Effects of Risk-Based Firearm Seizure Laws in Connecticut and Indiana on Suicide Rates, 1981–2015

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Objective: This study evaluated whether risk-based firearm seizure laws in Connecticut and Indiana affect suicide rates.

Methods: A quasi-experimental design using annual state-level panel data from the 50 states between 1981 and 2015 was used. When analyses controlled for a range of risk factors for population-level suicide rates, the effects of Connecticut and Indiana's firearm seizure laws on firearm and nonfirearm suicide rates were evaluated by using the synthetic-control methodology and difference-in-place placebo tests. Sensitivity analyses employed regression-based difference-in-differences analyses with randomization inference.

Results: Indiana's firearm seizure law was associated with a 7.5% reduction in firearm suicides in the ten years following its enactment, an effect specific to suicides with firearms and larger than that seen in any comparison state by chance alone. Enactment of Connecticut's law was associated with a 1.6% reduction in firearm suicides immediately after its passage and a 13.7% reduction in firearm suicides in the post–Virginia Tech period, when enforcement of the law substantially increased. Regression-based sensitivity analyses showed that these findings were robust to alternative specifications. Whereas Indiana demonstrated an aggregate decrease in suicides, Connecticut's estimated reduction in firearm suicides was offset by increased nonfirearm suicides.

Conclusions: Risk-based firearm seizure laws were associated with reduced population-level firearm suicide rates, and evidence for a replacement effect was mixed.

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Noncriminalizing firearm seizure laws are important in the United States, where strong gun rights protections make it difficult to legally prohibit many individuals at risk of injuring themselves or others from possessing firearms. Even when individuals are prohibited by federal law from owning firearms, they may be allowed to keep the guns they have because they live in states without legal mechanisms to remove them. In 1999, Connecticut became the first state to enact firearm seizure legislation following a mass shooting at the state lottery headquarters (1). Indiana followed suit in 2005 after the fatal shooting of a police officer in Indianapolis. Indiana's law permits warrantless seizure of a person's firearms if a police officer believes the person has a "mental illness" and is "dangerous," defined as an imminent or future "risk of personal injury" to self or others (2). Connecticut's law requires an "independent investigation" by police if they believe that a person poses "a risk of imminent personal injury" to self or others, followed by a warrant request, with several formal checks on the judge's ability to order the seizure and retention of firearms by law enforcement (1).

Connecticut's law is thus more stringent, although the "warrant first" requirement is often circumvented in practice (3). There was an eight-year lag after the enactment of Connecticut's firearm seizure legislation during which time very few guns were seized, but seizure rates increased fivefold following the mass shooting at Virginia Tech on April 16, 2007 (3). By contrast, Indiana's enactment in 2005 corresponded almost immediately with meaningful levels of enforcement (4,5). Of the 762 individuals exposed to firearm seizures between 1999 and 2013 in Connecticut, 21 committed suicide (six via firearm) (3). In Indiana, 404 people were exposed to firearm seizure in Marion County (Indianapolis) between 2006 and 2013 (5), although outcomes of these cases are unknown.

Four additional states (California, Washington, Oregon, and Florida) have recently passed risk-based firearm seizure laws. Although the specifics of each piece of legislation vary, all of these laws (also called red flag, risk warrant, gun violence restraining order, or extreme risk protection order laws) allow firearm seizures that are time limited, with a level of judicial oversight and due process, and that apply to persons who are not already prohibited from owning guns. To date, 19 other states have proposed such legislation, and federal policies are being considered. However, little information is available regarding the effect of such legislation. One exception comes from a recent evaluation of Connecticut's law, which found decreased firearm suicide rates among individuals subjected to firearm seizures; the study

also found a partial replacement effect, whereby reductions in firearm suicides were offset by increases in nonfirearm suicides (3).

Although firearm seizure laws in Indiana and Connecticut were enacted in response to firearm homicides, data show that these laws have functioned primarily as a means of permitting law enforcement to remove guns from individuals perceived as being at risk of suicide (3–7). In this study, we evaluated the effect of firearm seizure legislation in Connecticut and Indiana on state-level suicide rates. We examined firearm and nonfirearm suicide rates separately, with the expectation that any observed effects would be specific to firearm suicides and to test whether these effects were offset by increased nonfirearm suicides.

METHODS

Study Design and Data

We merged several sources of state-level panel data from 1981 to 2015 to evaluate the effects of firearm seizure legislation on suicide rates in Indiana and Connecticut. The outcome variables, firearm and nonfirearm suicide rates per 100,000 population, came from the Centers for Disease Control and Prevention's (CDC's) Web-Based Injury Statistics Query and Reporting System (WISQARS) (8). The key independent variable was the enactment of firearm seizure legislation in Connecticut (October 1, 1999) and Indiana (July 1, 2005). We also evaluated the effect of Connecticut's increased enforcement of its firearm seizure law, which, following Swanson and colleagues (3), we dated to the mass shooting at Virginia Tech (April 16, 2007).

Finally, we selected state-level covariates shown to be associated with state-level suicide rates, including age, sex, race-ethnicity, high school completion, poverty, unemployment, spirit alcohol consumption, violent crime, population density, and household gun ownership. For age, we calculated the percentage of each state's population ages 15 to 24 (9) and ≥65 (10) by using data from the U.S. Census. U.S. Census data were also used to calculate the percentage of each states' population that was white, black, and Hispanic (10,11); percentage of adults with a high school diploma (9,12); percentage below the federal poverty threshold (13,14); percentage male (9,10); and the population density of each state (residents per square mile) (15). We used data from the Bureau of Labor Statistics to calculate the annual unemployment rate (13,14), and data from the National Institute on Alcohol Abuse and Alcoholism were used to calculate average per capita gallons of spirit ethanol consumption (16). We used the FBI's Uniform Crime Report to obtain annual state-level violent crime rates. Finally, we calculated a widely used proxy for household firearm ownership rates (17,18), represented as firearm suicides as a percentage of all suicides, by using data from the CDC's WISQARS.

All data were publicly available. The study did not involve human participants, and institutional review board approval was not required.

Statistical Analysis

We employed the synthetic-control method to examine the impacts of firearm seizure laws on state-level suicide rates in Indiana and Connecticut (19). The synthetic-control method constructs a weighted combination of donor states to best fit the prelaw characteristics and suicide trends of Indiana and Connecticut, estimating an empirically derived counterfactual (19–21). Pre- and postlaw comparisons between Indiana or Connecticut and their synthetic-control units thus allowed for a comparison between rates of suicide observed in the affected states to expected rates had the legislation not been implemented. This methodology produces control units with better preintervention fit to the treated unit compared with other methodologies, allowing for more valid inferences regarding the effect of policy change (19–21).

The prelaw period was used to generate synthetic controls for firearm and nonfirearm suicide rates in Indiana and Connecticut. As recommended, we limited analysis of outcomes to no more than ten years postintervention (20). Connecticut's preenactment period was 1981 to 1998 and postenactment was 1999 to 2009. The preenactment period for tests of Connecticut's increased enforcement was 1981 to 2006 and postenforcement was 2007 to 2015. Indiana's preenactment period was 1981 to 2004, and postenactment was 2005 to 2015. States were excluded from the donor pool if they enacted similar legislation during each state's respective follow-up period (21). Ultimately, Connecticut's enactment donor pool included 48 states, excluding Indiana, and its postenforcement donor pool included 47 states, excluding Indiana and California. Indiana's donor pool included 47 states, excluding Connecticut and California. Outcome data were smoothed by using three-year moving averages, [(y-1)+y+(y+1)]/3, to reduce year-to-year volatility in suicide rates (22).

Predictors were averaged across the preintervention period, and mean scores for each synthetic-control unit and its target state were calculated. Following Abadie and colleagues (20), we entered three preintervention observations of the dependent variable. For Connecticut, we entered firearm and nonfirearm suicide rates from 1982, 1990, and 1998 for postenactment analyses, and 1982, 1994, and 2006 for postenforcement analyses. For Indiana, we entered rates from 1982, 1993, and 2004. All covariates were entered in the construction of the synthetic-control units. Degree of fit between synthetic-control units and their respective states was assessed with the root mean square prediction error (RMSPE), a measure of the spread of the preintervention synthetic-control trends around the target state's trends.

Because the synthetic-control method does not provide standard measures of statistical inference, we employed "so-called placebo" tests (20,21,23). Like permutation tests, so-called placebo tests iteratively construct a sampling distribution. Specifically, so-called difference-in-place placebo tests iteratively construct synthetic-control units for each state in the donor pool and run analyses as though each state

TABLE 1. States contributing to the construction of synthetic-control units for Indiana and Connecticut, by type of suicide and weight[a]

State	Weight
Indiana	
Firearm suicide	
AK	.054
ME	.168
MI	.165
MN	.128
MS	.099
NH	.077
ND	.191
SC	.062
VT	.050
WI	.005
Nonfirearm suicide	
KY	.250
ME	.039
MA	.054
MN	.148
NH	.071
OH	.176
SC	.069
UT	.126
VA	.068
Connecticut	
Enactment	
Firearm suicide	
HI	.129
MA	.544
NH	.244
RI	.069
VT	.014
Nonfirearm suicide	
MN	.072
MT	.123
NE	.252
NJ	.342
NY	.21
Enforcement	
Firearm suicide	
FL	.112
HI	.270
NY	.420
ND	.047
RI	.152
Nonfirearm suicide	
CO	.058
FL	.012
HI	.005
NE	.357
NH	.109
NJ	.433
OR	.027

[a] Indiana's donor pool consisted of 47 states, excluding Indiana, Connecticut, and California. Connecticut's donor pool consisted of 48 states, excluding Connecticut and Indiana, during the postenactment period (1999–2009) and 47 states, excluding Connecticut, Indiana, and California, during the postenforcement period (2007–2015). Only states with nonzero weights are listed.

had implemented legislation in the specified year. By arbitrarily assigning the law to each state and comparing its effect relative to each state's unique synthetic-control unit, difference-in-place placebo tests generate a distribution of effects for states where no law was enacted. The distribution provides information on the rarity of observing an effect as large as that in the target state by chance alone. We excluded states with poor prelaw fit to their synthetic counterparts, defined as RMSPE values ≥5 times those in Indiana or Connecticut (20,23).

Sensitivity Analysis

Sensitivity analyses employed regression-based difference-in-differences tests. The data set was employed with a pre-post indicator variable representing the firearm law enactment, coded 0 prior to the enactment of the law and 1 afterward, with the year of enactment coded as a fraction according to the day the law was enacted. Time (pre versus post) was entered as a fixed effect, and the interaction between time and the state of interest (Indiana or Connecticut) was used as an estimator of the differential effect of the law on suicide rates. Negative binomial regression was used to account for the dispersion observed in the data, and standard errors were adjusted to account for clustering. After testing for variance inflation to ensure efficient model specification, analyses controlled for all predictors entered in the synthetic-control analyses. Finally, because difference-in-differences tests with a small number of treated clusters can underestimate standard errors (24), follow-up analyses employed randomization inference to account for within-group correlation of model errors (24,25). All analyses were conducted in Stata 15.0.

RESULTS

There were 15,130 firearm suicides in Indiana and 4,020 in Connecticut from 1981 to 2015. Indiana's rate of 7.21 per 100,000 population was more than twice as high as Connecticut's rate of 3.28 during this period. Table 1 shows states with nonzero weights in the construction of synthetic Indiana and Connecticut across firearm and nonfirearm suicide rates. The synthetic controls for Indiana's firearm (RMSPE=.123) and nonfirearm (RMSPE=.102) suicide rates evidenced a good fit to the preintervention data. For Connecticut's postenactment analyses, the synthetic controls showed a good fit to the state's nonfirearm suicide rate (RMSPE=.147) and an acceptable fit to the firearm suicide rate (RMSPE=.289). The synthetic controls for Connecticut's postenforcement analyses evidenced an acceptable fit to the state's firearm (RMSPE=.203) and nonfirearm (RMSPE=.182) suicide rates.

Table 2 presents preintervention means for all predictors entered into the models for Indiana and Connecticut. In general, each state closely mirrored its synthetic control in terms of gender, age, race-ethnicity, education, unemployment, and gun ownership rates. Each state's poverty rates were slightly lower than those of its respective synthetic-control units, and there were some differences in spirit alcohol consumption. Both states evidenced higher population density than their synthetic controls, and Indiana's violent crime rate was higher than its synthetic control. Each state's synthetic-control units closely approximated preintervention rates of firearm and nonfirearm suicide.

Figure 1 provides a panel of suicide rates in Indiana and Connecticut relative to their synthetic counterparts before and after implementation of firearm seizure legislation, providing a comparison between states' actual suicide trends and trends that would be expected had no legislation been implemented. Indiana's firearm suicide rate closely tracked its synthetic control across the prelaw period, with each showing an average rate of 7.30 firearm suicides per 100,000 population between 1981 and 2004. A distinctive gap emerged after the Indiana law's enactment in 2005, with mean postenactment firearm suicide rates of 6.98 and 7.55 for Indiana and its synthetic control, respectively, showing a 7.5% reduction in firearm suicide rates in Indiana across the postenactment period. Given the 5,105 firearm suicides in

TABLE 2. State-level and synthetic-control unit characteristics of Indiana and Connecticut before enactment and increased enforcement of firearm seizure laws[a]

	Indiana (enactment, 2005)			Connecticut (enactment, 1999)			Connecticut (enforcement, 2007)		
		Synthetic-control unit			Synthetic-control unit			Synthetic-control unit	
Covariate	State level	Firearm Suicides	Nonfirearm suicides	State level	Firearm suicides	Nonfirearm suicides	State level	Firearm suicides	Nonfirearm suicides
Male (%)	.49	.49	.49	.48	.49	.49	.48	.49	.49
Age 15–24 (%)	15.48	15.26	15.44	14.38	15.17	14.69	13.77	14.68	14.29
Age ≥65 (%)	12.28	12.27	11.86	13.43	12.83	13.12	13.50	13.45	13.03
White (%)	87.51	85.87	86.93	81.69	81.68	79.15	79.42	59.47	79.50
Black (%)	8.39	8.78	8.21	8.53	3.52	8.85	8.96	9.36	7.80
Hispanic (%)	2.91	1.69	2.77	7.61	4.71	7.80	8.92	11.17	8.77
High school graduate (%)	.58	.59	.57	.61	.61	.59	.62	.60	.62
Below the federal poverty threshold (%)	11.18	12.60	12.28	7.89	9.34	12.04	8.00	12.85	9.66
Unemployment rate (%)	5.63	5.63	5.63	5.02	5.02	5.43	4.73	5.43	4.66
Spirit alcohol consumption (mean gallons of ethanol per capita)	.64	.91	.74	.96	1.19	.80	.90	.77	.90
Population density (per square mile)	161.05	70.33	155.92	675.49	538.60	454.23	688.07	392.80	502.65
Violent crime rate (per 100,000 population)	402.66	323.64	363.08	438.69	444.17	521.93	398.15	574.04	400.47
Gun ownership rate (firearm suicide/total suicide ratio)	.61	.60	.60	.43	.37	.46	.41	.36	.46
Preenactment suicide rate (per 100,000 population)[a]									
T1 firearm	7.02	7.02	—	3.56	3.83	—	3.56	3.67	—
T1 nonfirearm	4.92	—	4.88	4.84	—	4.95	4.84	—	4.86
T2 firearm	7.85	7.80	—	3.95	3.91	—	4.36	4.01	—
T2 nonfirearm	4.57	—	4.55	5.20	—	5.13	5.02	—	4.88
T3 firearm	6.48	6.71	—	2.97	3.27	—	2.40	2.70	—
T3 nonfirearm	5.19	—	5.22	4.69	—	4.81	5.40	—	5.37

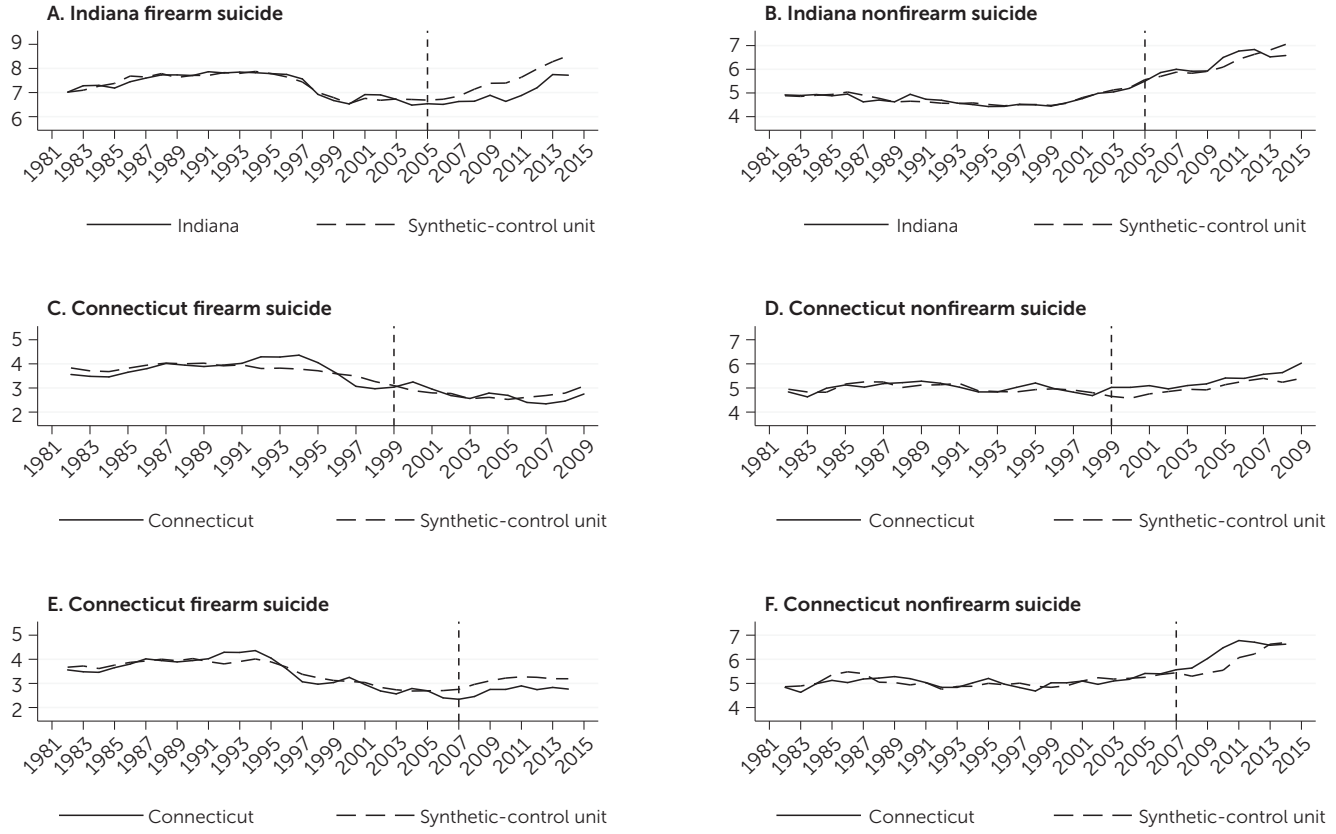
[a] A synthetic-control unit is a weighted combination of other states that best fit the characteristics and suicide trends in Indiana and Connecticut before enactment of the firearm seizure laws. Separate synthetic-control units were constructed for firearm and nonfirearm suicides. Three preenactment observations of the dependent variables were included in all synthetic-control analyses, with one observation each from the beginning, middle, and end of the preenactment period. Thus times 1, 2, and 3 correspond to 1982, 1990, and 1998 for Connecticut's enactment analysis and to 1982, 1994, and 2006 for Connecticut's enforcement analysis. For Indiana's enactment analysis, preenactment observations of the dependent variables are from 1982, 1993, and 2004.

Indiana between 2005 and 2015 (8), these analyses indicate that 383 firearm suicides might have been prevented across 10 years as a result of the law. This effect was specific to firearm suicides, with Indiana evidencing 1.0% more nonfirearm suicides than its synthetic counterpart postenactment. Given the 4,428 nonfirearm suicides in Indiana between 2005 and 2015 (8), this suggests that 44 nonfirearm suicides might be attributed to the law.

Both Connecticut and its synthetic counterpart showed mean firearm suicide rates of 3.75 per 100,000 population from 1981 to 1998, and Connecticut's mean postenactment rate was 1.6% lower than that of its synthetic counterpart. Postenactment nonfirearm suicides were 5.7% higher in Connecticut than for its synthetic counterpart. When the point of intervention was moved forward to the period following the Virginia Tech shooting (postenforcement), firearm suicide rates in Connecticut (2.69 per 100,000 population) were distinctly lower than in the synthetic control (3.12 per 100,000 population), a 13.7% mean decrease in firearm suicides from 2007 to 2015. Given that there were 933 firearm suicides in Connecticut between 2007 and 2015 (8), these estimates suggest that enforcement of Connecticut's law might have prevented an estimated 128 firearm suicides during this eight-year period. There was a 6.5% postenforcement increase in Connecticut's nonfirearm suicide rates, and this increase coincided precisely with the law's increased enforcement. There were 2,153 nonfirearm suicides in Connecticut between 2007 and 2015 (8), and this suggests that 140 nonfirearm suicides might be attributed to the increased enforcement of the law.

Figure 2 provides a panel of so-called placebo tests applying the synthetic-control method to each state in the donor pool to generate a random distribution of postlaw effects to test the rarity of obtaining effects as large as those seen in Indiana and Connecticut by chance alone. When we arbitrarily assigned exposure to the law's enactment to other states in the donor pool, there was a very low probability of observing a treatment effect on firearm suicide rates as large as that seen in Indiana (Figure 2A). Of the 47 states in Indiana's donor pool for which synthetic controls were

FIGURE 1. Synthetic-control analyses of suicide rates in Indiana and Connecticut before and after enactment and increased enforcement of firearm seizure laws, by type of suicide[a]



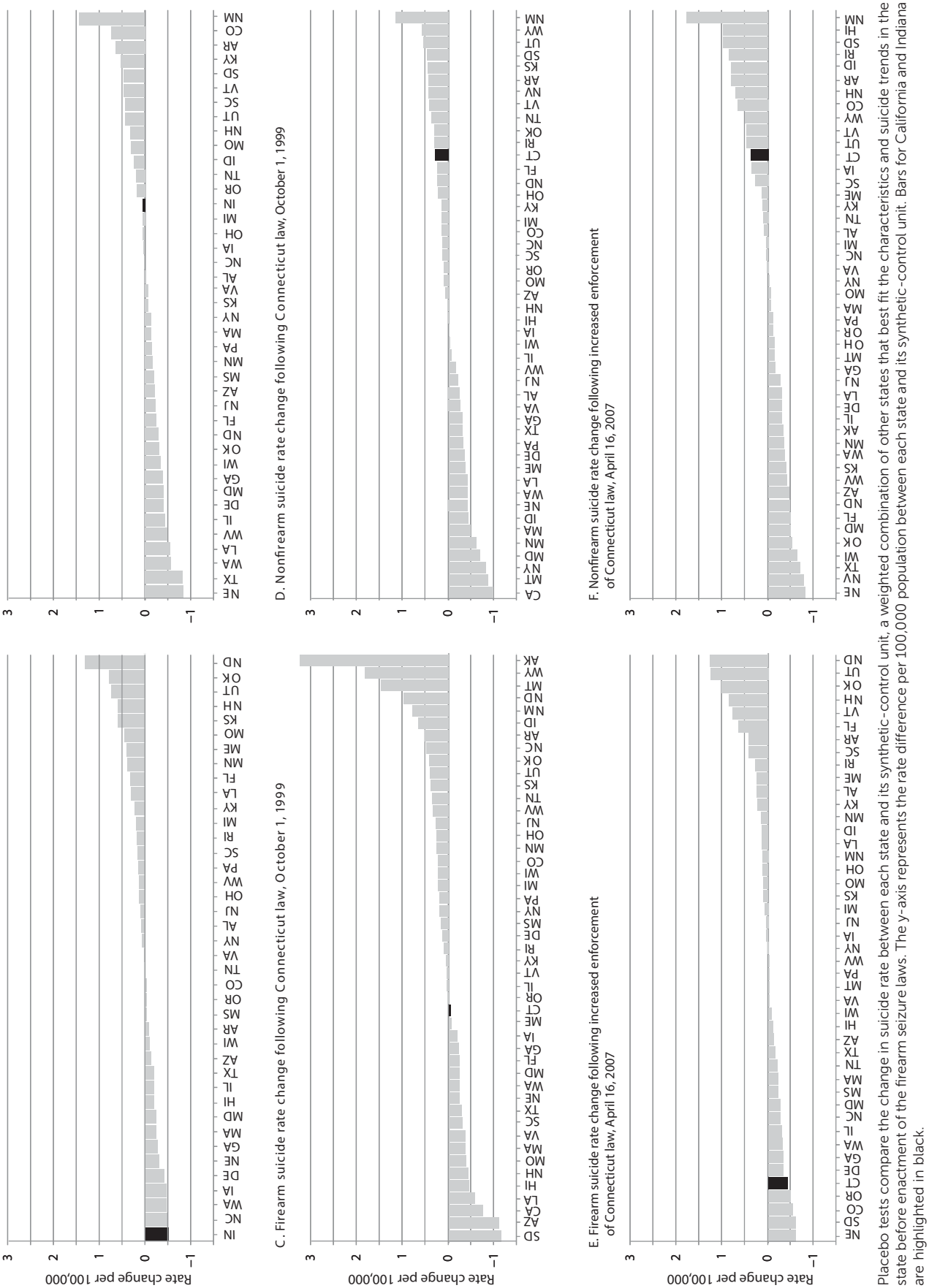
[a] The analysis compares suicide rates in Indiana and Connecticut with those of a synthetic-control unit, a weighted combination of other states that best fit the characteristics and suicide trends in Indiana and Connecticut before enactment of the firearm seizure laws. The y-axis represents suicide rate per 100,000 population. Dashed vertical lines correspond to Indiana's enactment of firearm seizure law on July 1, 2005 (panels A and B); Connecticut's enactment of firearm seizure law on October 1, 1999 (panels C and D); and Connecticut's increased enforcement of firearm seizure law on April 16, 2007 (panels E and F).

constructed, seven were excluded because of poor preenactment fit. Of the remaining 39 states for which placebo tests were calculated, none exhibited mean postenactment decreases as large as Indiana's. Indiana's 1.0% increase in nonfirearm suicides relative to its synthetic control was similar to the variation observed in states that enacted no firearm seizure legislation, with 13 of 39 states displaying larger increases (Figure 2B).

Of the 48 states in Connecticut's donor pool, two were excluded because of poor preenactment fit, resulting in the construction of placebo tests for 46 states. Seventeen states showed a greater decrease than Connecticut in firearm suicides relative to their synthetic controls (Figure 2C), and 11 states demonstrated larger postenactment increases in nonfirearm suicide rates compared with Connecticut's 5.7% increase (Figure 2D). When the point of intervention was moved to 2007 (enforcement after the Virginia Tech shooting), two states were removed from firearm suicide analyses because of poor preenforcement fit and none were removed from nonfirearm suicide analyses. The 13.7% post-2007 decrease in firearm suicides in Connecticut was large compared with other states, with just four states showing a larger average decrease from 2007 to 2015 (Figure 2E). Connecticut's 6.5% increase in nonfirearm suicide rates was less unusual, with 11 states showing larger increases (Figure 2F).

Finally, sensitivity analyses employed difference-in-differences estimates by using negative binomial regression models with panel data from comparison states included in the synthetic-control analyses. Indiana's seizure law was associated with an estimated 5% reduction in overall suicide rates ($p<.01$) (Table 3). This effect was driven by a 10% reduction in firearm suicide rates ($p<.001$), which was partially offset by a 10% increase in nonfirearm suicide rates ($p<.001$). The enactment and increased enforcement of Connecticut's law were associated with a 16% and 12% reduction in firearm suicide rates, respectively ($p<.001$ for both), with no evidence of a replacement effect. Randomization inference results accounting for within-group correlation of model errors did not alter the significance of these findings, except that Indiana's increase in nonfirearm suicide rates was rendered nonsignificant ($p=.06$). [Results of this analysis are presented in a table in an online supplement to this article.]

FIGURE 2. Placebo tests of the effects of firearm seizure laws implemented in Indiana and Connecticut on suicide rates, by type of suicide[a]



[a] Placebo tests compare the change in suicide rate between each state and its synthetic-control unit, a weighted combination of other states that best fit the characteristics and suicide trends in the state before enactment of the firearm seizure laws. The y-axis represents the rate difference per 100,000 population between each state and its synthetic-control unit. Bars for California and Indiana are highlighted in black.

DISCUSSION

This study found that firearm seizure legislation was associated with reductions in state-level firearm suicide rates and that these effects were robust to alternative specifications. Using panel data from the 50 states and controlling for population-level risk factors, Indiana's synthetic-control analyses showed a 7.5% decrease in firearm suicides in the first decade postenactment. On the basis of this finding, we estimated that Indiana's firearm seizure law may have prevented 383 firearm suicides in the first ten years after its enactment while contributing to 44 nonfirearm suicides. Although synthetic-control analyses showed that the enactment of Connecticut's legislation was associated with only a 1.6% reduction in firearm suicides, the reduction increased to 13.7% following increased enforcement of the law after the 2007 Virginia Tech shooting. Thus we estimated that the increased enforcement of Connecticut's firearm seizure law may have prevented 128 firearm suicides between 2007 and 2015 while contributing to 140 nonfirearm suicides.

Differences across states after enactment of the laws were generally specific to suicides with firearms, and evidence for a replacement effect was mixed. Little evidence of a replacement effect was found in Indiana, and results showed a substantial aggregate decrease in suicides. Connecticut's increased enforcement of the law was associated with a sustained decrease in firearm suicides coupled with a sustained increase in nonfirearm suicides, compared with its synthetic counterpart. Although so-called placebo tests showed that Connecticut's increase in nonfirearm suicides was not atypical, increased enforcement appears to have resulted in a moderate aggregate increase in suicides.

Our estimates are arguably high compared with those of Swanson and colleagues (3), who estimated that 72 suicides were prevented in Connecticut between 1999 and 2013. Our higher rates for 2007–2015 could have been partly due to observed continuous increases in enforcement after 2007 and differences in methodology. Thus, although convergence between results across methods suggests that many firearm suicides may have been prevented, our point estimate may be higher than the true count.

Some limitations should be considered. First, our analysis was conducted at the state level, and thus we were unable to look at regional variations in the implementation of firearm seizure laws in Indiana and Connecticut, and variations in other laws (for example, Connecticut's Permit-to-Purchase legislation introduced in 1995) complicate the policy picture. Second, we were unable to account for precise variations in the enforcement of this legislation over time. Finally, although we included a variety of identified risk factors for population-level suicide rates, it is possible that there were additional factors for which we could not account. Despite these limitations, use of the synthetic-control methodology provided a rigorous analysis of the effect of firearm seizure laws on suicide rates, and these results were robust to alternative specifications.

TABLE 3. Association between enactment and enforcement of firearm seizure laws in Indiana and Connecticut and suicide rates[a]

Suicide method and state	Annualized suicide rate per 100,000 population			
	IRR	95% CI	p	RMSPE
All methods				
Indiana	.95	.92–.99	<.01	1.87
Connecticut[b]	1.00	.96–1.04	.92	2.06
Connecticut[c]	1.01	.97–1.04	.68	2.00
Firearm				
Indiana	.90	.87–.94	<.001	1.61
Connecticut[b]	.84	.80–.89	<.001	1.62
Connecticut[c]	.88	.85–.92	<.001	1.61
Nonfirearm				
Indiana	1.10	1.06–1.14	<.001	.78
Connecticut[b]	1.03	.99–1.07	.09	.81
Connecticut[c]	.97	.94–1.00	.09	.78

[a] Results are estimates. IRR, incidence rate ratio. RMSPE, root mean square prediction error. All analyses controlled for several percentages (state population Hispanic, white, black, male, age 15–24, age ≥65, high school graduate, below the federal poverty threshold, unemployed) and for the violent crime rate (per 100,000 population), population density (residents per square mile), and spirit alcohol consumption (mean gallons of ethanol per capita).
[b] Effects of 1999 enactment of law
[c] Effects of 2007 meaningful enforcement of law

CONCLUSIONS

Even though risk-based firearm seizure laws have typically been enacted in response to mass homicides, the laws have functioned primarily as a means of seizing firearms from suicidal individuals. These findings suggest that firearm seizure legislation is associated with meaningful reductions in population-level firearm suicide rates, with mixed evidence for a replacement effect.

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