Original article

California's comprehensive background check and misdemeanor violence prohibition policies and firearm mortality

Alvaro Castillo-Carniglia, PhD, MSc [a,b,*], Rose M.C. Kagawa, PhD, MPH [a],
Magdalena Cerdá, DrPH, MPH [a,c], Cassandra K. Crifasi, PhD, MPH [d],
Jon S. Vernick, JD, MPH [d], Daniel W. Webster, ScD, MPH [d], Garen J. Wintemute, MD, MPH [a]

[a] Violence Prevention Research Program, Department of Emergency Medicine, UC Davis School of Medicine, Sacramento, CA
[b] Society and Health Research Center, Facultad de Humanidades, Universidad Mayor, Santiago, Chile
[c] Department of Population Health, New York University School of Medicine, New York
[d] Center for Gun Policy and Research, Johns Hopkins Bloomberg School of Public Health, Baltimore, MD

ARTICLE INFO

ABSTRACT

Purpose: In 1991, California implemented a law that mandated a background check for all firearm purchases with limited exceptions (comprehensive background check or CBC policy) and prohibited firearm purchase and possession for persons convicted within the past 10 years of certain violent crimes classified as misdemeanors (MVP policy). We evaluated the population effect of the simultaneous implementation of CBC and MVP policies in California on firearm homicide and suicide.
Methods: Quasi-experimental ecological study using the synthetic control group methodology. We included annual firearm and nonfirearm mortality data for California and 32 control states for 1981—2000, with secondary analyses up to 2005.
Results: The simultaneous implementation of CBC and MVP policies was not associated with a net change in the firearm homicide rate over the ensuing 10 years in California. The decrease in firearm suicides in California was similar to the decrease in nonfirearm suicides in that state. Results were robust across multiple model specifications and methods.
Conclusions: CBC and MVP policies were not associated with changes in firearm suicide or homicide. Incomplete and missing records for background checks, incomplete compliance and enforcement, and narrowly constructed prohibitions may be among the reasons for these null findings.

Introduction

Firearm violence is one of the leading causes of death and injury in the United States, resulting in more than 38,000 deaths in 2016 [1]. Firearm ownership and access are risk factors for death from both suicide and homicide [2—6], and firearm access is a necessary precondition for committing firearm-related violent crimes.

Federal law prohibits certain categories of individuals from purchasing or possessing firearms; examples include persons convicted of felonies or domestic violence misdemeanors [7]. To help prevent prohibited persons from acquiring firearms, the Brady Handgun Violence Prevention Act requires that purchases from federally licensed retailers be subject to a background check. Since Brady's inception in 1994, more than 3 million attempted purchases by prohibited persons have been denied [8]. Sales by unlicensed private parties are exempt from background check requirements in many states; however, it is estimated that more than 20% of all firearm acquisitions do not involve background checks [9]. About 80% of all firearms acquired for criminal purposes—96% of those acquired by prohibited persons—are obtained through private-party transfers [10].

Among legal purchasers of firearms, as in the general population, a history of violence is strongly associated with an increase in risk for future violence [11]. A prospective study of California handgun purchasers found that individuals with a

single prior conviction for a nonprohibiting violent misdemeanor crime (such as assault and battery) were nearly five times as likely as those with no prior criminal history to be arrested for a subsequent firearm-related or violent offense [12]. For purchasers with multiple such prior convictions, risk was increased by a factor of 15.

In 1991, California mandated background checks for nearly all firearm sales (a comprehensive background check [CBC] policy) and a 10-year prohibition on gun purchase and possession for persons convicted of most violent misdemeanor crimes (a misdemeanor violence prohibition [MVP] policy). These policies are complementary. Expanded background check requirements are meant to create an additional barrier to firearm access for prohibited persons; nationally, they are associated with a lower proportion of private-party firearm sales conducted without background checks (26% vs. 57%) [9]. Expanded prohibitions reflect an intent to reduce violence through preventing access to firearms by larger numbers of high-risk individuals.

We know little about the effectiveness of CBC policies. Studies showing clear benefits have focused on permit-to-purchase (PTP) laws, a particularly rigorous subset of CBC policies that require a background check and a permit, typically issued by a law enforcement agency, to purchase a firearm [13–17]. Some cross-sectional, ecological studies of CBC policies have shown negative associations between CBC laws and firearm mortality [18,19]. However, a more rigorous time-series analysis found no effect on firearm suicide and homicide rates from repealing CBC policies in two states [20]. Newly enacted CBC policies led to increases in background checks, presumably the principal mechanism by which they would exert intended effects on violence, in only 1 of 3 states studied [21].

Incomplete compliance and enforcement have been suggested as possible reasons for these findings. The possibility of these mechanisms of action is reinforced by studies showing benefits to more thorough background checks [22,23] and by well-known instances of violence, including mass shootings, where prohibited persons purchased firearms because the data on which their background checks were performed were incomplete [24].

Evaluations of MVP policies have yielded positive results, but the literature is sparse. At the individual level, a controlled longitudinal study of California's MVP policy found that denial of firearm purchase because of a prior violent misdemeanor conviction was associated with a substantial reduction in risk of arrest for future violent or firearm-related crimes [25]. A recent multistate population-level study found similar benefits from MVP policies for intimate partner homicide [15].

The objective of our study was to evaluate the effects of California's CBC and MVP policies on firearm-related homicide and suicide. Given their simultaneous implementation and limited possibilities for estimating individual policy effects (both were intended to prevent high-risk people from acquiring firearms), we evaluated the two policies together.

Methods

Design and study sample

We used a quasi-experimental design at the state level, with California as the treated state and "treatment" defined as the simultaneous implementation of CBC and MVP policies in 1991. The control units, also known as the donor pool, were 32 states that did not have CBC or MVP policies at the start of the study period and did not implement them or other major firearm policy changes during that period (Table 1). The main analysis considered the preintervention period to be all years before the intervention for which data were available (1981–1990) and assessed effects for 10 years postintervention (1991–2000).

Data sources and variables

Outcomes: Our main outcomes were the annual rates of firearm-related homicides and suicides per 100,000 people, available from the US Centers for Disease Control and Prevention [1]. As these data do not include numbers when there are fewer than 10 cases, we performed simple imputation using linear regression. This resulted in the imputation of 2 years for New Hampshire, South Dakota, Vermont, and Wyoming, and 1 year for Delaware. We rejected multiple imputation because inference in the synthetic control group method does not rely on variance estimates (the main concern in single imputation methods) but on permutation tests (see Supplemental Material).

To account for potential spurious associations and explore the influence of additional exogenous factors, we included rates of non−firearm-related homicides and suicides as negative control outcomes. The rationale is that these outcomes should not be affected by policies restricting access to firearms, but if there is a relationship, it should be in the opposite direction (i.e., other

Table 1
States with nonzero weights in synthetic California for firearm and nonfirearm homicide and suicide rates[*]

State	Firearm homicide[†]	Nonfirearm homicide[†]	Firearm suicide[‡]	Nonfirearm suicide[‡]
Alaska	0	0.021	0	0
Arizona	0	0.015	0	0
Colorado	0	0.123	0	0
Georgia	0.101	0	0	0
Louisiana	0.259	0	0	0
Nevada	0	0.2	0	0.308
New Mexico	0	0.039	0	0
Ohio	0	0	0.681	0.237
Texas	0	0.603	0.319	0
Virginia	0.566	0	0	0
Wisconsin	0.073	0	0	0.455
RMSPE synthetic control/all control states	0.299/2.408	0.230/1.675	0.294/2.191	0.482/1.811

[*] States in the donor pool (n = 32): Alabama, Alaska, Arizona, Arkansas, Colorado, Delaware, Florida, Georgia, Idaho, Kansas, Kentucky, Louisiana, Maine, Minnesota, Mississippi, Montana, Nevada, New Hampshire, New Mexico, Ohio, Oklahoma, Oregon, South Carolina, South Dakota, Texas, Utah, Vermont, Virginia, Washington, West Virginia, Wisconsin, Wyoming.

[†] Covariates included in the homicide models are percentage Hispanic; percentage black; percentage male; percentage living below the federal poverty line; percentage unemployment; percentage of population aged 15−29 years; percentage of population aged older than or equal to 65 years; number of gallons of ethanol from spirits consumed per capita; percentage veterans; gun availability (annual); outcomes at 1984, 1987, and 1990.

[‡] Covariates included in the suicide models are the same as[†], plus the natural logarithm of the states' populations.

methods would be substituted for firearms, increasing the rates of non—firearm-related deaths). A decline in the rates of non—firearm-related homicides and suicides associated with the implementation of CBC and MVP policies would likely be the result of other unmeasured confounders.

Covariates: Based on previous research [17,20] and model performance (lowest root mean square prediction error [RMSPE]), we defined the following set of covariates: percentage of people 15—29 years of age; percentage of people older than or equal to 65 years of age; logarithm of the population (which improved the RMSPE only for the suicide models); percentages of the population who were white, Hispanic, and males [1]; living below the federal poverty line, veterans [26], and unemployed [27]; the per capita consumption of gallons of ethanol from spirits by people aged older than or equal to 14 years [28]; and as an indicator of gun availability, firearm suicides as a percentage of total suicides [29,30]. We also included as predictors in the models the values of each of the outcomes at three time points in the preintervention period; using three time points yielded the lowest RMSPE: 1984, 1987, and 1990 [31,32].

In generating the final models, we removed variables with low V-weights, that is, variables with low predictive values in final models. Variables tested but not included were additional age and race/ethnicity categories; percentages of people with different categories of marital status and religion; an indicator for state mental health parity laws; a measure of the crack epidemic, which incorporates cocaine-induced emergency room visits, deaths, arrests, among other proxies [33]; and a violent crime index [34].

Statistical analyses

For the main analysis, we used the synthetic control group method, which aims to generate a trend counterfactual to the observed outcome by creating a weighted average of the states in the donor pool [32].

The policy effect is estimated as the difference between the values in the treated state (California) and the values in the synthetic control group (synthetic California) in the post-intervention period. Consistent with other studies that have used this method [20], we averaged the annual differences across the 10 years after CBC and MVP implementation (to the year 2000); in secondary analyses, we also considered 5 years (to 1995) and 15 years (to 2005) after the intervention. We did not include longer postintervention periods to avoid forecasting counterfactual trends too far removed from the preintervention period.

Given that the synthetic control group method does not produce traditional measures of uncertainty (e.g., 95% confidence intervals), inference is based on permutation tests, also known as placebo tests (see Supplemental Material).

To account for imperfect fit in the preintervention period, we provided estimates that subtracted the preintervention average difference between California and the synthetic control from the postintervention difference (as in a difference-in-difference estimator) [35,36]. In addition, we showed results produced by states that had a comparable fit in the preintervention period, that is, RMSPE less than or equal to 5 and less than or equal to 2 times the RMSPE for California [13].

We conducted multiple sensitivity analyses, which included removing states that prohibited firearm purchases by people convicted of domestic violence before the national enactment of such a law in 1996, testing for a delayed and gradual effect of CBC/MVP policies, restricting the population to the age groups that have the greatest risk of firearm-related homicide and suicide, and changing the methodological approach to estimate the results (see Supplemental Material).

All analyses were conducted using Stata 14.1 (StataCorp, College Station, Texas, USA).
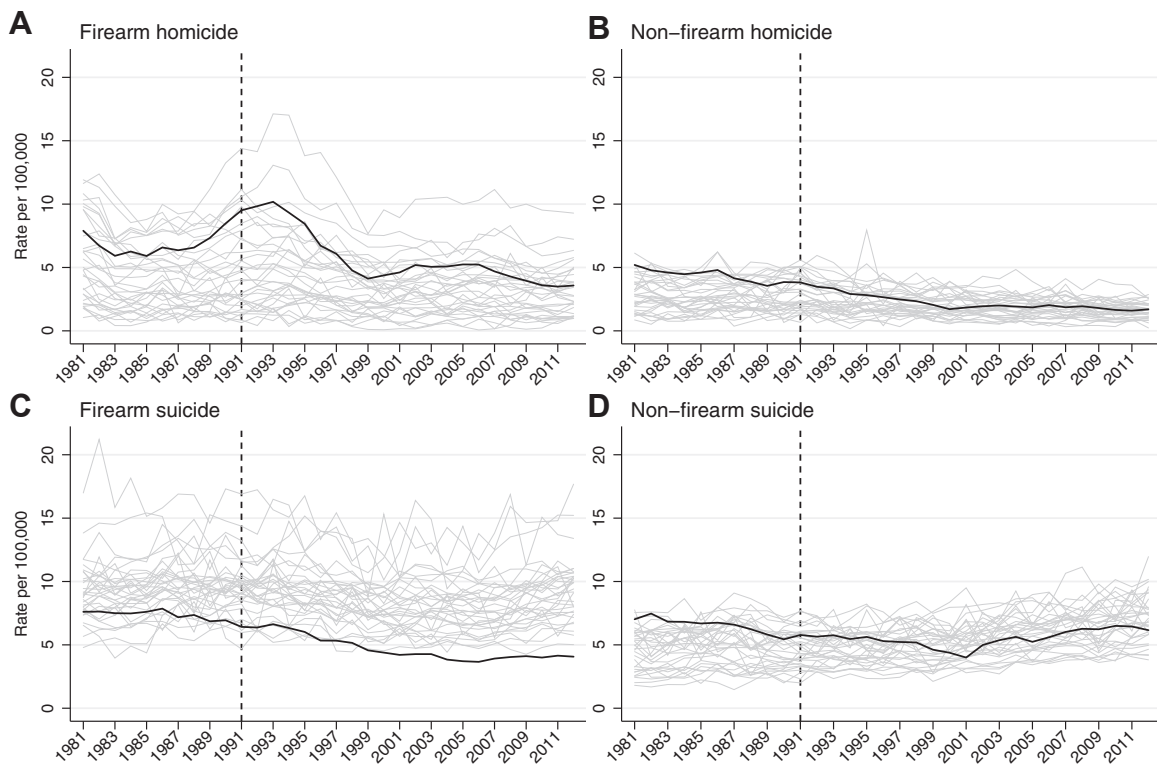
Fig. 1. Trends in annual rate of firearm homicides (A), non-firearm homicides (B), firearm suicides (C) and non-firearm suicides (D) per 100,000 people in California and all control.

Results

Annual trends in firearm and nonfirearm homicide and suicide rates are in Figure 1. California experienced a large increase in firearm-related homicides from the mid-1980s until the early 1990s (peaking at 10.2/100,000 people in 1993). A sharp decline followed until approximately 2000, then relative stabilization until 2012. Non−firearm-related homicides showed a stable decline, from the beginning of the time series until the first years of the 2000s.

For firearm-related suicides, there was an overall decline, concentrated mostly between the years 1997 and 2000. Non-−firearm-related suicides showed a similar trend but with an increase from 2002 to the last years of the series.

Results from the synthetic control group method

Of the 32 states in the donor pool, 11 had nonzero weights and were included in one or more of the synthetic controls for the four outcomes (Table 1). None of the states with imputed data were included in the synthetic controls.

Levels and trends for firearm homicide rates in the preintervention period were similar for California and synthetic California, although the increase in the 2 years before 1991 was slightly higher in California (Fig. 2A). For firearm suicides, California witnessed a similar trend compared with synthetic California until 1988, but a small relative decline thereafter (Fig. 2C). Nonfirearm outcomes for California and all control states are shown in Figure 1B and D. Both were well balanced in the preintervention period in relation to the trend in synthetic California.

Estimated absolute and relative effects of CBC and MVP policies on each outcome and the results from the permutation tests are presented in Table 2. The 10-year postintervention period

provided our primary results. The average difference in the rate of firearm homicides between California and synthetic California in the postintervention period was 0/100,000; for firearm suicides, it was −0.7/100,000, corresponding to a 10.9 percent decrease. Five of the 32 states eligible to serve as controls experienced larger effects for firearm suicides over the same time period in the permutation tests. However, after restricting the comparison states to those with a reasonable preintervention fit (≤2 times the RMSPE for California), no states (out of 11) experienced a decrease larger than California. Consistent results were observed for firearm homicides and suicides at both 5 and 15 years postintervention.

In the 10 years following implementation, the average differences in nonfirearm homicides and suicides were −0.3/100,000 (−9.7 percent) and −0.4/100,000 (−7.0 percent), respectively. For nonfirearm suicides, only one state experienced a larger decrease than California, regardless of the number of control states used as comparison. For the nonfirearm homicide rate, the decline observed after policy implementation was within the range that would be expected given random variation.

Results from sensitivity analyses were consistent with those of the main analysis (see Supplemental Material).

Discussion

This study evaluated the association between rates of firearm-related homicides and suicides and California's simultaneous enactment of two policies aimed at preventing firearms acquisition by people who are at increased risk of interpersonal and self-directed violence: a comprehensive background check requirement and a firearm prohibition for persons convicted of violent misdemeanors. Enactment was not associated with significant and specific changes in rates of fatal firearm violence.
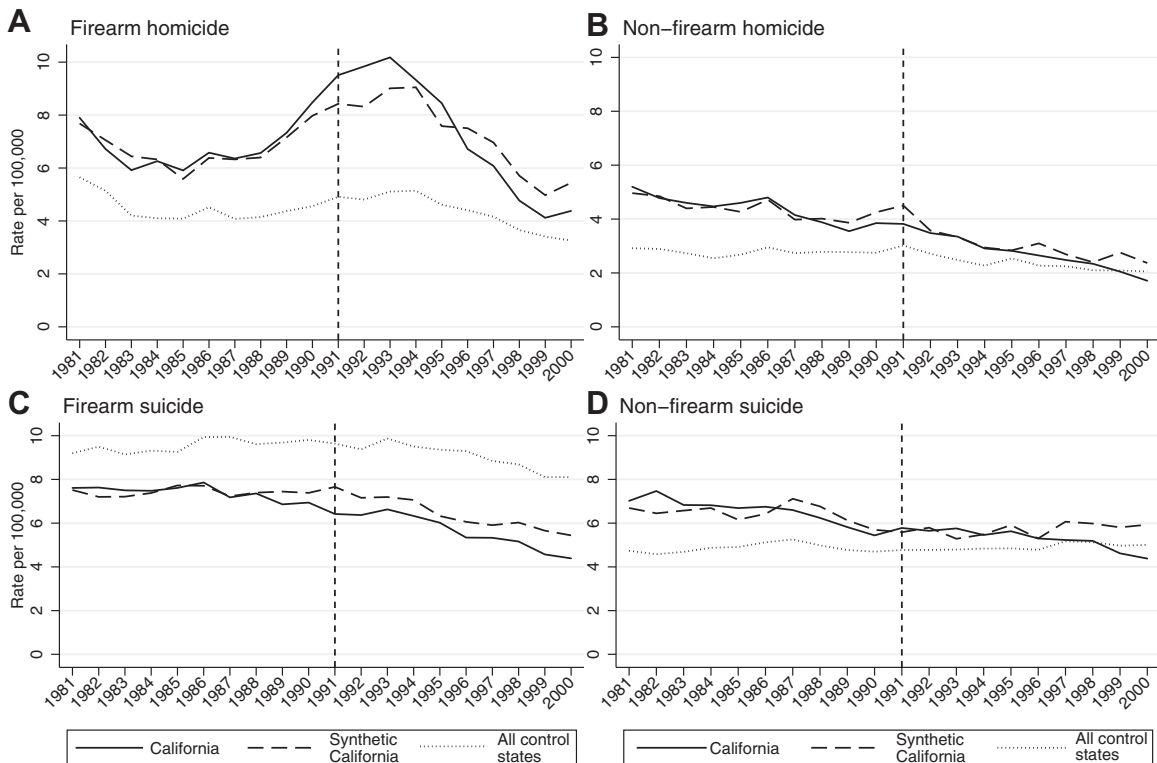
Fig. 2. Trend in annual rate of firearm homicides (A), non-firearm homicides (B), firearm suicides (C) and non-firearm suicides (D) per 100,000 people in California, synthetic California, and average for all control states, 1981−2000.

Table 2
Association between CBC and MVP policies and firearm-related and non-firearm-related homicides and suicides in California for 3 post-implementation periods

	Firearm homicide	Nonfirearm homicide	Firearm suicide	Nonfirearm suicide
Five years postimplementation				
California's rate per 100,000[*]	9.5	3.3	6.4	5.7
Counterfactual rate per 100,000[†]	8.5	3.4	7.1	5.6
Estimated absolute effect of CBC/MVP[‡]	1.0	−0.1	−0.7	0.1
Estimated relative effect (%) of CBC/MVP[§]	11.8	−2.9	−9.9	1.8
Number of states with effect ≥ CA				
All control states[‖]	27/32	11/32	4/32	14/32
≤5 × CA RMSPE	26/30	11/32	3/30	14/32
≤2 × CA RMSPE	15/17	6/18	1/11	12/28
Ten years postimplementation (main results)				
California's rate per 100,000[*]	7.3	2.8	5.7	5.3
Counterfactual rate per 100,000[†]	7.3	3.1	6.4	5.7
Estimated absolute effect of CBC/MVP[‡]	0.0	−0.3	−0.7	−0.4
Estimated relative effect (%) of CBC/MVP[§]	0.0	−9.7	−10.9	−7.0
Number of states with effect ≥ CA				
All control states[‖]	17/32	6/32	5/32	1/32
≤5 × CA RMSPE	16/30	6/32	4/30	1/32
≤2 × CA RMSPE	10/17	2/18	0/11	1/28
Fifteen years postimplementation				
California's rate per 100,000[*]	6.6	2.5	5.1	5.2
Counterfactual rate per 100,000[†]	6.8	2.9	6.2	6.0
Estimated absolute effect of CBC/MVP[‡]	−0.2	−0.4	−1.1	−0.8
Estimated relative effect (%) of CBC/MVP[§]	−2.9	−13.8	−17.7	−13.3
Number of states with effect ≥ CA				
All control states[‖]	11/32	3/32	3/32	1/32
≤5 × CA RMSPE	10/30	3/32	2/30	1/32
≤2 × CA RMSPE	5/17	0/18	0/11	1/28

[*] Mean rate per 100,000 people in California after CBC and MVP implementation.
[†] Mean rate per 100,000 people in synthetic California after CBC and MVP implementation.
[‡] Average difference between California and synthetic California in the postintervention period.
[§] Percentage difference compared with synthetic California.
[‖] Results from the permutation test (control states = 32). To generate comparable estimates across control states, effects were computed as a difference in difference (DiD): $DiD_{state} = (Outcome_{post}^{state} - Outcome_{post}^{Synth}) - (Outcome_{pre}^{CA} - Outcome_{pre}^{Synth})$. Because the hypothesis of the study is that CBC and MVP are associated with reductions in mortality from firearms, we counted only states with reductions in mortality larger than those in California.

Firearm-related suicide rates during the 10 years after policy implementation were, on average, 10.9 percent lower in California than in synthetic California, a difference greater than for any of the 11 control states with a comparable model fit. Non—firearm-related suicides also decreased by 7.0 percent; however, a decrease exceeding that was seen in 27 of 28 states with RMSPE less than or equal to two times the RMSPE for California. This suggests that the policies' estimated impact on firearm suicide may be part of broader changes in suicide risk around the time that California's CBC and MVP policies were implemented. Still, the difference between changes in firearm and non-firearm suicides (3.9 percentage points) may indicate a preventive role of CBC/MVP policies in firearm suicide, although this study was not designed to test whether this difference is statistically meaningful.

Firearm-related homicide rates rose substantially from the mid-1980s through the early 1990s and fell thereafter. Both the increase and the decline were greater in California than in synthetic California; the net difference during the 10 years postintervention was practically 0. Sensitivity analyses testing for delayed and gradual effects did not change the overall conclusions. It is worth noting that the negative slope observed in California in the years following CBC/MVP implementation was more pronounced than the slope observed for the control states; however, the difference in slopes between California and the control group was not statically significant (see Supplemental Material).

Our findings conflict with those of studies associating CBC policies with a reduction in firearm homicide and suicide in Connecticut, where implementation occurred in 1995, and Missouri, where firearm homicide and suicide increased following CBC repeal in 2007 [13,14,17]. However, these states had PTP laws, a particularly rigorous form of CBC policy that several studies have found to be effective [37—39]. Consistent with our findings, repeal by

Indiana and Tennessee in 1998 of CBC policies without a PTP component was recently found not to be associated with changes in rates of firearm homicide or suicide [20].

Other mechanisms for our findings are plausible; however, several or all may be in play simultaneously. One well-documented example, which would diminish the population-level effects of both CBC and MPV policies, is that the criminal and mental health records on which background checks were performed were very incomplete in the 1990s, including in California [37—42]. For example, in 1990, only 25 percent of criminal records were accessible via the interstate identification index, the primary source of arrest and conviction information for background checks [37]. Centralized records of mental health prohibitions were almost nonexistent [37]. As a result, background checks almost certainly produced a large number of false negative results, which is a shortcoming that may have limited the effectiveness of the CBC and MVP policies.

Purchases by undetected prohibited persons would likely decrease the population-level effects of CBC policies and may account in part for negative findings here, in the assessment of CBC repeal in Indiana and Tennessee [20], and in an earlier study of trends in homicide and suicide following the Brady Handgun Violence Prevention Act [43]. Increased thoroughness of background checks and improvements in the data used to perform them are associated with reductions in violent crime, firearm homicide, and firearm suicide [22,23,44—46]. It is therefore important to note that the quality and completeness of the records on which background checks are performed have improved since our study period [47].

Incomplete compliance with and enforcement of background check and prohibition requirements may also play a role. After CBC policies were implemented in Colorado, Delaware, and

Washington, an overall increase in background checks was detected only in Delaware, and incomplete compliance and enforcement were reported in the two western states [21]. Enforcement may not be a law enforcement priority; in the 1990s, chief law enforcement officers in Montana and Arizona sued successfully to avoid conducting background checks [48]. The vigor with which firearm laws are enforced is variable and susceptible to a variety of external factors [49].

Finally, the population-level effect of CBC and MVP policies may be small if only a small number of transactions or individuals are affected. In California, on average, 0.54 percent of handgun purchases were denied before CBC and MVP policy implementation (data available from 1982 to 1990); this rose to 1 percent in the 10 years following implementation (1991–2000) [50]. The increase represents an annual average increase in denials of handgun purchases by approximately 1250 people considered to be at risk—a number too small, perhaps, for a decrease in firearm-related violence among those individuals to produce a detectable change in state-level, population-based outcome measures. A similar argument has been advanced as partial explanation for the lack of observed effects on homicide of the Brady Act [51,52]. California's MVP policy has been shown to have a substantial beneficial effect on those directly affected, however [25], and a multistate population-level analysis has associated MVP policies with a decrease in intimate partner homicide [15].

Limitations

We carefully identified states that were "at risk" of implementing CBC and MVP policies and used additional criteria to select control states in sensitivity analyses (e.g., excluding states that had banned firearm purchases by people convicted of domestic violence before 1996, when this policy was adopted nationwide). Although we are fairly confident that no other major firearm policies were implemented during the study period in our study states, we cannot be certain about other policies (e.g., criminal, public health, or social policies) or idiosyncratic changes at the local level that may have affected firearm violence, including firearm homicide, the frequency of which was particularly unstable during our study period. Finally, in 1998, the National Instant Criminal Background Check System was launched; the interim provisions of the Brady Law, including a 5-day waiting period, were removed; and the federal background check requirement for handgun sales by licensed retailers was extended to rifles and shotguns. These changes may have had mixed and varying effects on our control states in the final two years of our study period; California had a waiting period throughout the time of our study.

Conclusions

Our findings suggest that the simultaneous implementation of CBC and MVP policies did not result in population-level changes in the rates of firearm-related homicides and suicides in California. A combination of inadequate criminal and mental health records, incomplete compliance and enforcement, the absence of a permit requirement, and the small size of the population directly affected by the laws may account for these findings.

Acknowledgments

Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.annepidem.2018.10.001.

References

[1] Centers for Disease Control and Prevention. Web-based Injury Statistics Query and Reporting System (WISQARS). Atlanta, GA: National Center for Injury Prevention and Control; 2015. https://www.cdc.gov/injury/wisqars/fatal.html. [Accessed 6 April 2018].

[2] Branas CC, Richmond TS, Culhane DP, Ten Have TR, Wiebe DJ. Investigating the link between gun possession and gun assault. Am J Public Health 2009;99(11): 2034–40.

[3] Kellermann AL, Rivara FP, Rushforth NB, Banton JG, Reay DT, Francisco JT, et al. Gun ownership as a risk factor for homicide in the home. N Engl J Med 1993;329(15):1084–91.

[4] Webster DW, Starnes M. Reexamining the association between child access prevention gun laws and unintentional shooting deaths of children. Pediatrics 2000;106(6):1466–9.

[5] Kellermann AL, Rivara FP, Somes G, Reay DT, Francisco J, Banton JG, et al. Suicide in the home in relation to gun ownership. N Engl J Med 1992;327(7):467–72.

[6] Wintemute GJ, Parham CA, Beaumont JJ, Wright M, Drake C. Mortality among recent purchasers of handguns. N Engl J Med 1999;341(21):1583–9.

[7] 18 U.S. Code § 922(g) - Unlawful acts.

[8] Karberg JC, Frandsen RJ, Durso JM, Buskirk TD, Lee AD. Background Checks for Firearm Transfers, 2015. Statistical Tables Bureau of Justice Statistics. Washington D.C: U.S. Department of Justice; 2017. Contract No.: NCJ, 250978.

[9] Miller M, Hepburn L, Azrael D. Firearm Acquisition Without Background Checks: Results of a National Survey. Ann Intern Med 2017;166(4):233–9.

[10] Vittes KA, Vernick JS, Webster DW. Legal status and source of offenders' firearms in states with the least stringent criteria for gun ownership. Inj Prev 2013;19(1):26–31.

[11] Campbell JC, Webster D, Koziol-McLain J, Block C, Campbell D, Curry MA, et al. Risk factors for femicide in abusive relationships: results from a multisite case control study. Am J Public Health 2003;93(7):1089–97.

[12] Wintemute GJ, Drake CM, Beaumont JJ, Wright MA, Parham CA. Prior misdemeanor convictions as a risk factor for later violent and firearm-related criminal activity among authorized purchasers of handguns. JAMA 1998;280(24):2083–7.

[13] Rudolph KE, Stuart EA, Vernick JS, Webster DW. Association Between Connecticut's Permit-to-Purchase Handgun Law and Homicides. Am J Public Health 2015;105(8):e49–54.

[14] Webster D, Crifasi CK, Vernick JS. Effects of the repeal of Missouri's handgun purchaser licensing law on homicides. J Urban Health 2014;91(2):293–302.

[15] Zeoli AM, McCourt A, Buggs S, Frattaroli S, Lilley D, Webster DW. Analysis of the Strength of Legal Firearms Restrictions for Perpetrators of Domestic Violence and Their Association With Intimate Partner Homicide. Am J Epidemiol 2018;187:1449–55.

[16] Center for Gun Policy and Research. Permit-to-purchase licensing for handguns: Johns Hopkins University. https://www.jhsph.edu/research/centers-and-institutes/johns-hopkins-center-for-gun-policy-and-research/publications/PTP-policy-brief.pdf. [Accessed 6 April 2018].

[17] Crifasi CK, Meyers JS, Vernick JS, Webster DW. Effects of changes in permit-to-purchase handgun laws in Connecticut and Missouri on suicide rates. Prev Med 2015;79:43–9.

[18] Santaella-Tenorio J, Cerda M, Villaveces A, Galea S. What Do We Know About the Association Between Firearm Legislation and Firearm-Related Injuries? Epidemiol Rev 2016;38(1):140–57.

[19] Crandall M, Eastman A, Violano P, Greene W, Allen S, Block E, et al. Prevention of firearm-related injuries with restrictive licensing and concealed carry laws: An Eastern Association for the Surgery of Trauma systematic review. J Trauma Acute Care Surg 2016;81(5):952–60.

[20] Kagawa RMC, Castillo-Carniglia A, Vernick JS, Webster DW, Shev A, Crifasi CK, et al. Repeal of Comprehensive Background Check Policies and Firearm Homicide and Suicide. Epidemiology 2018;29(4):494–502.

[21] Castillo-Carniglia A, Kagawa RMC, Webster DW, Vernick JS, Cerda M, Wintemute GJ. Comprehensive background check policy and firearm background checks in three US states. Inj Prev 2017. https://doi.org/10.1136/injuryprev-2017-042475 [Epub ahead of print].

[22] Sen B, Panjamapirom A. State background checks for gun purchase and firearm deaths: an exploratory study. Prev Med 2012;55(4):346–50.

[23] Sumner SA, Layde PM, Guse CE. Firearm death rates and association with level of firearm purchase background check. Am J Prev Med 2008;35(1):1–6.

[24] Dewan S, Oppel RA. For the Military: a Long History of Failure to Report Crimes. New York, NY: New York Times; 2017.

[25] Wintemute GJ, Wright MA, Drake CM, Beaumont JJ. Subsequent criminal activity among violent misdemeanants who seek to purchase handguns: risk factors and effectiveness of denying handgun purchase. JAMA 2001;285(8): 1019–26.

[26] U.S. Census Bureau. American Community Survey, 2010 American Community Survey 5-Year Estimates. Suitland, MD: U.S. Census Bureau; 2016.

[27] Bureau of Labor Statistics. Local Area Unemployment Statistics. http://www.bls.gov/lau/home.htm. [Accessed 6 April 2018].

[28] Surveillance report #98: apparent per capita alcohol consumption — national, state, and regional trends, 1977–2012. http://pubs.niaaa.nih.gov/publications/surveillance98/pcyr19702012.txt. [Accessed 10 September 2017].

[29] Azrael D, Cook PJ, Miller M. State and local prevalence of firearms ownership measurement, structure, and trends. J Quant Criminol 2004;20(1):43–62.

[30] Cook PJ, Ludwig J. The social costs of gun ownership. NBER Working Paper. Cambridge, MA: National Bureau of Economic Research; 2004.

[31] Kaul A, Klößner S, Pfeifer G, Schieler M. Synthetic Control Methods: Never Use All Pre-Intervention Outcomes Together With Covariate. 2017. MPRA Paper No. 83790. https://mpra.ub.uni-muenchen.de/83790/. [Accessed 15 January 2018].

[32] Abadie A, Diamond A, Hainmueller J. Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program. J Am Stat Assoc 2010;105(490):493–505.

[33] Fryer RG, Heaton PS, Levitt SD, Murphy KM. Measuring Crack Cocaine and Its Impact. Econ Inq 2013;51(3):1651–81.

[34] Federal Bureau of Investigation. Uniform Crime Reports. http://www.fbi.gov/about-us/cjis/ucr/leoka. [Accessed 6 April 2018].

[35] Maclean JC, Saloner B. Substance Use Treatment Provider Behavior and Healthcare Reform: Evidence from Massachusetts. Health Econ 2018;27(1): 76–101.

[36] Brazil N. Large-Scale Urban Riots and Residential Segregation: A Case Study of the 1960s. U.S Riots Demogr 2016;53(2):567–95.

[37] Bureau of Justice Statistics. Survey of Criminal History Information Systems. Washington, DC: United States Department of Justice; 1991.

[38] Bureau of Justice Statistics. Survey of Criminal History Information Systems. Washington, DC: United States Department of Justice; 1992.

[39] Bureau of Justice Statistics. Survey of Criminal History Information Systems. Washington, DC: United States Department of Justice; 1993.

[40] Bureau of Justice Statistics. Survey of Criminal History Information Systems. Washington, DC: United States Department of Justice; 1995.

[41] Bureau of Justice Statistics. Survey of Criminal History Information Systems. Washington, DC: United States Department of Justice; 1997.

[42] Bureau of Justice Statistics. Survey of Criminal History Information Systems. Washington, DC: United States Department of Justice; 1999.

[43] Ludwig J, Cook PJ. Homicide and suicide rates associated with implementation of the Brady Handgun Violence Prevention Act. JAMA 2000;284(5):585–91.

[44] Swanson JW, Easter MM, Robertson AG, Swartz MS, Alanis-Hirsch K, Moseley D, et al. Gun Violence, Mental Illness, And Laws That Prohibit Gun Possession: Evidence From Two Florida Counties. Health Aff (Millwood) 2016;35(6):1067–75.

[45] Swanson JW, McGinty EE, Fazel S, Mays VM. Mental illness and reduction of gun violence and suicide: bringing epidemiologic research to policy. Ann Epidemiol 2015;25(5):366–76.

[46] Swanson JW, Robertson AG, Frisman LK, Norko MA, Lin HJ, Swartz MS, et al. Preventing gun violence involving people with serious mental illness. In: Webster DW, Vernick JS, editors. Reducing gun violence in America: informing policy with evidence and analysis. Baltimore: Johns Hopkins University Press; 2013. p. 33–51.

[47] SEARCH. Improving the national instant background screening system for firearm purchases: The National Consortium for Justice Information and Statistics. www.search.org. [Accessed 6 April 2018].

[48] Printz v. United States (95-1478), 521 U.S. 898 (1997).

[49] Crifasi CK, Francis MM, Webster DW, Wintemute GJ, Vernick JS. Changes in the legal environment and enforcement of firearm transfer laws in Pennsylvania and Maryland. Inj Prev 2018. https://doi.org/10.1136/injuryprev-2017-042582 [Epub ahead of print].

[50] State of California Department of Justice. Dealer record of sale transactions. https://oag.ca.gov/sites/all/files/agweb/pdfs/firearms/forms/dros_chart.pdf. [Accessed 6 April 2018].

[51] Cook PJ, Ludwig J. The limited impact of the Brady Act: evaluation and implications. In: Webster DW, Vernick JS, editors. Reducing Gun Violence in America: Informing Policy with Evidence and Analysis. Baltimore: The Johns Hopkins University Press; 2013.

[52] Wintemute GJ. Impact of the Brady Act on homicide and suicide rates. JAMA 2000;284(21):2719–20. author reply 20-1.