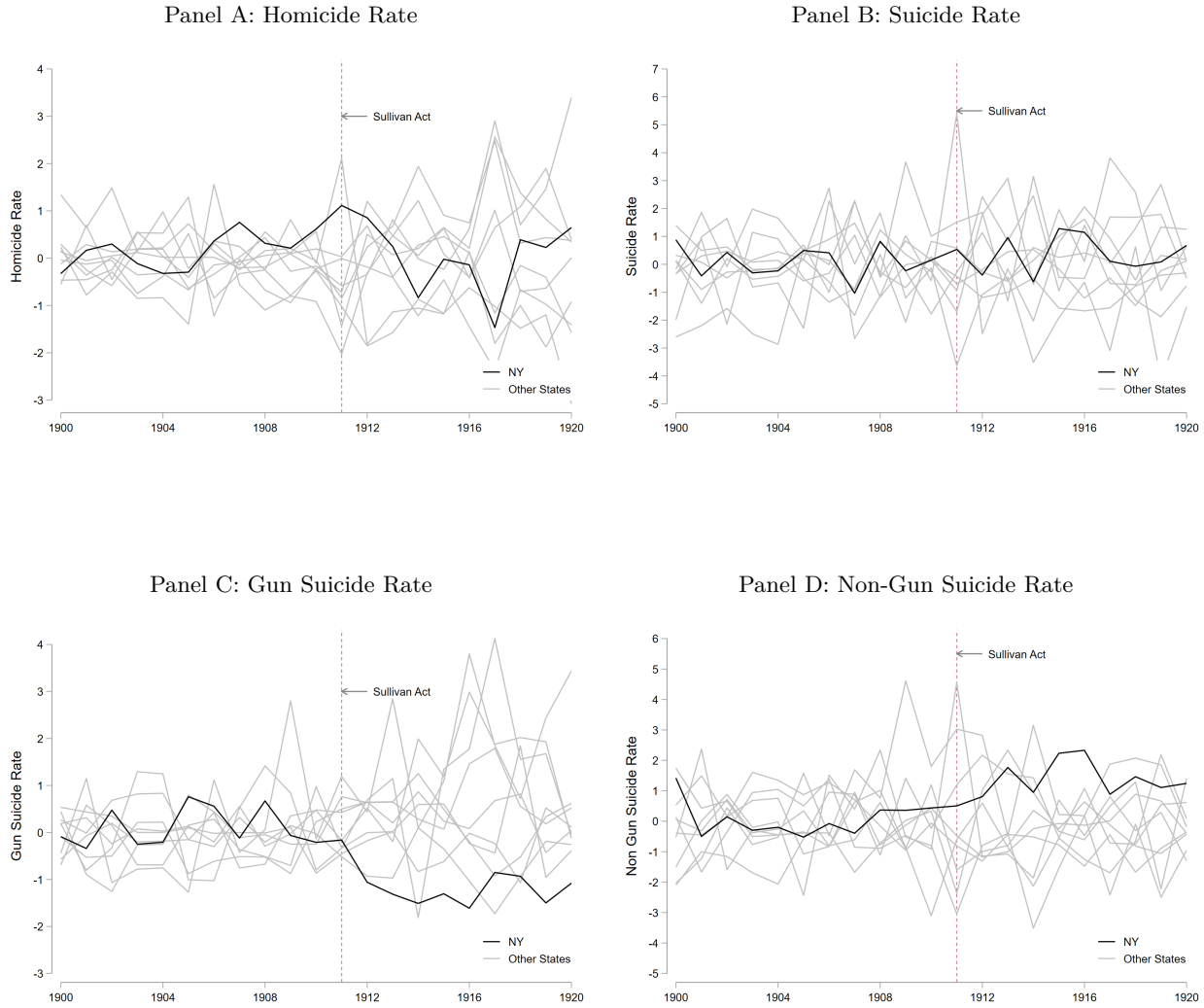


Panel D: Non-Gun Suicide Rate



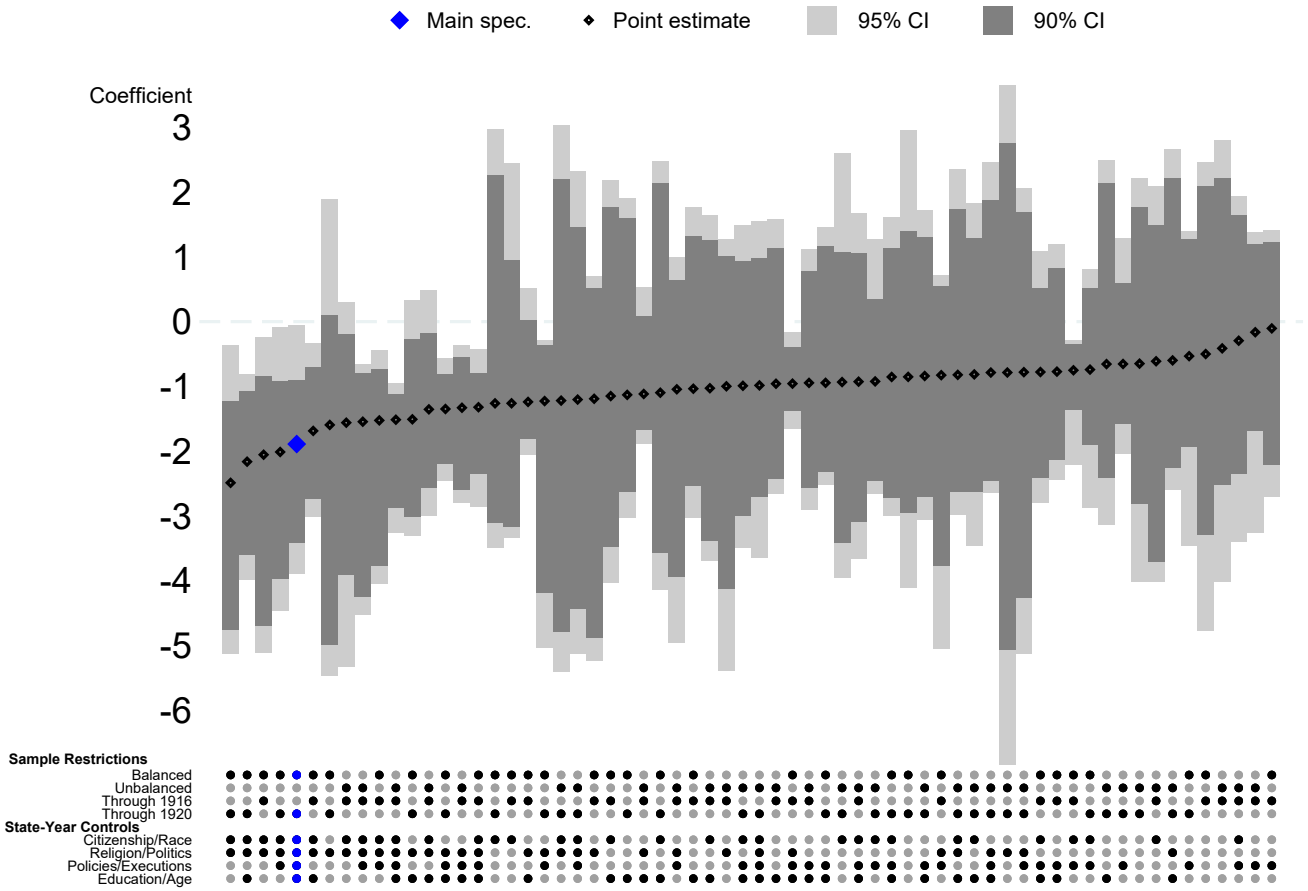
Notes: This graph plots annual mortality rates (per 100,000) for NY, non-NY states, and synthetic NY for the outcomes corresponding to each panel. Following guidance from Ferman et al. (2020), synthetic NY is constructed using the outcome variable in each year prior to treatment as predictor variables. Data were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Figure 3
Synthetic Control Placebo Results



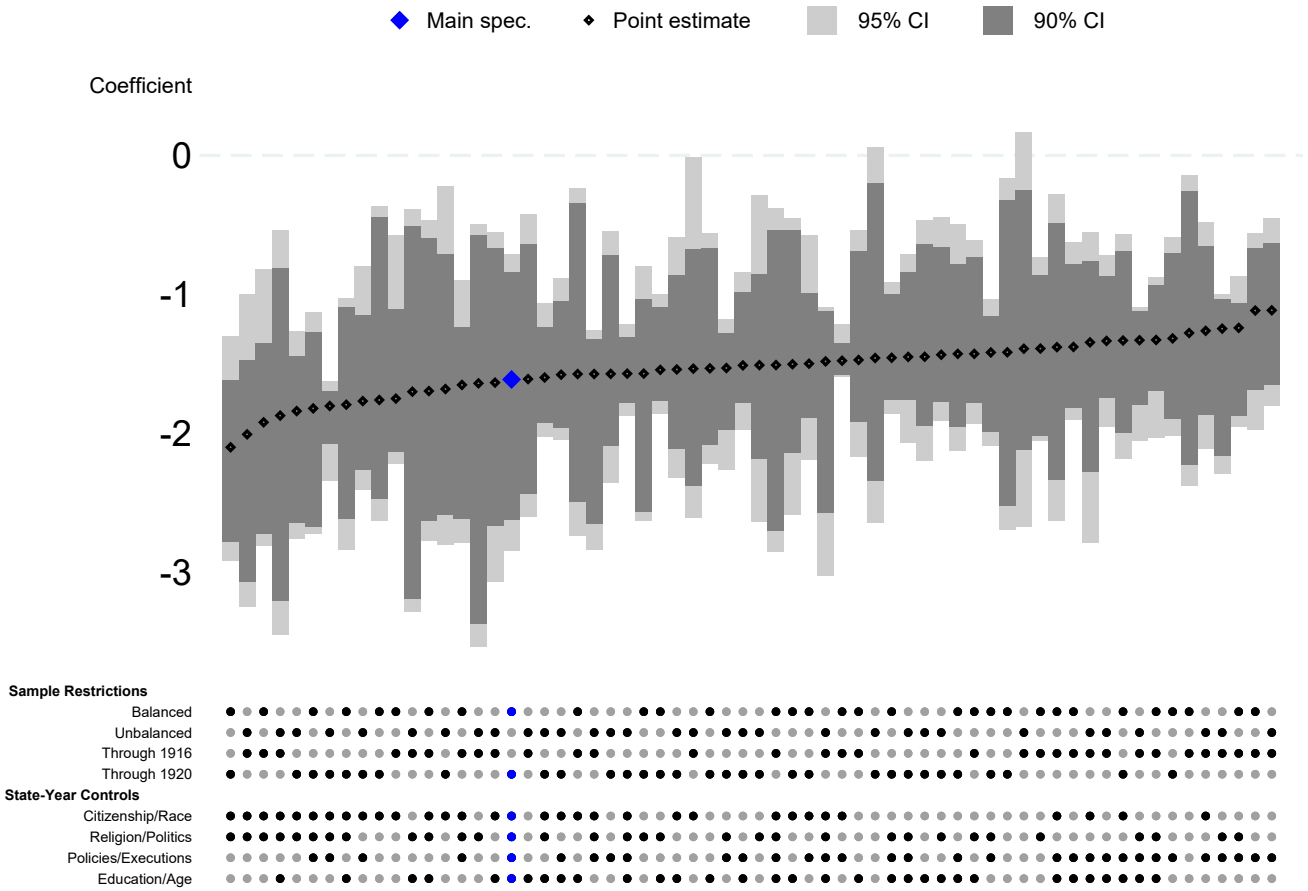
Notes: This graph plots the difference in mortality rates (per 100,000) between each state's raw data and the associated constructed synthetic control for the outcomes corresponding to each panel. Following guidance from Ferman et al. (2020), the synthetic control for each state is constructed using the outcome variable in each year prior to treatment as predictor variables. Data were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Figure 4
Effect on Overall Suicide Rate: Robustness Check



Notes: This graph plots the estimated difference-in-differences coefficients along with the 90 and 95 percent confidence intervals that correspond to the specifications indicated in each column above, including state and year fixed effects. The balanced panel includes 10 states and the unbalanced panel includes 15 states. Confidence intervals are constructed using the Wild bootstrap procedure suggested by Cameron et al. (2008) and the estimates are weighted by state population. Mortality rates (per 100,000) were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Figure 5
Effect on Gun Suicide Rate: Robustness Check



Notes: This graph plots the estimated difference-in-differences coefficients along with the 90 and 95 percent confidence intervals that correspond to the specifications indicated in each column above, including state and year fixed effects. The balanced panel includes 10 states and the unbalanced panel includes 15 states. Confidence intervals are constructed using the Wild bootstrap procedure suggested by Cameron et al. (2008) and the estimates are weighted by state population. Mortality rates (per 100,000) were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Table 1
Summary Statistics

	NY			Other States		
	Pre	Post	Overall	Pre	Post	Overall
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes:						
Homicide Rate	3.07	4.72	3.93	1.98	3.73	2.89
Suicide Rate	14.33	15.13	14.75	12.04	13.81	12.96
Gun Suicide Rate	3.60	3.06	3.32	2.90	3.81	3.37
Non-Gun Suicide Rate	10.73	12.07	11.43	9.12	10.00	9.58
State Characteristics:						
Fraction Illiterate	0.05	0.05	0.05	0.05	0.04	0.05
Fraction Foreign Born	0.29	0.29	0.29	0.22	0.21	0.22
Fraction Non-White	0.02	0.02	0.02	0.02	0.02	0.02
Fraction Age < 10	0.19	0.19	0.19	0.20	0.20	0.20
Fraction Catholic	0.26	0.28	0.27	0.21	0.25	0.23
Per Capita Executions	0.09	0.13	0.11	0.05	0.05	0.05
Fraction Democrat	0.41	0.31	0.36	0.40	0.41	0.40
Observations	11	10	21	99	90	189

Notes: The data were obtained from the U.S. Census Bureau's Mortality Statistics (1900-1920). Rates are calculated per 100,000 population. All summary stats are weighted by state population.

Table 2
Difference-in-Differences Results

	(1)	(2)	(3)
Homicide Rate	-0.04	-0.68	-0.43
(standard error)	(0.23)	(0.25)	(0.24)
[wild p-value]	[0.869]	[0.076]	[0.122]
Suicide Rate	-0.85	-2.16	-1.89
(standard error)	(0.58)	(0.41)	(0.42)
[wild p-value]	[0.286]	[0.019]	[0.045]
Gun Suicide Rate	-1.42	-1.79	-1.61
(standard error)	(0.23)	(0.24)	(0.29)
[wild p-value]	[0.002]	[0.000]	[0.002]
Non-Gun Suicide Rate	0.54	-0.42	-0.30
(standard error)	(0.40)	(0.35)	(0.32)
[wild p-value]	[0.281]	[0.249]	[0.284]
Number of Observations	210	210	210
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Demographic Controls		Yes	Yes
Policy Controls			Yes

Notes: The outcome variables are annual state mortality rates (per 100,000). Demographic measures include controls for the fractions of the state population that are non-white, illiterate, foreign born, Catholic, Democrat, and under the age of 10. Policy controls include indicators for state-level suffrage and alcohol prohibition laws, and the number of state executions. The estimates are weighted by state population, standard errors correcting for clusters at the state level are presented in parentheses, and Wild bootstrapped p-values are presented in brackets.

Table 3
Difference-in-Differences Results: Alternative Dependent Variables

	(1) Inv Hyp Sine	(2) Log Rate	(3) Quart Root
Homicide	-0.146	-0.131	-0.021
(standard error)	(0.150)	(0.077)	(0.040)
[wild p-value]	[0.323]	[0.095]	[0.583]
Suicide	-0.119	-0.112	-0.059
(standard error)	(0.029)	(0.027)	(0.014)
[wild p-value]	[0.022]	[0.029]	[0.032]
Gun Suicide	-0.433	-0.344	-0.151
(standard error)	(0.105)	(0.074)	(0.033)
[wild p-value]	[0.010]	[0.001]	[0.009]
Non-Gun Suicide	-0.022	-0.021	-0.011
(standard error)	(0.031)	(0.028)	(0.014)
[wild p-value]	[0.418]	[0.436]	[0.407]
Number of Observations	210	210	210
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Demographic/Policy Controls	Yes	Yes	Yes

Notes: The outcome variables are annual state mortality rates (per 100,000) transformed according to each column title. Demographic measures include controls for the fractions of the state population that are non-white, illiterate, foreign born, Catholic, Democrat, and under the age of 10. Policy controls include indicators for state-level suffrage and alcohol prohibition laws, and the number of state executions. The estimates are weighted by state population, standard errors correcting for clusters at the state level are presented in parentheses, and Wild bootstrapped p-values are presented in brackets.

Table 4
Difference-in-Differences Results: Extension to Additional States and Laws

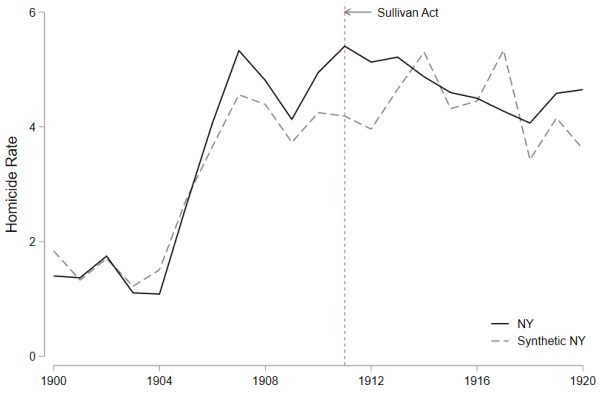
	Drop New York				Keep New York			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Homicide	All Suicide	Gun Suicide	Non-Gun Suicide	Homicide	All Suicide	Gun Suicide	Non-Gun Suicide
Panel A: Any Law								
Any Law	0.02	-1.01	-0.86	-0.14	-0.26	-1.16	-1.14	-0.01
(standard error)	(0.54)	(0.62)	(0.31)	(0.48)	(0.32)	(0.41)	(0.22)	(0.39)
[wild p-value]	[0.98]	[0.26]	[0.05]	[0.86]	[0.49]	[0.09]	[0.01]	[0.98]
Panel B: Law Types								
May Issue Law	-0.03	-0.56	-1.19	0.62	-0.46	-1.00	-1.54	0.54
(standard error)	(0.95)	(0.71)	(0.51)	(0.32)	(0.42)	(0.38)	(0.27)	(0.29)
[wild p-value]	[0.98]	[0.58]	[0.15]	[0.06]	[0.34]	[0.18]	[0.01]	[0.12]
License Law	0.40	-1.76	-0.52	-1.21	0.49	-1.67	-0.34	-1.31
(standard error)	(0.60)	(1.22)	(0.43)	(0.86)	(0.57)	(1.22)	(0.40)	(0.90)
[wild p-value]	[0.58]	[0.67]	[0.69]	[0.63]	[0.46]	[0.69]	[0.77]	[0.65]
Other Law	-0.77	-0.91	-0.39	-0.45	-0.77	-0.94	-0.41	-0.46
(standard error)	(0.42)	(0.65)	(0.45)	(0.35)	(0.45)	(0.66)	(0.42)	(0.37)
[wild p-value]	[0.27]	[0.81]	[0.79]	[0.48]	[0.31]	[0.79]	[0.78]	[0.55]
Number of Observations	667	667	667	667	697	697	697	697
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic/Policy Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The outcome variables are annual state mortality rates (per 100,000) corresponding to each column title. Demographic measures include separate controls for the fractions of the state population that are non-white, illiterate, foreign born, Catholic, Democrat, and under the age of 10. Policy controls include indicators for state-level suffrage and alcohol prohibition laws, and the number of state executions. The estimates are weighted by state population, standard errors correcting for clusters at the state level are presented in parentheses, and Wild bootstrapped p-values are presented in brackets.

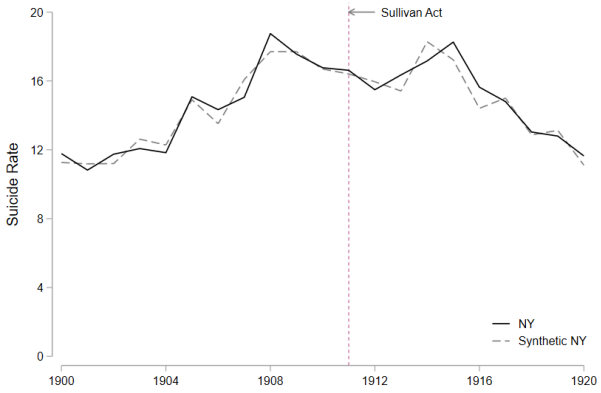
8 Appendix

Figure A1
Synthetic Control Results: matching on pre-treatment outcomes and covariates

Panel A: Homicide Rate

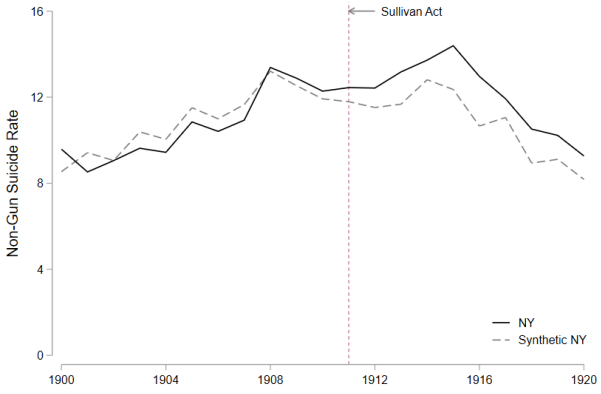


Panel B: Suicide Rate



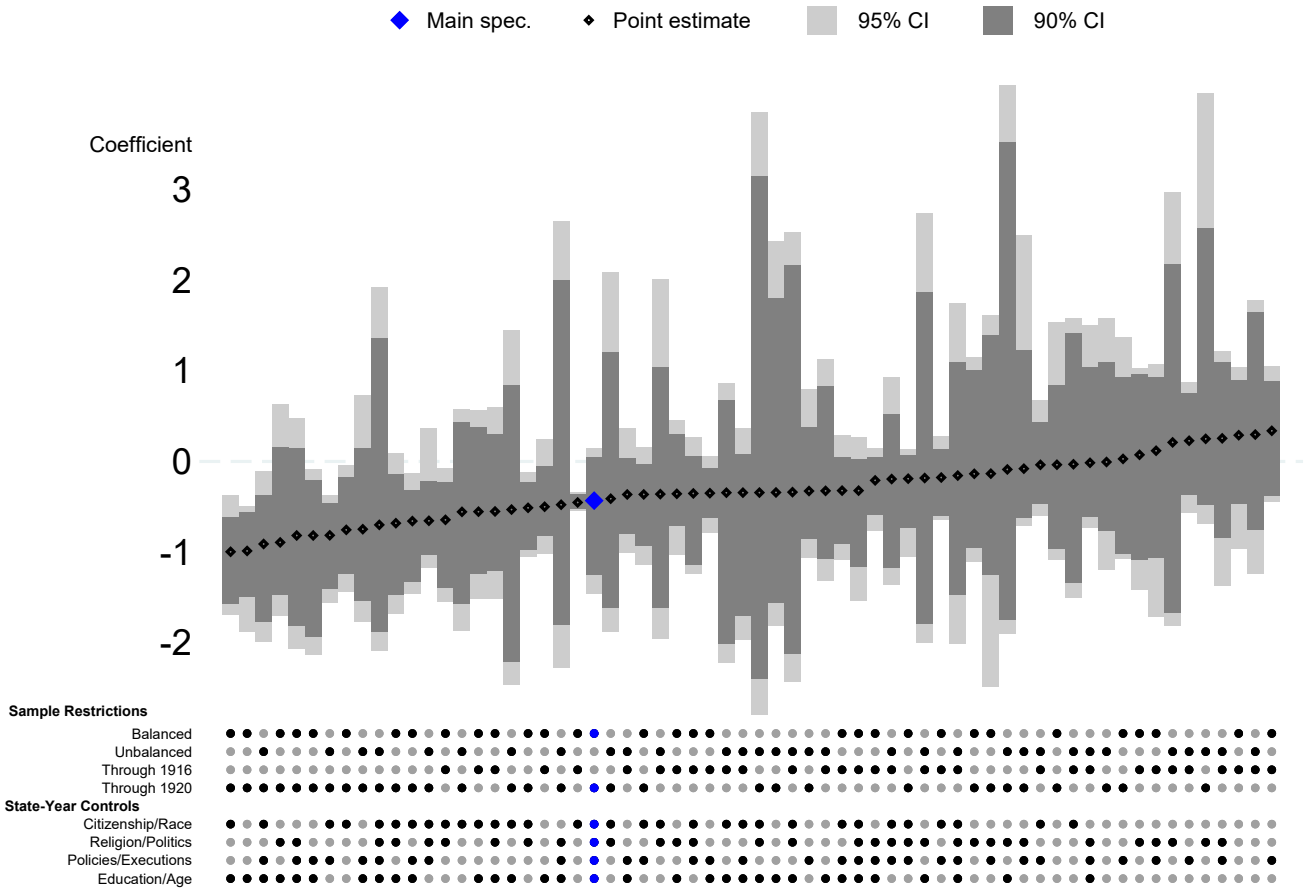
Panel C: Gun Suicide Rate

Panel D: Non-Gun Suicide Rate



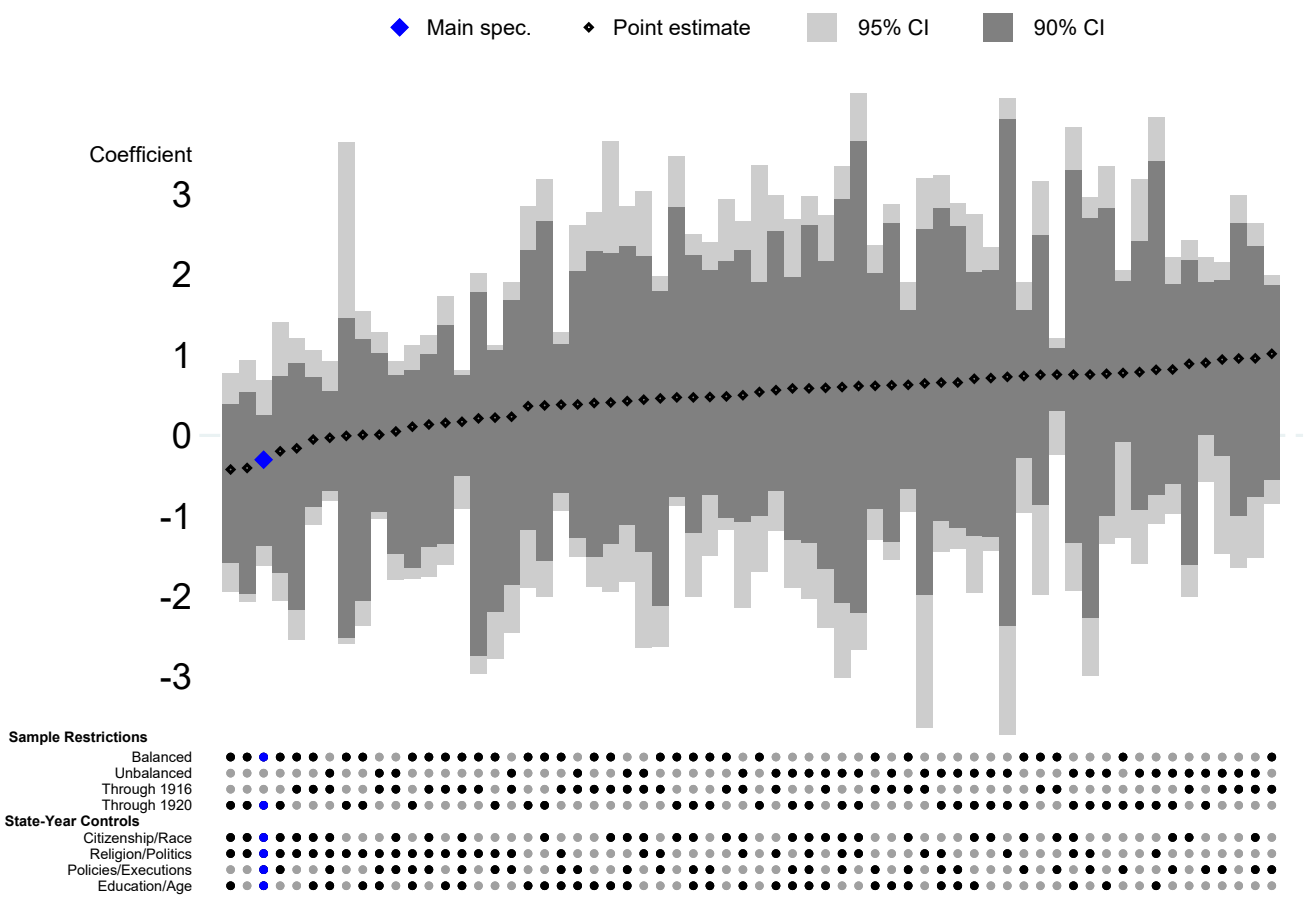
Notes: This graph plots annual mortality rates (per 100,000) for NY, and synthetic NY for the outcomes corresponding to each panel. Synthetic NY is constructed using the outcome variable and the covariates listed in Table 1 as predictor variables. Data were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Figure A2
Effect on Homicide Rate: Robustness Check



Notes: This graph plots the estimated difference-in-differences coefficients along with the 90 and 95 percent confidence intervals that correspond to the specifications indicated in each column above, in addition to state and year fixed effects. The balanced panel includes 10 states and the unbalanced panel includes 15 states. Confidence intervals are constructed using the Wild bootstrap procedure suggested by Cameron et al. (2008) and the estimates are weighted by state population. Mortality rates (per 100,000) were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Figure A3
Effect on Non-Gun Suicide Rate: Robustness Check



Notes: This graph plots the estimated difference-in-differences coefficients along with the 90 and 95 percent confidence intervals that correspond to the specifications indicated in each column above, in addition to state and year fixed effects. The balanced panel includes 10 states and the unbalanced panel includes 15 states. Confidence intervals are constructed using the Wild bootstrap procedure suggested by Cameron et al. (2008) and the estimates are weighted by state population. Mortality rates (per 100,000) were obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1920).

Table A1
Dates for Ancillary Analysis of Other State Laws

State	Law Type	Law Date
California	1923	May Issue
Indiana	1925	May Issue
Oregon	1925	May Issue
Michigan	1925	May Issue
New Jersey	1925	May Issue
Missouri	1918	Ownership
Montana	1918	Ownership
North Carolina	1919	Ownership
Connecticut	1923	Other
New Hampshire	1923	Other

Notes: *May Issue* laws establish a process to obtain a concealed carry license that includes subjective determination of eligibility by the local authority; *Ownership* laws establish a licensing process to own a handgun, and *Other* laws that require waiting periods or background checks and are described as "close and longstanding cousins" of the Sullivan Act.