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By

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Abstract

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Professor Jennifer Ahern, Chair

Firearm violence is responsible for a substantial amount of morbidity and mortality in the United States, particularly among young Black men (via assault) and older white men (via self-harm). Successful prevention efforts rely on the identification of modifiable risk factors, which can inform new interventions, as well as the evaluation of existent interventions. Individual-level and ecologic studies have found firearm ownership to be a strong risk factor for firearm mortality, but ownership itself can be difficult to intervene on. However, firearm access may be subject to community-level interventions through zoning regulations on firearm dealers and to individual-level intervention via targeted prohibitions among those at high risk of harming others or themselves. Despite general acceptance of the social-ecological model in violence and injury epidemiology, the cross-level risks associated with firearm availability have rarely been explored.

The objective of this dissertation was to identify modifiable risk factors and to evaluate a firearm violence intervention, focusing particularly on cross-level impacts of firearm availability (i.e., how community firearm availability impacts individual risk and vice versa). To do this, we first examined whether community firearm and alcohol availability were independently or jointly associated with individual-level risk of (fatal and nonfatal) firearm assault or self-harm. We conducted two population-based case-control studies of California residents, 2005-2015, drawing on statewide hospitalization, emergency department, firearm transfer, and alcohol license data. The first of these studies focused on firearm assault and homicide and the second focused on firearm self-harm and suicide. We use case-control-weighted g-computation to assess marginal risk differences under various scenarios of interest and to assess additive interactions between measures of firearm and alcohol availability.

Second, we explored whether removal of firearms from high-risk individuals via gun violence restraining orders (GVROs) was associated with the rate of firearm violence in San Diego County, which issued a plurality of orders in the state. California's GVRO law, enacted in 2016, permits the temporary removal of firearms from people at high risk of committing firearm violence. Using statewide hospitalization and emergency department data, we conducted a quasi-experimental study using the synthetic control method to determine whether GVRO

implementation was associated with decreased rates of (fatal and nonfatal) overall firearm violence, firearm assault, or firearm self-harm in San Diego County during the first four years of implementation.

Overall, we found weak cross-level associations between firearm availability and firearm violence. Non-pawn firearm dealer and off-premise alcohol outlet density were associated with modest increases in the individual-level risk of firearm assault, but pawn dealer and off-premise alcohol outlet density were null. Firearm sales density (but not firearm dealer density) was associated with elevated risk of firearm self-harm, but alcohol availability was null or of negligible magnitude. Finally, we found that GVRO implementation was not associated with a reduction in firearm violence of any kind in San Diego County from 2016-2019.

This project will be of interest to policymakers and researchers interested in understanding and preventing firearm violence. Despite our modest findings, this study could inform local interventions relating to community firearm availability. It also provides the first quantitative evaluation of the GVRO law in California, which served as a template for similar laws in 16 other states. Risk factors for firearm violence come from multiple social-ecological levels, including the surrounding social, physical, and policy environments. Successful strategies for prevention will therefore include individual-level interventions, such as GVROs (which may be effective at the individual-level), as well as broad measures that reduce environmental risk. This work adds to the limited literature on cross-level risk associated with firearm availability, but additional research into modifiable risk factors and interventions to prevent firearm violence is needed.

This work is dedicated to my parents, Charles and Nancy Pear.

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The analyses, interpretation, and conclusions presented in this dissertation are attributable solely to the authors. The views expressed herein do not necessarily reflect the views of the National Collaborative on Gun Violence Research, the Heising-Simons Foundation, the California Department of Public Health, or the California Department of Justice.

1. Introduction

Firearm violence contributes substantially to morbidity and mortality in the United States (US), resulting in around 40,000 deaths annually.[1] In recent years, both firearm homicide and suicide have been increasing. The age-adjusted rate of firearm homicide increased more than 30% from 2014 to 2017, where it plateaued around 4.6 deaths per 100,000 people.[1] Since the mid-2000s, firearm suicide has been steadily increasing.[2,3] Between 2006 and 2018, the age-adjusted rate increased 25%, to over 7 deaths per 100,000 people.[1] Preliminary data suggests that rates of homicide further increased in 2020—nearly 25% nationwide—and has remained high in 2021.[4,5] During the same period, it appears as if the rate of suicide declined about 5% overall,[6] but increased substantially among people of color.[7,8]

These average rates belie the large disparities in firearm violence by age, race, and sex, that result in specific populations bearing a disproportionate burden of firearm-related harms. For example, in California, the highest rate of firearm homicide is among Black men in their 20s, who are killed at an annual rate of nearly 90 per 100,000 residents.[3] In contrast, older white men have the highest rate of firearm suicide, peaking at nearly 50 per 100,000 residents among those who are 80 years of age and older.[3] These patterns are similar to those observed nationwide.[2]

In addition to loss of life, the ramifications of firearm violence can include a range of long-term physical and psychological disabilities, such as paralysis, disfigurement, mental health disorders, and substance abuse.[9,10] Along with the personal and financial costs incurred by victims and their families,[11] firearm violence has extensive societal costs. Taking into account the direct costs of fatal and nonfatal firearm injuries (medical and mental health treatment, emergency transportation, police and criminal justice services, and insurance claims processing) as well as the costs associated with loss of work (including the employers' costs) and decreased quality of life, it is estimated that the annual cost of firearm injuries in the US exceeds $174 billion.[12]

Risk factors

Given the scale and severity of this problem, its wide-ranging and diffuse consequences, and its contribution to health disparities, it is a public health imperative to identify risk factors for firearm violence, which can, in turn, inform interventions and policy. While firearm homicide and suicide are distinct in many ways, findings from prior research indicate that there are causes common to both. Among the most important of these are access to firearms and alcohol.

<u>Firearm access</u>

Firearm access is, of course, necessary for the occurrence of all firearm violence. The challenge with studying firearm access is that there are no registries of firearms or firearm owners in the US. As a result, previous studies have relied primarily on interviews, survey data, or proxies, such as the proportion of suicides committed with a firearm,[13] to measure firearm accessibility. Despite the measurement limitations inherent to this line of inquiry, access to firearms has been associated with death from homicide and suicide in a range of studies.[14-20]

These previous studies primarily measured firearm accessibility either individually or ecologically, often using large geographic units such as states, leaving the multilevel role of local firearm accessibility mostly unexplored. However, local firearm access is likely to play an important role in individual risk for firearm violence for at least two reasons: 1) individual ownership may be mediated through local access to firearm dealers, and 2) a high density of firearm dealers could signal local social norms relating to firearm ownership and use in the area. In addition, firearm dealers can be subject to regulations through zoning laws, making this a potentially modifiable exposure (in contrast to other measures of accessibility, such as prevalence of firearm ownership).

Nevertheless, only a few studies have evaluated firearm dealer density in relation to firearm violence. A recent study evaluating county-level Federal Firearms Licenses (FFLs) density and intimate partner homicide (IPH) in 16 states found that FFLs were associated with increased rates of firearm IPH in non-rural areas.[21] This is consistent with a prior study by Weibe and colleagues that examined FFL density and homicide, finding that county-level FFL density in metropolitan cores was associated with increased firearm homicide rates.[22] A nationwide state-level study found no association between FFL density and firearm homicide, but it did find firearm-dealing pawn shop density to be associated with increased firearm mortality overall and with firearm suicide rates; however, this "correlation analysis" did not attempt to control for confounding (from, e.g., urbanicity or demographic differences between states).[23] The existent literature suggests that firearm dealer density may play a role in firearm mortality, but the small number of studies and their individual limitations (e.g., ecologic design, uncontrolled confounding) preclude drawing firm conclusions.

The dearth of research on firearm dealers and firearm violence may be due, in part, to concerns about measurement error. As is illustrated above, researchers have used publicly available FFL data to approximate the density of firearm retailers. All dealers that sell firearms are required to have an FFL. Historically, the number of FFLs was badly inflated by individuals who became "dealers" in order to purchase firearms for themselves at wholesale prices, without ever intending to sell. The law was changed in 1993 and 1994 to reduce the number of FFLs that were not actual dealers, resulting in a 70% drop in the number of FFLs by 1998.[24] The impact of this misclassification was suggested in the aforementioned Wiebe study, which examined the association between FFL density and firearm homicide before and after the law changed. The estimated risk ratio increased from 1.69 in 1993 to 12.72 in 1999,[22] suggesting misclassification was masking a large portion of the association. FFL data, while more useful than in the early 1990s, still provide only an approximation of active firearm dealers, as individual license holders may sell firearms very rarely and year-long licenses may be misclassified for many months after a store closes.

This dissertation seeks to improve upon these previous studies by using a more accurate measure of firearm dealer density, drawing on California's Dealer Record of Sale (DROS) data, which contain all legal firearm transactions for the state (with the exception of certain intrafamilial transfers). Handgun transactions have been recorded since before the system was digitized in the mid-1970s, and long gun (rifle and shotgun) transaction records have been mandatory since 2014. We use these data to measure *active* dealers—i.e., pawn and non-pawn firearm dealers that are selling firearms to community members in a given month—in each county throughout the state, providing a less biased measure of association between firearm dealer density and firearm violence. We also use DROS data to quantify firearm sales density as an alternative measure of firearm availability, which allows us to determine which measure is more salient for violence prevention.

Alcohol availability

Another major risk factor for both firearm homicide and suicide is alcohol availability and use. There is a great deal of research on the association between alcohol outlet density and violence in general, but very few studies have examined firearm violence in particular. However, alcohol outlet density could affect firearm violence directly by increasing the availability of alcohol and indirectly by relaxing social norms about drinking.

Ecological studies have found alcohol outlet density to be associated with suicides[25,26] and assault,[27,28] but only a few studies have examined the relationship between alcohol outlet density and firearm violence, despite the fact that most homicides and suicides are committed with a firearm. A study of residents in Philadelphia found off-premise outlet density (e.g., liquor stores) to be associated with an increase in firearm assault, particularly among heavy drinkers.[29] However, a study using New York county data did not find a significant relationship with firearm crime.[30] Only one study, examining a single US city, has evaluated the association between outlet density and firearm suicide. Findings suggested off-premise outlets may be associated with elevated risk of firearm suicide, but these results were not statistically significant.[31]

Prior work has not explored the joint effects of firearm and alcohol availability on firearm violence, but risks are likely highest in areas with easy access to both alcohol and firearms. In areas where firearms are more accessible, individuals drinking together may be more likely to encourage each other to use a firearm during the commission of a crime or to share firearms among the group for this purpose. In addition, suicidal individuals may be more likely to purchase or have access to a firearm for self-harm, and to have a firearm at hand when drinking and experiencing suicidal ideation.

Chapters 2 and 3 of this dissertation extend our limited knowledge about firearm and alcohol availability with regard to firearm violence: these are the first statewide, multilevel analyses examining the joint effects of alcohol outlet density and firearm dealer density on firearm violence. These case-control studies use DROS and alcohol license data along with comprehensive death and hospitalization data to identify residents with a fatal or nonfatal firearm assault injury (Chapter 2) or firearm self-harm injury (Chapter 3). We use case-control-weighted g-computation to assess risk differences under various scenarios of interest and to assess additive interactions. Findings from these analyses can inform local policy interventions on alcohol outlets and firearm dealers via zoning regulations to reduce firearm violence; given the challenge of intervening directly on firearm access, such alternative approaches warrant further exploration.

Targeted interventions

Along with community-level interventions that reduce firearm access, a comprehensive firearm violence prevention strategy must include targeted individual-level interventions. These include firearm prohibitions aimed at limiting access to firearms among those at highest risk of committing violence, and background checks, which allow for enforcement of these prohibitions. These laws reduce access to firearms among those at risk of harming others or themselves, but no set of prohibitions will adequately capture all individuals who are at increased risk, particularly if this risk is temporary.

Extreme risk laws were created to fill this gap, allowing for temporary firearm removal from people who pose a significant danger to themselves or others but who are not otherwise prohibited from firearm ownership. In 2016, California became the first state in the nation to enact a gun violence restraining order (GVRO) law. As of July 2021, 19 states and the District of

Columbia have enacted some form of extreme risk law—16 of which are based on California's model—many in response to the school shooting in Parkland, Florida in 2018.

Despite the recent spate of legislation, there is very little evidence on the effectiveness of extreme risk laws at preventing firearm violence. To date, only four such studies have been published. Two of these were conducted by Swanson and colleagues, one focusing on the law in Connecticut and the other on the law in Indiana. Both found that one firearm suicide was prevented for every 10-20 cases in which guns were seized.[32,33] Additionally, a population-level reduction in firearm suicide was found in a synthetic control study of the laws in these same states.[34] The early evidence for extreme risk laws reducing firearm suicide is promising but limited. Apart from a single study that includes a year of post-implementation data from California and Washington, all of the existing studies have evaluated these laws in only two states which, unlike other states with similar legislation, have extreme risk laws based on warrants rather than restraining orders. Unlike GVROs, risk warrants are only accessible to law enforcement officers, and they cannot prevent a person who does not already own firearms from purchasing them. Additional evidence is needed to judge the effectiveness of GVROs at reducing firearm violence, particularly as more states continue to adopt such legislation.

Chapter 4 directly addresses this need. It is the first study to evaluate the association between implementation of California's GVRO law with firearm violence and the first evaluation of extreme risk laws to include firearm homicide and assault as an outcome. We use the synthetic control method to compare rates of fatal and nonfatal firearm violence in San Diego County after the law was implemented with expected rates in San Diego in the absence of implementation. San Diego County was the focus of this analysis as it issued a plurality of GVROs in California over the study period, 2016-2019.[35] This exploratory study can serve as a pilot for a statewide analysis, to follow when the number of orders is sufficiently high. Findings will be of interest to policymakers and violence prevention researchers nationwide.

This dissertation closes by synthesizing findings from chapters 2-4 and offering directions for future research. Firearm violence is a multifaceted, deeply complicated public health problem. Culturally, it is rooted in our history of the right to bear arms, the legacy of slavery, and a patriarchal ideal of masculinity. It is also intertwined with vulnerabilities at the individual level (e.g., poor mental health[36]), interpersonally (e.g., suicide in a social network[37]), and at the community level (e.g., over- and under-policing[38]). Because of its complexity, firearm violence demands multipronged solutions, simultaneously targeting cultural, political, community/environmental, and individual risks. This work is intended to contribute to the search for such solutions by exploring potentially modifiable community-level risk factors and evaluating the impact of a new and promising firearm violence prevention policy.

2. Community-Level Risk Factors for Firearm Assault and Homicide: The Role of Local Firearm and Alcohol Availability

Introduction

In the United States (US), firearms cause 3 of every 4 homicides.[1] Firearm homicide is the leading cause of death for Black boys and men between the ages of 15 and 34 and the fifth leading cause of injury-related death in the population overall. In 2019, firearm assault resulted in over 14,000 preventable deaths and many more nonfatal injuries,[1] leaving those directly and indirectly affected with lasting socioemotional and economic consequences.[12,39]

Access to firearms is a necessary cause of firearm injury and death. Firearm dealer density is one aspect of firearm access that can be modified by municipalities or counties through zoning regulations. A small number of studies have evaluated community firearm dealer density in relation to firearm homicide using Federal Firearm Licenses (FFLs).[21-23,40-43] However, these data are imprecise, as individuals with FFLs may sell firearms infrequently and businesses may close many months prior to the license expiring. The existing evidence suggests firearm dealer density may play a role in firearm mortality, but given the small number of studies and their limitations with regard to data and generalizability beyond the study population, it is not yet possible to draw conclusions.

Alcohol is another major risk factor for firearm violence. An estimated 37% of firearm homicide victims and 34% of firearm homicide perpetrators were under the influence of alcohol at the time of the offense.[44,45] Alcohol use impairs executive functioning and increases impulsive, violent behavior,[46] which increases the chance of assaults or other violent interactions. Like firearms, alcohol must be purchased through a licensed retailer, and access can be modified through rules and regulations on retailer density. There is a robust body of literature showing that alcohol availability is associated with increased interpersonal violence.[47-49] However, few studies have examined the relationship between alcohol outlet density and firearm assault in particular, and, among those that have, findings are mixed.[29,30,42,50] These are primarily limited to smaller, single-city studies.

The joint association of alcohol and firearm availability with firearm assault has not previously been examined, though a synergistic interaction seems likely given the mechanisms through which each exposure is expected to impact violent behavior, with firearms plausibly magnifying the risk of violence stemming from alcohol and vice versa. Aims of this study were to evaluate the associations in California between 1) community-level firearm availability and individual-level risk of firearm assault victimization, 2) community-level alcohol availability and individual-level risk of firearm assault victimization, and 3) both community-level firearm and alcohol availability and individual-level risk of firearm assault victimization, and to quantify the interaction between these two exposures. Because rates of fatal and nonfatal injuries from firearm assault are substantially higher in young Black men than in other groups, there is a particular need to identify interventions that may reduce risk in this population. Therefore, our final aim was to additionally explore the above associations in this high-risk subgroup.

Methods

Study design

We conducted a density-sampled population-based case-control study of California residents from January 2005 through September 2015. Cases were all Californians injured or killed by a firearm assault resulting in an in-state hospital visit or death. We selected controls from the general population of California—the primary study base from which cases arose[51,52]—in a 4:1 ratio with cases. As is detailed below, we characterized firearm availability at the county level and alcohol availability at the ZIP code tabulation area (ZCTA) level. A case-control-weighted analysis approach was used to facilitate estimation of a broad range of measures of association, including risk differences and additive interactions. Analyses were adjusted for individual demographics and a range of community socioeconomic characteristics hypothesized to confound the associations of interest.

Data and Measures

Exposures: The exposures were community-level firearm and alcohol availability measures. We used Dealer Record of Sale (DROS) data to measure firearm availability monthly from January 2004 through September 2015 (the most recent data available). Unlike measures based on counts of FFLs, which previous studies relied on, the DROS sales data can provide a true measure of active firearm dealers. They have not previously been used for this purpose. DROS data, maintained by the California Department of Justice (CA DOJ), contain statewide records for every legal handgun transfer since before the 1970s and every long gun transfer since 2014. California has had a universal background check law since 1991 (mandating all handgun purchasers pass a background check prior to completing the transaction), so DROS should contain a complete record of handgun sales as of that time. Although handguns are more commonly used in firearm homicides and assaults,[53] we used sales records for both handguns and long guns, when available. To determine the most appropriate spatial unit of analysis, we geocoded paired DROS dealer and purchaser address data to explore how far people traveled to purchase their firearms. We found that 70% of purchasers and dealers were in the same county, making it the best performing administrative boundary we explored (others were Public Use Microdata Areas, cities, and ZIP codes).

To examine whether firearm violence is more sensitive to measures of firearm dealers or firearm sales, we measured firearm availability in 2 ways: with active firearm dealer density and with firearm sales density. Both measures were calculated monthly for each county, per 100,000 residents. We then calculated county-specific 12-month moving averages, which served as the exposure of interest, to improve stability. Firearm dealers with at least 1 sale in a month were considered "active" in that month. Pawn and non-pawn dealer densities were measured separately because pawn shops have been linked to crime guns previously and may, therefore, have a different association with firearm assault victimization than non-pawn dealers.[54-56] We classified dealers as pawn shops if they had "pawn" or "loan" in the business name or email address or if they had a DROS record for redeeming pawned firearms. To capture the flow of new firearms into the community, firearm transactions were limited to sales, which excludes pawn redemptions, private party transfers, curio/relics, loans, and non-roster peace officer transfers.

Alcohol outlet density was measured at the ZCTA level, the smallest geographic area available in the data, 2004-2015. Studies of alcohol outlet density tend to employ small geographic units in analysis, including ZIP codes.[27,57,58] We estimated the 12-month ZCTA density of off-premise outlets and bars/pubs separately, as they have been shown to have differing associations with violence.[59] Consistent with the approach to firearm dealer density, alcohol outlet density was measured per 100,000 population. Annual alcohol outlet data were from the California Department of Alcoholic Beverage Control (ABC). We limited the outlets to off-premise (license types 20 and 21) and bars/pubs (license types 23, 40, 42, 48, 61, and 75) and linked the geocoded ABC data to the 2010 ZCTA map to get the number of outlets in each ZCTA-year. To better approximate mid-year exposures, we calculated a moving weighted average of the current and prior year's density (e.g., the density for February 2005 was calculated as $\frac{2}{12}$(2005 density) + $\frac{10}{12}$(2004 density)).

Cases: Cases were individuals who experienced a fatal or nonfatal firearm assault injury in California (hereafter "firearm assault"), from January 2005 through September 2015, as captured in mortality and hospital discharge data. Mortality data were from the California Department of Public Health's Comprehensive Death Files. Injury data were from emergency department and hospital discharge records for the state, provided by the Office of Statewide Health Planning and Development (OSHPD). Only nonfatal cases were included from the hospital data to avoid double counting individuals with fatal injuries that were treated at the hospital.

Both data sources use the International Statistical Classification of Diseases and Related Health Problems (ICD) to identify injuries. The mortality data use the 10th edition (ICD-10) and the hospital data use the 9th edition (ICD-9). Firearm assault was identified with ICD-10 codes X93-95 and with ICD-9 codes E965.0-E965.4. Details of these codes are presented in Appendix A Table 1.

Cases were limited to California residents. For individuals with missing data and a record linkage number in the OSPHD data, which identifies unique individuals, we used other records for the same individual to fill in missing sex, race, and ZIP code (linked to ZCTA).

Controls: To sample controls from the general population of California, we used American Community Survey[60] data to estimate the annual ZCTA population by age, sex, and race using a crosswalk with county-level population data from the Census Bureau (for ZCTAs that crossed county boundaries, we generated estimates for each unique ZCTA-county unit and later summed these to create single ZCTA estimates). We then used these annual data to estimate monthly values by linearly interpolating changes in subgroup population counts. We refined the at-risk population estimates by removing cases from the population corresponding to the case's ZCTA, age, sex, and race. Finally, we calculated the probability of being in each ZCTA-county age-sex-race group at each year-month. For each study month, controls were randomly sampled from this multinomial distribution at a ratio of 4 controls for every incident case.

Covariates: Covariates were determined *a priori* based on theory and previous literature. We used a directed acyclic graph to visualize the relationships between variables and to determine the minimum set of confounders to control for (presented in Appendix A Figure 1). The following confounders were included in analyses: individual age, race/ethnicity, sex; ZCTA percent aged 15-24, percent Hispanic, percent non-Hispanic Black, percent male, urbanicity, median household income z-score (hereafter "income"), percent aged 25 years and older with at least a Bachelor's degree (hereafter "education), unemployment rate in the civilian work force over aged 16 (hereafter "unemployment"), percent of vacant housing units; and county non-firearm violent crime rate and property crime rate. We additionally controlled for year to account for secular trends in firearm violence and included an indicator for when DROS started recording long gun transactions in 2014.

The individual demographic variables are recorded in the mortality and hospital data. Race/ethnicity included the following categories: non-Hispanic white, non-Hispanic Black, Hispanic, Asian, Native American, and multiracial. ZCTA-level income, education, vacancy, and unemployment data were from the American Community Survey 5-year estimates for 2007-

2011 and 2012-2016.[61] To characterize urbanicity, we use 2010 census data to calculate the percent of the population in each ZCTA that was in an urban area (>=50,000 people), an urban cluster (2,500-49,999 people), or rural area (<2,500 people); the ZCTA was then categorized as urban, suburban, or rural based on the category with a plurality. ZCTA demographic data (age, sex, race) were calculated monthly using the interpolated population data describe above and were then transformed into 12-month rolling averages. Annual county crime data are publicly available from CA DOJ's Crimes & Clearances.[62] We calculated a rolling weighted average of the current and previous year's rates, as we did with alcohol outlet density.

<u>Statistical analysis</u>

We used case-control-weighted (CCW) G-computation to estimate risk differences for various contrasts of interest. G-computation is a parametric substitution estimator, i.e., the outcomes are estimated under exposure regimes of interest (which are substituted into the model). CCW G-computation corrects for the case-control sampling by reweighting: cases are weighted according to the population prevalence of the outcome and controls are weighted with (1-prevalence)/(control:case ratio).[63,64] The CCW G-computation formula is as follows:

$$\begin{aligned}\Psi (P) = {} & E_w\{[\, I(Y_{i=1}) * q(t)[E(Y \mid A_1=1, A_2=1, W_i)]\,] + [\, I(Y_{i=0}) * \tfrac{\bar{q}(t)}{J(t)}[E(Y \mid A_1=1, A_2=1, W_i)]\,]\} \,/ \\ & E_w[\, I(Y_{i=1})q(t) + I(Y_{i=0}) \tfrac{\bar{q}(t)}{J(t)}\,] \; - \\ & E_w\{[\, I(Y_{i=1}) * q(t)[E(Y \mid A_1=0, A_2=0, W_i)]\,] + [\, I(Y_{i=0}) * \tfrac{\bar{q}(t)}{J(t)}[E(Y \mid A_1=0, A_2=0, W_i)]\,]\} \,/ \\ & E_w[\, I(Y_{i=1})q(t) + I(Y_{i=0}) \tfrac{\bar{q}(t)}{J(t)}\,],\end{aligned}$$

where Ψ is the risk difference at the true data distribution (P); q_0 and $\bar{q}_0$ are the case and control weights, respectively, at each year-month (t); $E(Y \mid A_1=1, A_2=1, W=w)$ is the expected value of the outcome (Y) when both exposures (A_1 and A_2) are set to a given value, such as 1, adjusting for confounders (W); and E_w is the expectation over W.

We estimated 2 target parameters for 2 populations: the entire statewide population and the subgroup of Black boys and men between the ages of 15 and 39. The first parameter is the overall risk difference ($RD_{overall}$) comparing firearm assault in a scenario in which firearm availability (dealers or sales) and alcohol availability are both set to high density to a scenario in which both are set to low density. To avoid positivity violations, "high" and "low" were determined by the observed highest and lowest values in each county or ZCTA over the study period (Appendix A Table 2). The second parameter of interest is a targeted intervention ($RD_{targeted}$) comparing the outcome under the observed values of the exposure with a scenario in which only those counties in the top quartile of firearm availability and ZCTAs in the top quartile of alcohol availability are set to low density. For each parameter, we also calculated the corresponding risk ratio (RR) to provide additional context for understanding the differences of interest.

Along with these joint interventions, we examined each exposure independently (non-pawn dealer, pawn dealer, off-premise outlet, bar/pub outlet, and firearm sales density) and calculated additive interactions by subtracting the sum of the individual $RDs_{overall}$ ("expected") from the joint $RD_{overall}$ ("observed"). We used bias corrected and accelerated nonparametric bootstrapped confidence intervals for all estimates (n runs = 400).[65] To determine whether results were sensitive to our choice to include available long gun information, we re-estimated the $RD_{overall}$ for the entire population after removing long guns from the DROS data.

The parametric model underlying the G-computation estimates was a CCW logistic regression that accounted for nonindependence of observations from individuals being nested within counties, zip codes, and year-months with cluster-robust standard errors (using the Hubert-White sandwich estimator). We included interactions between the firearm availability and alcohol availability variables and retained those with $p<0.20$.[66] To examine nonlinearity, we visualized the bivariate relationships with each continuous variable and the log-odds of the modeled outcome with scatterplots (including smoothed lines of fit). Variables that appeared nonlinear and were significantly associated with the outcome were modeled with restricted cubic splines. To ensure degrees of freedom were being spent wisely, we chose to not include splines on nonlinear variables that were not significant at alpha=0.10 when modeled with or without a spline.

Analyses were performed with R 4.0.2, Stata/MP 13.1, and ArcGIS 10.7. This study was approved by the California Health and Human Services Agency's Committee for the Protection for Human Subjects; University of California, Berkeley's Committee for the Protection for Human Subjects; and the University of California, Davis Institutional Review Board.

Results

From January 2005 through September 2015, there were 69,743 assaultive firearm injuries in California among residents. Records missing ZCTA, age, sex, or race were dropped (n = 1,165; 1.7% of cases). We sampled 274,312 controls, giving us a total sample size of 342,890. We then dropped 245 observations (30 cases and 215 controls) with missing community-level covariate data. Several variables were very right skewed. To minimize the potential bias from extreme outliers, we removed all observations with an exposure or covariate value more than 10-times the interquartile range (IQR) below the 1st quartile or greater than the 3rd quartile of a variable's distribution (n = 692 cases and 5,970 controls). Cases and controls both had extreme values of ZCTA percent unemployed, vacant housing, non-Hispanic Black, young, and male. In the course of visualizing the data, we identified and removed an additional 11 observations with extreme covariate values. This yielded a final sample size of 335,972 (67,850 cases and 268,122 controls). Our final sample of controls closely matched the demographics of the Californian population from which they were drawn (Appendix A Table 3).

Table 2.1 presents the individual- and community-level characteristics of study participants. Firearm assault cases were more likely than controls to be male (89.6% vs. 49.3%), Black (34.7% vs. 5.9%) or Hispanic (46.7% vs. 37.8%), and between the ages of 10 and 29 (68.6% vs. 29.4%). They also lived in areas with lower median household incomes ($45,029 vs. $60,447) and fewer college graduates (14.9% vs. 25.5%) than controls. Alcohol outlet density was similar between assault cases and controls, but firearm dealer and sales density were lower among cases.

Table 2.2 displays the unadjusted associations between monthly risk per 100,000 of firearm assault, weighted to be representative of the state population, by tertile of each exposure. The risk of firearm assault injury was highest in the lowest tertile of each measure of firearm availability. The lowest rate was among those in the middle tertile of pawn dealers. Conversely, the risk of assault injury increased with higher off-premise outlet density and bar/pub density.

Figure 2.1 displays the adjusted RDs for firearm assault per 100,000 per month from the statewide CCW G-computation analyses. The corresponding relative risks (RR) are presented in Appendix A Table 4. Considering each exposure individually, we found that high (vs. low) density of non-pawn firearm dealers ($RD_{overall}$: 0.04, 95% CI: 0.01, 0.08) and off-premise alcohol outlets ($RD_{overall}$: 0.02, 95% CI: 0.01, 0.04) were slight risk factors for firearm assault, but pawn dealers and bar/pub outlets were not. The $RD_{overall}$ for the joint exposures of firearm dealers and alcohol outlets was 0.06 injuries per 100,000 (95% confidence interval [CI]: 0.02, 0.10), indicating a small increased risk of firearm injuries when firearm dealers and alcohol outlets are both high density (this is a statewide difference of about 22 injuries per month, or 263 injuries per year). There is, however, no evidence of additive interaction—the risk for assault is not greater than expected in areas with a high density of both firearm and alcohol availability (Table 2.3). The targeted intervention results were small and the CIs often included the null. Results using firearm sales density rather than firearm dealer density were similar.

Figure 2.2 displays the results from our subgroup analysis of Black boys and men between the ages of 15 and 39. We found a similar overall pattern, but the absolute differences were substantially larger in this group than in the overall population (the relative contrasts were very similar—RRs are in Appendix A Table 5). As above, in the individual exposure models, we found firearm assault to be associated with high density of non-pawn dealers ($RD_{overall}$: 1.05, 95% CI: 0.22, 1.78) and off-premise outlets ($RD_{overall}$: 0.58, 95% CI: 0.27, 0.96). These associations drove the joint $RD_{overall}$ for firearm dealers and alcohol outlets together, which was 1.18 injuries per 100,000 (95% CI: 0.26, 2.05) in this higher risk population (equivalent to about 5 injuries per month and 58 injuries per year). Again, there was no evidence of an additive interaction (Table 2.3). The $RD_{targeted}$ for the joint exposures was much smaller and the 95% CI included the null.

Among young Black men, using firearm sales density rather than firearm dealer density resulted in a smaller $RD_{overall}$ for the joint association ($RD_{overall}$: 0.98, 95% CI: 0.21, 1.81). This was because of weaker independent associations with firearm sales compared with dealers ($RD_{overall}$: 0.64, 95% CI: 0.06, 1.49) and with off-premise outlets ($RD_{overall}$: 0.28, 95% CI: 0.12, 0.42). Firearm sales density remained a risk factor for firearm assault in the targeted model; the observed firearm sales density distribution was associated with 0.58 more monthly firearm assaults per 100,000 than a hypothetical intervention reducing sales only among ZCTAs in the top quartile of the distribution (95% CI: 0.30, 0.76).

Results of our sensitivity analysis removing long guns from the DROS data are presented in Appendix A Table 6. Because pawn redemption of long guns was reported prior to 2014, removing long guns slightly changed the pre-2014 data as well, as we used redemptions to classify active dealers as pawn or non-pawn. Without long guns, non-pawn firearm dealers and firearm sales were less strongly associated with firearm assault, resulting in attenuated joint associations as well. The associations with alcohol outlets and pawn shops were not meaningfully different from the primary results.

Discussion

Using comprehensive statewide data from California over more than a decade, we found that community-level firearm and alcohol availability were associated with increased risk of firearm assault after adjusting for confounding. We estimate that about 263 additional firearm assault injuries would occur under high vs. low firearm dealer and alcohol outlet density conditions, nearly 60 of which would be among young Black men. While the joint associations were larger than the independent associations, we did not find evidence of additive interaction between the exposures. Associations were stronger among Black boys and men between the ages of 15 and 39 than among the overall population, suggesting community-level interventions may be an important part of a comprehensive strategy to reduce firearm assault in those at highest risk. By identifying modifiable community-level risk factors, we hope to provide the foundation for politically feasible, local solutions (e.g., zoning interventions) to policymakers and public health practitioners working to prevent firearm violence.

Our first study aim was to evaluate the association between community-level firearm availability and individual risk of firearm assault injury. In models estimating the $RD_{overall}$ for the statewide population, we found that the density of non-pawn dealers and firearm sales were both associated with increased risk of firearm assault and had similar point estimates. While our estimates are not directly comparable to previous studies, our qualitative conclusions are consistent with previous work finding community firearm availability (mostly measured with FFL density) to be associated with increased rates of firearm homicide and firearm intimate partner homicide.[21,22,41,67,68] City-level spikes in firearm sales have also been found to be associated with increased interpersonal firearm injury in a study drawing on the same data sources that we used here.[69] It should be noted that these prior studies were ecological and were estimating conditional associations, so they were answering different (though related) questions.

There are several mechanisms through which firearm dealers may increase risk of firearm assault. Most firearms used in assaults were acquired through an unlawful transaction; in one study of inmates convicted of firearm crimes, only about 11% purchased their firearm from a dealer.[70] Nevertheless, dealers act as point sources from which firearms flow into a community. The higher the prevalence is of firearm ownership in a community, the easier it is for a firearm to change hands between friends or family members (the primary source of firearms used in assaults).[71,72] Dealers also supply firearms (knowingly or unknowingly) to traffickers and to straw purchasers, who then move the weapons to the illicit market.[73-75] Many firearms stay proximate to the original dealer; about 1/3rd of crime guns traced by the ATF originated from a dealership in the same community in which the crime occurred[76] and 45% were recovered within 25 miles of the original dealer.[77]

Our findings that density of non-pawn dealers was more strongly associated with firearm assault in young Black men than density of firearm sales suggests that in this population, non-pawn dealers are playing a role beyond merely providing firearms to the community. For example, dealers also supply ammunition; this could partially explain their importance above and beyond introducing new firearms. We were somewhat surprised to find a null association with pawn shops, as they have been linked to crime in the past.[74,78] This may be due, in part, to the fact that pawn shops typically do not rely on firearm sales to operate and are therefore not a major source of new firearms in the community (statewide, pawn shops sold about 20% of firearms over the study period).

Our second study aim was to evaluate the association between community-level alcohol availability and individual risk for firearm assault injury. We found modest positive associations between off-premise outlets and firearm assault, but no evidence of an association with bars/pubs. Consistent with our results, previous studies of single cities in the US found off-premise outlets, but not on-premise outlets, to be associated with firearm assault.[29,48] However, gun crime was not associated with alcohol outlet density in a study of counties in New York,[30] and a study of gunshot wounds in Chicago found that the association with alcohol outlet proximity was heterogeneous across the city.[79] In addition to geographic differences with our study, prior investigations have estimated conditional rather than marginal parameters, making it hard to compare results directly.

Routine activities theory suggests that violent crimes occur when a “likely offender” crosses path with a “suitable target” when no bystander is present and able to intervene.[80] This theory helps to makes sense of the null finding with bars/pubs, as these outlets have bartenders and bouncers who can serve as "guardians" to prevent violence at the outlet. On the other hand, off-premise alcohol consumption is less monitored, resulting in more opportunities for violence to occur.

Our third study aim was to evaluate the joint association between community-level firearm and alcohol availability and individual-level risk of firearm assault. We found that the joint associations were larger than the individual associations, but that the magnitude was consistent with adding the individual associations together. Despite there not being an additive interaction, multipronged community-level interventions that target both firearm dealers and off-premise outlets simultaneously are likely to have a larger impact on firearm assault, particularly among those at highest risk, than are interventions targeting either firearm dealers or off-premise outlets alone.

While our results were attenuated when we removed long guns from the data, this is likely an artifact of how we defined "high" and "low" density in our analyses. These values were determined empirically, by taking the highest and lowest value of each exposure over the study period for each county (for firearm variables). In January 2014, when long gun reporting became mandatory in DROS, there was a sudden and sustained increase in the number of active firearm dealers and firearm sales in all counties (Appendix A Figure 2). As a result, the "high" values in the primary analysis were likely being drawn from this period, whereas the "low" values were being drawn from the period prior to 2014. Without long guns, this sharp increase did not occur in 2014, so the difference between "high" and "low" was smaller in the sensitivity analysis.

Limitations

This study adds considerably to the limited literature on the relationship between community firearm and alcohol availability and firearm assault by using comprehensive data—including accurate measures of active firearm dealers, legal firearm sales, and both fatal and nonfatal firearm assaults—and by using rigorous methods to estimate absolute scale measures that are most relevant for quantifying an exposure's population health impact. Nevertheless, our study faced limitations. We cannot be sure that results generalize outside of our study state. Because firearms are so tightly regulated in California, it is possible that associations may be stronger elsewhere.

There may have been a small amount of measurement error in our exposure and outcome data. By using mortality and hospitalization data, we only captured firearm injuries that were severe enough to cause death or warrant going to the hospital. Given the nature of gunshot wounds, however, the great majority of firearm assault injuries appear in hospitalization data.[81] Furthermore, we could directly measure neither the proportion of community members who owned or had access to a firearm nor the flow of illegal firearms into communities; but by focusing on dealers, which are a major source of new firearms in surrounding areas, we were targeting upstream drivers that are modifiable by local interventions and policy.

We were also limited by a degree of uncontrolled confounding, as we did not have individual-level measures of, for example, socioeconomic status. Finally, our findings, like other spatial analyses, are subject to the modifiable areal unit problem; it is possible our results would be different had we used different geographic units of analyses.[82] However, we empirically determined the county to be the best administrative unit to measure firearm availability and ZCTA is a common measure of alcohol outlet density[27,57,58] (and was the smallest unit of analysis available to us).

Conclusions

Firearm violence is a complex problem that is best addressed by multiple interventions targeting different social-ecological levels.[83,84] Community-level interventions can be a particularly important focus for public health strategies, as they shift the burden away from individuals, have the potential to address systemic problems, and can often be enacted locally (thereby avoiding, to an extent, the politicized firearm policy environment). Firearm dealers and alcohol outlets are both modifiable exposures, as they are subject to rules and regulations, such as zoning laws. Our findings suggest that regulations limiting the density of non-pawn firearm dealers and off-premise alcohol outlets may contribute to a multi-level approach to firearm violence prevention, particularly among those at highest risk. Additional research on community-level firearm and alcohol availability in other states is needed to further inform firearm violence prevention efforts across the country.

Table 2.1: Characteristics of Cases and Controls

	Cases[a]	Controls
Total	67,850	268,122
Individual Characteristics		
Sex, N (%)		
Male	60,775 (89.57)	132,310 (49.35)
Race/ethnicity, N (%)		
Non-Hispanic white	8,106 (11.95)	107,336 (40.03)
Non-Hispanic Black	23,575 (34.75)	15,890 (5.93)
Hispanic	31,654 (46.65)	101,251 (37.76)
Asian	2,341 (3.45)	36,192 (13.50)
Native American	170 (0.25)	1,127 (0.42)
Multiracial	2,004 (2.95)	6,326 (2.36)
Age Group, N (%)		
0-9	367 (0.54)	35,965 (13.41)
10-19	17,948 (26.45)	38,729 (14.44)
20-29	28,568 (42.10)	39,978 (14.91)
30-39	11,618 (17.12)	37,648 (14.04)
40-49	5,671 (8.36)	38,104 (14.21)
50-59	2,571 (3.79)	33,683 (12.56)
60-69	779 (1.15)	22,370 (8.34)
70-79	227 (0.33)	12,923 (4.82)
80+	101 (0.15)	8,722 (3.25)
ZCTA-Level Characteristics		
Alcohol Outlet Density per 100,000 residents, Median (25th, 75th pctl)		
Off-premise	69.23 (55.51, 89.58)	65.84 (50.05, 85.63)
Bar/pub	10.67 (6.03, 18.17)	10.65 (5.50, 17.79)
Urbanicity, N (%)		
Urban	64,820 (95.53)	248,717 (92.76)

Suburban	2,437 (3.59)	13,615 (5.08)
Rural	593 (0.87)	5,790 (2.16)
Demographics, Median (25th, 75th pctl)		
% Male	49.33 (48.53, 50.23)	49.35 (48.57, 50.14)
% Non-Hispanic Black	9.08 (2.94, 19.92)	3.12 (1.43, 6.92)
% Hispanic	53.47 (32.30, 69.92)	32.01 (17.23, 54.65)
% Age 15-24	16.17 (14.42, 17.58)	14.63 (12.42, 16.53)
Median household income	45,029 (37,072, 56,835)	60,447 (47,049, 80,310)
% Bachelor's degree+	14.94 (8.02, 23.91)	25.54 (15.07, 39.95)
% Unemployed	11.82 (9.47, 14.41)	9.37 (7.38, 11.94)
% Vacant housing units	6.94 (5.29, 8.89)	6.13 (4.50, 8.19)
County-Level Characteristics		
Firearm Dealer or Sales Density per 100,000 residents, Median (25th, 75th pctl)		
Non-pawn dealer	0.45 (0.39, 0.97)	0.71 (0.41, 1.06)
Pawn dealer	0.57 (0.42, 1.03)	0.77 (0.47, 1.12)
Firearm sales	25.77 (20.60, 47.11)	31.43 (21.72, 50.60)
Crime Rate per 1,000 residents, Median (25th, 75th pctl)		
Property crime	28.92 (25.04, 36.43)	27.45 (23.13, 33.21)
Non-firearm violent crime	3.82 (3.19, 4.17)	3.40 (2.62, 4.14)

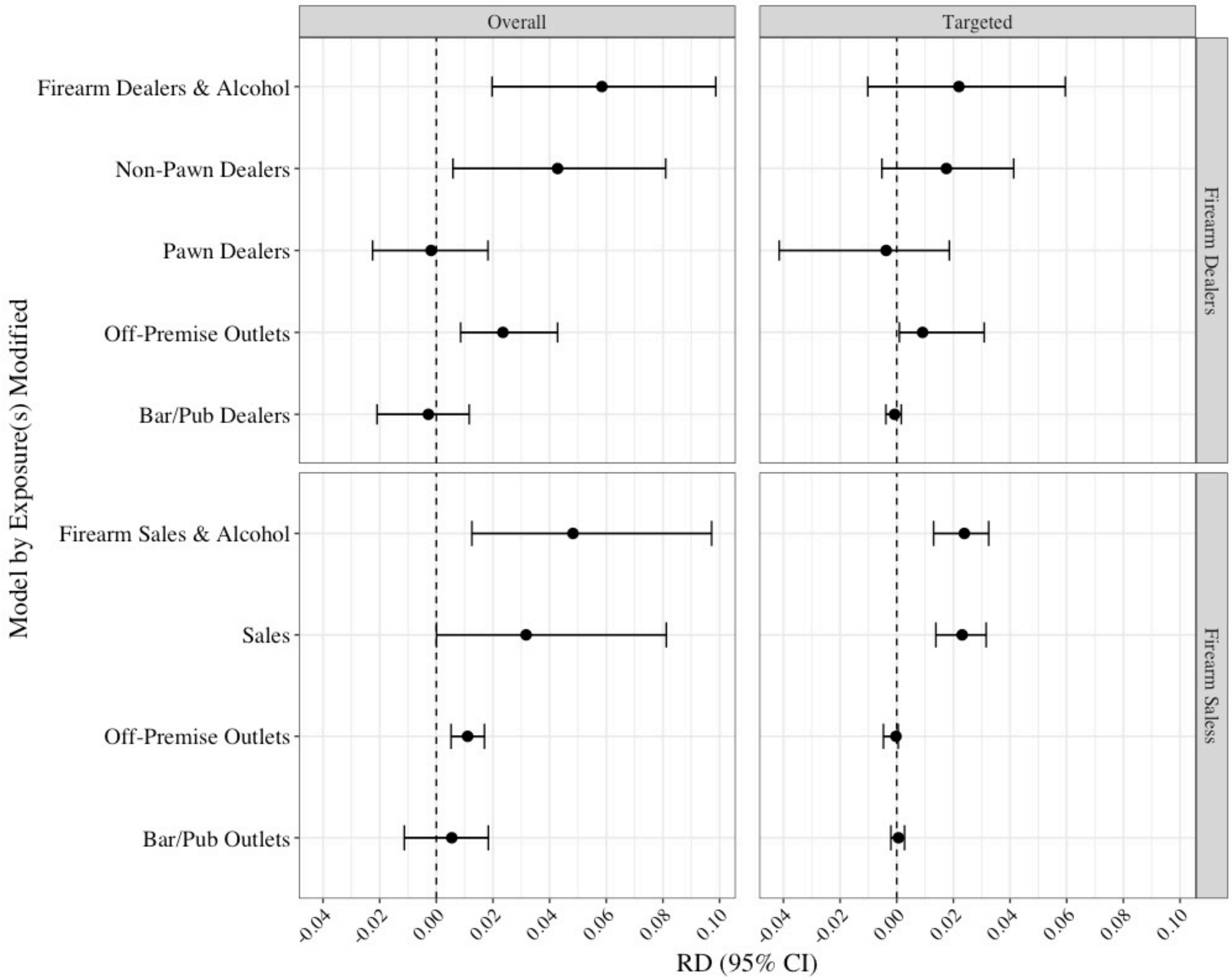
a. 2,835 individuals have multiple assaultive firearm injuries.

Table 2.2: Risk of Firearm Assault per 100,000 Residents per Month by Tertile of Firearm Dealer, Firearm Sale, and Alcohol Outlet Density

	Tertile[a]	Homicides/Assaults[b]
12-month Firearm Dealer or Sales Density per 100,000 residents		
Non-pawn dealers	1	2.30
	2	1.14
	3	1.17
Pawn dealers	1	2.52
	2	0.98
	3	1.16
Firearm sales	1	2.37
	2	1.14
	3	1.10
12-month Alcohol Outlet Density per 100,000 residents		
Off-premise outlets	1	1.15
	2	1.67
	3	1.72
Bar/pub outlets	1	1.39
	2	1.53
	3	1.59

a. 1 is the lowest tertile and 3 is the highest.
b. Estimates are weighted to be representative of the population.

Figure 2.1: Adjusted Risk Differences for Firearm Homicide/Assault per 100,000 Residents per Month[a]



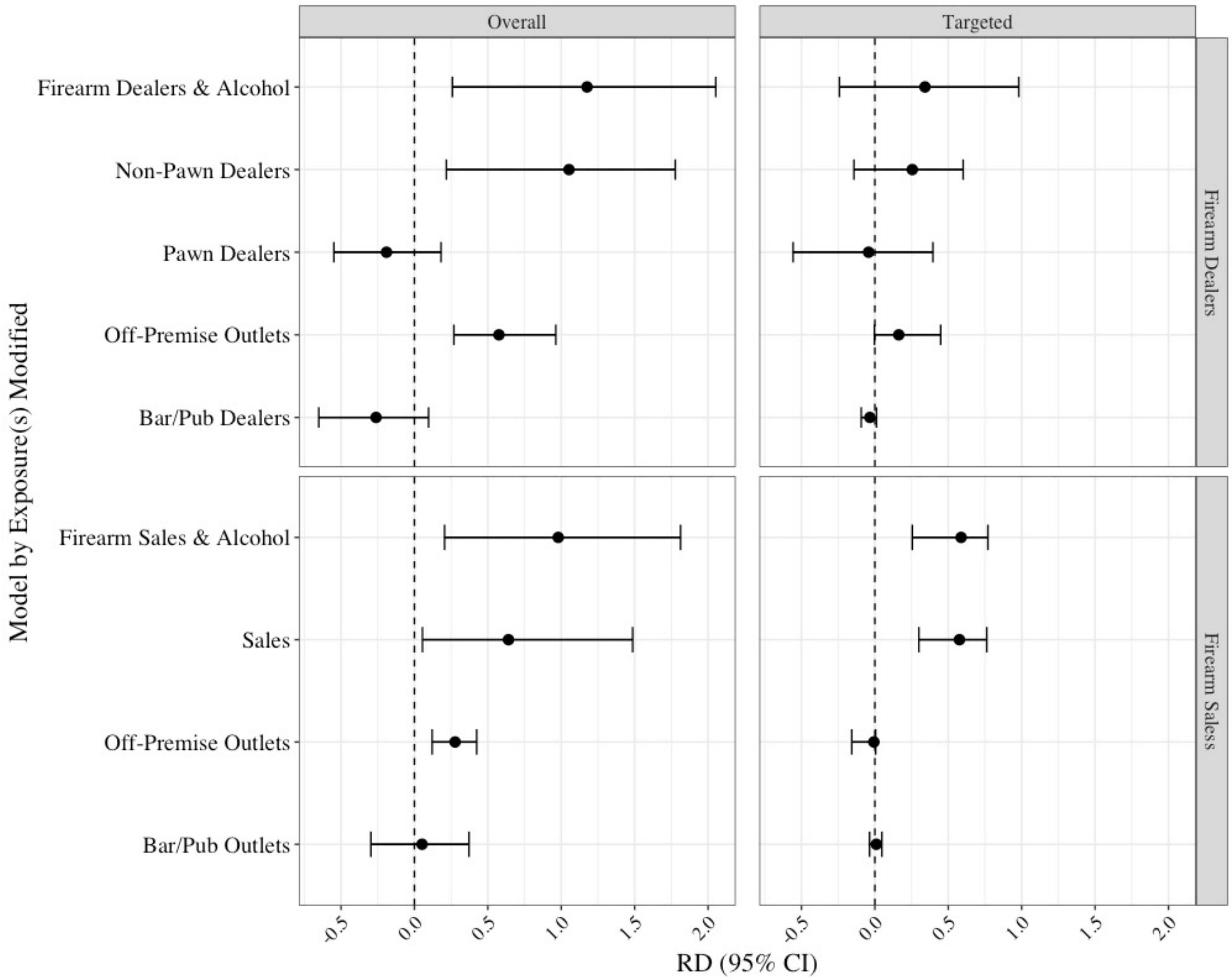
a. "Firearm dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Table 2.3: Additive Interaction Estimates

Study Population	Interactions Tested[a]	RD Overall Interaction (95% CI)
Total population	Firearm dealers & alcohol outlets	-0.003 (-0.023, 0.007)
	Firearm sales & alcohol outlets	0.000 (-0.009, 0.011)
Black boys and men aged 15-39	Firearm dealers & alcohol outlets	-0.001 (-0.327, 0.229)
	Firearm sales & alcohol outlets	0.010 (-0.175, 0.248)

a. "Firearm dealers" includes both non-pawn and pawn dealers. "Alcohol outlets" include both off-premise outlets and bars/pubs.

Figure 2.2: Adjusted Risk Differences for Firearm Homicide/Assault per 100,000 Residents per Month Among Black Boy and Men Aged 15-39[a]



a. "Firearm dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

3. The Joint Role of Firearm and Alcohol Availability in the Risk of Firearm Suicide and Self-Harm

Introduction

Firearms are the most commonly used and deadliest method of suicide in the United States.[85] Firearm suicide is the 10th leading cause of death and the 4th leading cause of death by injury.[1] The age-adjusted rate of firearm suicide increased by 23% from 2006 to 2019, affecting all racial/ethnic groups.[1] However, risk is concentrated among older white men who suffer both the highest rate and the greatest number of deaths from this cause.[2,3]

Firearm ownership, also concentrated among older white men,[86,87] is a major risk factor for firearm suicide.[20] Suicide attempts are often impulsive acts[88] that are made more deadly when lethal means, such as firearms, are readily available.[85] Because firearm ownership is difficult to intervene on directly, it is important to identify alternatives, such as community-level interventions that can create a safer environment for at-risk individuals.

Firearm dealers are the primary source of new firearms in a community. Thus, it is plausible that areas with higher firearm dealer density or firearm sales density would have elevated rates of firearm suicide. However, only a few studies have evaluated this relationship and findings are mixed.[23,43,89,90] Previous research comprised ecological, cross-sectional, and often state-level studies that primarily used Federal Firearm Licenses (FFLs) to measure firearm dealers, which do not account for sales and can be imprecise with regard to timing of dealer openings and closings. It is important to examine this relationship more rigorously, as findings could inform real-world interventions leveraging zoning laws to limit firearm dealer density.

In addition to firearm access, alcohol use is a major risk factor for firearm suicide: an estimated 35% of firearm suicide victims in the United States consumed alcohol shortly before death.[44] Alcohol outlet density, as a modifiable measure of community-level alcohol availability and norms, provides another opportunity for intervention. Alcohol outlet density has been linked to local drinking behavior, including heavy and binge drinking,[91,92] which, in turn, increases risk for suicide.[93] Outlet density has also previously been associated with suicide by any means.[26,94] However, the single study that examined the relationship between alcohol outlet density and firearm suicide, in particular, found a null association (although the point estimate suggested the odds may be elevated in areas with high off-premise outlet density).[31]

The joint effects of alcohol outlet density and firearm dealer density on firearm self-harm are unknown, but it is plausible that risk is highest in areas where both alcohol and firearms are easily accessible. In this study of California residents, we aim to 1) quantify the association between firearm dealers, measured by dealer density and sales density, with firearm self-harm (fatal and nonfatal); 2) quantify the association between alcohol outlet density and firearm self-harm; and 3) quantify the joint association between both firearm dealer (or sales) and alcohol outlet density and firearm self-harm, testing for additive interaction between the two exposures. We additionally explore these associations in older white men, the subset of the population at highest risk for firearm suicide.

Methods

Study design

We conducted a density-sampled case-control study of California residents from January 2005 through September 2015. Cases were Californians at least 10 years of age who were injured or killed by an intentional self-directed gunshot wound (it is extremely rare for self-directed injuries to be coded as intentional for children under 10). We selected 4 controls for every case from the general population aged 10 and over, drawing directly from the primary study base.[51,52] We examined the associations between county-level firearm availability and/or ZIP code tabulation area (ZCTA) level alcohol availability and individual-level risk of fatal or nonfatal self-inflicted firearm injury using case-control-weighted G-computation to estimate risk differences and additive interactions, controlling for individual and community-level confounders.

<u>Data and Measures</u>

Exposures: The first exposure of interest was community firearm availability. This was measured monthly with Dealer Record of Sale (DROS) data, maintained by the California Department of Justice, from January 2004 through September 2015 (the most recent data available). These data comprise records for nearly every legal handgun transfer (including private party transfers and gun show sales) in California since 1991 and every long gun transfer since 2014. Thus, DROS data can be used to identify which firearm dealers are actively selling firearms and how many firearms they sell each month. To determine the best spatial unit of analysis, we geocoded paired DROS dealer and purchaser address data to determine the travel distance between firearm dealers and purchasers. As 70% of purchasers and dealers were in the same county, we chose this as the unit of analysis (worse-performing administrative boundaries were Public Use Microdata Areas, cities, and ZIP codes).

We measured firearm availability as: 1) active firearm dealer density and 2) firearm sales density, so that we could determine which of these measures is more relevant to firearm self-harm. For each of these measures, we calculated monthly densities per 100,000 residents for each county. We used these to calculate a 12-month moving average to increase stability. “Active” firearm dealers were defined as dealers with at least 1 sale in a given month. Pawn shop and non-pawn dealer densities were measured separately, as they were hypothesized to have different associations with the outcome (only pawn shops have been linked to firearm suicide).[23] DROS data do not distinguish pawn shops from other dealers, so we classified dealers as pawn shops if they had “pawn” or “loan” in the business name or email address or if they had a DROS record for redeeming pawned firearms. To focus on new firearms entering a community, we limited transactions to sales (excluding pawn redemptions, private party transfers, curio/relics, loans, and non-roster peace officer transfers).

The second exposure of interest was community alcohol availability, measured with ZCTA-level alcohol outlet density, 2004-2015. This is consistent with previous ZIP code-level studies of alcohol outlet density,[26,95] but avoids misalignment issues that arise when using ZIP codes over time. ZCTAs, created by the Census Bureau, approximate ZIP codes and are stable for 10-year periods. Using annual alcohol outlet data from the California Department of Alcoholic Beverage Control (ABC), we estimated the monthly ZCTA density per 100,000 residents for off-premise outlets (license types 20 and 21) and bars/pubs (license types 23, 40, 42, 48, 61, and 75) separately, as they were previously found to have different associations with violence.[59] We linked the geocoded ABC data to the 2010 ZCTA map to get the number of outlets in each ZCTA-year. To estimate exposures throughout the year, we calculated a moving weighted average of the current and prior year’s density (e.g., the density for April 2005 was calculated as $\frac{4}{12}$(2005 density) + $\frac{8}{12}$(2004 density)).

Cases: Cases were all individuals in California with a fatal or nonfatal self-inflicted firearm injury (hereafter "firearm self-harm") from January 2005 through September 2015. Fatal case data were from the California Department of Public Health's Comprehensive Death Files and nonfatal case data were from the Office of Statewide Health Planning and Development's (OSHPD's) emergency department and hospital discharge records. To avoid double counting fatal injuries, we only use nonfatal cases from the OSHPD data. The death data used the International Statistical Classification of Diseases and Related Health Problems, 10th edition (ICD-10) and the injury data used the 9th edition (ICD-9); firearm self-harm was identified with ICD-10 codes X72-74 and with ICD-9 codes E955.0-E955.4 (see Appendix B Table 1 for details). We restricted cases to California residents (for whom we have exposure data). Whenever possible, we used linked individual-level OSHPD records to fill in missing demographic characteristics (e.g., race, ZIP code).

Controls: To sample controls from the residential population in California, we first estimated the annual ZCTA population by age, sex, and race using a crosswalk with county-level population data from the Census Bureau[60] (since ZCTAs can cross county boundaries, we generated estimates for each unique ZCTA-county unit and later summed these to create single ZCTA estimates). Those under 10 years of age were excluded to reflect the study base of our cases. We estimated monthly values by linearly interpolating changes in subgroup population counts and refined the at-risk population estimates by removing cases from the population corresponding to the case's ZCTA, age, sex, and race. We then calculated the probability of being in each ZCTA-county age-sex-race group at each year-month and randomly sampled controls from this multinomial distribution for every study month at a 4:1 ratio with incident cases.

Covariates: We used theory and previous literature to determine covariates *a priori* using a directed acyclic graph to visualize their relationships (Appendix B Figure 1). We controlled for the following confounders: individual age, race/ethnicity, sex; ZCTA percent aged 55 and older, percent non-Hispanic white, percent male, urbanicity, median household income z-score (hereafter "income"), percent aged 25 years and older with at least a bachelor's degree (hereafter "education), and unemployment rate in the civilian work force over age 16 (hereafter "unemployment"). We also controlled for year to account for secular trends in firearm self-harm and included an indicator for when DROS started recording long gun transactions in 2014.

Individual demographic variables were taken directly from the hospital and death data. Race/ethnicity included the following categories: non-Hispanic white, non-Hispanic Black, Hispanic, Asian, Native American, and multiracial. ZCTA income, education, and unemployment data were from the American Community Survey 5-year estimates for 2007-2011 and 2012-2016.[61] We used 2010 census data to calculate the percent of the population in each ZCTA that was in an urban area (>=50,000 people), an urban cluster (2,500-49,999 people), or a rural area (<2,500 people) and then defined each ZCTA as urban, suburban, or rural based on the category with a plurality. ZCTA demographic data (age, sex, race) were calculated monthly using the interpolated population data describe above, transformed into 12 month rolling averages.

<u>Statistical analysis</u>

To estimate risk differences and additive interactions, we used case-control-weighted (CCW) G-computation. G-computation is a parametric substitution estimator, i.e., the outcomes are estimated under specified exposure regimes, which are substituted into the parametric model.

The case-control sampling scheme is overcome by reweighting: cases are weighted with the population prevalence of the outcome and controls are weighted with (1-prevalence)/(control:case ratio).[63,64] The CCW G-computation formula is as follows:

$$\Psi\,(P) = E_w\{[\,I(Y_{i=1}) * q(t)[E(Y \mid A_1{=}1, A_2{=}1, W_i)]\,] + [\,I(Y_{i=0}) * \frac{\bar{q}(t)}{J(t)}[E(Y \mid A_1{=}1, A_2{=}1, W_i)]\,]\} \,/$$
$$E_w\,[\,I(Y_{i=1})q(t) + I(Y_{i=0})\,\frac{\bar{q}(t)}{J(t)}\,] -$$
$$E_w\{[\,I(Y_{i=1}) * q(t)[E(Y \mid A_1{=}0, A_2{=}0, W_i)]\,] + [\,I(Y_{i=0}) * \frac{\bar{q}(t)}{J(t)}[E(Y \mid A_1{=}0, A_2{=}0, W_i)]\,]\} \,/$$
$$E_w\,[\,I(Y_{i=1})q(t) + I(Y_{i=0})\,\frac{\bar{q}(t)}{J(t)}\,],$$

where Ψ is the risk difference at the true data distribution (P); q_0 and $\bar{q}_0$ are the case and control weights, respectively, at each year-month (t); $E(Y \mid A_1{=}1, A_2{=}1, W{=}w)$ is the expected value of the outcome (Y) when both exposures (A_1 and A_2) are set to a given value, such as 1, adjusting for confounders (W); and E_w is the expectation over W.

We estimated 2 parameters for 2 populations: the entire state population 10+ years old and the highest-risk subpopulation, white men 50+ years old. The first parameter is the overall risk difference ($RD_{overall}$) comparing firearm self-harm rates when a given exposure (non-pawn, pawn, firearm sales, off-premise alcohol outlet or bar/pub density) is set to high density with firearm self-harm rates when the exposure is set to low density. “High” and “low” were determined empirically, using the observed highest and lowest values in each county or ZCTA over the study period to avoid positivity violations (Appendix B Table 2). The second parameter estimates the population attributable risk (RD_{PAR}) comparing the outcome under the observed value of a given exposure with a scenario in which the exposure is set to low density. We also calculated the corresponding risk ratios (RR) to provide additional context for understanding the absolute differences we present.

In addition to these individual exposures, we examined the joint exposures of firearm availability (measured with dealers or sales) and alcohol outlets (off-premise outlets and bars/pubs). We also calculated additive interactions by subtracting the sum of the individual RDs (“expected”) from the joint RD (“observed”). We examined additive interaction to better isolate cases that would only occur in the presence of high firearm and alcohol availability but not in the presence of either alone. Bias corrected and accelerated nonparametric bootstrapped confidence intervals were calculated for all point estimates (n runs = 400).[65] To determine whether results were sensitive to the inclusion of long gun data, we re-estimated the $RD_{overall}$ and $RR_{overall}$ for the full population after removing long guns from the DROS data.

The G-computation estimates were based on case-control-weighted logistic regression models using cluster-robust standard errors (Hubert-White sandwich estimator) to account for non-independence of observations from individuals being nested within counties, zip codes, and year-months. We included interactions between the firearm availability and alcohol availability variables and retained those with $p<0.20$.[66] We created scatterplots with smoothed lines of fit to visualize nonlinearity in the bivariate relationships between each continuous variable and the log-odds of the modeled outcome. To spend our degrees of freedom wisely and minimize overfitting, we modeled variables with restricted cubic splines only if they appeared nonlinear and they were significantly associated with the outcome (at alpha=0.10) when modeled with or without a spline.

Analyses were performed with R 4.0.2, Stata/MP 13.1, and ArcGIS 10.7. This study was approved by the California Health and Human Services Agency’s Committee for the Protection for Human Subjects; University of California, Berkeley's Committee for the Protection for Human Subjects; and the University of California, Davis Institutional Review Board.

Results

From January 2005 through September 2015, there were 17,277 intentional self-directed firearm injuries in California among residents. One case was removed for being under the age of 10. Records missing ZCTA, age, sex, or race were dropped (n=103), leaving us with 17,173 cases. With 68,692 controls, this gave us a sample size of 85,865 injuries. We dropped 24 cases and 72 controls with missing community-level covariate information. In all, 0.7% of cases and 0.1% of controls were dropped for missingness. Several continuous variables were highly skewed. To minimize the potential bias from these extreme outliers, we removed all observations with an exposure or covariate value more than 10-times the interquartile range (IQR) below the 1^{st} quartile or greater than the 3^{rd} quartile of the variable's distribution (n=500 cases and 910 controls). Cases and controls had extreme values of the exposures and ZCTA percent male. Cases additionally had extreme values of ZCTA percent unemployed. In the course of visualizing the data, we identified and removed an additional 3 observations with extreme covariate values. This yielded a final sample size of 84,356 (16,648 cases and 67,708 controls). Our controls closely matched the Californian population from which they were drawn (Appendix B Table 3).

Table 3.1 presents the individual- and community-level characteristics of study participants. Firearm self-harm cases were more likely than controls to be male (88.0% vs. 49.3%), white (76.5% vs 42.1%), 50+ years old (57.4% vs. 34.2%), and live outside of urban areas (16.1% vs. 7.7%). Alcohol outlet, firearm dealer, and firearm sales densities were all higher among cases than controls.

Table 3.2 displays the monthly risk per 100,000 of firearm self-harm, weighted to be representative of the state population, by tertile of each exposure. On average, the monthly rate of firearm self-harm was 0.41 injuries per 100,000. The risk of firearm self-harm injury increased from lowest to highest tertile across all exposures. The highest rate was among those in the highest tertile of pawn dealer density, at 0.56 injuries per 100,000, and the lowest was among those in the lowest tertile, at 0.29 injuries per 100,000.

Figure 3.1 presents the adjusted CCW G-computation results for firearm self-harm in the full population (corresponding RRs are in Appendix B Table 4). In single exposure models, only bar/pub density was associated with risk of firearm self-harm when measuring firearm availability with firearm dealers ($RD_{overall}$: 0.004, 95% CI: 0.0003, 0.008). Individually, this association is not meaningfully significant. Taken together, high densities of firearm dealers and alcohol outlets were associated with modestly elevated risk compared with low densities ($RD_{overall}$: 0.02, 95% CI: 0.01, 0.03). There was no evidence of additive interaction between firearm and alcohol availability (Table 3.3). Using sales rather than dealers to measure firearm availability did not affect the estimate for off-premise outlet and bar/pub density. However, firearm sales density was associated with an increased monthly risk of 0.06 injuries per 100,000 (95% CI: 0.03, 0.09), representing at 14% relative increase (95% CI: 1.07, 1.23), or about 18 suicides per month. This association entirely accounted for the overall joint association of both firearm sales and alcohol availability and firearm self-harm ($RD_{overall}$: 0.06, 95% CI: 0.03, 0.09).

As expected, the RD_{PAR} estimates were attenuated compared with the $RD_{overall}$, but they followed a similar pattern (Figure 3.1). In models using firearm dealer density to measure firearm availability, neither the individual nor the joint exposures were associated with firearm self-harm. When using firearm sales to measure firearm availability, the individual alcohol outlet density exposures remained null, but firearm sales and firearm sales together with alcohol availability were associated with small but significant increases in the risk of firearm self-harm (sales RD_{PAR}: 0.01, 95% CI: 0.01, 0.02; sales and alcohol RD_{PAR}: 0.02, 95% CI: 0.01, 0.02). This is equivalent to about 52 fewer firearm self-harm injuries per year (there were on average 1,513 such injuries per year during our study period).

Figure 3.2 presents the corresponding results for the high-risk subgroup analysis of white men aged 50+ (RRs are presented in Appendix B Table 5). The average monthly rate of firearm self-harm in this population was 1.94 injuries per 100,000. Overall, patterns were similar with the statewide analysis, but the magnitude was larger in the high-risk group. Associations with pawn dealer, off-premise alcohol outlet, and bar/pub density were null, but unlike the statewide analysis, non-pawn firearm dealer density was associated with increased risk of firearm self-harm among older white men ($RD_{overall}$: 0.08, 95% CI: 0.01, 0.17). This non-pawn association was the main contributor to the estimate for the joint association between firearm dealer and alcohol outlet density in this group ($RD_{overall}$: 0.11, 95% CI: 0.05, 0.22).

In analyses using firearm sales to measure availability, we found sales density to be associated with substantially increased risk of firearm self-harm among the high-risk population ($RD_{overall}$: 0.27, 95% CI: 0.13, 0.41). This corresponds to a 14% relative increase in risk (95% CI: 1.07, 1.22), mirroring the $RR_{overall}$ in the full population. Controlling for firearm sales, bar/pub density was also associated with slightly increased risk in older white men ($RD_{overall}$: 0.02, 95% CI: 0.002, 0.03). Together, high densities of firearm sales and alcohol outlets were associated with an additional monthly risk of 0.28 injuries per 100,000 (95% CI: 0.14, 0.41), due almost entirely to firearm sales. There was no evidence of interaction (Table 3.3).

Among the high-risk population, the RD_{PAR} was null for all individual and joint exposure models using firearm dealer density to measure firearm availability (Figure 3.2). However, when using firearms sales density, only off-premise alcohol outlet density was null. Compared with low firearm sales density, the observed density was associated with an increased monthly risk of 0.07 injuries per 100,000 among older white men (95% CI: 0.03, 0.09). The RD_{PAR} for bar/pub density was small, at 0.01 self-harm injuries per 100,000 (95% CI: 0.001, 0.02). Together, the joint association of firearm sales and alcohol availability had nearly the same point estimate as firearm sales alone (RD_{PAR}: 0.08, 95% CI: 0.04, 0.10), suggesting this relationship is mainly due to firearm sales.

Results from the sensitivity analyses removing long guns from the DROS data are displayed in Appendix B Table 6. This reduced the magnitude of our findings for the $RD_{overall}$ in the statewide population by about half in all firearm-related exposure models (single exposure alcohol outlet density models were unaffected). Firearm sales density remained the community feature most strongly associated with increased risk of firearm self-harm ($RD_{overall}$: 0.03, 95% CI: 0.02, 0.05).

Discussion

We found a strong relationship between firearm sales density and firearm self-harm, with risk 14% higher in communities with a high versus a low sales density, corresponding to about 221 additional firearm suicides per year. Among older white men, associations were substantially larger than those for the statewide population across all exposures of interest. In addition, non-pawn firearm dealer density was associated with increased risk for firearm self-harm only in this high-risk subset. Alcohol outlet density was almost always null and there was no additive interaction between firearm and alcohol availability. Finally, we found that firearm sales density remained significantly associated with firearm self-harm risk in RD_{PAR} models, suggesting that an intervention to reduce firearms sales density could have a meaningful impact on firearm self-harm, particularly among those at highest risk.

Few studies have previously evaluated the association between firearm dealer density and firearm suicide. One county-level study found firearm dealer density to be associated with increased firearm suicide rates,[89] and a state-level study found pawn shop density, but not non-pawn density, to be associated with increased rates.[23] However, a separate state-level study with better control of confounding than its counterpart found a null association.[43] None of these studies are directly comparable to ours, since we estimated the individual-level risk of firearm self-harm while the others used weaker ecological designs to examine changes in group-level rates of firearm suicide. We also examined different populations and controlled for different sets of confounders. In general, the findings of these past ecologic studies were not mirrored in this multi-level analysis. We found no association between firearm dealers and risk of self-harm in the statewide analysis, and only found elevated risk among older white men in the presence of high non-pawn dealer density.

Associations were much stronger when we measured firearm availability with firearm sales rather than dealers in both the general and high-risk populations. To our knowledge, ours is the first study to evaluate the association between community-level sales and individual-level risk of firearm self-harm. At the individual level, it is well established that purchasing a handgun is associated with extremely high relative risk of firearm suicide.[20] This risk extends to others living with the firearm owner, including children.[14,96] Although about 70% of firearm suicides are from handguns,[97,98] findings from our sensitivity analyses suggest that interventions targeting only handguns would have more limited benefits. However, the differences between high and low densities of firearm dealers and sales were smaller in the analysis without long guns, which may partially explain why the associations were attenuated. Future studies should use additional years of long gun data to better estimate the relationship between long gun sales and firearm self-harm. Currently, firearm transaction records are only available for the State of California. Our findings illustrate the public health value in allowing researchers access to such data, as sales (and ownership) data are essential to studying the role of community- and individual-level firearm accessibility in firearm violence.

Taken together, our findings suggest that the most likely mechanism is that firearm dealers play a role in firearm suicide through increasing the accessibility of firearms to at-risk individuals via sales. Community-level interventions could consider targeting local volume of firearm sales. Private companies can aid in this effort by ending or limiting sales of firearms. This has worked previously: Walmart's decision to stop selling handguns in 1994 was associated with a 3.3-7.5% reduction in the firearm suicide rate in affected counties.[99] More recently, Dick's Sporting Goods stopped selling assault rifles after the mass shooting in Parkland, Florida and has since been scaling back the number of stores selling firearms of any kind. Other "big-box" stores selling firearms should consider following suit. Firearm dealers have been willing to participate in other suicide prevention efforts, such as providing temporary storage of firearms during periods of heightened risk[100,101] and displaying educational material about suicide and firearm safety.[102] Such efforts, if successful, may reduce the risk associated with community-level firearm availability.

With respect to alcohol outlet density, our null findings in the full population analysis are consistent with a previous case-control study of Philadelphia residents.[31] Among those at highest risk, we found that a high density of bars/pubs was associated with a slightly increased risk of firearm self-harm (corresponding to a 1% relative increase). Similarly, a ZIP code-level ecological study of suicide and suicide attempts by any means in California found bar density to be a risk factor.[26] The specific role of bars in suicide deserves further exploration. Overall, there is little evidence that community-level interventions to reduce alcohol outlet density would substantially reduce risk of firearm suicide. Nevertheless, previous research indicates that certain subsets of the population may benefit and there may be other public health considerations supporting such an intervention.[94,103]

Limitations

This study's findings should be considered in light of its limitations. The outcome could be subject to nondifferential misclassification, as it can be challenging to distinguish between unintentional deaths and suicides. However, this is less of a concern with firearm deaths than certain other forms of suicide (e.g., drug overdose or single-car motor vehicle accidents). Another limitation is that comparison between high and low density was sometimes quite small, particularly for pawn density, because we did not extrapolate beyond the observed highest and lowest value for each community. Our estimates, therefore, reflect both the adjusted association of interest and the degree of underlying variation in the exposure variables. Furthermore, we had a limited set of individual-level covariates, so there may have been residual confounding.

We cannot be sure our results will generalize to states outside of California. It is possible that California's stringent firearm laws may lead to weaker associations compared with what might be observed in other states. To date, California is the only state that has made firearm purchase records available to researchers and there are not nationwide FFL-specific sales data, so this work cannot currently be replicated elsewhere.

Conclusions

Risk factors for firearm self-harm come from multiple social-ecological levels, including the social and physical environments in which we live. Successful suicide prevention strategies will therefore include individual-level interventions, such as gun violence restraining orders,[32,33] as well as broad measures that reduce risk in the environment. Our findings suggest that community-level approaches targeting firearm dealer density or alcohol outlet density are unlikely to reduce firearm suicide, but that reducing firearm sales density may help, particularly for the older white men at greatest risk.

Table 3.1: Characteristics of Cases and Controls

	Cases[a]	Controls
Total	16,648	67,708
Individual Characteristics		
Sex, N (%)		
Male	14,654 (88.02)	33,389 (49.31)
Race/ethnicity, N (%)		
Non-Hispanic white	12,738 (76.51)	28,535 (42.14)
Non-Hispanic Black	674 (4.05)	4,005 (5.92)
Hispanic	2,249 (13.51)	23,944 (35.36)
Asian	689 (4.14)	9,578 (14.15)
Native American	55 (0.33)	289 (0.43)
Multiracial	243 (1.46)	1,357 (2.00)
Age Group, N (%)		
10-19	629 (3.78)	11,079 (16.36)
20-29	2,035 (12.22)	11,480 (16.96)
30-39	1,915 (11.50)	10,951 (16.17)
40-49	2,514 (15.10)	11,046 (16.31)
50-59	3,430 (20.60)	10,081 (14.89)
60-69	2,493 (14.97)	6,681 (9.87)
70-79	1,881 (11.30)	3,847 (5.68)
80+	1,751 (10.52)	2,543 (3.76)
ZCTA-Level Characteristics		
Alcohol Outlet Density per 100,000 residents, Median (25th, 75th pctl)		
Off-premise	70.53 (52.83, 94.06)	65.79 (49.98, 85.54)
Bar/pub	11.89 (6.04, 21.21)	10.50 (5.38, 17.54)
Urbanicity, N (%)		
Urban	13,965 (83.88)	62,518 (92.33)
Suburban	1,594 (9.57)	3,587 (5.30)

Rural	1,089 (6.54)	1,603 (2.37)
Demographics, Median (25th, 75th pctl)		
% Male	49.32 (48.55, 50.15)	49.28 (48.53, 50.12)
% Non-Hispanic white	44.10 (21.60, 65.09)	36.82 (17.55, 57.65)
% Age 55+	21.53 (17.29, 26.65)	23.36 (19.00, 28.64)
Median household income	60,530 (47,348, 79,441)	62,374 (48,350, 84,109)
% Bachelor's degree+	26.07 (15.67, 40.33)	27.25 (15.93, 41.37)
% Unemployed	9.45 (7.45, 11.99)	8.26 (6.48, 10.73)
County-Level Characteristics		
Firearm Dealer or Sales Density per 100,000 residents, Median (25th, 75th pctl)		
Non-pawn dealer	0.87 (0.45, 1.52)	0.74 (0.41, 1.17)
Pawn dealer	0.90 (0.60, 1.57)	0.79 (0.49, 1.15)
Firearm sales	37.61 (24.11, 66.18)	34.27 (22.56, 59.92)

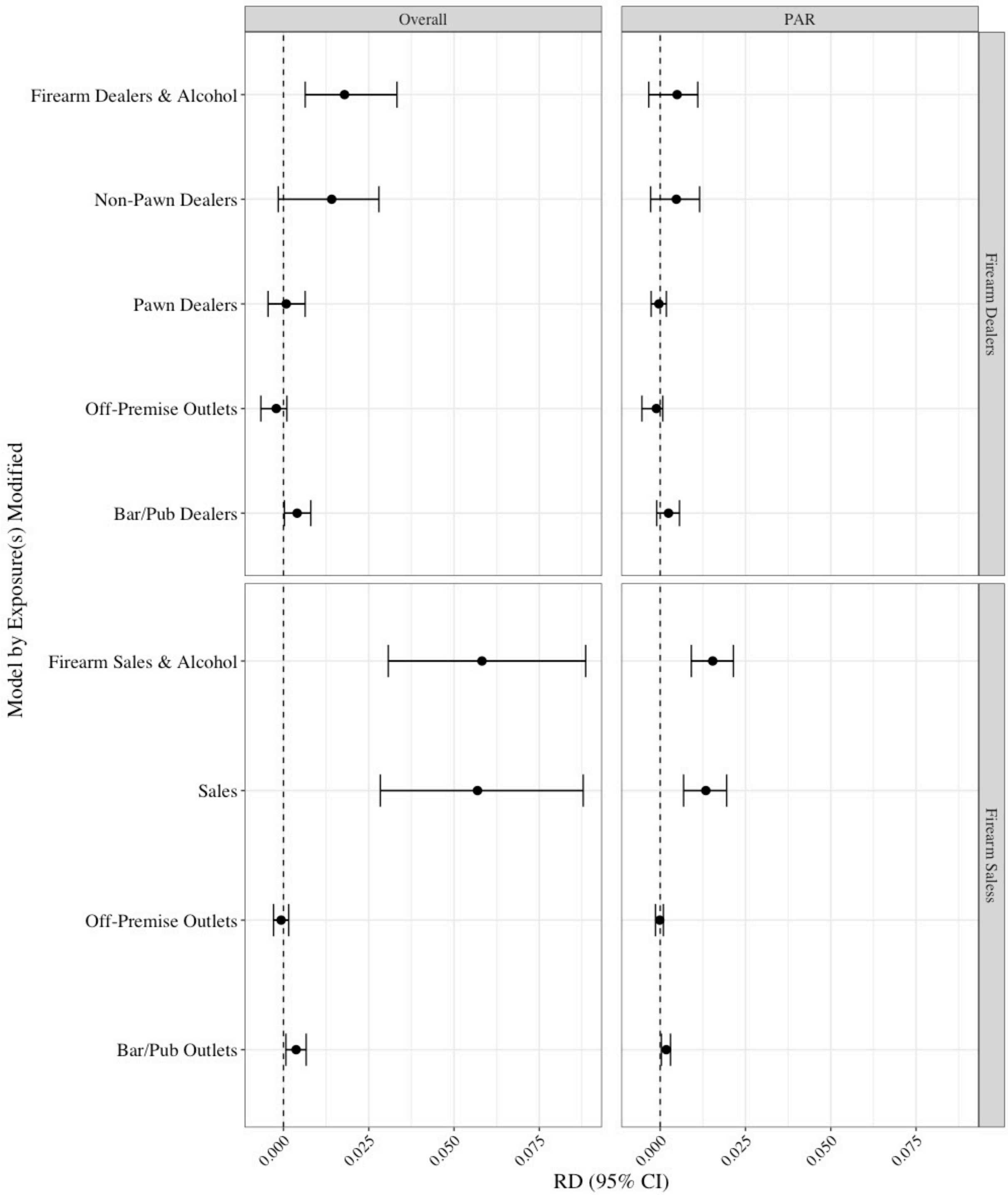
a. 70 individuals have multiple self-directed firearm injuries.

Table 3.2: Risk of Firearm Self-Harm per 100,000 Residents per Month by Tertile of Firearm Dealer, Firearm Sale, and Alcohol Outlet Density

	Tertile[a]	Firearm Self-Harm[b]
12-month Firearm Dealer or Sales Density per 100,000 residents		
Non-pawn dealers	1	0.30
	2	0.40
	3	0.54
Pawn dealers	1	0.29
	2	0.40
	3	0.56
Firearm sales	1	0.33
	2	0.41
	3	0.49
12-month Alcohol Outlet Density per 100,000 residents		
Off-premise outlets	1	0.35
	2	0.38
	3	0.50
Bar/pub outlets	1	0.36
	2	0.38
	3	0.49

a. 1 is the lowest tertile and 3 is the highest.
b. Estimates are weighted to be representative of the population.

Figure 3.1: Adjusted Risk Differences for Firearm Self-Harm per 100,000 Residents per Month[a]



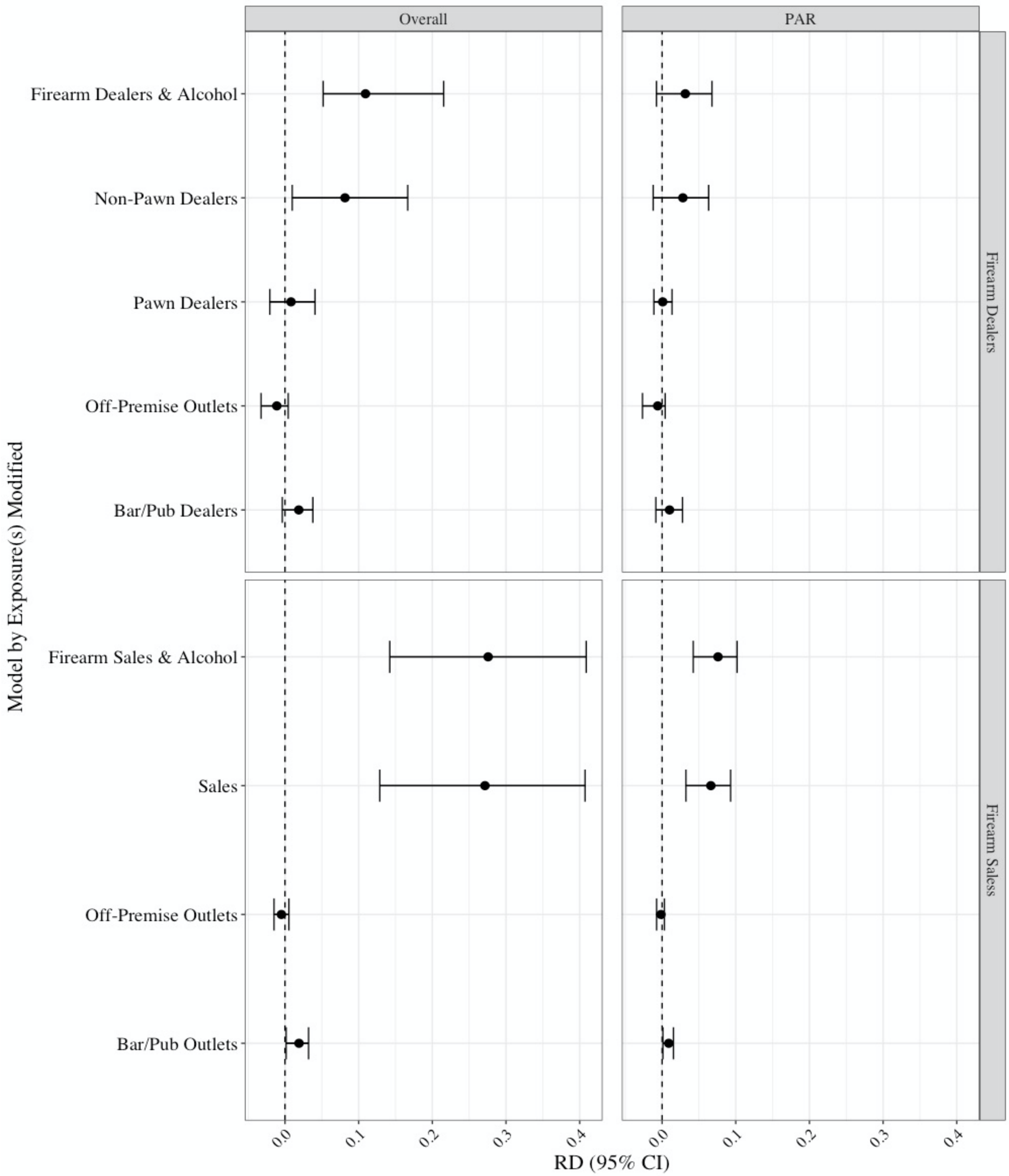
a. "Firearm dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Table 3.3: Additive Interaction Estimates

Study Population	Interactions Tested[a]	RD Overall Interaction (95% CI)
Total population	Firearm dealers & alcohol outlets	0.001 (-0.003, 0.011)
	Firearm sales & alcohol outlets	-0.002 (-0.003, 0.000)
White men aged 50+	Firearm dealers & alcohol outlets	0.012 (-0.009, 0.101)
	Firearm sales & alcohol outlets	-0.010 (-0.017, 0.000)

a. "Firearm dealers" includes both non-pawn and pawn dealers. "Alcohol outlets" include both off-premise outlets and bars/pubs.

Figure 3.2: Adjusted Risk Differences for Firearm Self-Harm per 100,000 Residents per Month Among White Men Aged 50+[a]



a. "Firearm dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

4. Implementation of California's Gun Violence Restraining Order Law and Firearm Violence Injury: A Synthetic Control Analysis

Introduction

Half of suicides and three-quarters of homicides in the United States are caused by gunshot wounds, with firearm violence resulting in nearly 40,000 deaths and many more injuries in 2019 alone.[1] Many acts of firearm violence—including two-thirds of public mass shootings—are preceded by explicit or implicit threats.[104] However, in most states, individuals indicating that they are at risk of harming others or themselves are legally permitted to possess and purchase firearms as long as they are not otherwise prohibited from ownership. Following a mass shooting that exposed this legal gap and its dangers, the California legislature enacted a targeted, risk-based policy for firearm violence prevention with the gun violence restraining order (GVRO) law,[105] which went into effect on January 1, 2016.

Under this law, when an individual poses a significant danger of self- or other-directed violence and has or could obtain access to a firearm, law enforcement or family members can petition a judge for a GVRO, which temporarily prohibits the subject of the order (the respondent) from purchasing or possessing firearms and ammunition. Upon service of the order, the respondent has 24 hours to relinquish their firearms to a licensed firearm dealer or law enforcement agency and 48 hours to provide proof of relinquishment to the court (however, in most cases relinquishment happens at the time of service). After 3 weeks, a hearing is held, at which time the judge determines whether the GVRO should be extended for one year. As of September 2020, coworkers and teachers have also been permissible petitioners, and the duration of the order after a hearing has been extended to a maximum of 5 years.

Sixteen states and the District of Colombia have since passed similar laws, most often called "extreme risk protection order" laws. There is also a push for such laws from the federal government, as President Biden recently instructed the Department of Justice to draft model legislation and asked congress to incentivize adoption among states.[106]

Despite their recent popularity, we know very little about whether these laws are working to prevent firearm violence. A small number of individual- and state-level studies using data from two states that have older and more limited "risk warrant" laws (which require the relinquishment of firearms but do not prohibit their purchase) found these laws to be associated with reductions in firearm suicide.[32-34] Only one study included data from states with GVRO-style laws, but these states (California and Washington) contributed only a single post-GVRO year in analysis. Nevertheless, when pooled with other early adopting states, the law was found to be associated with a modest reduction in firearm suicide among older adults.[107]

In short, we do not know whether GVROs are effective at preventing firearm violence. Despite GVROs and similar laws being implemented in response to mass shootings, no studies have been done on the impact of these laws on firearm homicide (though there are several cases in which GVROs were issued for potential mass shootings, none of which were carried out[108]). In addition, nonfatal firearm injuries have yet to be included in analysis, even though temporarily removing firearms from high-risk situations should reduce intentional firearm injuries of all kinds.

In this study, we aimed to begin addressing these gaps by quantifying the association between GVRO use and intentional firearm violence death and injury in San Diego County ("San Diego") during 2016-2019, the first 4 years of the law's implementation. In previous work, we found uptake of the law to be substantially higher in San Diego than any other county in the state,[35] providing an opportunity to leverage within-state variation in analysis. In addition to evaluating the impact on firearm violence overall, we also assessed firearm assault/homicide and self-harm/suicide individually. Given that many states have recently enacted similar laws and that the federal government is explicitly supporting their adoption, these findings should be of interest to policymakers and violence prevention researchers nationwide.

Methods

Study design and sample

To estimate the association between GVRO implementation and firearm violence in San Diego, we used the synthetic control method, a quasi-experimental comparative case study design.[109] This approach is akin to difference-in-differences combined with matching: the post-GVRO rate of firearm violence in San Diego (the treated unit) is compared to the outcome in the synthetic control unit, which is a convex combination of California control counties weighted to match the pre-GVRO (2005-2015) firearm violence trend in San Diego as closely as possible. To account for relevant differences between counties, key predictor variables were also included in the matching process. We then used a weighted combination of control units to predict the rate of firearm violence in San Diego after GVROs were implemented (2016-2019). In this way, the synthetic control estimates the counterfactual firearm violence trend in San Diego, had GVROs not been used.

As all of California was exposed to the GVRO law in 2016 but use of GVROs varied substantially across its 58 counties, we only included in the donor pool counties in California that both had stable rates of firearm violence and issued no or very few GVROs, as detailed below. Using California counties allowed us to include nonfatal firearm injuries, greatly increasing the observed number of outcome events, and obviated concerns about comparability due to differences in firearm laws and culture between states. Following CDC guidelines for unstable rates,[110] we excluded 26 counties from the donor pool for having, on average, fewer than 20 firearm violence injuries (fatal and nonfatal) per year in the pre-GVRO period.

To ensure we did not include exposed counties in the donor pool, we removed an additional 4 counties with a ratio of GVROs to expected firearm violence injuries > 0.1 (calculated as: total GVRO respondents 2016-2019 / [mean annual firearm violence injuries*4]). We determined the number of GVRO respondents per county using California Restraining and Protective Order System data, maintained by the California Department of Justice (CA DOJ). The 0.1 cutoff was guided by Swanson and colleagues' finding that 1 firearm suicide was prevented for every 10-20 cases of firearm removal pursuant to a risk-warrant in Connecticut and Indiana.[32,33] Based on this figure, if 10% of the population that went on to harm themselves with a firearm was first served a GVRO, we would expect a 1% reduction in firearm self-harm. We are assuming any lesser measure of association would be undetectable at a population level, such that these counties can safely be considered unexposed to GVROs. This left us with 27 control counties in the primary analysis.

Data and measures

The primary outcome was the biannual (6-month) county-level rate of firearm violence per 100,000 population. We aggregated individual-level firearm homicide and suicide data from the California Department of Public Health's Comprehensive Death Files and nonfatal firearm assault and self-harm data from the Office of Statewide Health Planning and Development's emergency department and hospital discharge records. The death data used the International

Statistical Classification of Diseases and Related Health Problems, 10th edition (ICD-10) and the injury data used the 9th edition (ICD-9) through September 2015, after which it used the 10th edition. Specific codes used to identify firearm violence are displayed in Appendix C Table 1. The mortality data constitute a complete census of deaths and the nonfatal data comprise all nonfatal injuries that were treated in emergency or inpatient settings in California-licensed hospitals. All records were limited to California residents, and hospitalization records resulting in a death were discarded to avoid double counting.

Our county-level denominators were created by, first, linearly interpolating monthly populations (overall and for subgroups by age, sex, and race/ethnicity) between annual American Community Survey (ACS) population data.[111] Second, we averaged the first and last 6 months of each year to get biannual counts. The ACS data for 2020 were not available, so interpolation could not be done for 2019. Therefore, both periods in 2019 were assigned the annual denominators for that year.

To maximize exchangeability between San Diego and the counties in the donor pool, we included county-level predictors of firearm violence in the synthetic control model. These were identified *a priori* based on the literature.[3,69,112,113] Predictors included: percent aged 15-24, aged 55 and older, non-Hispanic White, non-Hispanic Black, Hispanic, and male; non-firearm violent crime and property crime rates per 1,000; handgun sales per 1,000; unemployment in the civilian population aged 16 and older; and urbanicity.

Biannual demographic characteristics were measured directly using the interpolated denominators. Biannual crime rates were estimated with annual publicly available Crimes & Clearances data from CA DOJ,[62] which we divided by two. Biannual firearm sales data were measured with CA DOJ's Dealer Record of Sales (DROS) data, which contain records of nearly all legal handgun transfers in California (certain intrafamilial transfers are exempt). We were missing the last 3 months of DROS data in 2015, so we used the last 3 months of 2014 in its place. We used our interpolated denominators for the crime and firearm sales data to calculate rates per 1,000. Unemployment data was measured with the ACS 5-year estimates for 2005-2009 and 2010-2014.[114] Urbanicity was measured with the 2013 Rural-Urban Continuum Codes created by the United States Department of Agriculture.[115]

<u>Statistical analysis</u>

We used the synthetic control method to estimate the association between GVRO implementation and firearm violence in San Diego. To create the synthetic control, weights were assigned to counties in the donor pool such that the mean squared prediction error (MSPE) was minimized in the pre-GVRO period with respect to firearm violence and the predictor variables. The weighted average of the counties was then used to predict the firearm violence trend in the post-GVRO period in the absence of GVROs. Therefore, this method rests on the assumption that the relationship between the treated and control units stays the same before and after the intervention. We included the pre-GVRO outcome variable for every other biannual period in our model; assuming that only places with similar measured and unmeasured covariate values would have similar pre-intervention firearm violence trajectories, this should help to control for unmeasured confounding.[109]

Following Abadie and colleagues,[116] we conducted a series of "in-space" placebo tests to determine how likely it was that the difference in firearm violence between San Diego and its synthetic control after 2016 was due to chance. To do this, we iteratively assigned the intervention to each control county, repeating the synthetic control procedure for each in turn.

Counties with a poor pre-GVRO fit, defined as 20*San Diego's MSPE, were then dropped.[109,117] The remaining counties provided us with the distribution of the estimator under the null hypothesis. We then calculated the test statistic as the mean post-GVRO difference in the rate of firearm violence in the treated unit and its synthetic control. From here, we derived the p-value for a one-sided test by dividing the number of counties with a difference equal to or more negative than San Diego's by the number of placebo counties plus one.[118] As we hypothesize that the GVRO law would reduce firearm violence, a one-sided test was deemed most appropriate.

Additional analyses

We repeated the synthetic control method and in-space placebo tests in two secondary analyses, one evaluating firearm assault and the other evaluating firearm self-harm, to further understand the impact of GVRO implementation on firearm violence. Because the number of events was reduced when we separated assault from self-harm, we modeled these outcomes using annual data (which improved fit over biannual data). Biannual covariates and denominators were adjusted to annual percentages, rates, and counts, as appropriate. Along with the aforementioned predictors, we included the pre-GVRO outcome variable for every other year in these models.

We also conducted two sensitivity analyses to test the robustness of our findings. For the first, we repeated the primary and secondary analyses using a more restrictive donor pool, which, as before, included only counties with a stable rate of firearm violence, but now excluded counties that ever had ≥10 GVRO respondents in a given year. It is implausible that counties with fewer than 10 GVROs in a year would have an associated population-level change in firearm violence given the previously estimated number needed to treat (NNT) of 10-20 for firearm suicide.[32,33] This gave us a donor pool of 20 counties.

For the second sensitivity analysis, we repeated the primary and secondary analyses using 2018 as the year of implementation because it was not until then that GVROs were being used in large numbers (in San Diego, there were <10 respondents in 2016 and 2017, about 75 respondents in 2018 and over 250 respondents in 2019).[35] For predictors missing data in 2016 and 2017 (handgun sales rate, crime rates, and percent unemployed), values were carried forward from 2015.

Analyses were performed with R 4.0.2. This study was approved by the California Health and Human Services Agency's Committee for the Protection for Human Subjects; University of California, Berkeley's Committee for the Protection for Human Subjects; and the University of California, Davis Institutional Review Board.

Results

Biannual firearm violence

The synthetic control was created from a weighted combination of 6 donor pool counties assigned non-zero weights (Appendix C Table 2). Orange County received the largest weight (65.7%); this is an urban, coastal county, adjacent to San Diego. The balance of covariates between San Diego and its synthetic control appears reasonable (Table 4.1), with the largest difference being in the percent non-Hispanic white, which was about 5% higher in San Diego than its synthetic control.

San Diego had lower rates of firearm violence throughout the study period compared to the mean rates in the donor pool (Figure 4.1). Prior to the GVRO law's implementation, the mean biannual rate of firearm violence in San Diego was 5.95 injuries per 100,000 and the corresponding rate in the donor pool was 11.12. San Diego also experienced a marked decline in firearm violence from 2008 to 2010, from around 8 to around 5 injuries per 100,000. The donor pool mean stayed between 10 and 12 injuries per 100,000 from 2005 through 2017, but declined in the last 2 years of the study to just over 8 injuries per 100,000 by the end of 2019.

Results from the synthetic control analysis are visualized in Figure 4.2, which presents the trend of firearm violence in San Diego and in the synthetic control pre- and post-GVRO implementation. The pre-GVRO MSPE suggests a good fit (Appendix C Table 3). The post-GVRO rate of firearm violence was 5.05 per 100,000 in San Diego and 5.11 in synthetic San Diego. Results from the placebo test (Figure 4.3) suggest that this 1% difference is consistent with the null hypothesis of no association, as 12 of 23 in-space placebos with reasonable pre-intervention fits had differences equal to or more negative than San Diego's (Appendix C Figure 1).

Annual firearm assault and self-harm

We present results from the secondary, individual analyses of firearm assault and self-harm by taking each in turn. Seven counties in the donor pool were assigned non-zero weights in the assault analysis, with Sonoma County receiving the largest weight (38.6%; Appendix C Table 2). The annual rate of firearm assault in San Diego and the corresponding mean rate in the donor pool is presented in Figure 4.5. The patterns are similar to what was observed for firearm violence in Figure 4.1, though the rates are shifted upward: the mean pre-GVRO rate of firearm assault was 7.16 per 100,000 in San Diego and 16.64 in the donor pool. Firearm assault dropped in 2008-2010 in San Diego but not in the donor pool, whereas the donor pool but not San Diego experienced a decline beginning in 2018, mirroring the trend in overall firearm violence.

Table 4.2 presents the covariate balance between San Diego and its synthetic control. Two variables are poorly matched: percent non-Hispanic white is about 15% higher in the synthetic control than San Diego and the percent Hispanic is about 7% lower. However, the pre-GVRO model fit is reasonable (Appendix C Table 3), assuaging somewhat concerns about comparability.

Results from the synthetic control analysis are visualized in Figure 4.6A. San Diego and synthetic San Diego appear well matched pre-GVRO implementation. Post-GVRO implementation, the mean rate of firearm assault in San Diego (4.87 per 100,000) was 13% lower than predicted by the synthetic control (5.61 per 100,000; Appendix C Table 3). The placebo test results, displayed in Figures 4.7A and Appendix C Figure 2A, show that several placebo counties had post-GVRO differences more negative than San Diego, suggesting that GVRO implementation was not associated with firearm assault.

Turning to the self-harm analysis, the self-harm synthetic control unit was composed of 8 counties assigned non-zero weights, with Ventura receiving the largest (31.8%; Appendix C Table 2). We found rates to be quite similar between San Diego and the donor pool, with a mean pre-GVRO rate in San Diego of 4.80 per 100,000 and a mean rate in the donor pool of 5.69 per 100,000 (Figure 4.5). The rate was fairly steady in both groups across the study period. Both the covariate balance and the model fit were excellent (Table 4.2; Appendix C Table 3).

Results from the synthetic control analysis are displayed in Figure 4.6B. Post-GVRO implementation, firearm self-harm was 3% higher in San Diego (5.23 per 100,00) than synthetic San Diego (5.10 per 100,000; Appendix C Table 3). Results from the placebo test (Figures 4.7B and Appendix C Figure 2B) make clear that several counties had differences more negative than this, so we cannot reject the null hypothesis that GVRO implementation was not associated with firearm suicide in San Diego.

Sensitivity analyses

Results from the sensitivity analysis using a more restrictive donor pool are presented in Appendix C Table 4. The model fit was worse for firearm violence overall and firearm assault than the models using the primary donor pool, but it remained the same for firearm self-harm. Differences in the outcome were more extreme for all models, with post-GVRO rates of biannual firearm violence and annual firearm assault in San Diego 13% and 16% lower than the synthetic controls, respectively, and rates of firearm self-harm 11% higher in San Diego than the synthetic control. None of these differences were statistically significant.

Results from the sensitivity analysis using 2018 as the year of implementation are presented in Appendix C Table 5. The model fit was slightly better than models using 2016 for firearm violence and firearm assault, but slightly worse for firearm suicide. In these models, the rate difference between San Diego and its synthetic control changed directions from the primary analysis for both overall firearm violence and firearm self-harm. The rate difference for firearm assault was similar to findings from analogous models. Again, none of these differences were significant.

Discussion

Using the synthetic control method to compare post-GVRO implementation rates of firearm violence in San Diego with predicted rates in the absence of implementation, we found no evidence that GVRO implementation was associated with decreased overall firearm violence, firearm assault, or firearm self-harm at the population level in San Diego. These null findings were robust to alternative model specifications. Our results could reflect a true null or limitations of our study; further research is needed to determine which of these is the case.

This was the first study to evaluate the association between risk-based firearm removal laws and firearm violence overall (assault and self-harm together) and firearm assault. We evaluated all types of firearm violence because the mechanism through which we hypothesized the law to impact violence is the same for both assault and self-harm: removing firearms from high-risk individuals should make it more difficult or nearly impossible for respondents to carry out acts of firearm violence. Benefits may also diffuse to other household members, since they also lose access to the respondent's firearms.[119]

Nevertheless, we did not find evidence that GVRO implementation was associated with firearm violence of any kind in San Diego. The null firearm assault and overall firearm violence results may be partially explained by access to the black market. It is possible that respondents in danger of harming others are more likely to know how to access firearms unlawfully, which would undermine the intervention if they acquire illicit firearms after dispossession pursuant to a GVRO. Indeed, previous research found that most prisoners who possessed a firearm at the time of their offense obtained their firearms from the black market, theft, or a friend,[120] methods that would be unaffected by a GVRO.

Unlike firearm assault, firearm suicide has previously been evaluated with regard to risk-based temporary firearm removal laws. Contrary to what we found in San Diego, 2 previous state-level studies found these laws to be associated with a reduction in firearm suicide. Kivisto and Phalen used the synthetic control method to estimate the association between enactment of risk warrant laws in Indiana and Connecticut and firearm suicide rates in the 10 years following.[34] They found a significant 7.5% reduction in Indiana and a nonsignificant 1.6% reduction in Connecticut (though Connecticut's law was significantly associated with a 16% reduction in sensitivity analyses using difference-in-differences). Using these states along with California and Washington, which have GVRO laws, Saadi and colleagues used fixed-effects linear regression models to examine the association between enactment of risk-based firearm removal laws and firearm suicide in older adults.[107] They found these laws to be associated with significant 2.4% and 2.5% reductions in firearm suicide among those aged 55-64 and 65+, respectively. It should be noted that California and Washington only contributed one exposed year each, and for Washington, this was misclassified for all but 3 weeks (the law was enacted in December). Moreover, there were only 71 GVRO respondents statewide in California during the first year of implementation, and presumably many fewer in the first few weeks of Washington's law, so these findings are unlikely to accurately reflect associations for these states.

There are several possible reasons why our findings regarding firearm suicide diverged from these previous studies. First, our study takes place in California, which has more comprehensive firearm legislation than Indiana and Connecticut.[121] This could result in other mechanisms preventing high-risk individuals from possessing firearms in California, lessening the need for GVROs compared with these other states. This hypothesis is supported somewhat by the relatively slow uptake in California compared with other states that have fewer firearm regulations, such as Florida, which experienced rapid uptake of extreme risk protection orders following the law's enactment.[122]

Second, firearm suicide rates are substantially lower in California than in Indiana: in 2019, for example, the rate of firearm suicide per 100,000 was 4.01 in California and 8.47 in Indiana.[1] Moreover, the proportion of suicides completed with a firearm is larger in Indiana (0.59) than California (0.36).[1] This suggests that the need for GVROs for suicide prevention may be higher in Indiana, as the underlying at-risk population seems to be larger (relative to the state population). This is likely due, in part, to different state demographics and firearm access, with Indiana having a greater proportion of older white men (who are at highest risk[2,3]) and higher rates of firearm ownership, which is strongly associated with firearm suicide.[20,111,123]

Finally, it is possible that a population-level association is simply not detectable with such a rare exposure, though GVROs are supposed to be highly targeted to those at greatest risk and even small changes in a rare outcome can affect the population rate. Kivisto and Phalen's findings for Indiana rested on only 404 respondents,[34] just 50 more than the number of respondents in San Diego (n=354). By the numbers, a population-level association also seems feasible. In 2016, for example, there were 154 deaths and injuries from firearm self-harm (4.66 per 100,000) in San Diego. If the previously identified NNT of 10 for firearm suicide[32,33] holds in San Diego and all people who died by suicide were first served a GVRO, we would expect to see 15 fewer suicides, reducing the rate by 10%. Even if half of these victims were served a GVRO, there would be a notable change in the population rate of firearm suicide. Nevertheless, power is determined, in part, by the size of the donor pool, which was relatively small in our case. Perhaps when more GVROs have been issued, there will be a detectable association. In the meantime, there is an urgent need for individual-level effectiveness studies, as there may be strong individual-level benefits that are not being detected at the population level.

It is also, of course, possible that we identified a true null—that GVRO implementation was not associated with firearm violence, assault, or suicide at the county level in California from 2016 to 2019. Perhaps GVROs were not being implemented in the right cases. We previously found that the vast majority (>95%) of GVRO petitioners in California were law enforcement officers,[35] who are less likely to know about suicidal individuals unless alerted by a family member. In fact, we found that only around 15% of cases in the first 3 years of implementation were for danger of self-harm only (unpublished results), which is substantially lower than what has been reported in other states.[32,33,124-127] Further research on how these orders are being implemented in practice—and how they can be more accurately targeted to those at highest risk—is warranted.

Limitations

Like all research, this study was subject to limitations. First, some counties in the donor pool were not completely unexposed to GVRO implementation. We tried to limit the amount of exposure so that included counties were effectively unexposed; that is, we included counties only if the amount of exposure could not plausibly impact firearm violence rates. We believe the benefits of using within-state controls, which allowed us to include nonfatal injuries and to avoid between-state confounding, outweigh the bias that may stem from including control counties with a small amount of exposure. Moreover, to the degree that control counties were exposed, our results should be biased toward the null.

Second, while we tried to create a synthetic control that was exchangeable with San Diego by including predictors and pre-implementation rates of firearm violence in the model, it is possible that the synthetic control did not provide a good estimation of the counterfactual. This could happen if, for example, there were unmeasured differences between counties in the donor pool and San Diego that could compromise comparability. Including pre-implementation outcome variables is intended to adjust for unobserved factors, assuming that similar trends indicates similar values with respect to variables predictive of those trends.[109] Given that our synthetic controls fit the pre-intervention trends reasonably well, there was not likely to be a large degree of unmeasured confounding.

Third, there may be some amount of spillover between counties. This could occur primarily in cases where GVROs were issued for danger of other-directed violence. If the respondent frequently spent time on both sides of a county border (e.g., worked in Orange County and lived in San Diego County), it is possible that both counties would benefit from the respondent being disarmed. Similarly, control counties may benefit from adjacent counties with higher implementation. However, for this kind of spillover to have population-level impacts, many respondents would have to be in this type of situation, which seems unlikely. Again, if anything, this would make control counties more similar to San Diego, biasing toward the null.

Fourth, generalizability of our findings is uncertain. Since San Diego implemented far more GVROs than any other county in the state, it is likely that these null results will generalize to a statewide analysis for California. This might not be the case, however, if GVROs are being used in different circumstances outside of San Diego (perhaps another county is very good at targeting GVROs to those most likely to benefit). Differences between states, such as other firearm laws, make it difficult to know whether results will generalize beyond California. However, in states with a higher demand for this type of intervention (due to fewer other mechanisms preventing high-risk individuals from possessing firearms), results may be stronger.

Conclusions

This study was the first to analyze the association between GVRO implementation and firearm violence in California and was the first to evaluate the association between risk-based firearm removal laws and firearm assault in any state. We did not find evidence for a county-level reduction in firearm violence, firearm assault, or firearm self-harm following implementation of the law in San Diego, which issued the greatest number of orders in the state. Given the relatively small number of GVROs issued during the study period and our small donor pool, these results should be taken as preliminary. Further assessment after additional uptake of the law is warranted, as this study only included 2 post-implementation years in San Diego with substantial GVRO use. Importantly, these results do not preclude the possibility of there being an individual-level benefit to respondents, and previous research indicates that such a benefit exists for those in danger of self-harm.[32,33] Future research should prioritize studies of longer term effects and studies at the individual-level, notwithstanding the difficulty in identifying a comparison group. Despite our null findings, the state of the evidence overall supports GVROs and related legislation as tools that may be useful in preventing firearm injury and death.

Table 4.1: Mean Predictor Values Pre-GVRO Implementation

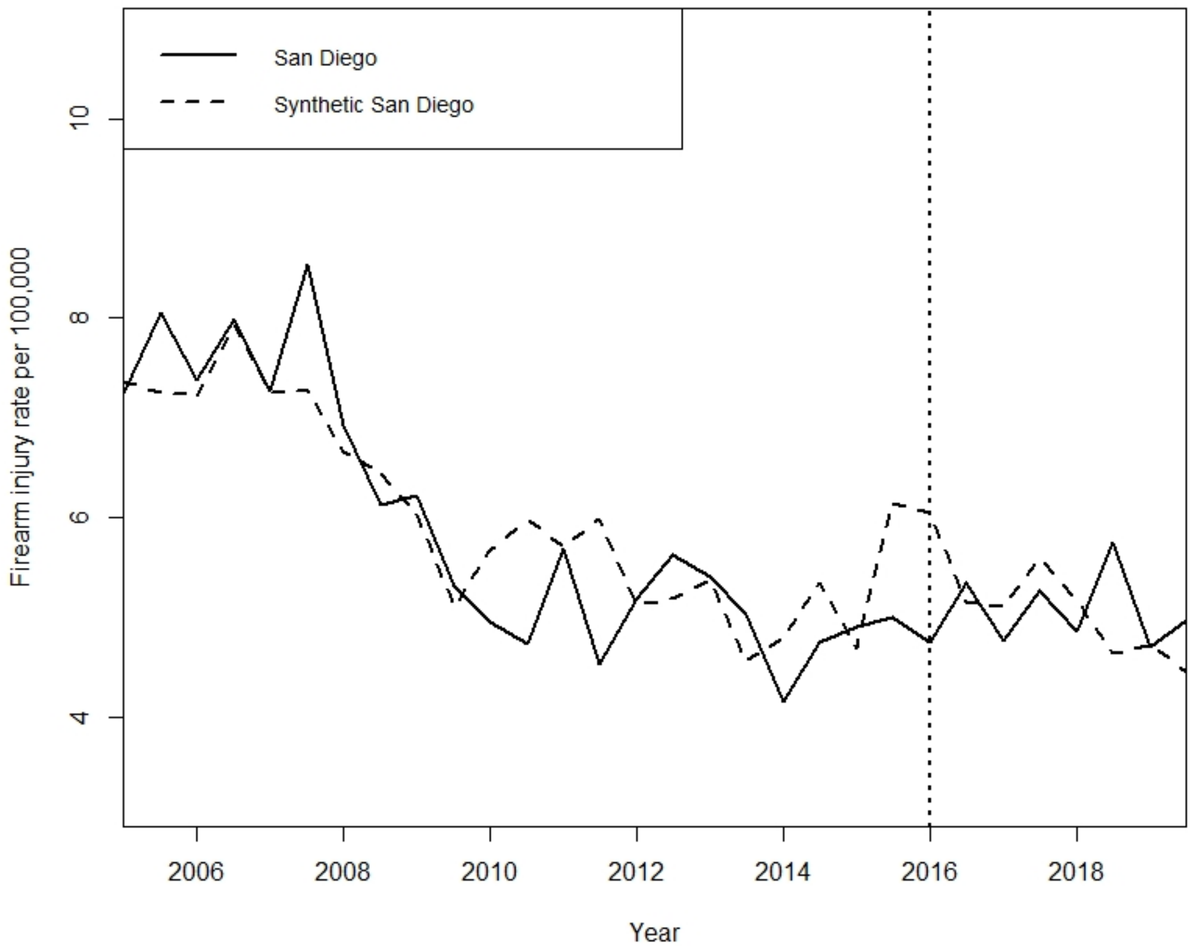
Predictor	San Diego	Synthetic San Diego	Donor Pool Mean
Aged 15-24, %	15.59	14.01	14.84
Aged 55+, %	22.38	23.29	23.71
Non-Hispanic white, %	48.68	43.00	48.41
Non-Hispanic Black, %	4.81	2.82	4.66
Hispanic, %	31.90	33.16	33.56
Male, %	50.21	49.57	49.96
Unemployed, %	8.29	8.09	10.35
Urbanicity (RUCC)[a]	1.00	1.02	1.85
Property crime rate[b]	12.20	12.23	15.16
Non-firearm violent crime rate	1.55	1.17	1.74
Handgun sales rate	2.70	2.18	3.25

a. 2013 Rural-Urban Continuum Code, as assigned by the United States Department of Agriculture. Values range from 1 (most urban) to 9 (most rural).
b. Biannual rates are per 1,000 residents

Figure 4.1: Mean Biannual Rate of Firearm Violence in Donor Pool Counties and in San Diego[a]

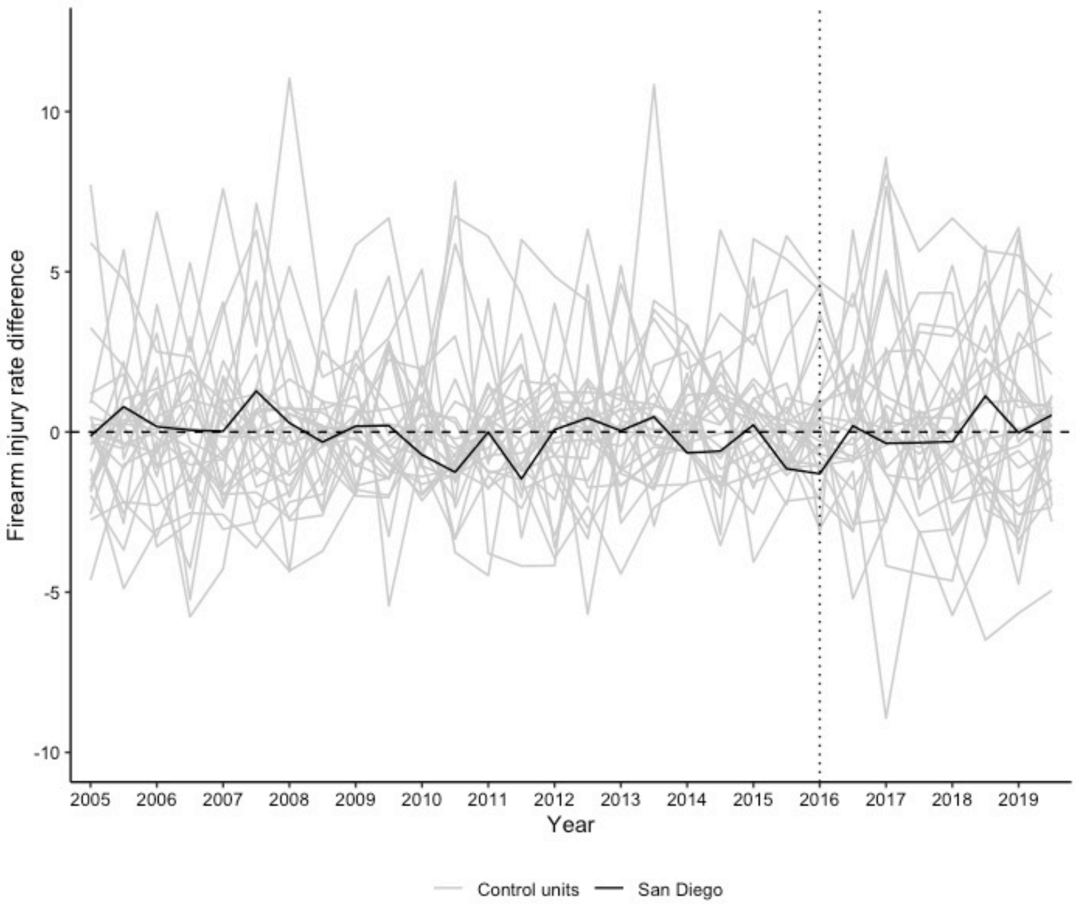
a. The red line denotes the start of the GVRO implementation period.

Figure 4.2: Biannual Firearm Violence Rate in San Diego and Synthetic San Diego[a]



a. The dotted vertical line indicates implementation of the GVRO law.

Figure 4.3: Placebo Test Results for the Association Between GVRO Implementation and the Biannual Firearm Violence Rate in San Diego[a]



a. Each line displays the difference between the observed and predicted rate of firearm violence injury. The vertical dotted line indicates the implementation of the GVRO law.

Figure 4.5: Mean Annual Rate of Firearm Assault and Self-harm in Donor Pool Counties and in San Diego[a]

a. The black line denotes the start of the GVRO implementation period.

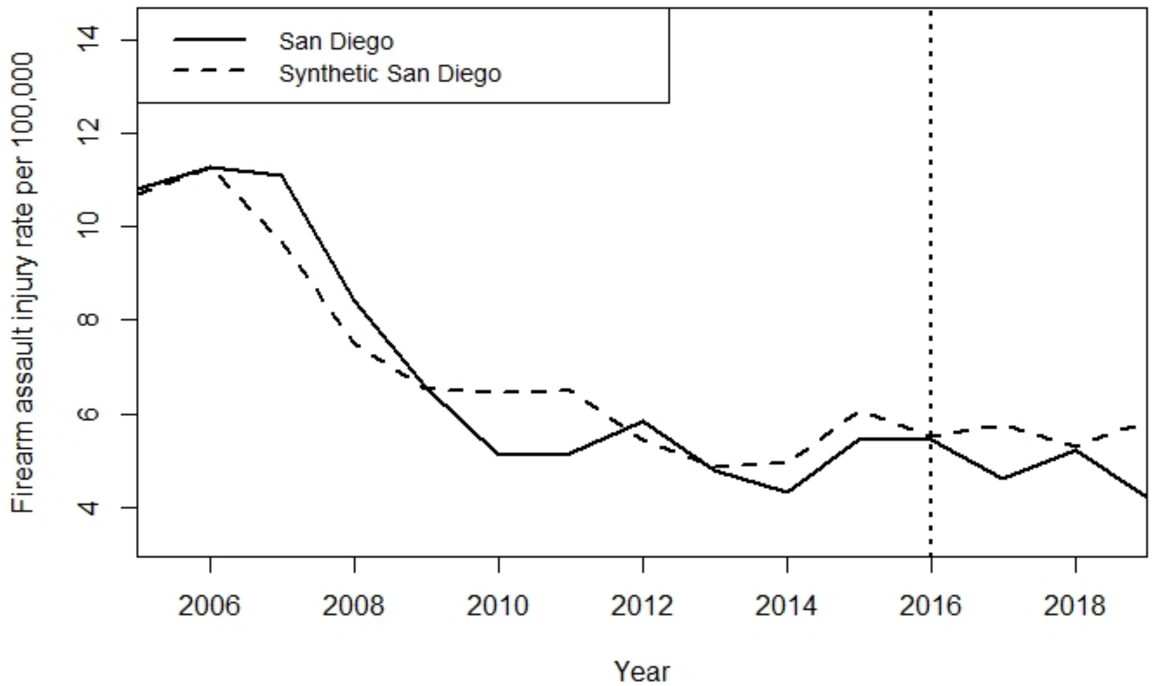
Table 4.2: Mean Predictor Values Pre-GVRO Implementation, Firearm Assault and Self-Harm Models

Predictor	San Diego	Synthetic Control, Assault	Synthetic Control, Self-Harm	Donor Pool Mean
Aged 15-24, %	15.67	14.86	15.05	14.89
Aged 55+, %	22.17	27.76	22.98	23.47
Non-Hispanic white, %	48.92	64.09	46.97	48.67
Non-Hispanic Black, %	4.83	2.62	4.61	4.68
Hispanic, %	31.74	24.67	34.56	33.36
Male, %	50.21	49.82	49.82	49.97
Unemployed, %	8.29	8.48	8.83	10.35
Urbanicity (RUCC)[a]	1.00	1.69	1.47	1.85
Property crime rate[b]	24.51	21.38	25.25	30.44
Non-firearm violent crime rate	3.12	3.08	2.60	3.49
Handgun sales rate	5.43	5.18	5.76	6.52

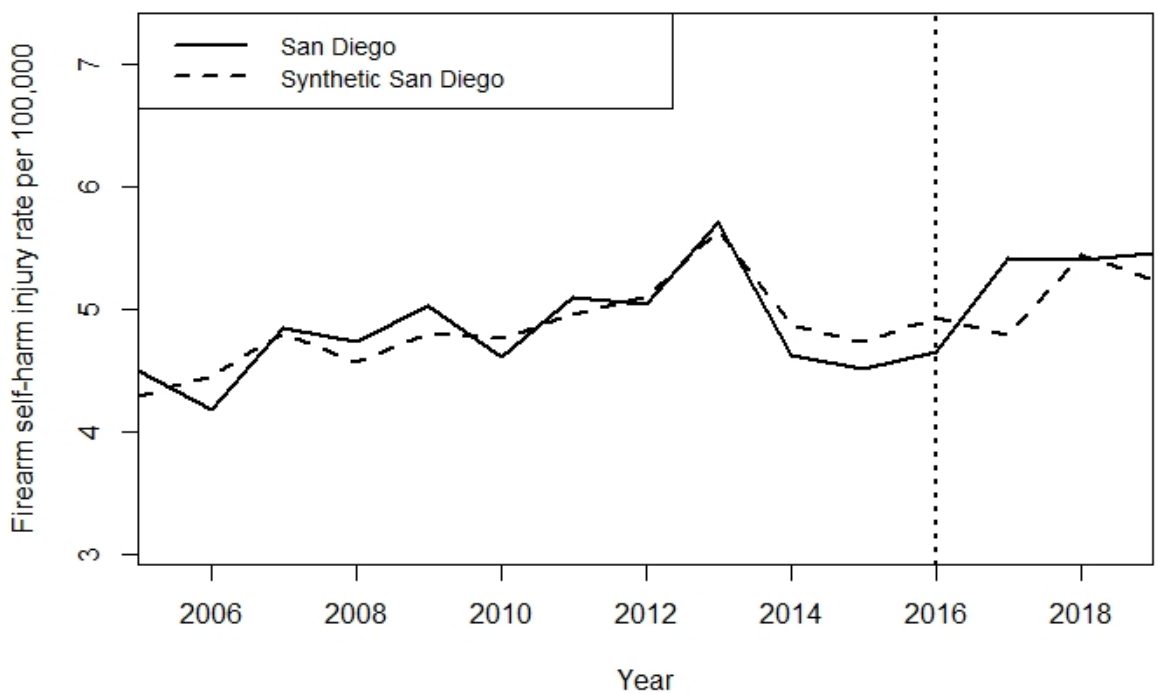
a. 2013 Rural-Urban Continuum Code, as assigned by the United States Department of Agriculture. Values range from 1 (most urban) to 9 (most rural).
b. Annual rates are per 1,000 residents

Figure 4.6: Annual Firearm Assault and Self-Harm Rates in San Diego and Synthetic San Diego[a]

A. Assault



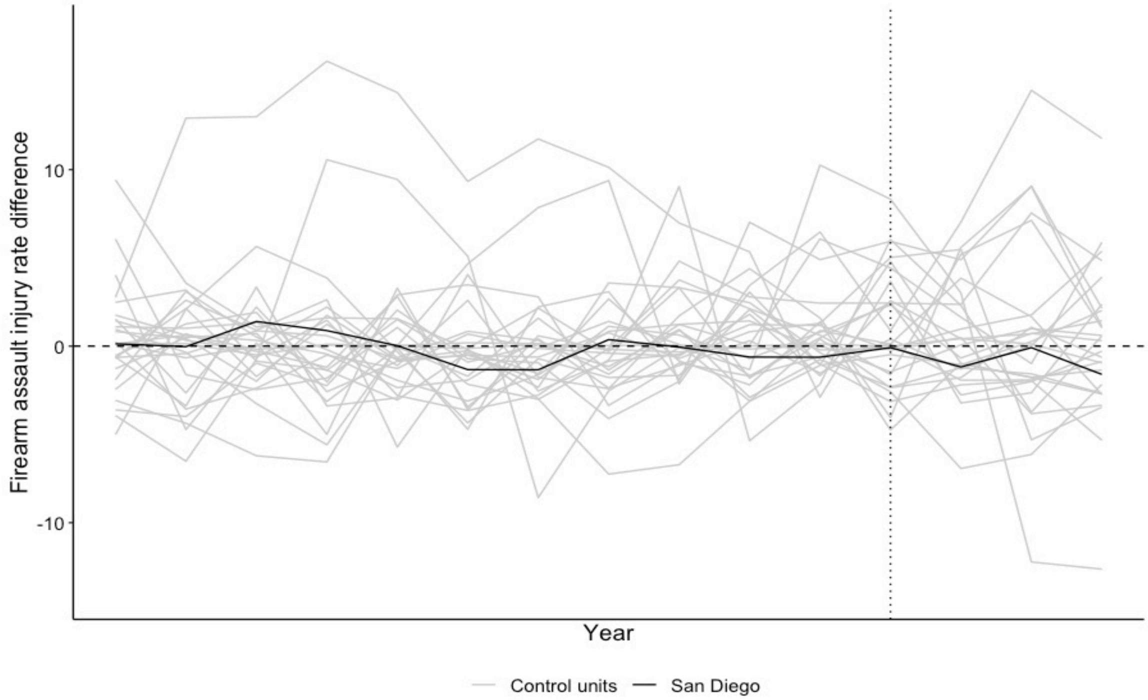
B. Self-Harm



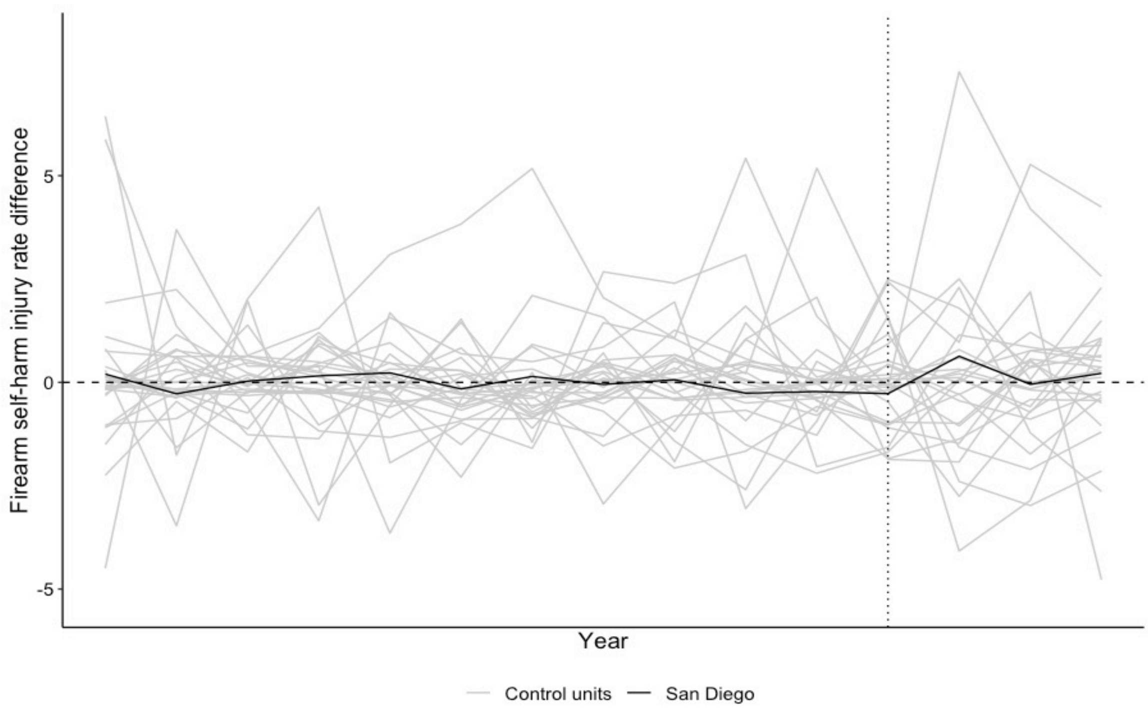
a. The vertical dotted line indicates implementation of the GVRO law.

Figure 4.7: Placebo Test Results for the Association Between GVRO Implementation and Annual Firearm Assault and Self-Harm Rates in San Diego[a]

A. Assault



B. Self-Harm



a. Each line displays the difference between the observed and predicted rate of firearm injury. The vertical dotted line indicates the implementation of the GVRO law.

5. Conclusion

This dissertation sought to identify and evaluate interventions to reduce firearm violence with a particular focus on cross-level impacts of firearm availability (i.e., how community firearm availability impacts individual risk and vice versa). Chapters 2 and 3 examined whether community firearm and alcohol availability were independently or jointly associated with individual-level risk of firearm assault or self-harm, while Chapter 4 explored whether removal of firearms from high-risk individuals (via gun violence restraining orders [GVROs]) was associated with decreased rates of firearm violence in San Diego County.

In Chapter 2, we examined the association between firearm and alcohol accessibility and firearm assault (including homicide). We found non-pawn firearm dealer density and off-premise alcohol outlet density to be modestly associated with firearm assault in the statewide population, with substantially larger risk differences in the subpopulation of young Black men. Low densities of both exposures were together associated with about 263 fewer firearm assault injuries and deaths statewide per year compared to high densities. Pawn shop and bar/pub density were not associated with firearm assault, and we found no evidence of additive interaction between measures of firearm and alcohol availability. Measuring firearm availability with sales density rather than dealer density resulted in similar findings for the statewide population, but it reduced the magnitude of the risk difference for young Black men, suggesting that firearm dealers are introducing risk beyond merely providing firearms to the community for this high-risk subpopulation.

In Chapter 3, we examined the association between firearm and alcohol accessibility and firearm self-harm (including suicide). Here we found that measuring firearm availability with sales density resulted in much stronger measures of association than measuring it with firearm dealer density. Low firearm sales density was associated with about 221 fewer firearm self-harm injuries and deaths statewide per year compared with high density. Firearm dealers were unrelated to firearm self-harm in the general population and were only modestly associated with self-harm in older white men who are at greatest risk. Bar/pub density was associated with a very slight increase in risk of firearm self-harm (not of a meaningful magnitude) and off-premise alcohol outlet density had a null association. Again, we did not find evidence of additive interaction between measures of firearm and alcohol availability. As we found with firearm assault, the measures of association for firearm self-harm were much greater in the high-risk subset than the overall state population.

Chapter 4 evaluated the association between GVRO implementation and the rate of firearm violence in San Diego County, which issued a plurality of orders in California. We found no association between the law's implementation and overall firearm violence (fatal and nonfatal self-harm and assault). The relationship was also null when we evaluated firearm assault and firearm self-harm individually. These results were robust to different model specifications, including changing the implementation year to 2018, when implementation rapidly increased in San Diego County.

Taken together, our findings suggest weak cross-level associations between firearm availability and firearm violence. The magnitude of our findings regarding firearm and alcohol availability, where significant, were modest, perhaps suggesting that intervention efforts would be better spent elsewhere. However, we examined only the range of firearm availability observed in the data; interventions reducing firearm availability below values observed during the study period could not be assessed without introducing positivity violations, but these would theoretically have the potential for a greater impact on firearm violence. In addition, firearm violence prevention requires a multitude of approaches, and many interventions that individually have small associations with risk can, together, have a meaningful impact on the number of lives affected by firearm violence. There may, furthermore, be other benefits of limiting alcohol outlet density, such as decreased violence, high-risk drinking behaviors, and traffic accidents.[103,128]

Our null findings with regard to GVRO implementation should not be interpreted as evidence that the law is not working to prevent firearm violence. We were explicitly evaluating the association with county-level rates of firearm violence, which is only one (rather stringent) measure of success. Individual-level studies of GVRO-type laws found the impact on those affected by the intervention to be substantial, with 1 suicide prevented for every 10-20 gun removal cases.[32,33] This suggests that the law may be working on the individual level but, given the rarity of the outcome, it is not yet detectable at the population level.

This dissertation adds to the limited literature on cross-level risks for firearm violence and on the effectiveness of GVROs. We improved upon prior studies of community firearm availability by using a more accurate measure of active firearm dealers, drawing on Dealer Record of Sales (DROS) data rather than relying on Federal Firearms Licenses (FFLs), and by evaluating sales density as an alternative measure of availability (which is not possible with FFL data). We also used both fatal and nonfatal outcomes in all analyses, as there is not likely to be an etiologic difference between fatal and nonfatal gunshot wounds; whether such an injury is fatal is often merely a matter of aim and timely access to care. Moreover, because it is a rare outcome, including nonfatal cases has the additional statistical benefit of increasing power. In addition to including nonfatal cases, we substantially contributed to the GVRO literature by evaluating both self-harm and assault, which had not previously been done. Ours is also the first study to robustly evaluate the association between GVROs—rather than risk warrants—and firearm violence. As more states adopt these laws and the federal government considers a nationwide policy, this work is particularly important.

This dissertation is also subject to limitations, limiting our ability to ascribe causation to our findings. While we chose methods intended to minimize bias and used comprehensive data sources for both the exposures and outcomes, some bias likely remained. For example, there were several individual-level confounders that we could not control for in the analyses of community-level firearm and alcohol availability, such as education, income, and mental health and substance use disorders. This is because we selected controls from the statewide population using ACS data, leaving us with only individual age, sex, and race/ethnicity. However, drawing from the primary study base reduces the probability of selection bias,[52] which we weighed against the loss of individual confounders that we could have had by using hospital-based controls.

Confounding was also possible in the GVRO analysis. Like many other policy analyses, we were challenged by the presence of co-occurring policy changes.[129] We could not disentangle the impact of GVROs from other policies if they 1) were implemented at the same time as the GVRO law, 2) were plausibly related to changes in firearm violence, and 3) impacted firearm violence differently (possibly due to different implementation) in San Diego than in the control counties. Laws meeting the first two conditions include statewide medical marijuana regulations, a ban on carrying concealed weapons on school property, and measures making it easier for some prisoners to be released on parole.[130] As long as these did not differentially impact firearm violence in San Diego—which is unknown—they should not confound our measures of association.

We also may have had a small amount of information bias due to misclassification, though we do not expect that any of it was differential. For example, long gun sales have only been recorded in DROS since 2014, so earlier years of data underestimate the density of sales and, to a lesser degree, active dealers. Some pawn shops were likely misclassified as non-pawn shops as well. Because we did not have the FFL number, we could not distinguish between those that did and did not have a pawn license, thus we had to rely on a less sensitive approach to identify pawn shops (i.e., using clues from the business name and email address as well as pawn redemption transactions). We also may have had a degree of misclassification of firearm self-harm and assault. It can be challenging to identify whether cases of fatal gunshot wounds were intentional or not, and as a result, some self-harm cases may have been misclassified as unintentional firearm deaths. Further, those suffering from a firearm assault may be reluctant to report the injury as assaultive if they do not want the police to be involved, which would also result in misclassification. We expect such errors to be rare.

Along with these biases, we faced additional challenges to causal inference. For example, while we were able to ensure temporal ordering of our exposures and outcomes, we may not have modeled the most appropriate lag between the two. The exposures in chapters 2 and 3 were measured with 12-month moving averages, such that each person's exposure in the year preceding the outcome (or selection into the study) was captured. In Chapter 4, we evaluated firearm violence in the first 4 years following implementation of the GVRO law, but only in 2 of those years was implementation robust enough to plausibly affect firearm violence rates. In each analysis, it is possible that the impact on firearm violence was slower than what we captured in our models.

In addition, there could be a feedback loop in the relationship between firearm availability and firearm violence, such that the former increases the latter, which, in turn, increases firearm sales (resulting in an increased supply). While individuals commonly report purchasing firearms for protection,[86,87] and short-lived spikes in purchasing have been found to follow high-profile mass shootings,[131] it is less clear whether local firearm sales increase in response to local increases in violence. Future research could further explore this complex relationship using statistical approaches appropriate for the investigation of dynamic systems, such as agent-based models or loop analyses.[132,133]

The findings from this dissertation suggest several directions for future research. First, it would be instructive to identify what role non-pawn firearm dealers are playing in firearm assault among young Black men beyond providing firearms to the community through sales. This could be explored with mediation analyses and survey or interview data of nonfatal cases or individuals convicted of perpetrating firearm assault. Mediation analyses would be challenging, given the task of linking large, longitudinal, administrative datasets on individual-level firearm purchasing and firearm injuries, but such work has been done successfully with mortality data in the past.[20] To the extent possible, it would also be extremely valuable to extend the scope of this work in order to elucidate the relationship between the availability of firearms on the black market, including firearms manufactured at home, and firearm violence.

Second, researchers should explore strategies to partner with firearm dealers for suicide prevention, such as by temporarily holding firearms for community members in crisis, providing educational material about safe storage and local suicide prevention resources, or by training to identify warning signs of suicidality in their clients and refusing to sell firearms or ammunition when appropriate. Among firearm owners, firearm dealers are trusted in matters of firearm safety and they may, therefore, be better positioned than researchers or medical professionals to reach at-risk firearm purchasers.

Finally, individual-level effectiveness studies of GVROs that include assaultive outcomes are badly needed. The primary challenge with these studies is identification of an appropriate comparison group. This would, ideally, comprise individuals who are temporarily at high-risk of harming themselves or others with a firearm, are not prohibited from firearm ownership, and would have been issued a GVRO had such orders been available. We are embarking upon this work now, using a combination of police incident report data and electronic medical record data to identify would-be GVRO cases prior to the law's implementation.

There is much to be learned about the causes and prevention of firearm violence, including the role of firearm availability, which we explored here. We cannot claim to have identified causal associations in our analyses, but our findings suggest that individuals exposed to certain measures of community-level firearm accessibility are at modestly elevated risk of firearm violence victimization and that GVRO implementation has not reduced firearm violence rates. We have extended prior research in this area both in the content of our questions and the quality of our data, opening new avenues of research in the process. These results will be of interest to local policymakers, violence prevention researchers, and public health practitioners seeking new strategies to reduce firearm violence. This is a complex public health problem that has been historically underfunded, resulting in a dearth of research on this important topic; successful prevention strategies hinge on applying rigorous methods and using high-quality data, as we have tried to do here.

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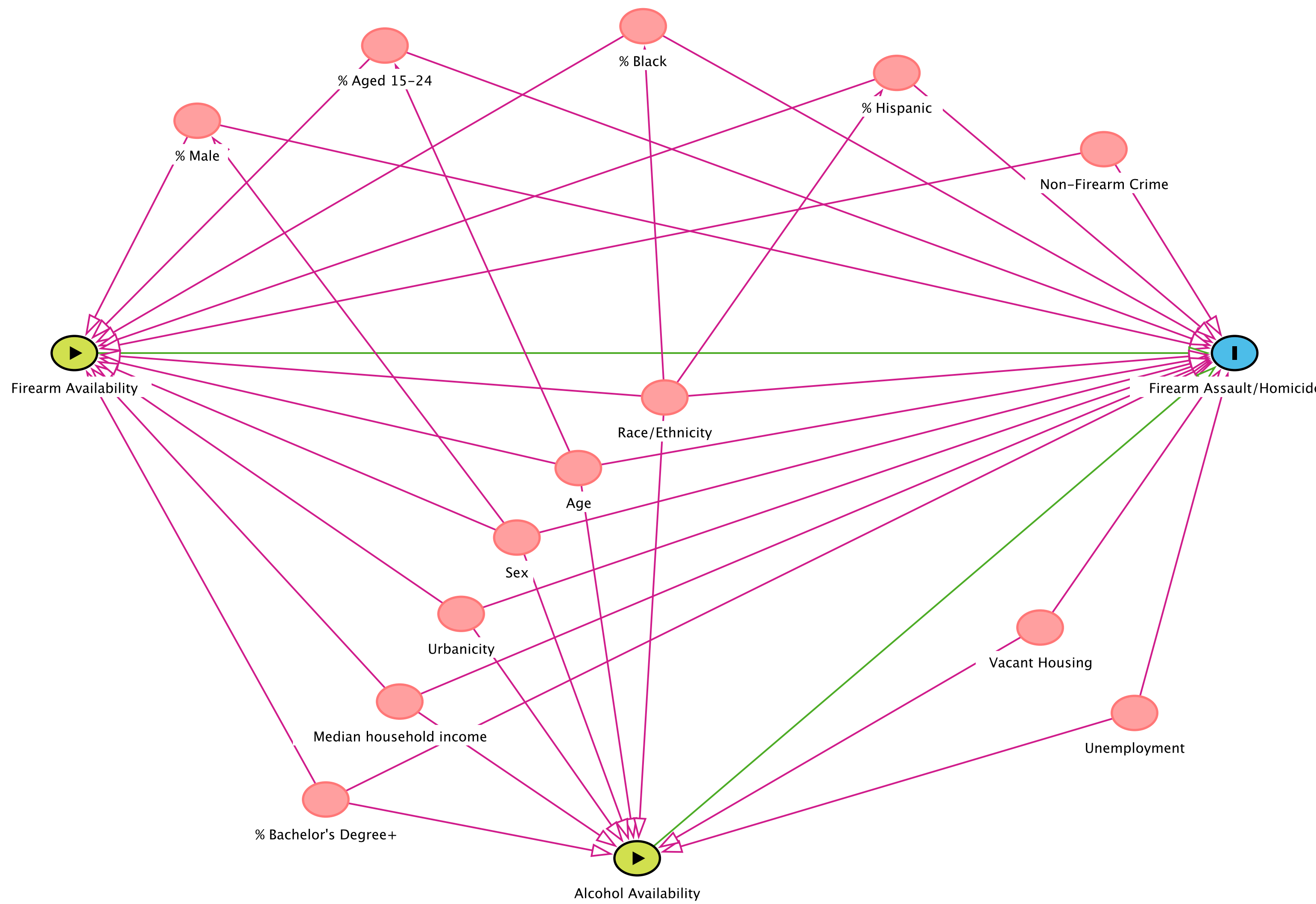
Appendix A
Appendix to Chapter 2

Appendix A Table 1: ICD-9 and ICD-10 Codes Used to Identify Firearm Assault

Description	ICD-9 Code	ICD-10 Code
Homicide & assault by:		
Handgun	E965.0	X93
Shotgun	E965.1	X94
Hunting rifle	E965.2	X94
Military firearms	E965.3	X94
Other and unspecified firearm	E965.4	X95

Appendix A Figure 1: Directed Acyclic Graph



Appendix A Table 2: Range of Firearm and Alcohol Availability Measures[a]

Exposure	Mean Highest Density	Mean Lowest Density	Mean Difference
County-level measures			
Non-pawn dealers	4.45	1.21	3.24
Pawn dealers	3.54	1.56	1.99
Firearm sales	151.51	21.14	130.37
ZCTA-level measures			
Off-premise outlets	117.89	93.42	25.56
Bars/Pubs	25.54	14.66	9.71

a. All densities are measured per 100,000 population.

Appendix A Table 3: Demographic Characteristics of Controls and Residents of California

	Assault Controls[a]	California State, 2010
Total	268,122	37,253,956
Sex, N (%)		
Male	132,310 (49.35)	18,517,830 (49.71)
Race/ethnicity, N (%)		
Non-Hispanic white	107,336 (40.03)	14,956,253 (40.15)
Non-Hispanic Black	15,890 (5.93)	2,163,804 (5.81)
Hispanic	101,251 (37.76)	14,013,719 (37.62)
Other	43,645 (16.28)	6,120,180 (16.43)
Age Group, N (%)		
0-9	35,965 (13.41)	5,037,172 (13.52)
10-19	38,729 (14.44)	5,414,870 (14.54)
20-29	39,978 (14.91)	5,510,358 (14.79)
30-39	37,648 (14.04)	5,147,047 (13.82)
40-49	38,104 (14.21)	5,298,950 (14.22)
50-59	33,683 (12.56)	4,766,848 (12.80)
60-69	22,370 (8.34)	3,135,755 (8.42)
70-79	12,923 (4.82)	1,738,749 (4.67)
80+	8,722 (3.25)	1,204,207 (3.23)

a. 2,835 individuals have multiple assaultive firearm injuries. Controls are those used in analysis, after making exclusions due to missingness and extreme outlying values.

Appendix A Table 4: Adjusted Relative Risks of Firearm Assault

Measure of Firearm Availability	Model by Exposure(s) Adjusted[a]	RR Overall (95% CI)	RR Targeted (95% CI)
Firearm Dealers	Dealers & Alcohol	1.04 (1.01, 1.07)	1.01 (0.99, 1.04)
	Non-Pawn	1.03 (1.00, 1.06)	1.01 (1.00, 1.03)
	Pawn	1.00 (0.98, 1.01)	1.00 (0.97, 1.01)
	Off-Premise	1.02 (1.01, 1.03)	1.01 (1.00, 1.02)
	Bars/Pubs	1.00 (0.99, 1.01)	1.00 (1.00, 1.00)
Firearm Sales	Sales & Alcohol	1.03 (1.01, 1.07)	1.02 (1.01, 1.02)
	Sales	1.02 (1.00, 1.06)	1.02 (1.01, 1.02)
	Off-Premise	1.01 (1.00, 1.01)	1.00 (1.00, 1.00)
	Bars/Pubs	1.00 (0.99, 1.01)	1.00 (1.00, 1.00)

a. "Dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Appendix A Table 5: Adjusted Relative Risks of Firearm Assault Among Black Boys and Men Aged 15-39

Measure of Firearm Availability	Model by Exposure(s) Adjusted[a]	RR Overall (95% CI)	RR Targeted (95% CI)
Firearm Dealers	Dealers & Alcohol	1.04 (1.01, 1.06)	1.01 (0.99, 1.03)
	Non-Pawn	1.03 (1.01, 1.05)	1.01 (1.00, 1.02)
	Pawn	0.99 (0.98, 1.01)	1.00 (0.98, 1.01)
	Off-Premise	1.02 (1.01, 1.03)	1.00 (1.00, 1.01)
	Bars/Pubs	0.99 (0.98, 1.00)	1.00 (1.00, 1.00)
Firearm Sales	Sales & Alcohol	1.03 (1.01, 1.06)	1.02 (1.01, 1.02)
	Sales	1.02 (1.00, 1.04)	1.02 (1.01, 1.02)
	Off-Premise	1.01 (1.00, 1.01)	1.00 (1.00, 1.00)
	Bars/Pubs	1.00 (0.99, 1.01)	1.00 (1.00, 1.00)

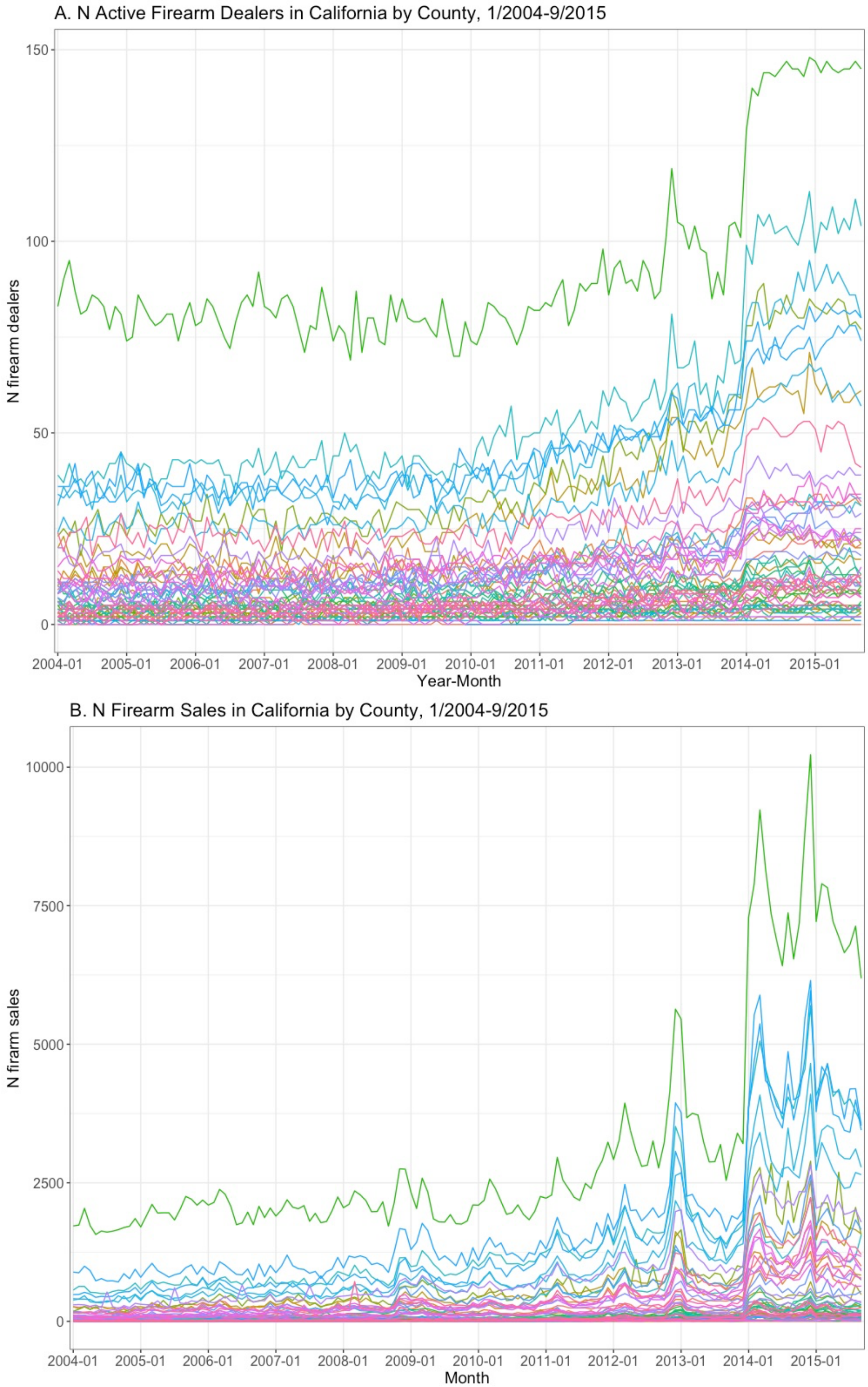
a. "Dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Appendix A Table 6: Sensitivity Analysis—Adjusted Relative Risks of Firearm Assault, Excluding Long Guns

Measure of Firearm Availability	Model by Exposure(s) Adjusted[a]	RD Overall (95% CI)	RR Overall (95% CI)
Firearm Dealers	Dealers & Alcohol	0.03 (0.01, 0.07)	1.02 (1.00, 1.05)
	Non-Pawn	0.02 (-0.01, 0.04)	1.01 (0.99, 1.03)
	Pawn	0.00 (-0.03, 0.02)	1.00 (0.98, 1.01)
	Off-Premise	0.02 (0.01, 0.04)	1.01 (1.01, 1.02)
	Bars/Pubs	0.00 (-0.02, 0.02)	1.00 (0.99, 1.01)
Firearm Sales	Sales & Alcohol	0.04 (0.02, 0.06)	1.03 (1.01, 1.04)
	Sales	0.01 (0.00, 0.03)	1.01 (1.00, 1.02)
	Off-Premise	0.01 (0.01, 0.02)	1.01 (1.00, 1.01)
	Bars/Pubs	0.01 (-0.01, 0.02)	1.00 (1.00, 1.02)

a. "Dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Appendix A Figure 2: Active Firearm Dealers and Sales by County[a]



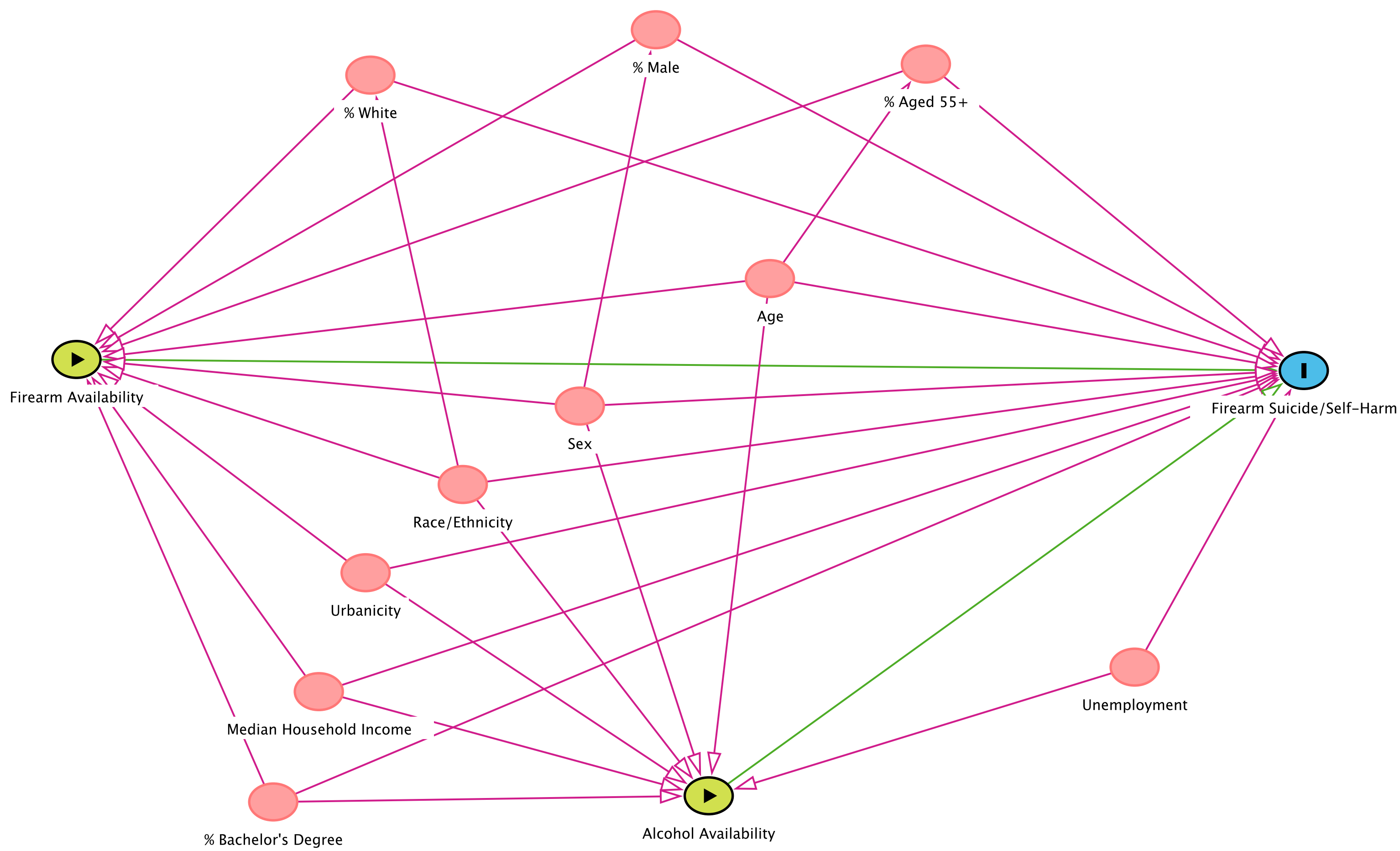
a. Each line is a county

Appendix B
Appendix to Chapter 3

Appendix B Table 1: ICD-9 and ICD-10 Codes Used to Identify Firearm Self-Harm

Description	ICD-9 Code	ICD-10 Code
Suicide & self-inflicted injury by:		
Handgun	E955.0	X72
Shotgun	E955.1	X73
Hunting rifle	E955.2	X73
Military firearms	E955.3	X73
Other and unspecified firearm	E955.4	X74

Appendix B Figure 1: Directed Acyclic Graph



Appendix B Table 2: Range of Firearm and Alcohol Availability Measures[a]

Exposure	Mean Highest Density	Mean Lowest Density	Mean Difference
County-level measures			
Non-pawn dealers	4.95	1.59	3.36
Pawn dealers	3.59	1.61	1.98
Firearm sales	164.33	21.55	142.78
ZCTA-level measures			
Off-premise outlets	123.24	102.69	22.58
Bars/Pubs	29.61	19.32	9.10

a. All densities are measured per 100,000 population.

Appendix B Table 3: Demographic Characteristics of Controls and Residents of California

	Self-Harm Controls[a]	**California State > 9 Yrs, 2010**
Total	67,708	32,216,784
Sex, N (%)		
Male	33,389 (49.31)	15,944,211 (49.49)
Race/Ethnicity, N (%)		
Non-Hispanic white	28,535 (42.14)	13,694,942 (42.51)
Non-Hispanic Black	4,005 (5.92)	1,919,614 (5.96)
Hispanic	23,944 (35.36)	11,370,655 (35.29)
Other	11,224 (16.58)	5,231,573 (16.24)
Age Group, N (%)		
10-19	11,079 (16.36)	5,414,870 (16.81)
20-29	11,480 (16.96)	5,510,358 (17.10)
30-39	10,951 (16.17)	5,147,047 (15.98)
40-49	11,046 (16.31)	5,298,950 (16.45)
50-59	10,081 (14.89)	4,766,848 (14.80)
60-69	6,681 (9.87)	3,135,755 (9.73)
70-79	3,847 (5.68)	1,738,749 (5.40)
80+	2,543 (3.76)	1,204,207 (3.74)

a. 70 individuals have multiple self-directed firearm injuries. Controls are those used in analysis, after making exclusions due to missingness and extreme outlying values.

Appendix B Table 4: Adjusted Relative Risks of Firearm Self-Harm

Measure of Firearm Availability	Model by Exposure(s) Adjusted[a]	RR Overall (95% CI)	RR PAR (95% CI)
Firearm Dealers	Dealers & Alcohol	1.04 (1.02, 1.08)	1.01 (0.99, 1.03)
	Non-Pawn	1.03 (1.00, 1.07)	1.01 (0.99, 1.03)
	Pawn	1.00 (0.99, 1.02)	1.00 (0.99, 1.00)
	Off-Premise	0.99 (0.98, 1.00)	1.00 (0.99, 1.00)
	Bars/Pubs	1.01 (1.00, 1.02)	1.01 (1.00, 1.01)
Firearm Sales	Sales & Alcohol	1.15 (1.08, 1.23)	1.04 (1.02, 1.06)
	Sales	1.14 (1.07, 1.23)	1.03 (1.02, 1.05)
	Off-Premise	1.00 (0.99, 1.00)	1.00 (1.00, 1.00)
	Bars/Pubs	1.01 (1.00, 1.02)	1.00 (1.00, 1.01)

a. “Dealers” includes both non-pawn and pawn dealers. “Alcohol” includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Appendix B Table 5: Adjusted Relative Risks of Firearm Self-Harm Among White Men Aged 50+

Measure of Firearm Availability	Model by Exposure(s) Adjusted[a]	RR Overall (95% CI)	RR PAR (95% CI)
Firearm Dealers	Dealers & Alcohol	1.06 (1.03, 1.11)	1.02 (1.00, 1.04)
	Non-Pawn	1.04 (1.01, 1.09)	1.01 (0.99, 1.03)
	Pawn	1.00 (0.99, 1.02)	1.00 (0.99, 1.01)
	Off-Premise	0.99 (0.98, 1.00)	1.00 (0.99, 1.00)
	Bars/Pubs	1.01 (1.00, 1.02)	1.01 (1.00, 1.01)
Firearm Sales	Sales & Alcohol	1.15 (1.08, 1.22)	1.04 (1.02, 1.06)
	Sales	1.14 (1.07, 1.22)	1.04 (1.02, 1.05)
	Off-Premise	1.00 (0.99, 1.00)	1.00 (1.00, 1.00)
	Bars/Pubs	1.01 (1.00, 1.02)	1.00 (1.00, 1.01)

a. "Dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Appendix B Table 6: Sensitivity Analysis—Adjusted Relative Risks of Firearm Self-Harm, Excluding Long Guns

Measure of Firearm Availability	Model by Exposure(s) Adjusted[a]	RD Overall (95% CI)	RR Overall (95% CI)
Firearm Dealers	Dealers & Alcohol	0.01 (0.00, 0.02)	1.03 (1.00, 1.06)
	Non-Pawn	0.00 (-0.01, 0.01)	1.01 (0.98, 1.04)
	Pawn	0.00 (0.00, 0.01)	1.01 (1.00, 1.02)
	Off-Premise	0.00 (-0.01, 0.00)	1.00 (0.99, 1.00)
	Bars/Pubs	0.00 (0.00, 0.01)	1.01 (1.00, 1.02)
Firearm Sales	Sales & Alcohol	0.04 (0.02, 0.05)	1.09 (1.05, 1.14)
	Sales	0.03 (0.02, 0.05)	1.09 (1.04, 1.13)
	Off-Premise	0.00 (0.00, 0.00)	1.00 (0.99, 1.00)
	Bars/Pubs	0.00 (0.00, 0.01)	1.01 (1.00, 1.02)

a. "Dealers" includes both non-pawn and pawn dealers. "Alcohol" includes both off-premise outlets and bars/pubs. Non-pawn dealers, pawn dealers, and firearm sales density were measured per 100,000 population at the county level. Off-premise outlets and bars/pubs were measured per 100,000 population at the ZCTA level.

Appendix C
Appendix to Chapter 4

Appendix C Table 1: ICD-9 and ICD-10 Codes Used to Identify Firearm Violence

	Injury Data		Mortality Data
Description	**ICD-9 Code (1/2005-9/2015)**	**ICD-10 Code (10/2015-12/2019)**	**ICD-10 Code**
Homicide & assault by:			
Handgun	E965.0	X93	X93
Shotgun	E965.1	X94	X94
Hunting rifle	E965.2	X94	X94
Military firearms	E965.3	X94	X94
Other and unspecified firearm	E965.4	X95.8, X95.9	X95
Suicide & self-inflicted injury by:			
Handgun	E955.0	X72	X72
Shotgun	E955.1	X73	X73
Hunting rifle	E955.2	X73	X73
Military firearms	E955.3	X73	X73
Other and unspecified firearm	E955.4	X74	X74

Appendix C Table 2: Donor Pool Weights by Model

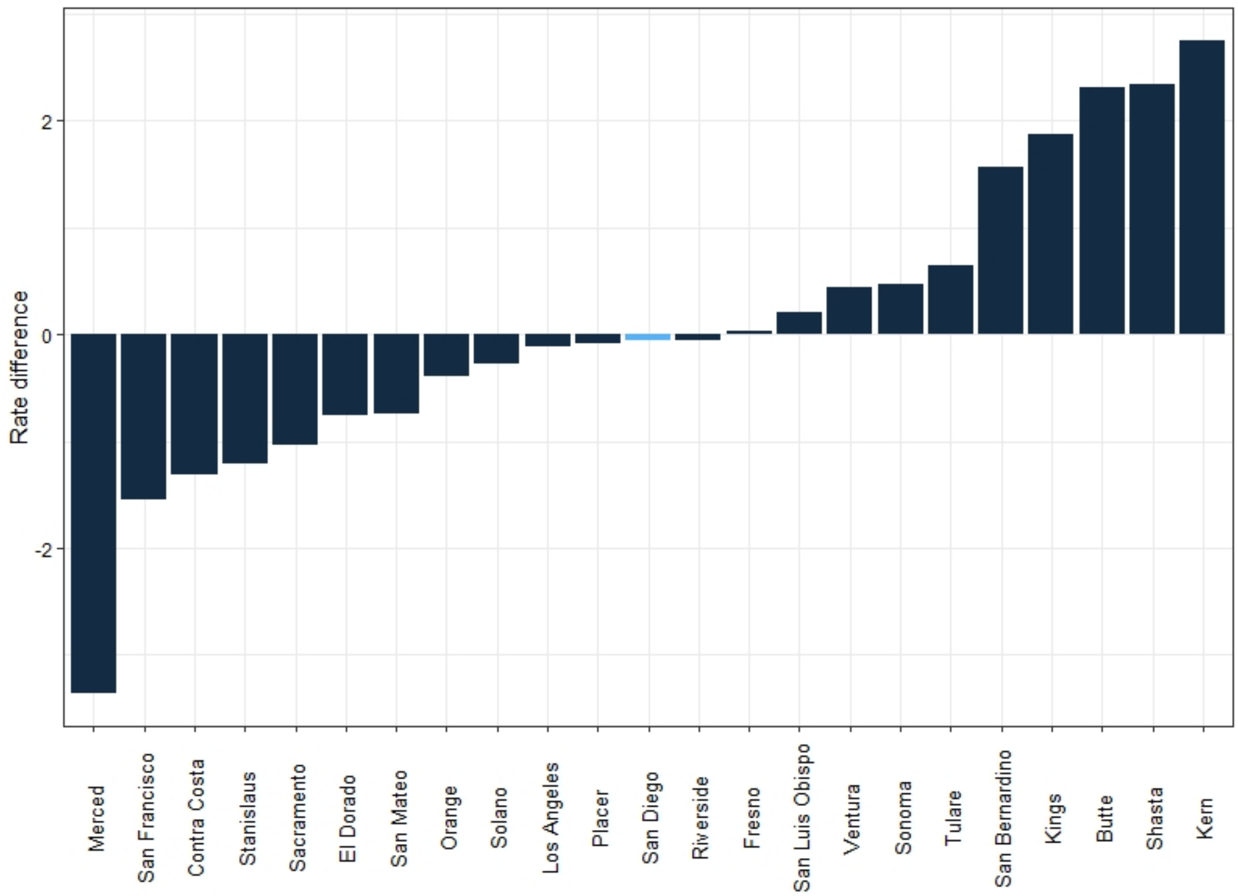
	Model Outcome		
Donor County	**Biannual Firearm Violence**	**Annual Firearm Assault**	**Annual Firearm Self-Harm**
Orange	0.657	0	0.160
Riverside	0.106	0	0
San Mateo	0.089	0	0
San Francisco	0.074	0.008	0
Los Angeles	0.059	0.162	0
San Luis Obispo	0.015	0.259	0.100
Sonoma	0	0.386	0
El Dorado	0	0.163	0
Shasta	0	0.021	0
Placer	0	0.001	0.061
Ventura	0	0	0.318
San Bernardino	0	0	0.186
Alameda	0	0	0.148
Kings	0	0	0.026
Sacramento	0	0	0.002
Butte	0	0	0
Contra Costa	0	0	0
Fresno	0	0	0
Humboldt	0	0	0
Kern	0	0	0
Madera	0	0	0
Merced	0	0	0
Monterey	0	0	0
San Joaquin	0	0	0
Solano	0	0	0
Stanislaus	0	0	0
Tulare	0	0	0

Appendix C Table 3: Synthetic Control Results, Primary & Secondary Analyses

		Outcome		
		Biannual Firearm Violence	**Annual Firearm Assault**	**Annual Firearm Self-Harm**
San Diego	**Rate in post-intervention period (per 100,000)**	5.05	4.87	5.23
Synthetic San Diego	**Rate in post-intervention period (per 100,000)**	5.11	5.61	5.10
Rate difference		-0.06	-0.74	0.13
Percent difference		-1%	-13%	+3%
Pseudo P-value[a]		12/23=0.52	8/23 = 0.35	10/15=0.67
Model fit (MSPE pre-GVRO)		0.42	0.66	0.03

a. The proportion of counties (donor pool plus San Diego) with a difference between the observed rate and the synthetic control rate in the post-treatment period equal to or more negative than San Diego's. Counties with a poor pre-period fit (MSPE >20* San Diego's MSPE) were exclude from the denominator.

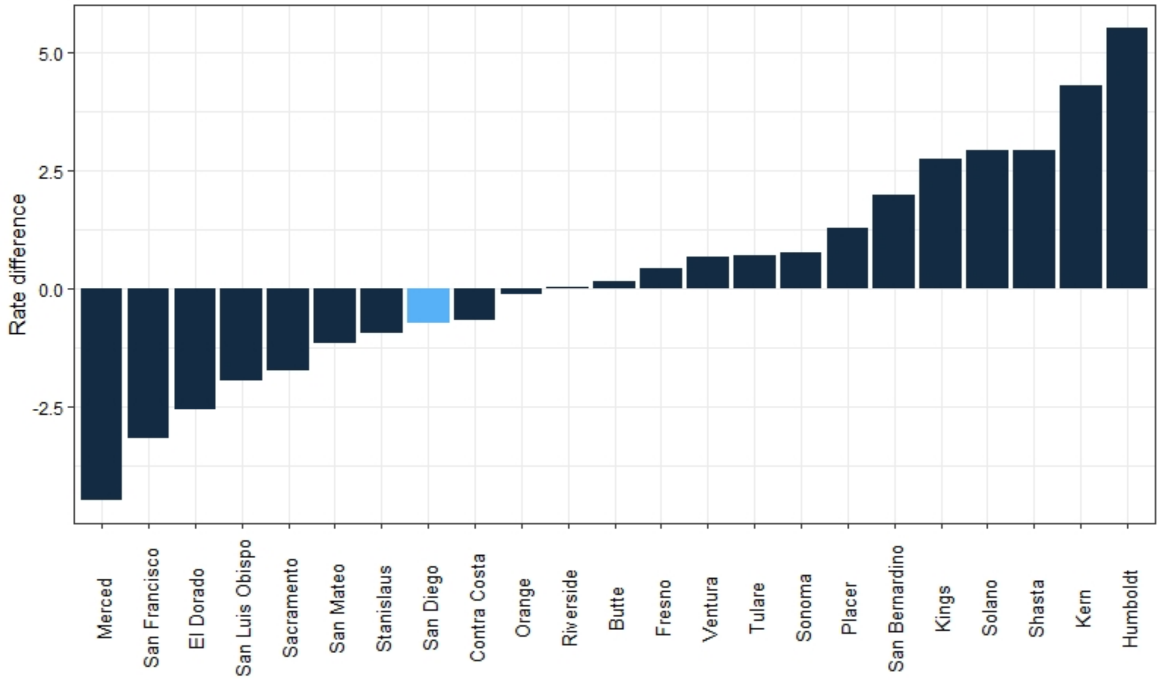
Appendix C Figure 1: Ranking of Post-GVRO Difference in Firearm Violence Between Treated Unit and Synthetic Control, San Diego and Placebo Counties[a]



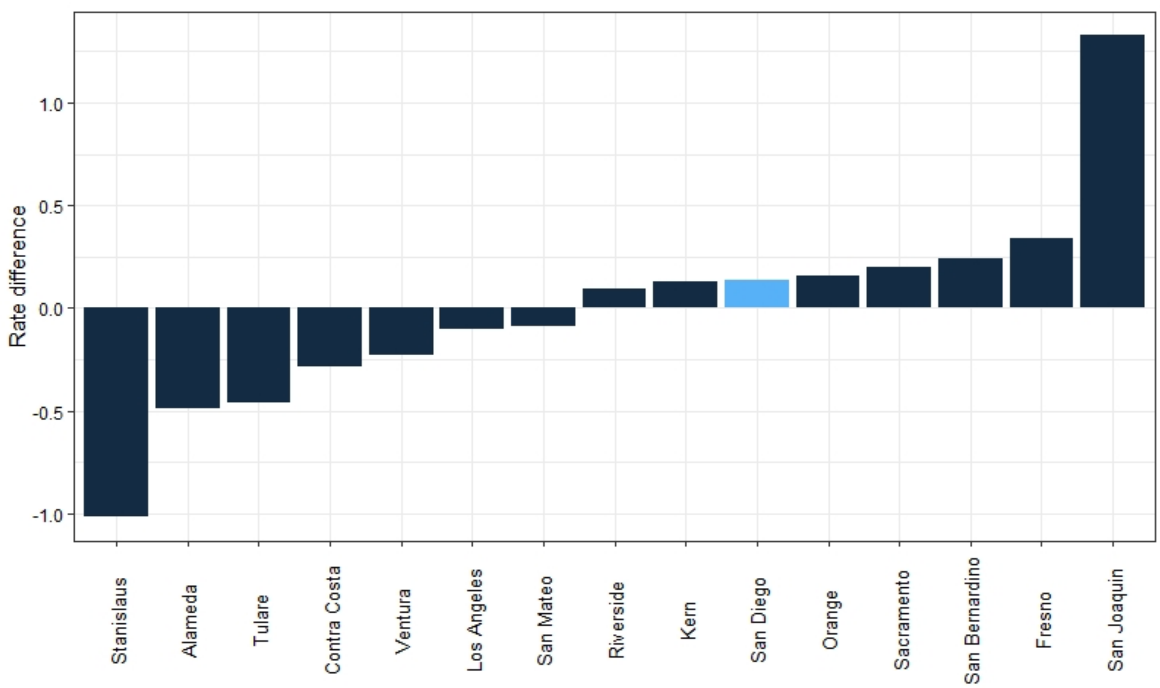
a. The rate difference is the mean post-GVRO firearm violence rate in the treated unit minus the corresponding value in its synthetic control. Only placebos with a good pre-treatment model fit (<20*MSPE of San Diego) were retained for ranking.

Appendix C Figure 2: Ranking of Post-GVRO Difference in Firearm Assault and Self-Harm Between Treated Unit and Synthetic Control, San Diego and Placebo Counties[a]

A. Assault



B. Self-Harm



a. The rate difference is the mean post-GVRO firearm violence rate in the treated unit minus the corresponding value in its synthetic control. Only placebos with a good pre-treatment model fit (<20*MSPE of San Diego) were retained for ranking.

Appendix C Table 4: Synthetic Control Results, More Restrictive Donor Pool (n=20)

		Outcome		
		Biannual Firearm Violence	**Annual Firearm Assault**	**Annual Firearm Self-Harm**
San Diego	**Rate in post-intervention period (per 100,000)**	5.05	4.87	5.23
Synthetic San Diego	**Rate in post-intervention period (per 100,000)**	5.78	5.83	4.73
Rate difference		-0.73	-0.96	0.51
Percent difference		-13%	-16%	+11%
Pseudo P-value[a]		7/21=0.33	7/18=0.39	6/7=0.86
Model fit (MSPE)		1.49	1.10	0.03

a. The proportion of counties (donor pool plus San Diego) with a difference between the observed rate and the synthetic control rate in the post-treatment period equal to or more negative than San Diego's. Counties with a poor pre-period fit (MSPE >20* San Diego's MSPE) were exclude from the denominator. Note: Including all placebos in the self-harm analysis resulted in a p-value of 0.71 (15/21).

Appendix C Table 5: Synthetic Control Results, 2018 Intervention

		Outcome		
		Biannual Firearm Violence	Annual Firearm Assault	Annual Firearm Self-Harm
San Diego	**Rate in post-intervention period (per 100,000)**	5.07	4.72	5.43
Synthetic San Diego	**Rate in post-intervention period (per 100,000)**	4.51	5.52	5.61
Rate difference		0.56	-0.81	-0.19
Percent difference		+12%	-15%	-3%
Pseudo P-value[a]		14/23=0.61	11/23=0.48	8/20=0.40
Model fit (MSPE pre-GVRO)		0.41	0.62	0.06

a. The proportion of counties (donor pool plus San Diego) with a difference between the observed rate and the synthetic control rate in the post-treatment period equal to or more negative than San Diego's. Counties with a poor pre-period fit (MSPE >20* San Diego's MSPE) were exclude from the denominator.