Association between Connecticut's permit-to-purchase handgun law and homicides

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April 6, 2015

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Abstract

Objectives

We estimate the effect of Connecticut's adoption of a handgun purchaser licensing law in October 1995 on subsequent homicides.

Methods

We compare Connecticut's homicide rates after the law change to rates we would have expected if the law had not been adopted, using the synthetic control method. To estimate the counterfactual and the law's effect on homicide rates, we use longitudinal data from a weighted combination of comparison states identified based on the ability of pre-law homicide trends in those states to predict pre-law homicide trends in Connecticut.

Results

We estimate that the law was associated with a 40% reduction in Connecticut's firearm homicide rates during the first 10 years that the law was in place. In contrast, there is no evidence for a reduction in nonfirearm homicides.

Conclusions

Consistent with prior research, this study demonstrates that Connecticut's handgun purchaser licensing law is associated with a subsequent reduction in homicide rates. As would be expected if the reduction is driven by the law, the policy's effects are only evident for homicides committed with firearms.

Homicide was the second leading cause of death for 15 to 34 year-olds in the United States from 1999 to 2011[1] and the second leading contributor to racial disparities in premature mortality among men.[2] Firearms are used in more than two-thirds of homicides in the U.S.[3] and firearm availability, especially to high-risk groups,[4,5] is positively associated with homicide risks.[6,7]

Given the importance of firearms in lethal violence, many federal and state policies are designed to prevent individuals with a history of violence, criminal behavior, substance abuse, or serious mental illness from accessing firearms. Although federal law exempts private, unlicensed sellers from background check requirements, seventeen of the 50 states and the District of Columbia have their own background check requirements for firearm sales by unlicensed private sellers,[8] and eleven have handgun purchaser licensing laws, also known as permit-to-purchase (PTP) laws. PTP laws require individuals to obtain a permit or license to purchase a handgun (from both licensed retail dealers and private sellers) that is contingent upon passing a background check. In eight states, individuals must apply for a PTP in person at the law enforcement agency initiating the background checks and issuing permits. In the other 42 states, pre-gun-sale background checks are initiated through a licensed gun dealer, although there is significant diversity among these policies.

We conduct the current study to estimate the impact of Connecticut's adoption of a PTP law in October 1995. Under this law, the state's Department of Emergency Services and Public Protection accepts prospective handgun purchaser's applications and fingerprints and, if the applicant passes the background check, issues permits to purchase handguns that are valid for 5 years.

Methods

Study Design and Data

To estimate the effect of Connecticut's PTP law on homicides, we compare Connecticut's homicide rates observed after the law change to the rates we would have expected if the law had not been adopted. To estimate the law's effect on homicide rates, we use longitudinal data from a weighted combination of comparison states with no PTP law change (henceforth, Connecticut's synthetic control). The comparison states are weighted based on the ability of their pre-law homicide trends and covariates to predict pre-law homicide trends in Connecticut.

States that are considered as potential comparison states for Connecticut are those that were "at-risk" to have a new PTP law implemented in 1995. Ten states (Hawaii, Illinois, Iowa, Missouri, Massachusetts, Michigan, Nebraska, New Jersey, New York, and North Carolina) and the District of Columbia are excluded from the pool of possible controls because they had a PTP law prior to 1995. We use annual covariate and outcome data from Connecticut and each of the 39 states in the control pool during the period 1984-2005. We conclude the post-law period in 2005 to limit extrapolation in our predictions of the counterfactual to 10 years, as has been done previously.[9] This study was determined not to be human subject research by the Institutional Review Board at the Johns Hopkins Bloomberg School of Public Health.

Outcomes

We examine two outcomes—firearm-specific homicide rates and nonfirearm-specific homicide rates (number of homicides per 100,000 state residents), obtained from the Centers for Disease Control and Prevention's compressed mortality data accessed through the Wide-ranging Online Data for Epidemiologic Research (WONDER) database (http://wonder.cdc.gov/mortSQL.html).

We would expect the impact of the PTP law—if any—to be limited to homicides committed with firearms. We would not expect nonfirearm homicides to be affected by the law.

Covariates

State-level covariates, measured annually, and their sources, follow. Population size, population density (log-transformed), proportion 0-18 years, proportion 15-24 years, proportion black (log-transformed), proportion Hispanic (log-transformed), proportion ≥ 16 years living at or below poverty, and income inequality as measured by the Gini coefficient are from the U.S. Census Bureau. Average per capita individual income and number of jobs per adult are from the Bureau of Economic Analysis. Proportion living in metropolitan statistical areas, law enforcement officers per 100,000 residents, and robberies per 100,000 residents are from the Federal Bureau of Investigation's Crime in the United States publications. The Census of State and Local Governments provides data on annual expenditures for law enforcement (current operation and capital outlay).

Statistical Analysis

We use the synthetic control group approach of estimating policy impacts of Abadie, Diamond, and Hainmueller (2010)[9] to create a weighted combination of states that exhibits homicide trends most similar to Connecticut's prior to the law's implementation (1984-1994). This weighted combination of states can be thought of as a "synthetic" Connecticut, whose homicide trends in the post-law period estimate the post-1994 trends that Connecticut would have experienced in the absence of the law change.

The algorithm for creating the weights has been described previously.[9] The vector of weights minimizes a measure of the distance between the vector of outcomes and covariates of Connecticut in the pre-law period and the weighted vector of outcomes and covariates of the control pool

states in the pre-law period.[9] The distance function minimized is $\sqrt{(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})'\mathbf{V}(\mathbf{X_1} - \mathbf{X_0}\mathbf{W})}$, where $\mathbf{X_1}$ is the vector of length k of pre-intervention outcomes and covariates that are predictive of homicide rates for Connecticut, $\mathbf{X_0}$ is the $k \times n$ matrix of k pre-intervention outcomes and predictive covariates for each of the n states in the control pool, $\mathbf{W}$ is the n-length vector of weights, and $\mathbf{V}$ is a $k \times k$ positive definite, diagonal matrix that minimizes the mean squared prediction error (MSPE). Note that no data from after the law change (1995 or after) is used in creating the weights and synthetic control. This method makes the following assumptions: 1) no interruptions in the law following passage in October 1995 and no effects of the law prior to 1995, 2) no interference between states (i.e., Connecticut's PTP law does not affect homicide rates in other states), 3) no unobserved confounders that change between the pre- and post-law period, and 4) linear relationships between homicide rates and covariates.

After creating the weights using the Synth package in R,[10] we compare homicide rates between Connecticut and its synthetic control in the 10 years following implementation of the PTP law: 1996-2005. We exclude 1995, as the law was not implemented until October of that year. We exclude 2001 from the nonfirearm homicide analysis because of the large increase in deaths attributable to the 2001 terrorist attacks, which had a disproportionate impact on Connecticut residents. The estimated number of homicides prevented by the law from 1996 to 2005 was calculated by multiplying the difference in homicide rates between Connecticut and its synthetic control by Connecticut's population size (in 100,000s) each year and summing across the years.

Statistical significance of the estimated differences in homicide rates between Connecticut and its synthetic control is assessed using a permutation-based test, also called a placebo or falsification test, that is similar to Fisher's Exact Test.[9,11] For each outcome, we repeat the analyses treating each of the 39 states in the control pool as the "treated" state, creating a synthetic control for each. We calculate the proportion of control states with an estimated rate of homicides prevented that is as or more extreme than the estimated rate prevented for Connecticut. This proportion is akin to

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the p-value and indicates how unusual Connecticut's estimated effect is compared to the states in the control pool.

However, not every control state's homicide rate over time is able to be well-approximated by a synthetic control. Lack of fit is determined by greater MSPE, which is the average of the squared differences between homicide rates in Connecticut and its synthetic control during the prelaw period. In cases of large MSPE, it is not appropriate to use the synthetic control as a comparison. Consequently, we calculate the proportions of control states with results as or more extreme than Connecticut for three separate control pools, including control states whose MSPE from their synthetic control was no more than 1) 20 times, 2) 5 times, and 3) 2 times that of Connecticut's synthetic control MSPE. This entire synthetic control process is conducted twice, once for firearm homicides and once for non-firearm homicides

Sensitivity Analysis

In the Supplementary Appendix, we consider an alternative approach in which we compare Connecticut's homicide rate trends to the 39 control states' average trends that are mean-shifted to the scale of Connecticut's homicide rates.

We use R version 3.0.2 for all analyses.[12]

Results

Using the predictive covariates as well as the pre-law outcome data, we construct a synthetic control for Connecticut for each of the two outcomes of interest. States with a nonzero weight contribute to the synthetic control and are listed in Table 1. Table 1 also shows how well each of the synthetic controls approximates Connecticut's homicide rates during the pre-law period, as measured by MSPE. The last row of this table shows that each synthetic control is a better fit than a simple

average of all the states in the control pool. For example, in the case of firearm homicides, the synthetic control has a MSPE of 0.157, which is an order of magnitude less than the MSPE if a simple average of all control states were used.

[Table 1 here]

Table S1 in the Supplementary Materials shows descriptive statistics for each of the covariates found to be predictive of homicide rates during the pre-law period (1984-1994). These variable summaries are provided for Connecticut, the pool of control states, and Connecticut's synthetic control optimized for 1) firearm and 2) nonfirearm homicides.

Figures 1 and 2 compare firearm and nonfirearm homicide rates over time between Connecticut and its outcome-specific synthetic control. Figure 1 compares Connecticut's firearm homicide rates to the rates of its synthetic control over the study period. The average firearm homicide rates over the study period for all states in the control pool are included for reference. Firearm homicide rates of Connecticut and its synthetic control track together prior to the law's implementation in October 1995; this was also evidenced by the low MSPE in Table 1. However, several years after the law's passage, the rates diverge markedly. Connecticut's firearm homicide rate continues to decline before leveling off in the early 2000s, while its synthetic control's firearm homicide rate levels off about five years earlier. Summing the differences between Connecticut and its synthetic control during the 1996-2005 period in Figure 1, we estimate that the law was associated with 296 fewer firearm homicides during this period (Table 2), a reduction of 40% relative to the counterfactual.

The permutation tests are consistent with this graphical intuition and indicate that Connecticut's divergent firearm homicide trends during the post-law period are unusual. None of the 30 potential control states with a MSPE $\leq$ 5 times that of Connecticut's have firearm homicide trends that diverge as widely from their synthetic controls as Connecticut's divergence (Table 2).

[Figure 1 here]

Figure 2 shows nonfirearm homicide trends in Connecticut in comparison to its synthetic control

and to all states in the control pool. Again, Connecticut's nonfirearm homicide rate trend tracks closely with that of its synthetic control's prior to the PTP law's implementation. But, in contrast to the firearm homicide data, rates for Connecticut and its synthetic control do not diverge following passage of the law, tracking together to the end of the study period. Summing the differences between Connecticut and its synthetic control from 1996 to 2005 in Figure 2, we estimate that the law is associated with 24 fewer nonfirearm homicides during this period than expected (Table 2). Again, the permutation tests are consistent with what seems apparent from the graph. Any divergence between Connecticut's nonfirearm homicide rates and that of its synthetic control during the post-law period was not unusual compared to the control states.

[Figure 2 here]

[Table 2 here]

Discussion

This study estimates the impact of Connecticut's PTP handgun law on homicides. Prior studies have suggested that PTP laws may be effective for preventing the diversion of guns to criminals,[13–15] and a sharp increase in gun homicides following the repeal of Missouri's PTP law suggests that PTP laws may reduce lethal violence.[16] Consistent with these prior studies, this study demonstrates that Connecticut's PTP law is associated with a subsequent reduction in homicide rates. As would be expected if the reduction is driven by the PTP law, the policy's effects are only evident for homicides committed with firearms.

Connecticut's firearm homicide rate trends depart substantially from that of its synthetic control for the period 1999-2005, several years after the law went into effect. This may call into question whether the estimated effect over the 10-year post-law period was due to the PTP law or to other unmeasured interventions that were put into place beginning in 1999 that selectively re-

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duced homicides committed with firearms. However, there are plausible explanations for a delayed policy effect. First, spikes in gun sales may occur just prior to a significant gun control law going into effect and the additional guns sold under less rigorous regulation could temporarily counteract preventive effects of the law.[17] Second, the number of high-risk handgun transactions blocked by the PTP law may accumulate over time until gun availability in the underground market is sufficiently constrained. The net effect of these two opposing forces—pre-law sales uptick and post-law downturn—may result in fewer high-risk gun acquisitions several years after implementation. Such a delayed effect was observed following Maryland's ban of small, poorly constructed handguns that were over-represented in crime.[17]

It is plausible that Connecticut's PTP law could reduce firearm homicide rates as substantially as the 40% reduction estimated. PTP laws make it easier for private sellers of firearms to discern if a potential purchaser has passed a background check. In addition, it also changed the process for handgun purchases from licensed firearm dealers. Rather than go directly to a gun shop where the owner or employee would review a purchase application and send the information to initiate a background check, the PTP law required prospective handgun purchasers to apply for a permit in person with a state law enforcement agency that photographed and fingerprinted applicants. Such a process may dissuade potential straw purchasers (those who buy guns for prohibited persons) or others purchasing handguns with the intention of committing a crime. It is also noteworthy that much of Connecticut is bordered by New York and Massachusetts, which have comprehensive PTP laws that provide law enforcement discretion to deny handgun purchase permits if there is reason to believe that the applicant poses a significant risk without meeting a legal disqualifying condition. This may reduce the ease of circumventing Connecticut's PTP law through out-of-state suppliers. Finally, Connecticut's PTP law increased the minimum legal age for handgun purchase from 18 to 21 years, blocking an age group (18-20 year-olds) with a high homicide offending rate.[18]

Estimating state laws' effects on health and safety requires researchers to estimate the counterfactual—

health and safety outcomes in a world in which the law was not implemented but all else remained equal. This is typically done by comparing outcomes over time between state(s) with the law and state(s) without the law. The synthetic control method used in this analysis is appropriate for the comparative case-study design and is related to the difference-in-differences approach to estimating intervention effects.[9] This method has gained popularity recently, and has been used in estimating economic and health policy effects.[9,19–22] The advantages of this approach and its assumptions have been discussed previously.[23]

The first assumption of the synthetic control approach is that there were no interruptions in the law and no effects prior to its implementation. The law could not go into effect prior to October 1, 1995 and there is no evidence that the law's implementation was interrupted. However, as stated above, it is plausible that more handguns were purchased just prior to the PTP law's implementation as a result of the law.

Assumption 2 states that the implementation of the PTP law affects Connecticut only and has no effect on other states' homicide rates. If this assumption is violated in this study, there is no appealing strategy for relaxing it for the synthetic control method. One approach would be to restrict the analysis to states that are not geographically near Connecticut. The drawback of this strategy is that states like Rhode Island and New Hampshire, which were large contributors to Connecticut's synthetic control, would be excluded, leaving fewer reasonable points of comparison for Connecticut's trends.

The third assumption is that there are no unmeasured confounders during the post-law period. This is an untestable assumption given the absence of randomization of PTP law implementation across states. However, the synthetic control group provides good fit to Connecticut's homicide rates during the 1984-1994 pre-law time period. In addition, intrastate correlation of state homicide rates across years 1984-2005 is very high, ranging from 0.84-0.97. Thus, a synthetic control that fits well during the pre-law period is likely to fit well during the post-law period as well.

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Fixed effects regression models are one popular way of estimating the effects of state laws while also controlling for variables that may potentially confound this estimate. However, we believe the fixed effects regression approach to be inappropriate in this case for several reasons. First, it assumes that all states and time periods are at-risk for PTP law implementation, and that the association between PTP law implementation and homicide rates is the same for all states. It is unlikely that the association between the law and homicide rate is the same for every state. Furthermore, we have very little data with which to evaluate this assumption, because only one other state implemented a PTP law during the study period (Nebraska implemented a PTP law 1991 that differs in important ways from Connecticut's). In addition, fixed effect regression models fail to recognize the comparative case-study design of both the data and research question and inappropriately extrapolate to the pool of control states.

The study goal is to estimate the effect of Connecticut's PTP law on homicides in Connecticut—not to extrapolate the effect of Connecticut's law on homicides in an average control state. The synthetic control approach allows us to estimate such an effect and appropriately restricts the interpretation to the state of Connecticut. In addition, the method of assessing significance of the estimated results is more appropriate than a large sample inferential technique, like regression, given the small number of units.[9] Other advantages of this method over standard regression methods include that 1) estimation of policy effects are data-driven (through the synthetic control weights) to produce the most accurate counterfactual and 2) it incorporates both graphical and numerical checks (via the MSPE) of how well the comparison approximates the case in the comparative case study.

This study has important policy implications as state and federal lawmakers consider options for reducing gun violence. The findings, in addition to other research,[16] suggest that PTP laws may reduce firearm-specific homicides. Following the process in place in six states now, the most recent federal legislation considered by Congress to require background checks for many private

party transactions would require prospective purchasers to go a federally licensed gun dealer who would process the purchase application and submit the information for the background check. Future research should compare the effectiveness of this approach versus the approach used in PTP laws. Other unexamined issues include standards of evidence to hold noncompliant gun sellers accountable and the significance of penalties for failure to comply with gun sales laws.

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Contributors

KER contributed to the study design and interpretation of results, and led the analysis, drafting, and revision of the article. EAS contributed to the study design, analysis, interpretation of results, and article revisions. JSV contributed to interpretation of the results and article revisions. DWW conceptualized the study and contributed to interpretation of results, drafting, and revision of the article.

Acknowledgements

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Table 1: States with non-zero weights in the synthetic Connecticut for 1) firearm homicide rates and 2) nonfirearm homicide rates. 39 states were included in pool of possible controls. Ten states with a similar law passed prior to 1995 were not included: Hawaii, Illinois, Iowa, Missouri, Massachusetts, Michigan, Nebraska, New Jersey, New York, North Carolina.

State	Weight	
	Firearm Homicides	Nonfirearm Homicides
California	0.036	0.000
Maryland	0.147	0.110
Nevada	0.087	0.121
New Hampshire	0.005	0.724
Rhode Island	0.724	0.046
MSPE[a] Synthetic control / all control states	0.157 / 1.663	0.090 / 0.740

[a]Mean squared prediction error

Table 2: Estimated number of homicides prevented, 1996-2005. Results from permutation tests including control states whose synthetic control's MSPE[a] that is no more than $20\times$, $5\times$, and $2\times$ that of the MSPE of Connecticut's synthetic control: proportion of control states with results as or more extreme than Connecticut.

	Firearm	Nonfirearm
Estimated homicides prevented	296	24
Proportion more extreme, $20 \times$ MSPE	3/38	13/39
Proportion more extreme, $5 \times$ MSPE	0/30	11/32
Proportion more extreme, $2 \times$ MSPE	0/24	8/26

[a]Mean squared prediction error

Figure 1: Firearm homicide rates. Connecticut (solid line) compared to synthetic Connecticut (dashed line) and all states in the control pool, equally weighted (dotted dashed line). The vertical line indicates when Connecticut's PTP law was implemented.
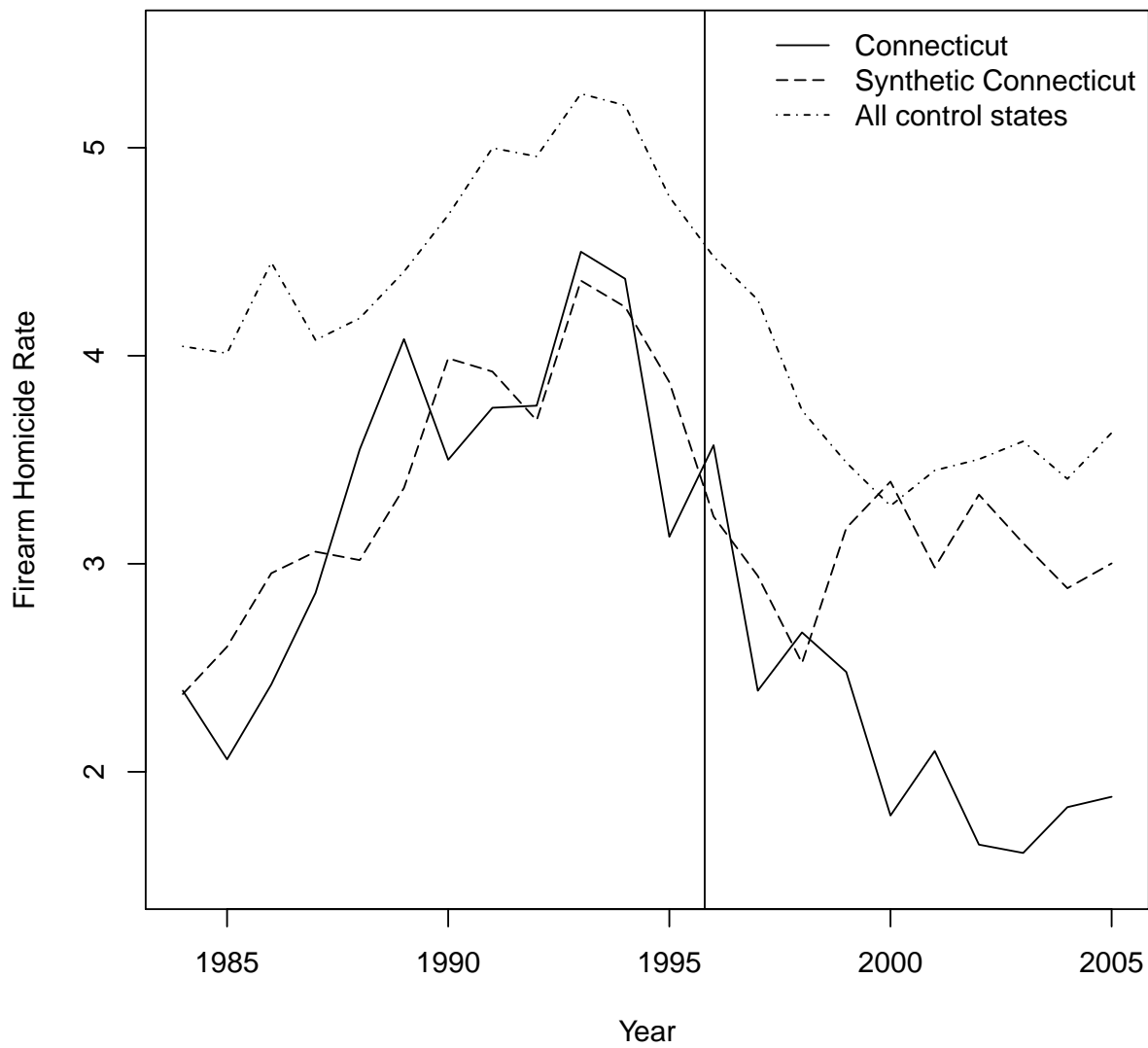
Figure 2: Nonfirearm homicide rates. Connecticut (solid line) compared to synthetic Connecticut (dashed line) and all states in the control pool (dotted dashed line). The vertical line indicates when Connecticut's PTP law was implemented. Rates for 2001 are not included due to the World Trade Center attacks.