

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

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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 permit, established a stringent set of rules for obtaining a permit, and introduced regulations governing the sale and possession of firearms. 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 the efficacy of its efforts to curb gun violence in New York. To analyze the effects of the Sullivan Act, we collected unique historical data including state mortality records, pistol permit data, and information on citations for carrying without a permit. Our main empirical strategy employs both synthetic control and difference-in-differences methodologies to estimate the effects of the Sullivan Act. Our initial analysis of gun permits and citations for illegal carrying reveal clear first-stage effects of the Sullivan Act on gun-related behaviors. Our main analyses on societal safety shows no evidence for an effect on homicides or overall suicides. However, we find clear evidence that the Sullivan Act led to a 35 percent decrease in per-capita suicide by firearm and suggestive evidence for a substitution away from firearm suicides to suicide by other means.

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1 Introduction

State legislatures began implementing a first wave of laws targeting concealable weapons in the 19th century. Unlike modern concealed-carry laws that typically establish legal pathways to carry concealed firearms, these early 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 permits and severely penalized concealed carrying without a permit. 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 right to carry a concealed firearm.

In this paper, we conduct the first rigorous analysis evaluating the effects of early concealed-carry legislation, focusing on one particularly prominent legislative act—New York State’s 1911 Sullivan Act. The objective of the Sullivan Act was to curb gun violence in the state of New York primarily through the establishment of a felony penalty for carrying a concealed weapon without a permit.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 (New York Times, May 11, 1911).” The Sullivan Act was also 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 the past century, this law is still an

¹The Sullivan Act also stipulated any possession of a concealable weapon as a misdemeanor offense. See 1911 N.Y. Laws ch. 195, sec. 1, 1897 and N.Y. Penal L. 1897 (1909)

²Though four states passed statutes restricting concealed carrying prior to 1911, these laws became more

enforced statute in New York State law. 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 restrictive gun control affects public safety.

To explore the effects of the Sullivan Act we bring together historical data from multiple sources that allow us to examine the effect of restrictive gun control leads on illegal gun ownership, gun carrying, and measures of public safety. In particular, our data include measures for permitted handguns, felony citations for illegal gun carrying, and gun-related mortality outcomes. Our primary empirical strategy employs a synthetic control design to compare homicides and suicides in New York to other states before and after the implementation of the Sullivan Act. We also explore estimates using a difference-in-differences framework, and show that the main findings are sufficiently regular to be evident in descriptive plots of the data.

Our analysis reveals that the restrictions brought about by the Sullivan Act led to stark increases in permitted handguns and citations for illegal gun carrying, which suggest effects on both citizens' gun-carrying behaviors and law enforcement of the Sullivan statutes. While our estimates show that these changes had no clear effects on homicide or suicide rates, we find large decreases in gun suicide, and increases, albeit noisy, in non-gun related suicide. While the reduction in gun-related suicide rates is more precisely identified and often larger in magnitude than the increase in non-gun suicides, that we find no effect on overall suicide rates suggests a substitution away from guns toward alternative methods for suicide due to the passage of Sullivan Act. Our robustness checks highlight that the effects are apparent following but not prior to the passage of the Sullivan Act, and that the estimates are similar under alternative modeling decisions.

Our estimates offer a first look at the effects of a particularly prominent conceal-carry law in the early 20th century. From a historical perspective, our analysis of the Sullivan Act

common after the passage of the Sullivan Act, and by 1938 all but two states restricted or required a permit for concealed carrying (Warner, 1938).

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.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 permitting process.⁴ 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 in terms of consensus opinion 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 New York. Moreover, as one of the more restrictive gun regulations in U.S. history, the Sullivan Act provides a unique opportunity to credibly identify effects on gun-carrying and public safety.⁶ 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

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

⁴The pattern of moving toward shall-issue laws continued into the 21st century, with several states recently expanding to unrestricted carry provisions that eliminate the need for a permit to carry a concealed firearm. In 2014, Illinois became the 50th state to legalize a permit process for concealed carrying.

⁵Since the early work of 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) found a deterrent effect of concealed-carry laws on crime. Similarly, Bronars and Lott (1998), Lott (1998), Moody (2001), Plassmann and Tideman (2001), Olson and Maltz (2001), Mustard (2001), and Moody et al. (2014) found supporting evidence for a deterrent effect. 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 that relaxed carrying laws have either no significant effect on crime or slight increases in certain types of crime.

⁶Brabner-Smith (1933) characterized the Sullivan act as “the severest anti-firearm legislation in the United States.”

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, several studies have demonstrated a reduction in suicide rates following the implementation of state-level restrictive gun policies including mandatory handgun purchase delays (Edwards et al., 2018) and child access prevention laws (Webster et al., 2004). On the other hand, 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 by establishing a clear reduction in gun-related suicides and evidence for substitution toward non-gun suicides following the establishment of a may-issue permitting process and felony penalties for concealed carrying without a permit.

2 Historical Context

In the wake of two tragic New York City events, State Senator Timothy D. Sullivan’s bill to establish concealed carrying without a permit as a felony offense had little opposition. The first tragedy occurred on August 9, 1910, as New York City mayor, William J. Gaynor, was posing for a photograph while waiting to board the German steamship at the Hoboken Pier. J. J. Gallagher stepped out from the crowd and shot the mayor, hitting him in the neck. Gallagher had been dismissed from the New York City Dock Department on July 19th, and had been “haunting the Mayor’s office in a vain attempt to get his job back” (New York 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 New York

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. The law applying to the carrying of concealed weapons is farcical, and does not meet present conditions” (New York Times, January 30, 1911). In response, Senator Timothy D. Sullivan introduced his bill to the state legislature in early 1911 (New York Times, Jan 30, 1911).⁷ Only a few voices in the state legislature opposed the bill (New York Times, May 11, 1911).⁸

The Sullivan Act was signed on May 30, 1911 and went in effect on September 1, 1911 (New York Times, Aug 29, 1911). Under the act, citizens were required to obtain a permit to possess and carry a concealable weapon.⁹ Possession of a firearm without a permit was a misdemeanor offense and carrying without a permit 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 the state’s first gun regulation, prior to the law the state penal code stipulated that it was a misdemeanor offense to carry a concealed weapon without a license in cities or villages that required them (New York Penal Code 1905). For example, at least as early as 1897, New York City required a pistol permit to carry a pistol in the city and the annual permit fee contributed to the police department’s pension fund (New York City Charter 1897). However, our subsequent analysis demonstrates a dramatic increase in pistol permits following the 1911 legislation.

⁷Historical 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 (see Welch (2009)).

⁸For instance, New York State Senator Ferris “Your bill won’t stop murders. You can’t force a burglar to get a license to use a gun” (New York Times, May 11, 1911).

⁹Concealable 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. 1897 (1914))

¹⁰See Hansen (1976) and Kopel (2016) for a detailed historical account of the passage of the Sullivan Act.

Obtaining a 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 in “good reason.” The police force held considerable authority in determining the issuance of permits. According to Kopel (2016), New York City’s police commissioners held the view that residents should not have handguns. Kopel (2016) argues that “[N]o matter the reason a New York City applicant might give for wanting a handgun (e.g., target shooting, self-defense), the applicant would be told that the reason was not good enough.” On November 27th, 1911 the New York 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.”

Indeed, 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 ... New York State, than where there are few or no regulatory provisions” (Brabner-Smith, 1933). Our analysis provides a systematic approach to offer needed quantitative evidence of the effects of the Sullivan Act on gun-related behaviors, law enforcement practices, and gun-related mortality outcomes.

3 Data

We utilize newly digitized historical data from multiple sources. Our main analysis uses mortality records from the U.S. Census Bureau’s Mortality Statistics. The U.S. Census Bureau

began publishing state-level mortality statistics in 1900. Initially, only ten states participated in the publication including Connecticut, Indiana, Massachusetts, Maine, Michigan, New Hampshire, New Jersey, New York, Rhode Island, and Vermont. By 1916, 26 states were included.¹¹ These data allow us to compare homicides and suicides in New York to other states over the same years. For suicides, these measures provide a breakdown of suicide by cause of death, allowing us to measure effects on gun suicide.¹² For our main analysis, we restrict the sample to a balanced panel of states for the years 1900 through 1916, which is the final year prior to the U.S.’s entry into World War I.¹³ We calculate mortality rates per 100,000 using state population estimates obtained from the St. Louis Federal Reserve Bank.

We also use archived data specific to New York as a supplement our main analysis. Sources for these data include New York City police reports, New York City’s Department of Health, and various sources gathered by Monkkonen (2006). We use data from annual New York City police reports to measure the number of pistol permits and the number of citations for carrying a dangerous weapon. These measures provide insight into initial effects on gun-related behaviors following the enactment of the Sullivan Act.¹⁴

Department of Health data include the monthly number of homicides and suicides in New York City for the years 1909-1913. These data allow us to explore descriptive changes in monthly mortality outcomes surrounding the 1911 Sullivan Act that may be obscured by the annual U.S. Census reports.

Finally, we use data from Monkkonen (2006) who gathered more detailed incident information for homicides occurring in New York City from various archived sources.¹⁵ These data allow us to graphically explore homicides separately by those that are gun-related, which is not possible using the U.S. Census data in our sample.

¹¹It wasn’t until 1933 that all of the lower 48 states were included in the Census Bureau’s publication.

¹²Unfortunately, the homicide data collected by the Census do not provide a similar breakdown until 1910

¹³Our estimates are similar with the inclusion of states that began reporting to the Census after 1900.

¹⁴Note that the annual police reports do not clarify whether a citation for “carrying a dangerous weapon” was limited to concealed carry without a permit.

¹⁵These include the New York City Police Department, the City Inspector, the New York City Municipal Archives, and daily newspaper archives.

In tables 1 and 2, we show descriptive statistics for the data used in our analysis. Table 1 Panel A shows our main mortality outcomes and highlights high homicide and suicide rates in NY relative to the other states in our sample. Panel B of Table 1 shows mean outcomes used in our ancillary analysis and the years where these measures are available. In Table 2 we show the means prior to and following the 1911 Sullivan Act. These show increases in all outcomes in Panel A with the exception of gun suicide rates in New York. The summary measures in Panel B are consistent with increases in felony gun carrying, pistol permits, and homicides, and decreases in misdemeanor gun carrying. We explore these patterns in more detail in subsequent sections.

4 Graphical Evidence

We begin by exploring graphical patterns of pistol permits and citations for carrying a dangerous weapon in New York City leading up to and following the Sullivan Act. The degree to which these outcomes are responsive to the Sullivan Act can offer proof of concept demonstrating that the SA led to changes in gun-related behaviors. We then examine graphical patterns of mortality outcomes in New York using the various sources of data mentioned previously.

4.1 Gun-Related Behaviors

The Sullivan Act required a permit to possess a handgun and elevated the penalties for carrying a concealable weapon without a permit from a misdemeanor to a felony classification. While local magistrates did issue concealed-carry permits prior to 1911, the requirements introduced by the Sullivan Act, if binding, likely led to changes in compliance of citizens seeking permits and changes in policing of officers tasked with enforcing the new statutes. To provide insight on changes in compliance, Figure 1 shows the number of issued pistol permits from New York City annual police reports surrounding the Sullivan Act. Figure 1 highlights

a sharp increase in the number of permits following the enactment of the Sullivan Act in September of 1911. We view this as evidence that the Sullivan Act may have contributed to a more-than-doubling of the annual number of pistol permits issued in New York City.

Using the same data source, we next explore the trends in felony and misdemeanor citations for carrying a dangerous weapon. While the data from the annual police reports does not differentiate between handguns and other dangerous weapons, changes in policing behaviors coincident with the Sullivan Act can help inform whether the law was enforced.¹⁶ Moreover, handgun use and carrying were the clear focal point of historical records discussing the implementation and enforcement of the Act.

Figure 2 shows a stark increase in felony citations for carrying a dangerous weapon starting in 1911 and continuing through 1915. Misdemeanor citations show a slight increase in 1911, but a decrease in 1912 before continuing an upward trend. This is consistent with a substitution toward felony citations for concealed-carrying violations, followed by increases in misdemeanor offenses that include handgun possession without a permit in the years following the Sullivan Act. Taken together, Figures 1 and 2 provide evidence that the Sullivan Act increased both civilian compliance with the Act’s permit requirement, and the law enforcement of the statutes in New York City.

4.2 Mortality Outcomes

We next explore graphical trends in homicide and suicide surrounding the passage of the Sullivan act. We initially continue to focus on data from New York City for two main reasons. First, the available New York City data include monthly data allowing us to explore short-run dynamics and annual data that break down homicides into gun-related and non-gun related incidents. These details are not available in the Census data used in our main analysis. Second,

¹⁶Citations made by the detective division were not included in the Annual Report of the Police Department of the City of New York until 1911. Thus, the drastic 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 presents a compelling argument of changes in policing behavior as a result of the Sullivan Act.

much of the historical dialogue surrounding the Sullivan Act focuses around happenings in and around New York City, so these data provide outcome measures in the location where the Sullivan Act policies may be most salient. After focusing on New York City data, we shift our focus to plots using our primary data source that includes statewide mortality outcomes from the Census.

Figure 3 presents monthly homicide and suicide counts in New York City using the available data from 1909-1913. The figure provides no clear evidence that homicides or suicides changed with the enactment of the Sullivan Act in September of 1911. Similarly, Figure 4 reveals no apparent changes in annual gun or non-gun homicides in New York City.

Focusing on statewide annual mortality data digitized from U.S. Census reports, Figure 5 shows per capita (per 100,000 population) measures of homicides, total suicides, gun suicides, and non-gun suicides for the years 1900-1916 separately for NY and the average of the remaining nine states in the data. Panel A shows an increase in homicide rates from 1904 to 1907 before leveling-off in later years; the increase was particularly pronounced in New York. Panel B also shows a similar increasing trend in suicide rates before leveling off. For gun suicides, Panel C shows evidence of an absolute and relative decrease in New York's gun suicide rate coinciding with the Sullivan Act. Finally, non-gun suicide rates in Panel D follow a similar pattern as overall suicide rates in each case.

5 Empirical Strategy

Our empirical approach compares homicide and suicide rates in New York to similar states before and after the implementation of the New York Sullivan Act. Given the various sensible methods to construct a credible counterfactual, we opt for a transparent three-pronged approach. We start by showing residual plots that build upon our graphical analysis in the previous section, but condition on state and year fixed effects in our sample of 10 states. In our preferred approach, we employ a synthetic control design that constructs a counterfactual

from a weighted average of outcomes in our control states to compare with mortality outcomes in New York. Finally, we present traditional difference-in-difference estimates. We next discuss the latter two approaches in more detail.

5.1 Synthetic Control Method

The 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.” The method is well suited for our context where we have one treatment group, few control groups, and few observational units (Cameron and Miller, 2015). 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:

$$\|X_1 - X_0W\|_V = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)}, \quad (1)$$

where X_1 and X_2 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. In practice, our pre-treatment characteristics include the outcome variable in addition to measures for the fraction of the population that are less than age 25, black, catholic, literate, and foreign born.¹⁷

Our donor pool of control states consists of the nine available states that report mortality outcomes consistently from 1900 through 1916. This sample restriction is due to the balanced

¹⁷Linearly interpolated age, race, education, and foreign born measures were obtained from the decennial census. Fraction Catholic was interpolated using the 1906, 1916, and 1926 Census of Religious Bodies.

sample requirement in the SCM as well as our desire to utilize the longest feasible pre-treatment window to facilitate a credible synthetic control.¹⁸ To avoid overfitting, we match on the characteristics mentioned above for the pre-treatment years 1900, 1902, 1906, and 1908.¹⁹

5.2 Difference-in-Differences Model

As an alternative strategy, we explore the effects of the Sullivan Act in a difference-in-differences framework employing the following baseline regression,

$$y_{st} = \alpha \times (s = NY \ \& \ t \geq 1911) + X_{st}\Gamma + \theta_s + \delta_t + \varepsilon_{st}, \quad (2)$$

where s indexes state and t indexes year. Similar to our SCM model, y_{st} measures our mortality outcomes of interest including the homicide rate, suicides rate, gun suicide rate, and non-gun suicide rate. θ_s are state fixed effects, δ_t are year fixed effects and ε_{st} represent unobserved factors. We also test the robustness of our results to a number of control variables in the vector X , including demographic measures for race, age, education, foreign born, and proportion of the state’s population that are Catholic; in addition to indicators for prohibition laws. Finally, we weight the regressions by the average population of each state. Our parameter of interest is α , 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 17 years, provides relatively few state clusters.²⁰ As pointed out by Bertrand et al. (2004), few clusters may lead to downwards-biased standard errors. To adjust for this, we also calculate p-values

¹⁸Models that extend the sample to the 15 states that report from 1906-1916 yield similar results.

¹⁹Following Ferman et al. (2017), we also exclude all covariates and simply match on the outcomes in each year prior to treatment. These plots, shown in Appendix Figure A1, are consistent with our main results.

²⁰For ease of comparison, we settled on showing estimates using the same balanced sample as our SCM approach. Estimates using all possible states (26 states over 17 years) yield similar results.

for our difference-in-difference estimates using the wild bootstrap procedure suggested by Cameron et al. (2008).

6 Results

Figure 6 shows residual plots for our four outcomes obtained from a population weighted regression of the dependent variable on state and year fixed effects.²¹ Panel C of Figure 6 shows a clear decrease in gun-related suicide rates in New York relative to the other states coinciding with the enactment of the Sullivan Act. There is also evidence for an increase in non-gun suicide rates in Panel D. The plots for homicide and overall suicide in panels A and B are relatively noisy and do not provide a clear pattern.

We next show results from our SCM analysis. This approach is intuitively appealing in that it highlights the pre-treatment similarities between the synthetic control and New York, and visually identifies potential dynamic effects of the Sullivan Act. In each graph in Figure 7, the solid line represents the outcome for New York and the dashed line measures the synthetic counterfactual, which is constructed using the process outlined in Equation 1.²² Across all four panels, synthetic New York nearly reflects actual New York prior to treatment. Panels A and B reveal no distinctive difference in the post-treatment evolution of New York homicide or suicide rates relative to the synthetic control. In contrast, there is a clear decline in gun suicide rates in Panel C and an increase in non-gun suicide rates in New York relative to synthetic New York in Panel D. These effects persist through 1916. These results suggest an average decrease in gun suicide rates of 1.06 or 35 percent each year and an average increase in non-gun related suicide rates increase by 1.5 or 24 percent each year.

To provide support for our SCM results we explore a series of placebo exercises and an approach to inference following Abadie, Diamond, and Hainmueller (2010). We first show

²¹Similar to our main SCM analysis, we restrict the sample to a balanced panel of states for the years 1900, the first year for which data is available, through 1916, which is the final year prior to the U.S.'s entry into WWI.

²²SCM results in a three-state counterfactual: Maine (.126), New Jersey (.766), and Vermont (.108).

a graphical comparison of the SCM 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 New York (Figure 7) to potential effects in other placebo states that were not subject to the restrictive gun regulations of the SA. 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 New York are a matter of chance. Intuitively, a credible match in pre-treatment New York 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 that the post RMSPE to pre RMSPE ratio to be high in NY relative to other states for outcomes with an observed effect in Figure 7 (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 $1/10 = 0.1$.

We report the results for this exercise in Figure 8. The dark solid lines in each graph represent the difference between actual New York and synthetic New York, and the gray lines represent the difference between a control state and its synthetic state. Figure 8 panels A and B confirms that homicides and suicides are not apparent outliers in the post treatment period. P-value calculations for these outcomes are 0.4 and 0.3, respectively. For the breakdown of suicides into gun and non-gun related suicides in panels C and D, we also see patterns that reinforce our findings in FigureXXX 7. The pattern for gun suicides in NY show a lower envelope to the differences seen in the placebo states. Non-gun suicides are also an apparent outlier relative to other states, but in the opposite direction. For each outcome, the constructed p-value is 0.1. Together with the estimates in Figure 7 we view these results as supportive of a causal conclusion that the Sullivan Act led to a large decrease in gun suicide rates. Moreover, there is suggestive evidence for substitution from gun to non-gun methods

to commit suicide, leading to no apparent effect on overall suicides.

We next highlight the results of our alternative approach using difference-in-difference estimates from the OLS estimation of Equation 2. Column 1 of Table 3 shows baseline estimates from a model that only includes state and year fixed effects. We then add flexibility to the model by including indicators for state-level alcohol prohibition laws in Column 2; demographic controls for the share of the state population that is black, under the age of 20, literate, Catholic, and foreign born in Column 3. We also show the standard error clustered at the state level in parentheses and the associated wild bootstrap p-value.

Consistent with the findings in the graphical analysis, residual analysis, and synthetic control analysis, the estimates in Table 3 suggest no clear effect on homicides following the enactment of the Sullivan Act. The results also support a clear decrease in gun suicides following the enactment of the laws. These estimates suggest a decrease of 1.303 to 1.814, which is 36-50 percent from the mean, and roughly comparable to our SCM estimates. Moreover, the estimates are statistically significant with the wild bootstrap p-values ranging less than or equal to 0.013 across the four specifications. While the effect on total suicide rates show a consistent negative estimate and the effect on non-gun suicides is consistently positive, the estimates are relatively noisy.

To test the robustness of our difference-in-difference results and reduce the potential influence of outliers, in Table 4 we consider the following alternative transformations of the dependent variable: inverse hyperbolic sine (IHS), log, and quartic root.²³ 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.²⁴ Columns 1, 2, and 3 present the results with the full set of policy and demographic controls. We continue to observe no significant effect for homicides, suicides and non-gun suicides. The effect on gun suicides continues to

²³Unconditional 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 $\ln(y + 1)$.

²⁴Several recent examples that employ a similar transformation to deal with zeros include Anderson et al. (2016) and Tarozzi et al. (2014).

be negative and statistically different from zero. To facilitate a comparison of the estimates in Column 3 with the estimates in columns 1-2, we calculated the semi-elasticities from the marginal effects at the mean. These semi-elasticities in Column 3 suggest that that gun suicides decreased by 38.5 percent. In summary, the results presented in Table 4 are similar to those presented in Table 3.

7 Conclusion

Passed in 1911, New York 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 gun debates, and a model law for those advocating for tighter gun control. As such, our analysis of this law is potentially reflective of the effects of the nationwide movement toward restrictive concealed-carry policies in the early 1900s.

Using unique historical data from multiple sources, we analyze the effect of the 1911 Sullivan Act on gun-related behaviors and mortality outcomes. We find clear evidence of a decrease in gun suicides, and suggestive evidence of a substitution toward alternative means of suicide. In particular, the Sullivan Act led to a 35 percent decrease in the gun suicide rate, an increase—albeit noisy—in non-gun suicide rates, and no effect on overall suicide rates. We also find no evidence that the Sullivan Act affected the homicides rate. Our estimates are robust to alternative approaches to modeling, including synthetic control and difference-in-difference methods. Our results also show that where there are impacts on mortality, these estimated effects are apparent following and not prior to the Sullivan act, and that they persist for several years following the law.

The clear link between firearms and suicides stresses the relevance of this area of inquiry. In the U.S., roughly two-thirds of all gun deaths are suicides, and guns are the method for approximately half of all suicides. The extent which gun restrictions can reduce suicide and how to design effective gun policy remain divisive, but critical policy-relevant questions. Our

study adds historical insight and an important data point to these questions by demonstrating that stricter Sullivan-Act gun policies significantly decreased gun suicides, but not overall suicides.

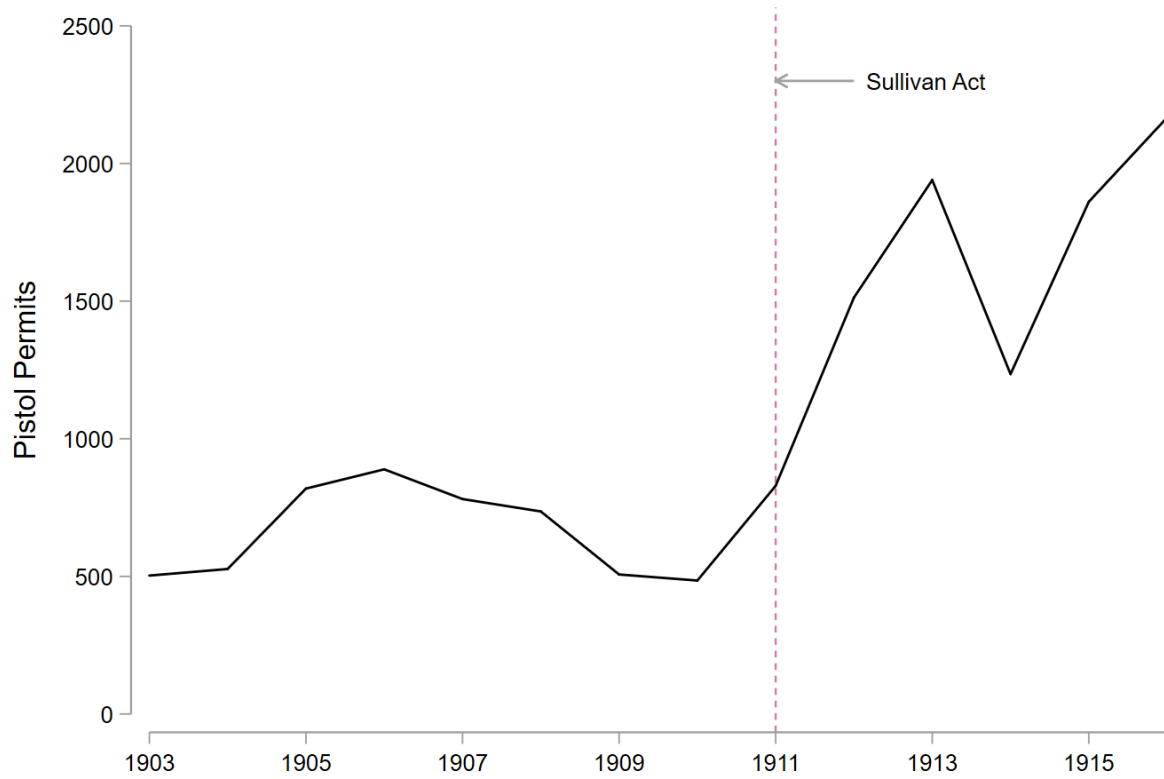
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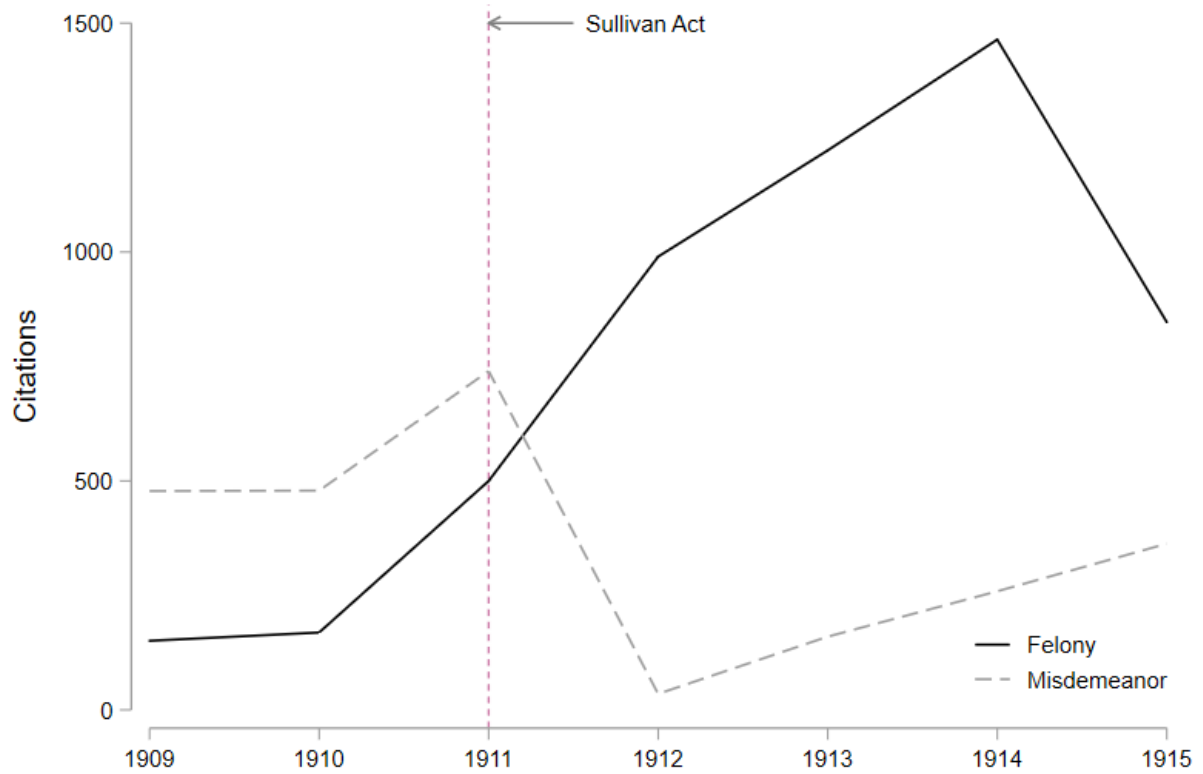
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Figure 1
Annual NYC Pistol Permits



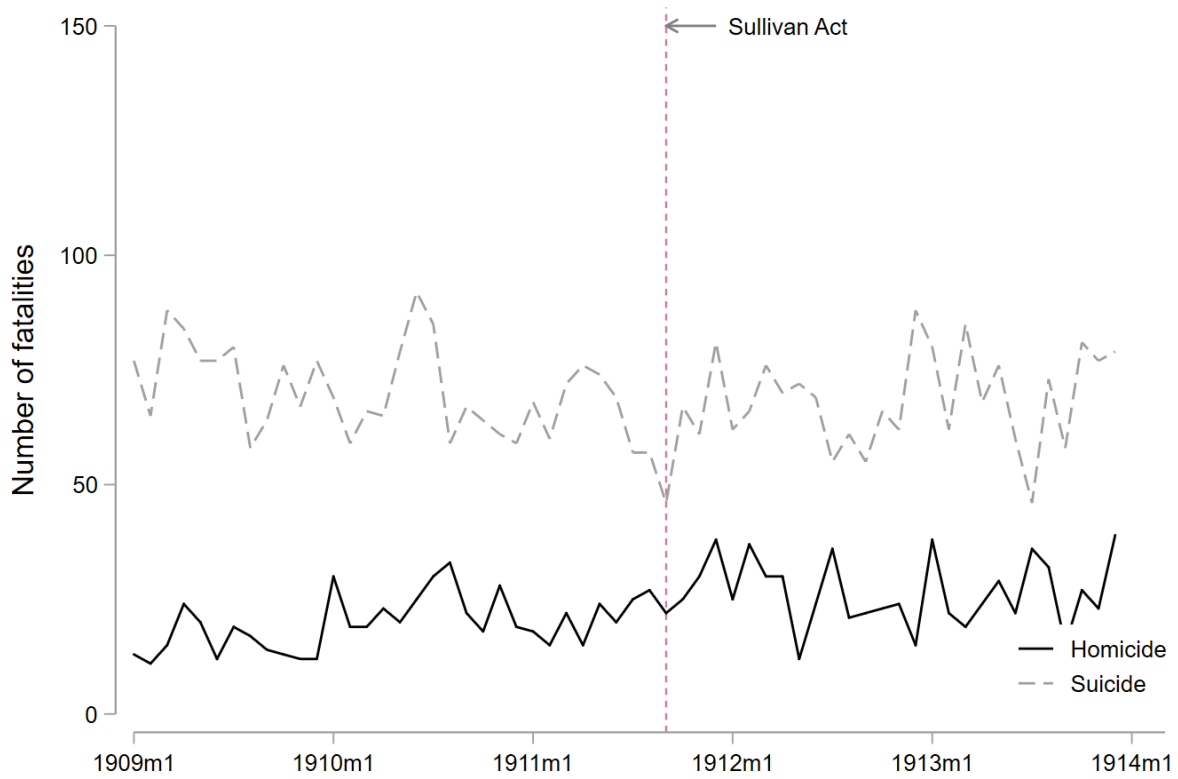
Notes: The data represent the annual number of issued pistol permits obtained from the Annual Report of the Police Department of the City of New York 1903-1916.

Figure 2
Annual Citations for Gun Carrying



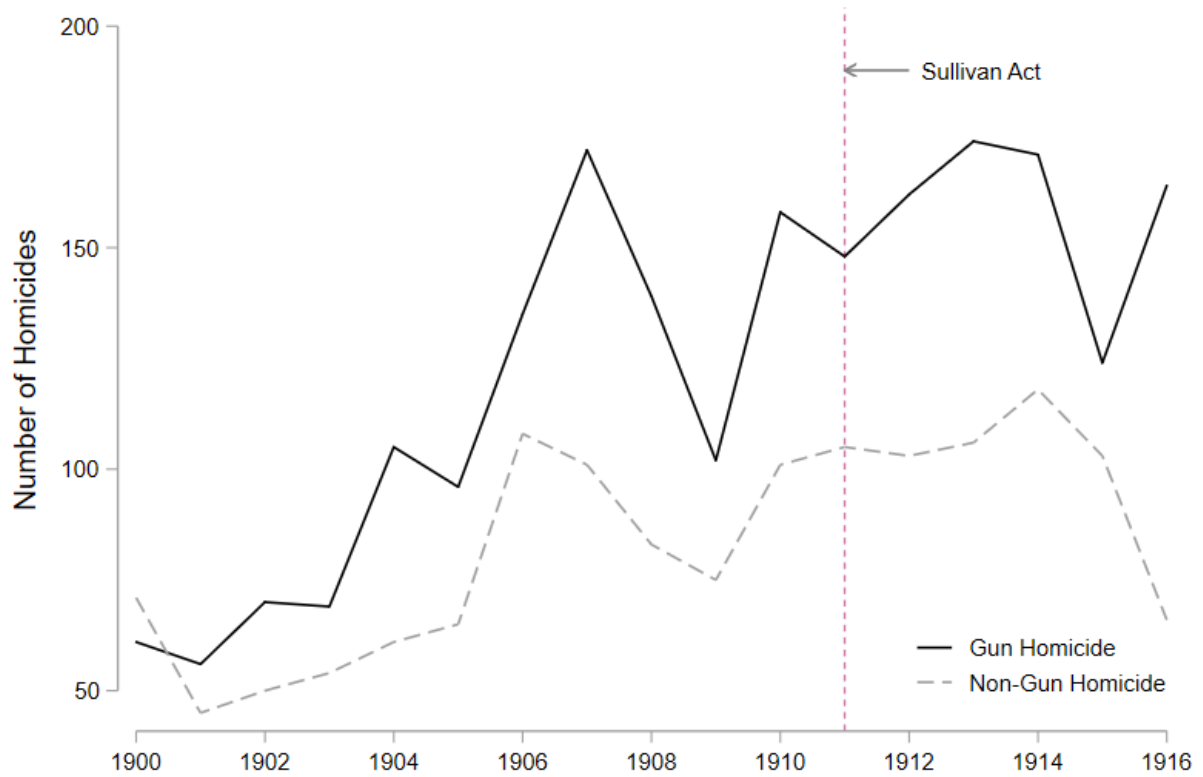
Notes: The data represent the annual number of felony and misdemeanor citations for carrying a dangerous weapon in New York City obtained from the Annual Report of the Police Department of the City of New York 1909-1915.

Figure 3
Monthly Homicides and Suicides in NYC



Notes: The data represent the monthly mortality reports in New York City obtained from the Board of Health, of the Department of Health, of the City of New York for the year 1909-1913.

Figure 4
Annual Homicides in NYC: Gun and Non-Gun



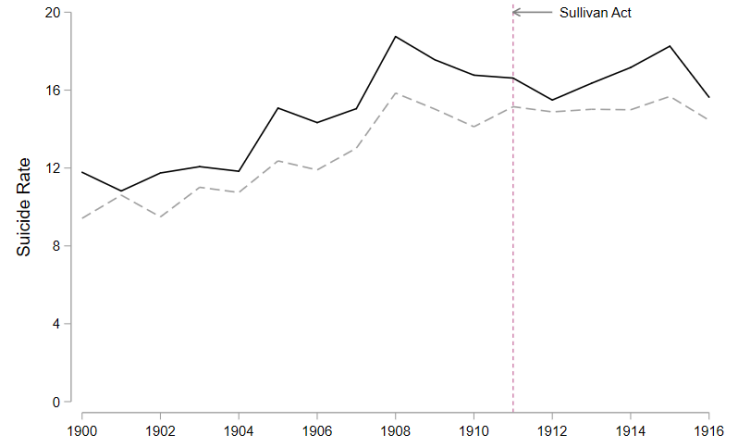
Notes: The data represent the annual mortality reports in New York City obtained from Monkkenon (2006): Police Departments, Arrests and Crime in the United States, 1860-1920 (ICPSR 7708).

Figure 5
Mortality rates in NY and other States

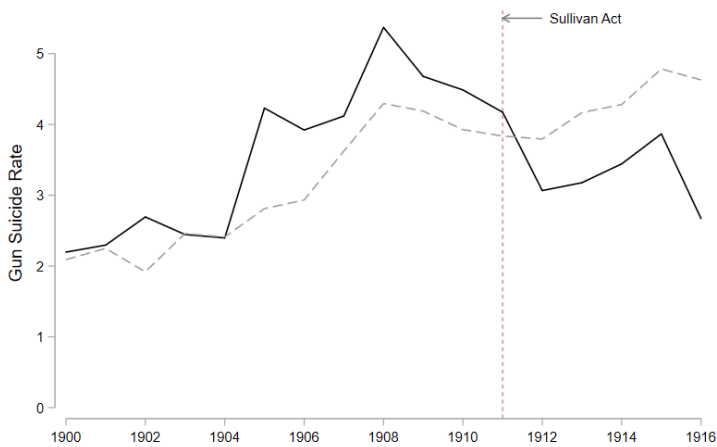
Panel A: Homicides



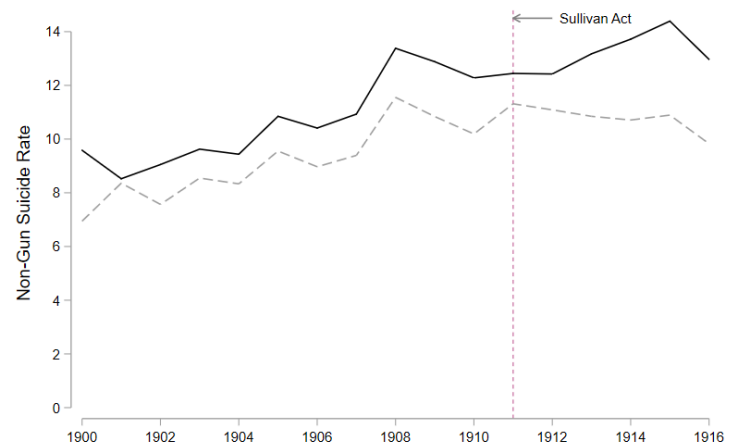
Panel B: Suicides



Panel C: Gun Suicide



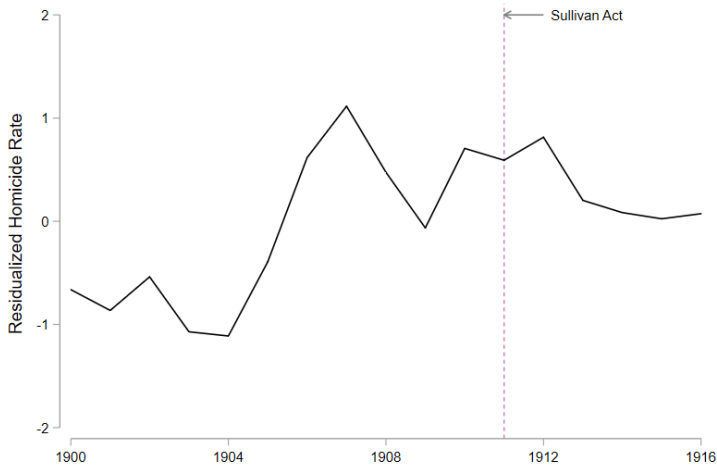
Panel D: Non-Gun Suicide



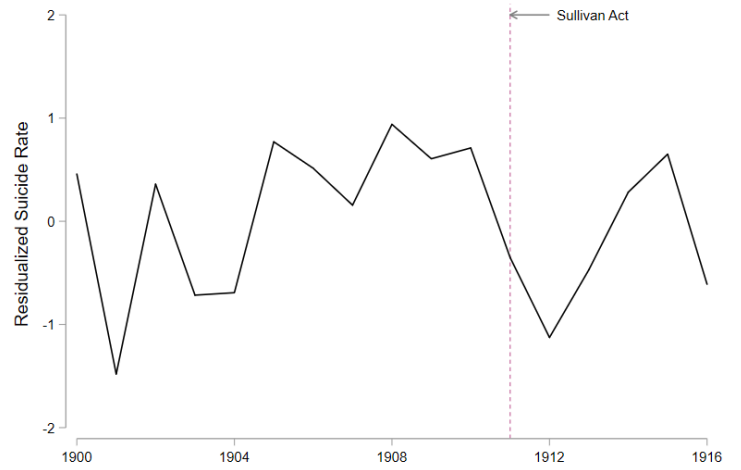
Notes: The data represent annual state mortality rates (per 100,000) obtained from from the U.S. Census Bureau's Mortality Statistics (1900-1916).

Figure 6
Residualized Mortality Rates: NY

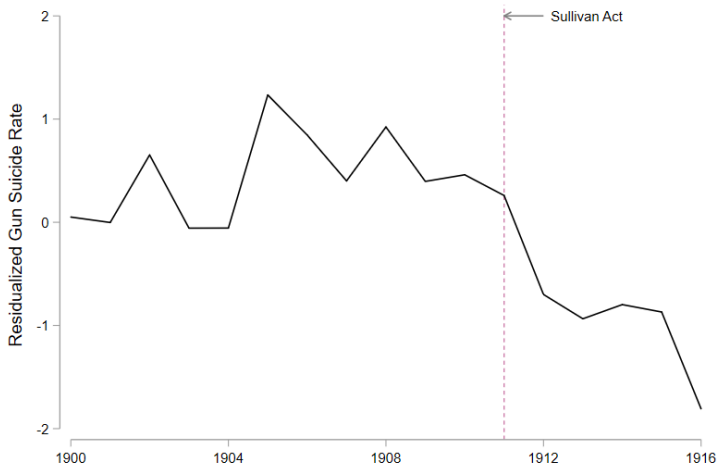
Panel A: Homicide



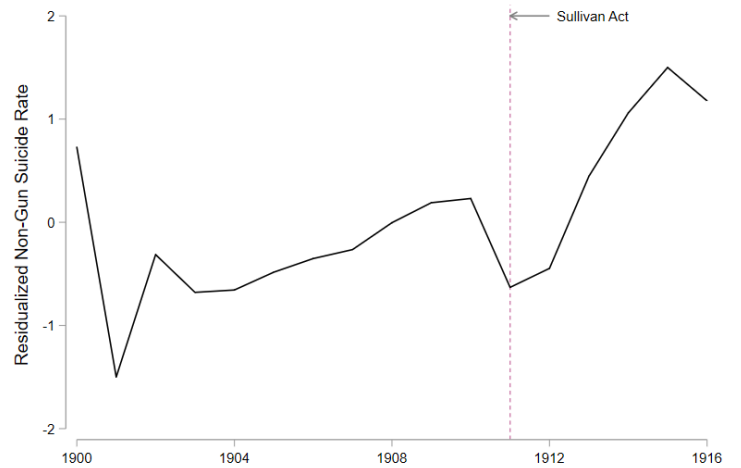
Panel B: Suicide



Panel C: Gun Suicide



Panel C: Non-Gun Suicide



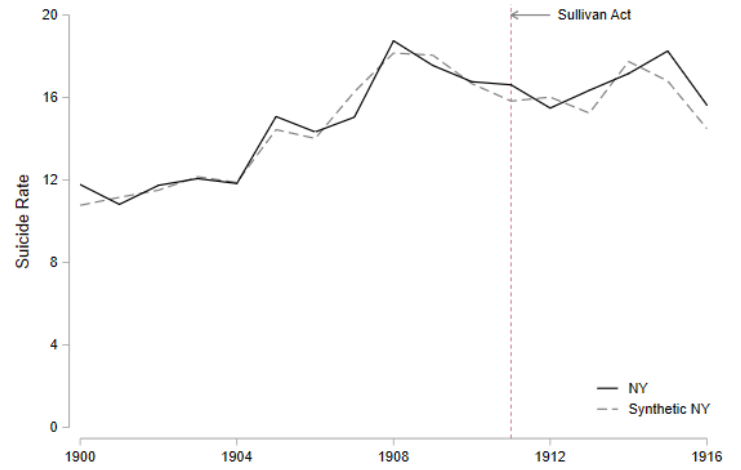
Notes: The data represent annual state mortality rates (per 100,000) obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916).

Figure 7
Synthetic Control Results

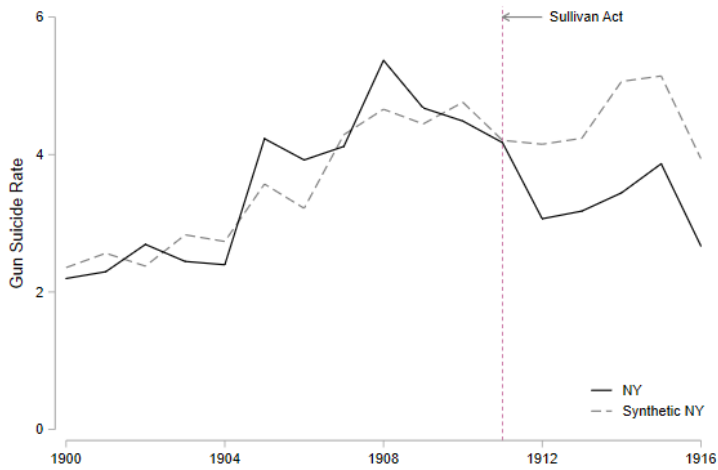
Panel A: Homicide



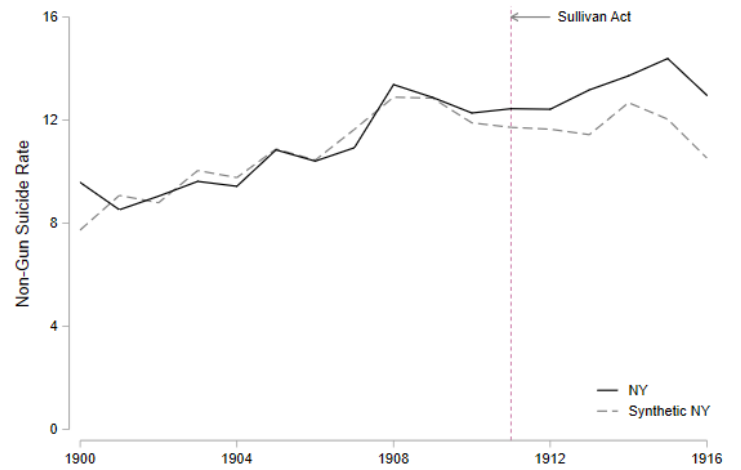
Panel B: Suicide



Panel C: Gun Suicide



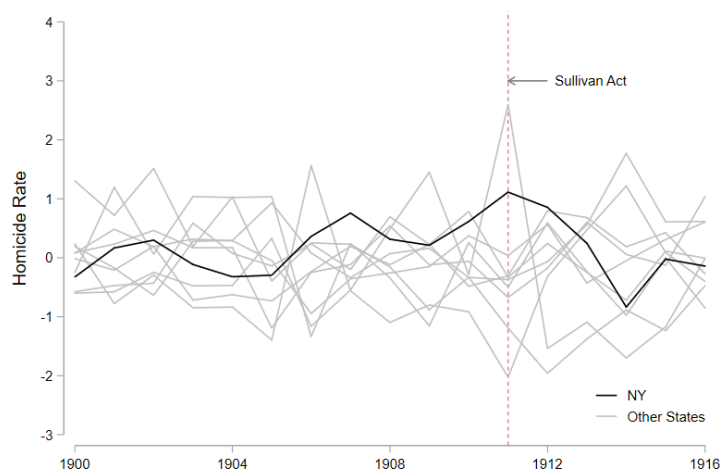
Panel D: Non-Gun Suicide



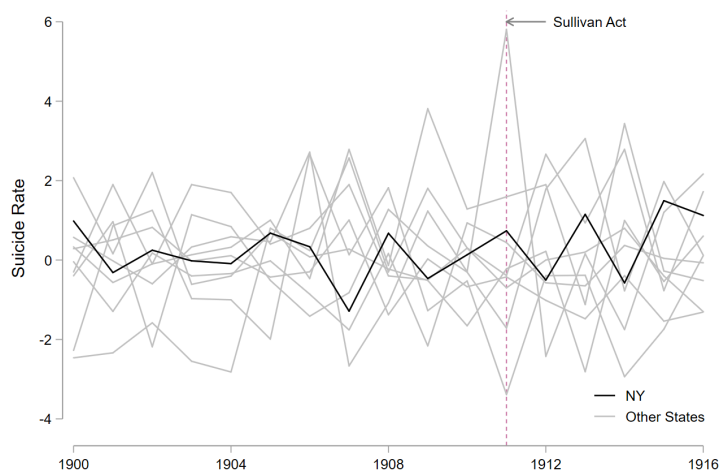
Notes: Data was obtain from the U.S. Census Bureau's Mortality Statistics (1900-1916).

Figure 8
Synthetic Control Placebo Results

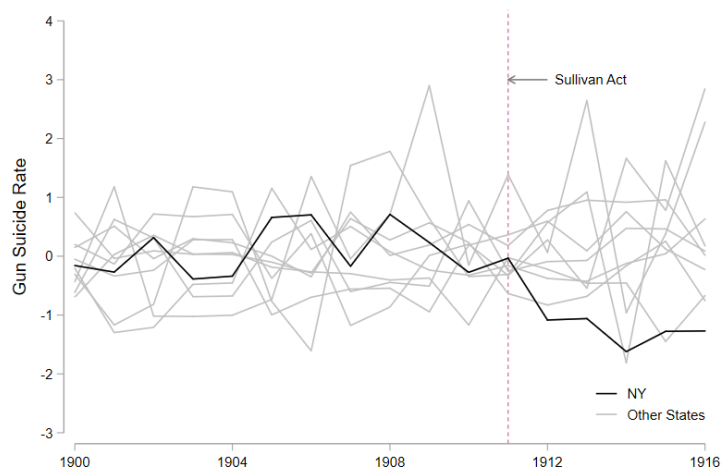
Panel A: Homicide



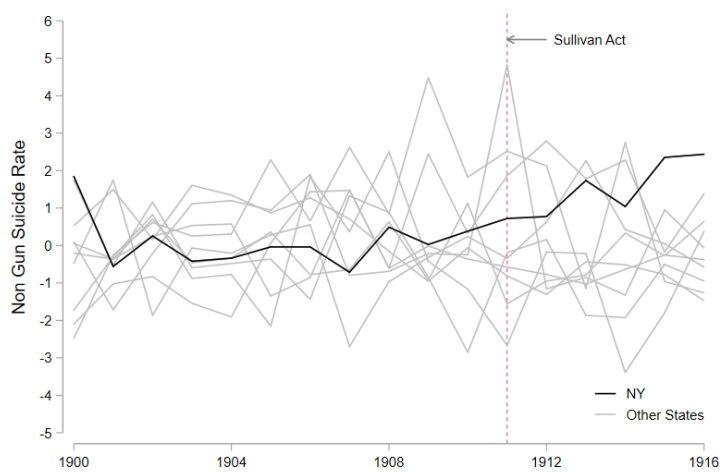
Panel B: Suicide



Panel C: Gun Suicide



Panel D: Non-Gun Suicide



Notes: The data represent annual state mortality rates (per 100,000) obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916).

Table 1
Summary Statistics

<i>Panel A: Main sample using annual Census mortality data</i>		
	<u>NY</u>	<u>Other states</u>
Homicide Rate	3.803 (1.629)	2.611 (1.423)
Suicide Rate	15.218 (2.455)	13.153 (2.913)
Gun Suicide Rate	3.520 (0.939)	3.325 (1.050)
Non-Gun Suicide Rate	11.697 (1.815)	9.815 (2.185)
N	17	153

<i>Panel B: Ancillary analysis using annual NY city data</i>		
Felony Gun Carrying (1909-1915)	763.286 (509.447)	
Misdemeanor Gun Carrying (1909-1915)	359.143 (233.921)	
Pistol Permits (1903-1916)	942.000 (441.126)	
Gun Homicides (1900-1916)	203.667 (358.245)	
Other Homicides (1900-1916)	177.694 (228.980)	

Notes: The data in Panel A was obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916). Rates are calculated per 100,000 population. The data in Panel B was obtained from various sources, including New York City police reports, New York City's Department of Health, and Monkkonen (2006). Standard deviations are displayed in parentheses.

Table 2
Summary Statistics

Panel A: Main sample using annual Census mortality data

	<u>NY</u>		<u>Other states</u>	
	Pre	Post	Pre	Post
Homicide Rate	3.069 (1.709)	4.945 (0.359)	1.972 (1.270)	3.623 (1.007)
Suicide Rate	14.335 (2.726)	16.591 (1.034)	12.037 (2.948)	14.919 (1.768)
Gun Suicide Rate	3.602 (1.142)	3.394 (0.549)	2.894 (0.981)	4.007 (0.756)
Non-Gun Suicide Rate	10.734 (1.627)	13.197 (0.764)	9.122 (2.253)	10.912 (1.541)
N	11	6	99	54

Panel B: Ancillary analysis using annual NY city data

	Pre	Post
Felony Gun Carrying (1909-1915)	160.000 (12.728)	1004.600 (366.737)
Misdemeanor Gun Carrying (1909-1915)	478.500 (0.707)	311.400 (268.526)
Pistol Permits (1903-1916)	736.769 (203.161)	1475.600 (458.688)
Gun Homicides (1900-1916)	19.117 (34.184)	441.275 (438.841)
Other Homicides (1900-1916)	33.165 (26.893)	363.775 (239.915)

Notes: The data in Panel A was obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916). Rates are calculated per 100,000 population. The data in Panel B was obtained from various sources, including New York City police reports, New York City's Department of Health, and Monkkenon (2006). Standard deviations are displayed in parentheses.

Table 3
Difference-in-Difference Results

	(1)	(2)	(3)
Panel A: Homicides			
Diff-in-Diff	0.245 (0.175)	0.239 (0.177)	-0.434 (0.221)
Number of Observations	170	170	170
Wild P-Value	0.328	0.329	0.275
Panel B: Suicides			
Diff-in-Diff	-0.543 (0.634)	-0.545 (0.640)	-1.388 (0.369)
Number of Observations	170	170	170
Wild P-Value	0.507	0.488	0.158
Panel C: Gun Suicides			
Diff-in-Diff	-1.308 (0.209)	-1.303 (0.211)	-1.814 (0.163)
Number of Observations	170	170	170
Wild P-Value	0.012	0.013	0.000
Panel D: Non-Gun Suicides			
Diff-in-Diff	0.745 (0.466)	0.737 (0.470)	0.463 (0.330)
Number of Observations	170	170	170
Wild P-Value	0.262	0.246	0.268
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Policy Controls		Yes	Yes
Demographic Controls			Yes

Notes: The outcome variables are annual state mortality rates (per 100,000) obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916). Controls include indicators for state-level alcohol prohibition laws and demographic controls for the share of the state population that is black, under the age of 20, literate, Catholic, and foreign born. Standard errors that are clustered on the state are presented in parentheses.

Table 4
Difference-in-Difference Results: Robustness Transformed Dependent
Variables

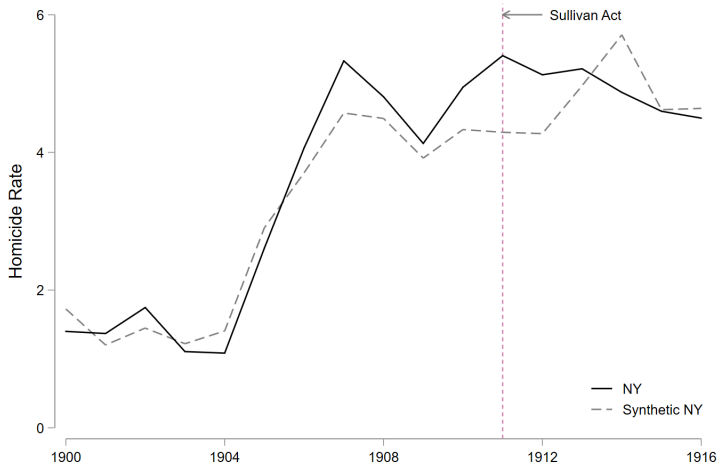
	(1) Inv Hyp Sine	(2) Log Rate	(3) Quart Root
Panel A: Homicides			
Diff-in-Diff	-0.208 (0.153)	-0.147 (0.074)	-0.060 (0.044)
Number of Observations	170	170	170
Wild P-Value	0.406	0.259	0.383
Panel B: Suicides			
Diff-in-Diff	-0.107 (0.029)	-0.099 (0.027)	-0.051 (0.014)
Number of Observations	170	170	170
Wild P-Value	0.113	0.139	0.146
Panel C: Gun Suicides			
Diff-in-Diff	-0.482 (0.080)	-0.378 (0.052)	-0.167 (0.024)
Number of Observations	170	170	170
Wild P-Value	0.014	0.004	0.005
Panel D: Non-Gun Suicides			
Diff-in-Diff	0.017 (0.029)	0.017 (0.027)	0.011 (0.013)
Number of Observations	170	170	170
Wild P-Value	0.532	0.491	0.394
Year Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: The outcome variables are annual state mortality rates (per 100,000) obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916). Controls include indicators for state-level alcohol prohibition laws and demographic controls for the share of the state population that is black, under the age of 20, literate, Catholic, and foreign born. Standard errors that are clustered on the state are presented in parentheses.

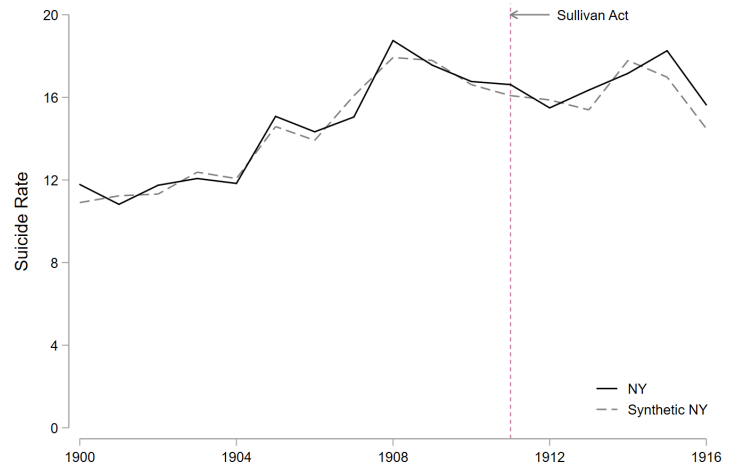
8 Appendix

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

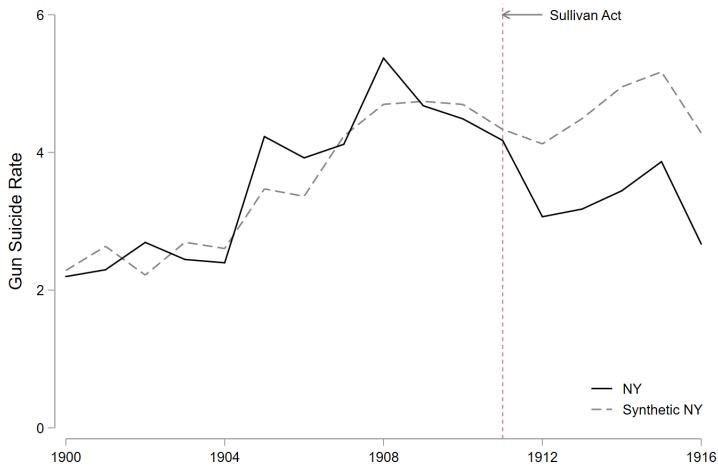
Panel A: Homicide



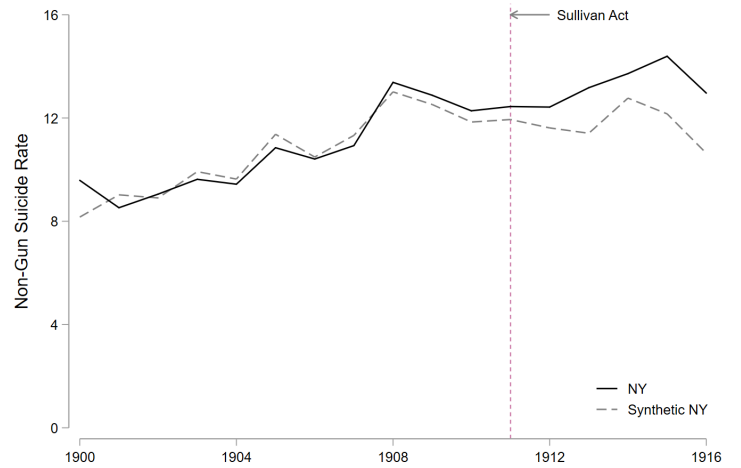
Panel B: Suicide



Panel C: Gun Suicide



Panel D: Non-Gun Suicide



Notes: The data represent state annual mortality rates per 1000 residents obtained from the U.S. Census Bureau's Mortality Statistics (1900-1916).