Title:
Association of the Operation Peacemaker Fellowship in Richmond, California with city-level firearm and nonfirearm homicide and assault: A quasi-experimental study

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Introduction

Interpersonal violence is a major driver of population health and health disparities in the United States (US). In 2016, homicides and assaults caused approximately 20,000 deaths and 1.7 million injuries.[1] Firearm-related violence is particularly concerning because it is highly fatal and disproportionately impacts young, black men.[1] However, there are few community-based violence prevention programs, firearms-centered or otherwise, that are scientifically-supported. Strategies such as Ceasefire[2,3] and Cure Violence,[4–6] which typically involve community mobilization, street outreach, and partnerships among frontline staff in police, probation, corrections, and social services sectors, have been tested in cities across the US, and while they are promising,[7,8] additional tools to combat community violence would be valuable. Identifying other effective interventions to address firearm violence is a top priority for public health and public policy researchers and practitioners.[9,10] One particularly novel program merits evaluation: the Operation Peacemaker Fellowship implemented in Richmond, California in 2010. Richmond is a racially and ethnically diverse, formerly industrial city of approximately 100,000 residents in the Northeast San Francisco Bay Area. In the mid-2000's, it was one of the most violent cities in the nation, with a homicide rate of 46 per 100,000, compared with 5 per 100,000 in similarly sized California cities.[11,12] Facing mounting pressure from residents and community leaders, in 2007 the City Council moved to create and fund the Office of Neighborhood Safety (ONS) to focus explicitly on reducing firearm violence.[13] Initial activities included street-level conflict mediation and intensive mentoring for at-risk youth in "hotspot" neighborhoods, but after an uptick in homicides in 2009, ONS leadership shifted focus to the 30 individuals that the Police Department believed were responsible for a majority of the city's firearm crimes. Although other programs have used this targeted approach,[2,4] ONS efforts are unique because they invite targeted individuals to participate in an intensive 18-month program known as the Operation Peacemaker Fellowship (hereafter, "Operation Peacemaker"). Among other supports, Operation Peacemaker provided participants with individually-tailored mentorship, 24-hour case management, cognitive behavioral therapy, internship opportunities, assistance with social service navigation, substance abuse treatment, and support, excursions, and stipends up to $1000 per month for successful completion of specific goals set by both the Fellow and ONS staff (a conditional cash transfer).[14,15]

Operation Peacemaker has received nationwide attention for its unique approach and apparent success – descriptively, firearm homicides and assaults declined 18% from the implementation of the program to 2015.[11,12] In addition, a detailed process evaluation documented high program uptake and positive outcomes for participants, including improved access to services, higher quality of life, and low rates of deaths, injuries, and crime perpetration.[14,15] However, to our knowledge, there are no rigorous quantitative studies of the city-level impacts of the program that control for the influence of other factors that may have contributed to declines during this time period, such as improvements in economic climate and declines in crime more broadly. In this study, we address this gap using a quasi-experimental design and comprehensive health and crime data on Richmond and comparison cities in California and nationwide to quantify the association between Operation Peacemaker and population-level reductions in violence. Although firearm violence was the focus of Operation Peacemaker, the program addressed several fundamental determinants of violence (e.g.

substance use) and thus could plausibly impact nonfirearm violence as well; we assess both firearm and nonfirearm violence.

Methods

Overall approach

We used a generalization of the synthetic control method[16,17] (SCM) to compare observed post-intervention patterns in firearm and nonfirearm violence in Richmond to those predicted in the absence of the program. Firearm and nonfirearm violence were measured using city-level statewide health records of deaths and hospital visits due to homicide and assault and jurisdiction-level nationwide crime records of homicides and aggravated assaults. For each outcome and data source, we identified the weighted combination of comparison units, selected from all those available, whose outcomes optimally predicted outcomes in Richmond in the pre-intervention period. We then used this weighted combination to predict "counterfactual" post-intervention outcomes in Richmond. This method is well-suited to situations involving one intervention unit and many controls and may better approximate "counterfactual" post-intervention outcomes than using any single control or an evenly-weighted combination of controls. The approach controls unobserved confounders (e.g. poverty, inequality) by assuming that the weighted combination of controls that can best predict the pre-intervention trends will continue to predict those trends in the post-intervention period. It also controls for secular trends in violence that are common across places.

Data and measures

First, we used death records from the California Department of Public Health Vital Records and emergency department and inpatient hospitalization discharge records from California's Office of Statewide Health Planning and Development. Records included all deaths and hospital visits statewide, by location of residence of the patient or decedent. We identified deaths due to homicide and injuries due to assault using ICD external cause of death or injury codes (Appendix). External cause of injury coding in California's hospital discharge records is compulsory, with ongoing quality assurance, and is regarded as 100% complete.[18] Previous research also indicates completeness and validity of homicide e-codes in death data.[19]

Second, we used nationwide crime records from the Return A Record Card Master Files and Supplemental Homicide Reports. These data are voluntarily reported by law enforcement agencies and compiled by the FBI Uniform Crime Reports (UCR) system. Records included aggravated assaults and homicides, the incident month, weapon used, and jurisdiction/agency logging the crime. Although subject to variable reporting,[20,21] these data capture incidents that do not appear in health data—for example, assaults or shootings that do not involve hospital visits or deaths—and thus serve as an important complementary source.

Due to data availability, we restricted health data analyses to 2005-2016 and crime data analyses to 1996-2017. We modeled outcomes as counts rather than rates, because for crime data, the geographic boundaries of law enforcement agencies do not always correspond to clear populations at risk, and for health data, models of counts achieved better pre-intervention fit than rates (see Statistical analysis). We present results for health data analyzed using rates as a sensitivity analysis. We combined fatal and nonfatal outcomes, because fatal outcomes were too infrequent to be assessed separately. We aggregated counts to the quarterly level to balance capturing short-term variation with ensuring that measures were stable enough for accurate modeling. Health data were analyzed at the Census Place level—the named cities and towns in which people reside. Crime data were analyzed at the jurisdiction level. In health data, we restricted candidate control cities to those with at least 5,000 residents and one homicide/assault in the pre-intervention period. In crime data, we followed data cleaning procedures used by FBI UCR[22] and restricted to candidate control jurisdictions with complete reporting over the study period. Operation Peacemaker began intensive work with its first 21 participants in June 2010. We hypothesized that effects of the program would be immediate and treated Quarter 3 of 2010 as the intervention start date for all analyses.

Statistical analysis

We used a generalization of SCM to predict post-intervention patterns in firearm and nonfirearm violence in Richmond in the absence of Operation Peacemaker.[16,17] SCM has been used in a variety of recent applications to study the impacts of programs and policies implemented in a single geographic unit with a defined start date.[23–26]

In traditional SCM, the pre-intervention outcomes in the treated unit are modeled as a function of the pre-intervention outcomes in candidate control units, with model coefficients ("weights") constrained to be nonnegative and sum to one with no intercept. Fitted weights are those that optimally predict the treated unit outcomes in the pre-intervention period. These weights are then used to construct a "synthetic" or predicted outcome series for the treated unit in the pre- and post-intervention periods from the optimally weighted combination of control unit outcomes. Good alignment between observed and predicted outcomes for the treated unit in the pre-intervention period indicates that the weighted combination of control units is effectively predicting outcomes in the treated unit. In contrast to differences-in-differences, this approach does not require equally-weighted control units or the assumption of parallel trends in intervention and control units in the absence of the intervention, but requires that the relationship between intervention and control units in the pre-intervention period continues in the post-intervention period.[17]

Doudchenko and Imbens generalized SCM by relaxing the constraints of no intercept and weights that are nonnegative and sum to one.[17] This added flexibility can achieve better pre-intervention fit, particularly when the outcome is high relative to the control pool, while retaining the strengths of the synthetic control approach. Adding flexibility increases the risk of over-fitting, because weights can take any positive or negative values in order to fit the pre-intervention data. Elastic net combines a linear model with penalties on the number and size of the weights to reduce the likelihood of overfitting and improve model performance. Following recommendations by Doudchenko and Imbens,[17] we used leave-one-out cross-validation, strictly on pre-intervention control units, to select the penalty parameters. On occasion, this form of cross-validation resulted in models that were still over-fit, as indicated by the large number (600+) of nonzero weights. When this occurred, we selected the penalty parameters that achieved the lowest in-sample pre-treatment model error while also being reasonably parsimonious (defined as fewer than 100 nonzero weights).

We summarized associations by comparing the total and average annual homicides and assaults observed in the post-intervention period to those predicted in the post-intervention period in the absence of the program. Statistical inference was conducted using placebo tests.[16,24] Specifically, we regarded each control unit, in turn, as the intervention unit and quantified the association between Operation Peacemaker and the outcomes. Because Operation Peacemaker did not occur in control units, this procedure provides a distribution of measured associations that are likely due to chance. The proportion of control units with associations more extreme than that estimated for the intervention unit is a measure of the degree to which the association may be due to chance. Following standard practice,[27,28] we restricted these calculations to control units in which the pre-intervention mean squared prediction error (MSPE) was less than 20, 5, or 2 times that of Richmond.

The generalized SCM can include covariates. However, the geographic boundaries of law enforcement agencies are not always well-defined, making it difficult to assign appropriate place-level covariates (e.g. from Census data). Additionally, previous research has found excellent and often superior pre-intervention fit without covariates.[17,23,29] We tested the sensitivity of our results to inclusion of sociodemographic covariates predictive of violence in the models of health outcomes, for which covariates can be directly assigned. Covariates included yearly measures of poverty, education, income inequality, household composition, housing costs, neighborhood characteristics (e.g. civic engagement), job availability, and unemployment (see Appendix for details). In addition, because other violence prevention efforts (e.g. Ceasefire) scaled up beginning in 2012, we tested the sensitivity of our results to restricting the post-intervention period to July 2010 – December 2011.

Results

Figure 1 details trends in observed and predicted firearm-related homicides and assaults using health and crime data, by quarter, before and after implementation of Operation Peacemaker. Figure 2 presents the nonfirearm outcomes. In the pre-intervention period, predicted outcomes constructed from optimally-weighted combinations of control cities/jurisdictions generally aligned with the observed outcomes, although the model fit was better for health data than crime data. Crime-based outcomes were more common than health-based outcomes in each quarter, likely reflecting differences in the incidents captured by each system.

In the post-intervention period, comparing observed outcomes to those predicted in the absence of the program indicated that Operation Peacemaker was associated with reductions in firearm homicides and assaults, but increases in nonfirearm homicides and assaults. Specifically, post-implementation, the program was associated with 182 fewer firearm homicides/assaults in health data (July 2010 – December 2016) and 644 fewer firearm homicides/assaults in crime data (July 2010 – December 2017), corresponding to average reductions of 28 and 117 cases per year, respectively (Table 1). During the same time periods, the program was associated with 594 more nonfirearm homicides/assaults in health data and 167 more nonfirearm homicide/assaults in crime data, corresponding to average increases of 93 and 30 cases per year, respectively (Table 1). Placebo tests (Table 1; Appendix Figures 1-4) indicate that these associations were unlikely to be due to chance for all outcomes except nonfirearm homicide/assaults in crime data. The program was associated with increases in nonfirearm homicides/assaults in crime data in the first 3.5 post-intervention years and reductions in these outcomes thereafter (Figure 2). Analyses considering two separate post-intervention periods (July 2010-December 2013 and January 2014-December 2015) still suggested these associations were likely due to chance (Appendix Table 5). Weights assigned to comparison cities varied by outcome and are presented in Appendix Tables 1-4.

Restricting the post-intervention period to just those quarters before scale-up of other violence prevention efforts (e.g. Ceasefire) in 2012 did not meaningfully alter the results (Appendix Table 6). Adding sociodemographic covariates in analyses of health data made the associations for both firearm homicides/assaults and nonfirearm homicides/assaults somewhat stronger, but did not substantively change the results (Appendix Figure 5; Appendix Table 7). Results from sensitivity analyses of health data using rate outcomes instead of counts were also consistent with the main results (Appendix Figure 6; Appendix Table 8).

Conclusions

To our knowledge, this is the first quasi-experimental study to examine the association of the Operation Peacemaker Fellowship, a novel firearm violence prevention program, with city-level homicide- and assault-related crimes, deaths, and injuries. We found that the program was associated with significant reductions in firearm violence, but possible increases in nonfirearm violence.

In interpreting these results, one important caveat is that we cannot disentangle the possible effects of Operation Peacemaker from other programs that were implemented in Richmond at the same time. Firearm violence in Richmond started declining prior to Operation Peacemaker, and there are likely many reasons for these declines. For example, ONS opened its doors in 2007 and became fully operational in 2008; the police department changed leadership in 2006 and reorganized their special investigation unit to focus on arresting those responsible for firearm crimes; in 2007, the Rising Youth for Social Equity (RYSE) Center, a key community organization in the city, was founded to provide a safe space for youth affected by firearm and nonfirearm violence in the city; various grassroots anti-firearm violence campaigns were initiated by Richmond residents and community leaders beginning in the mid-2000's; and one of these campaigns led to the implementation of Operation Ceasefire, with planning stages beginning in 2010 and active call-ins beginning in 2012.[30]

However, we believe the measured associations may be due to the program, because other major violence-related changes were offset in time from Operation Peacemaker and the nature and intensity of the program was unique. An ongoing ethnographic study of firearm violence in Richmond suggests that between mid-2010 and 2012, Operation Peacemaker appears to have been the only organization providing intensive support services to those actively involved with or most at risk for firearm violence. These individuals were also targeted by police in years prior to Operation Peacemaker and by Ceasefire in subsequent years, but the timing of Operation Peacemaker was distinctive, and no other program provided the same level of case management and opportunities (e.g. stipends, international travel) that Operation Peacemaker did. Analyses restricting to this time period showed results consistent with those of the main analysis. Furthermore, a previous process evaluation documented that Operation Peacemaker succeeded in deeply engaging and affecting participants in unprecedented and meaningful ways.[14,15] In contrast, prior evidence on Ceasefire and community policing has shown less substantial impacts effectiveness.[2,3,31] However, it is still possible that other less well-documented changes, or impacts of ongoing programs or program enhancements, may have coincided with Operation Peacemaker and contributed to declines. Future research on the timing, content, and funding levels of the various programs, and on population subgroups most likely to be affected by particular programs (e.g. youth) may help to disentangle their effects.

In the health data, the program was associated with increases in nonfirearm homicides and assaults. These increases are corroborated by forthcoming qualitative work documenting reports by residents, community leaders, and law enforcement that crimes not involving firearms, such as violent robberies and illicit drug transactions, have persisted or increased during the post-intervention period.[30] Our crime data provide little insight into this shift. However, a post-hoc examination of our health data revealed that, throughout the study period, a majority of nonfirearm violent victimization occurred among Black and Hispanic men ages 15-29 residing in the neighborhoods of North Richmond, Iron Triangle, and certain Southern parts of the city (the populations and places most strongly associated with gang violence[30]), and that after the implementation of Operation Peacemaker, the composition of nonfirearm violence shifted slightly away from deaths and hospitalizations towards emergency department visits. We propose several possible post-hoc explanations. First, removing key players in firearm violence from active participation may have inadvertently generated violent activity in the face of a power void. One previous study documented similar patterns of increasing violence following drug-related arrests.[32] Second, the emphasis on firearm violence over the past decade may have reduced local organization and law enforcement efforts to suppress other types of violence. Third, the program may have induced changes in the nature of violence, such as substitution of firearms for other weapons or bodily force. Reports indicate that as firearm carrying declined in the post-intervention period, the risk of being shot decreased.[30] Thus, altercations or retaliations may have been more likely to be pursued, because they were less likely to be fatal. Such altercations may also produce more total injuries, because close-range or hand-to-hand interactions may harm more individuals, as opposed to shootings which are more likely to be fatal but may also end quickly with fewer individuals involved. Some research suggests that the availability of firearms drives the fatality of violent encounters but not the overall amount of violence.[33] Substitution effects in the opposite direction – that greater community violence was associated with shifts from nonfirearm to firearm violence – have been documented previously.[34] Further research should examine each of these possibilities and the dynamics that might underlie this phenomenon.

Although health and crime data showed generally similar trends, there were some differences in results across the two sources. In particular, crime data showed larger declines in firearm violence while health data showed larger increases in nonfirearm violence. These differences may be due to differences in the types of incidents captured by each data collection system, the weights assigned to comparison cities used to construct the synthetic control, the quality of the pre-intervention model fit, or variability in reporting practices across places and times, particularly for crime data.[20,21] Reported nonfirearm crimes in Richmond dropped dramatically between 2013 and 2014. This pattern was not systematically observed in control jurisdictions, and may reflect the contributions of other violence prevention efforts or unknown changes in reporting practices. The reasons for this shift warrant further investigation.

This study has several limitations. First, all nonexperimental studies are at risk of residual confounding. We minimized the impact of potential bias by conducting a quasi-experimental study that leverages the program's well-defined start time and outcome trends in comparison cities to control for both unmeasured time-varying risk factors for violence and secular trends that are common across cities. Second, our approach assumes that similar violence prevention efforts did not happen systematically in control cities at the same time as Operation Peacemaker. To our knowledge, no other cities implemented Operation Peacemaker's unique package of interventions during the study period, but it is possible that other cities may have implemented elements of the program during the post-intervention period. If any of these cities were weighted in the synthetic control, we would expect the measured association to be biased towards the null. Finally, our analytic approach assumed that the relationship between intervention and control units did not change between the pre-intervention and post-intervention periods, and that other factors impacting the outcomes did not change in Richmond at precisely the same time as Operation Peacemaker. We think that these assumptions are reasonable, but violations are possible. For example, demographic shifts have occurred in Richmond in the post-intervention period, albeit slowly, and might change the relationship between Richmond and control units over time.

This study adds to the scant literature on community-based violence prevention programs and provides more definitive evidence for this program's effectiveness of Operation Peacemaker in reducing urban firearm violence. While Operation Peacemaker may have reduced firearm violence in Richmond, the co-occurring increase in nonfirearm violence raises concerns and should be further investigated. Future research should also consider which components of this multifaceted program, or synergies between components, may be most impactful. Replications of the Operation Peacemaker Fellowship are currently being conducted in other cities nationally and internationally. Implementers should monitor for possible increases in nonfirearm violence while evaluators have the opportunity to assess effects of this prevention model in other settings.

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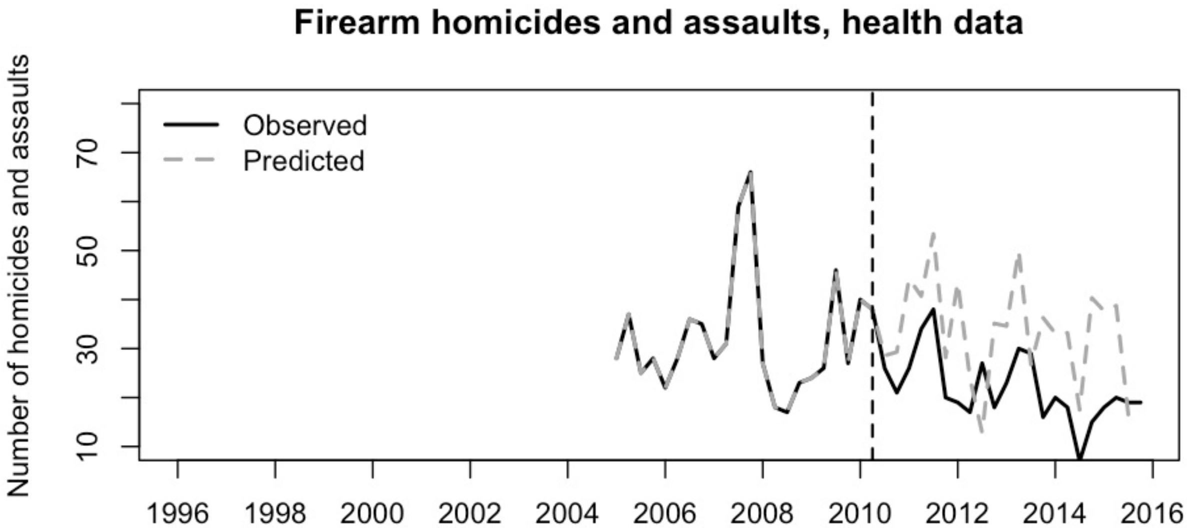
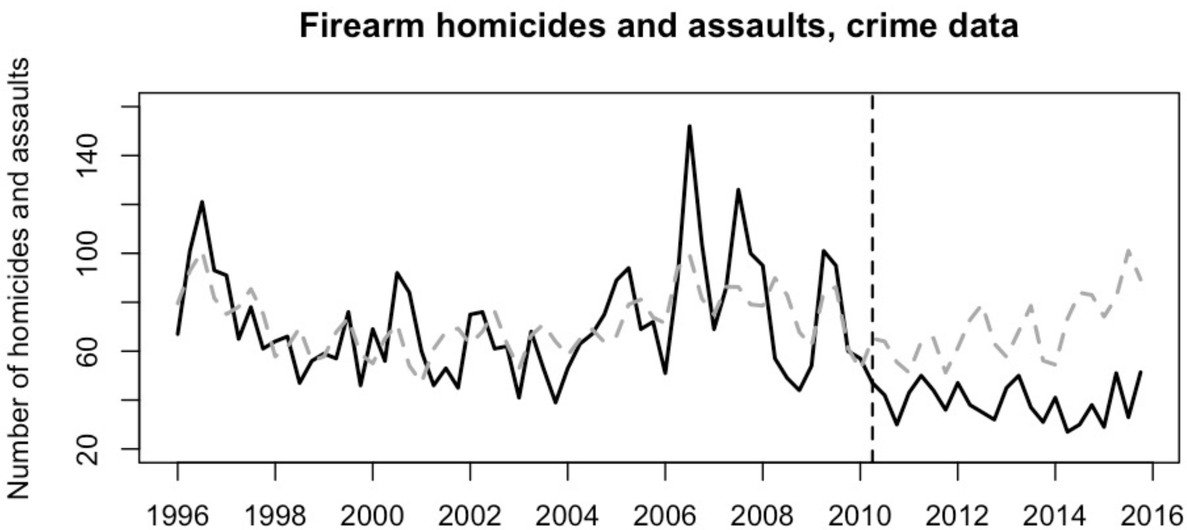
Figures

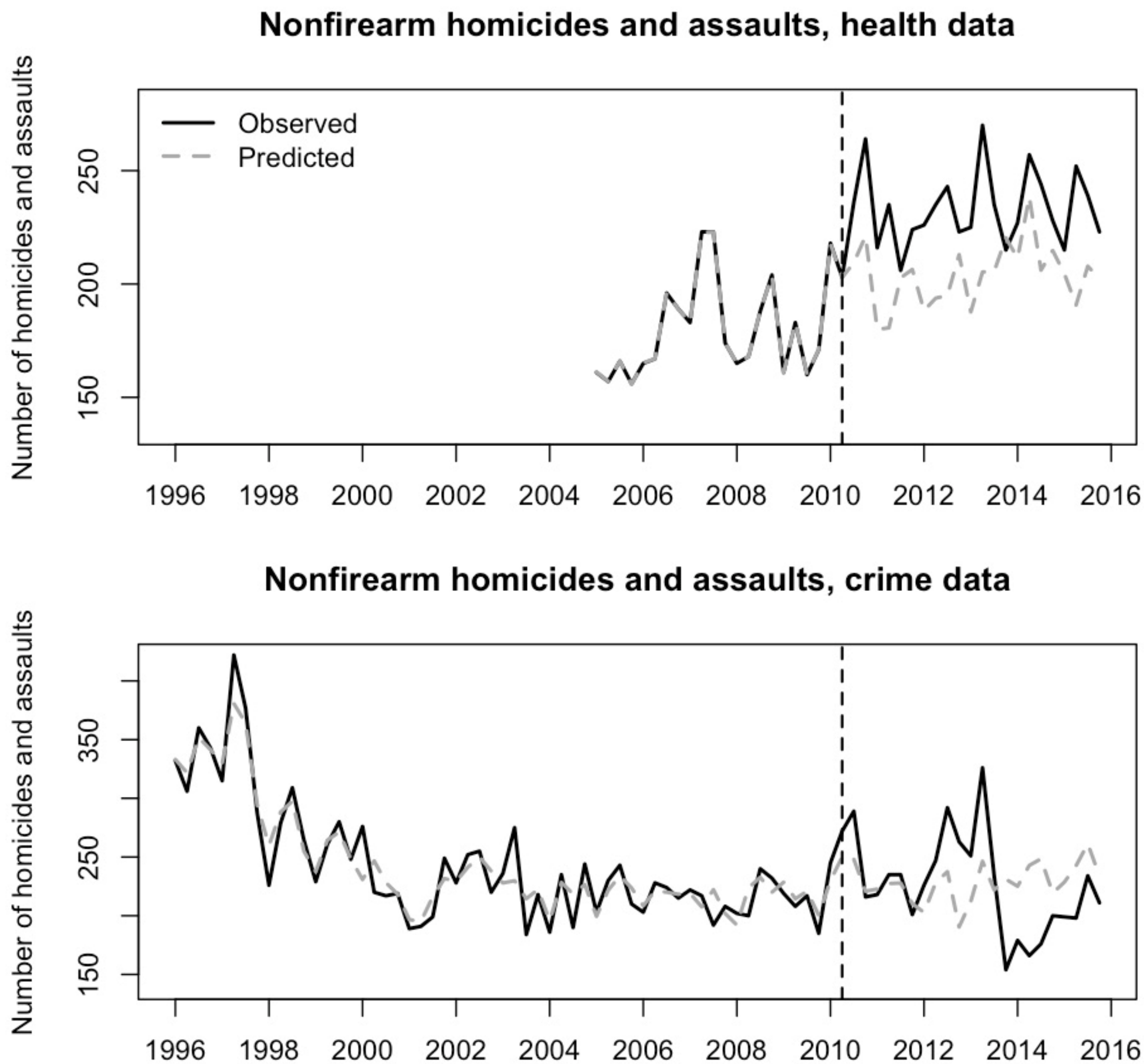
Figure 1: Observed and predicted firearm homicides and assaults, by quarter, from health and crime data, before and after implementation of the Operation Peacemaker Fellowship, Richmond, California, 1996-2016.





The vertical dotted line indicates the initiation of the Operation Peacemaker Fellowship in June 2010. Health data were available through 2016; crime data were available through 2015.

Figure 2: Observed and predicted nonfirearm homicides and assaults, by quarter, from health and crime data, before and after implementation of the Operation Peacemaker Fellowship, Richmond, California, 1996-2016.



The vertical dotted line indicates the initiation of the Operation Peacemaker Fellowship in June 2010. Health data were available through 2016; crime data were available through 2015.

Tables

Table 1: Summary of generalized synthetic control results for the association of the Operation Peacemaker Fellowship with firearm and nonfirearm homicides and assaults, Richmond, California.

	Homicides and assaults in health data (post-intervention period: 2005-2016)		Homicides and assaults in crime data (post-intervention period: 2005-2015)	
	Firearm	Nonfirearm	Firearm	Nonfirearm
Candidate control places/jurisdictions	625	630	641	753
Observed cases post-intervention	555	6,050	860	4,950
Predicted cases post-intervention without Operation Peacemaker	737	5,456	1,504	4,783
Average annual post-intervention difference in cases associated with Operation Peacemaker	-28	+93	-117	+30
	Proportion of control place/jurisdiction-quarters more extreme, average, post-intervention			
All control units	7/625 (0.01)	18/630 (0.03)	23/641 (0.04)	452/753 (0.60)
< 20x MSPE	7/580 (0.01)	18/630 (0.03)	23/641 (0.04)	452/753 (0.60)
< 5x MSPE	7/580 (0.01)	18/630 (0.03)	21/638 (0.03)	452/753 (0.60)
< 2x MSPE	7/580 (0.01)	18/630 (0.03)	15/622 (0.02)	451/752 (0.60)

MSPE: Mean squared prediction error in pre-intervention period models.

Appendix

ICD-9-CM and ICD-10-CM hospital visit codes and ICD-10 death codes used to identify homicides and assaults in health data

We identified deaths due to homicide and injuries due to assault using the following ICD external cause of death or injury codes:

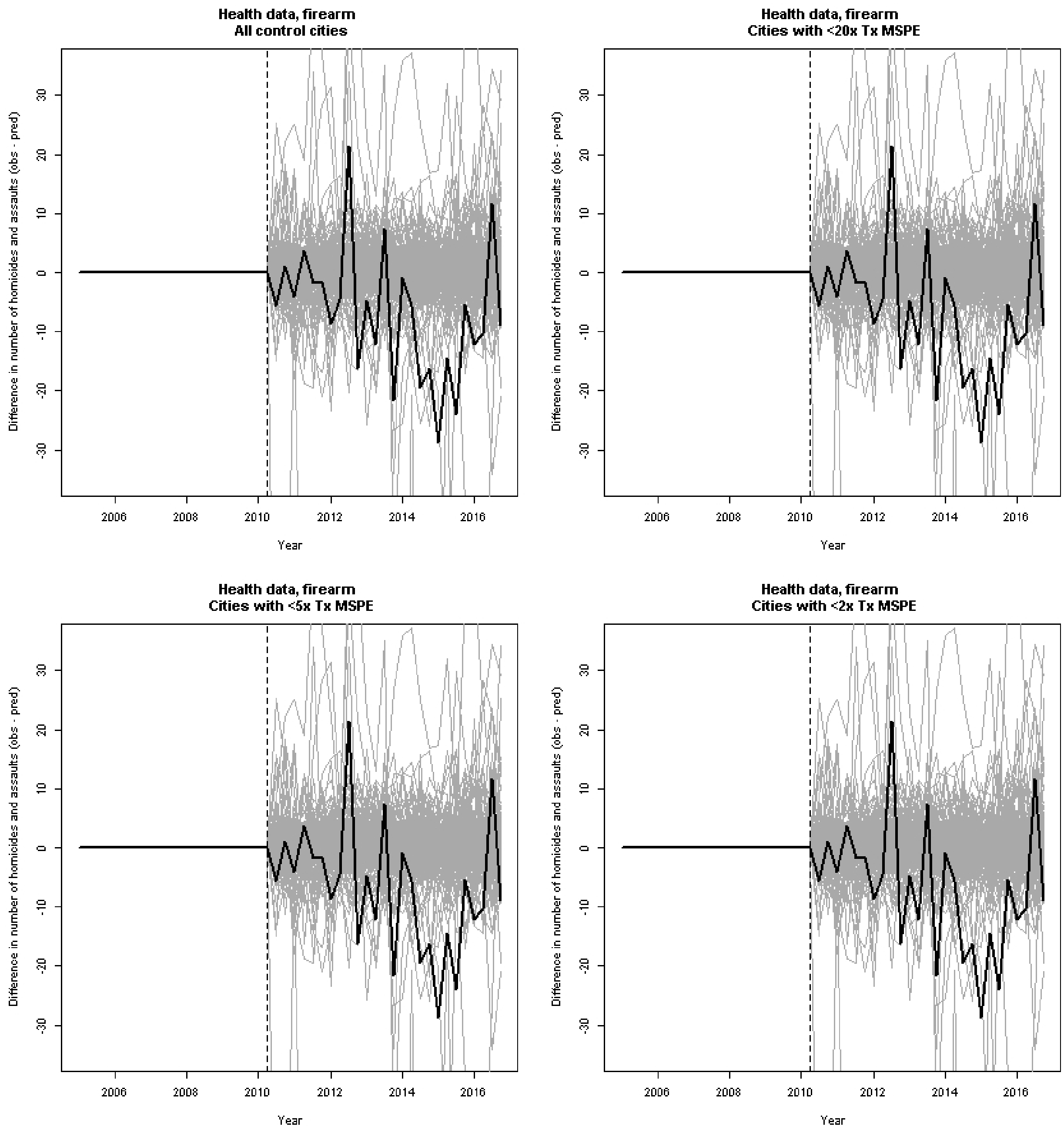
- ICD-9-CM hospital visit external cause of injury codes for firearm assault (2005-September 2015): E9650-E9654, E970
- ICD-9-CM hospital visit external cause of injury codes for nonfirearm assault (2005-September 2015): E960-E964, E9655-E9659, E966-E969, E971-E977
- ICD-10-CM hospital visit external cause of injury codes for firearm assault (October 2015 – December 2016): X93 – X94, X95.8 – X95.9, Y35.0
- ICD-10-CM hospital visit external cause of injury codes for nonfirearm assault (October 2015 – December 2016): X92, X96-Y09
- ICD-10 external cause of death codes for firearm homicide (2005-2016): X93-X95, Y35.0
- ICD-10 external cause of death codes for nonfirearm homicide (2005-2016): U01, U02, X85-X92, X96-Y09, Y871, Y35.1-Y35.7

Placebo tests for main analysis

Statistical inference was conducted using placebo tests (1,2). Specifically, we regarded each control unit, in turn, as the intervention unit and quantified the association between Operation Peacemaker and the outcomes. Because Operation Peacemaker did not occur in control units, this procedure provided a distribution of measured associations that are likely due to chance. The proportion of control units with associations more extreme than that of the intervention unit is a measure of the degree to which the association may be due to chance. Following standard practice (3,4), we restricted these calculations to control units in which the pre-intervention mean squared prediction error (MSPE) was less than 20, 5, or 2 times that of Richmond.

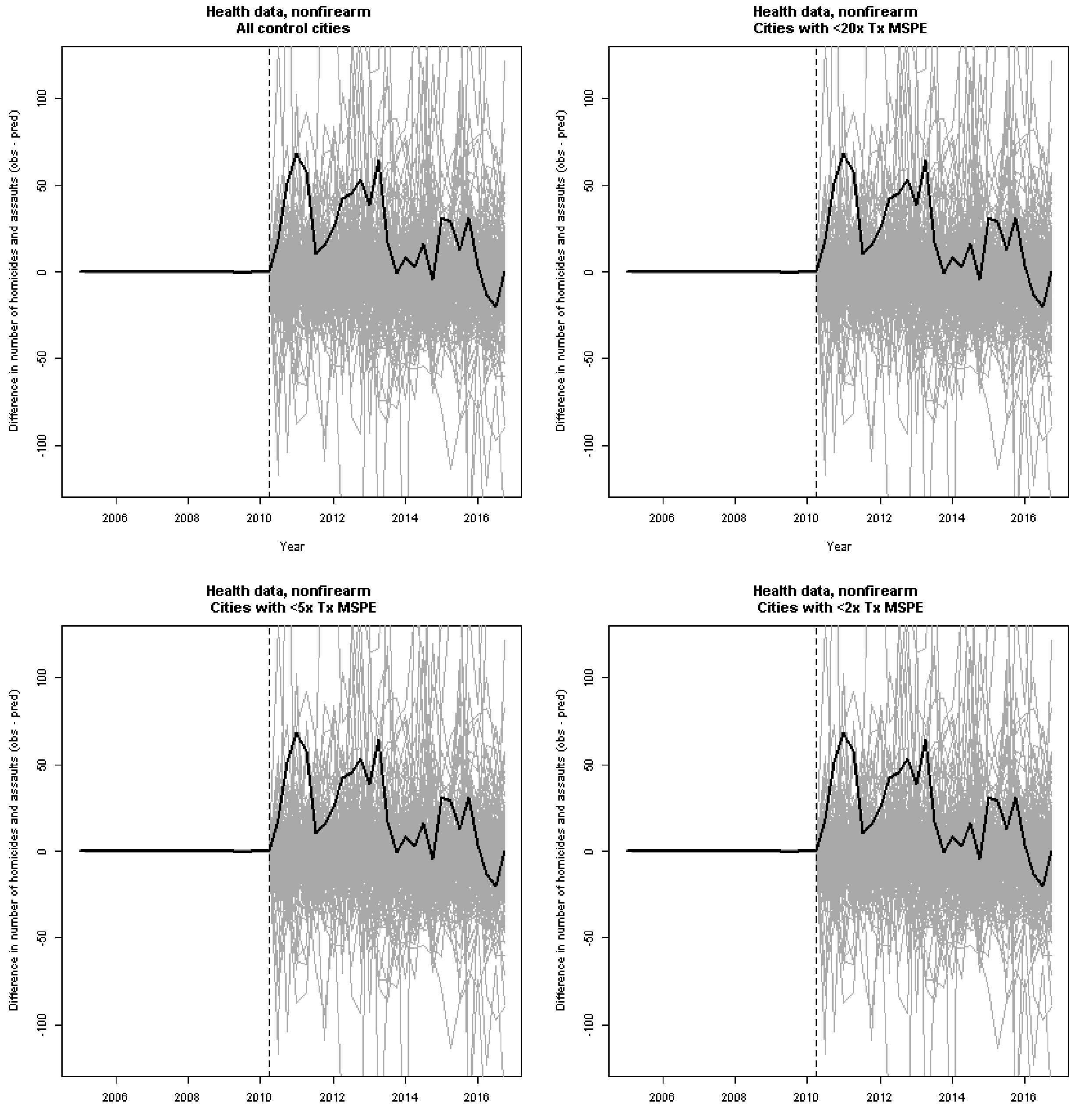
In the post-intervention period, comparing observed outcomes to those predicted in the absence of the program indicated that Operation Peacemaker was associated with reductions in firearm homicides and assaults, but increases in nonfirearm homicides and assaults. Results of the placebo tests (main text Table 1, Appendix Figures 1-4) indicated that the associations were unlikely to be due to chance for all outcomes except nonfirearm homicide/assaults in crime data.

Appendix Figure 1: Generalized synthetic control placebo test results for the association of the Operation Peacemaker Fellowship with firearm homicides and assaults in health data, Richmond, California.



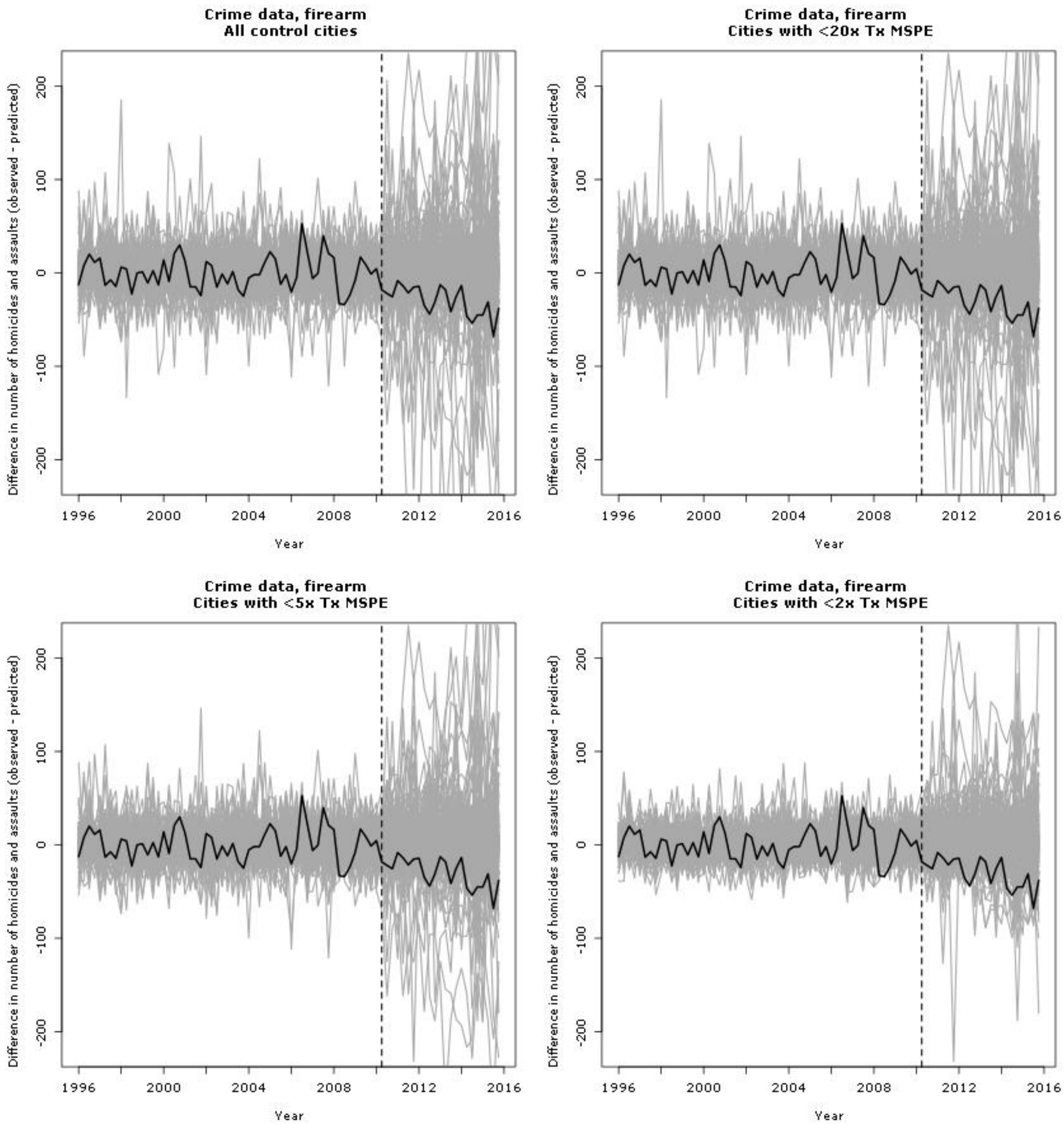
Each time series represents the difference between the observed and the predicted number of homicides and assaults. Richmond is in black and the control cities are in grey. The vertical dashed line indicates the start of the intervention. Each panel displays only those control cities with pre-intervention mean squared prediction error (MSPE) less than the multiple of Richmond's pre-intervention MSPE, as indicated by the title. Tx: treatment.

Appendix Figure 2: Generalized synthetic control placebo test results for the association of the Operation Peacemaker Fellowship with nonfirearm homicides and assaults in health data, Richmond, California.



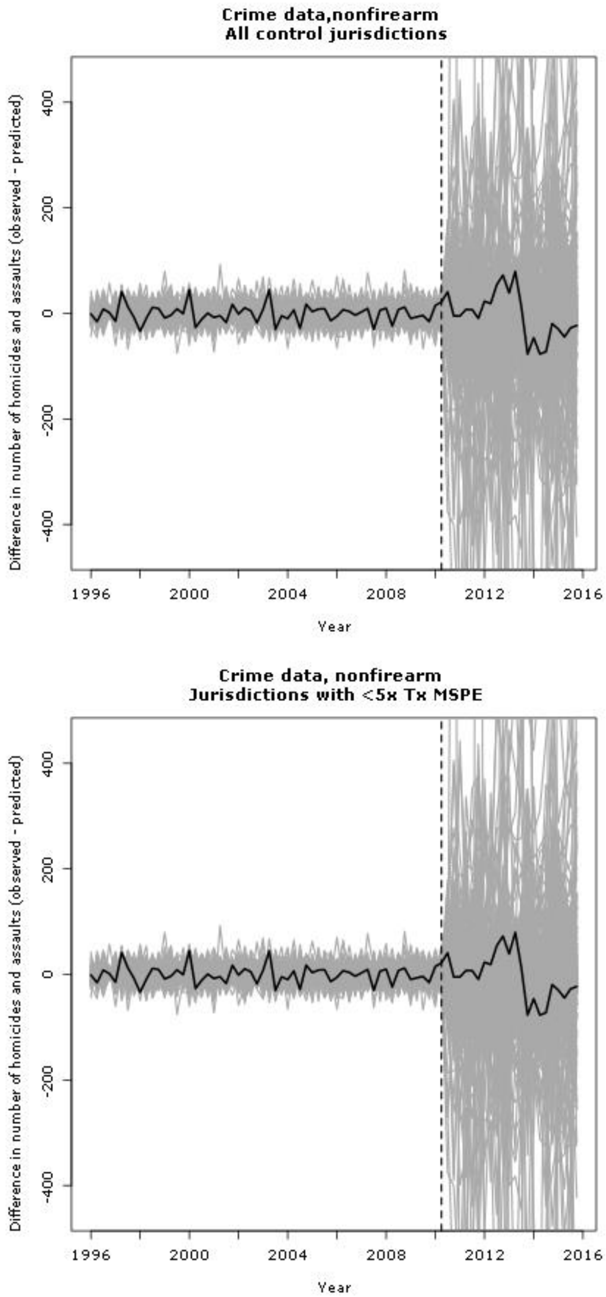
Each time series represents the difference between the observed and the predicted number of homicides and assaults. Richmond is in black and the control cities are in grey. The vertical dashed line indicates the start of the intervention. Each panel displays only those control cities with pre-intervention mean squared prediction error (MSPE) less than the multiple of Richmond's pre-intervention MSPE, as indicated by the title. Tx: treatment.

Appendix Figure 3: Generalized synthetic control placebo test results for the association of the Operation Peacemaker Fellowship with firearm homicides and assaults in crime data, Richmond, California.



Each time series represents the difference between the observed and the predicted number of homicides and assaults. Richmond is in black and the control jurisdictions are in grey. The vertical dashed line indicates the start of the intervention. Each panel displays only those control jurisdictions with pre-intervention mean squared prediction error (MSPE) less than the multiple of Richmond's pre-intervention MSPE, as indicated by the title. Tx: treatment.

Appendix Figure 4: Generalized synthetic control placebo test results for the association of the Operation Peacemaker Fellowship with nonfirearm homicides and assaults in crime data, Richmond, California.



Each time series represents the difference between the observed and the predicted number of homicides and assaults. Richmond is in black and the control jurisdictions are in grey. The vertical dashed line indicates the start of the intervention. Each panel displays only those control jurisdictions with pre-intervention mean squared prediction error (MSPE) less than the multiple of Richmond's pre-intervention MSPE, as indicated by the title. Tx: treatment.

Weights assigned to control cities/jurisdictions in main analysis

Weights assigned to comparison cities varied by outcome and are presented in Appendix Tables 1-4.

Appendix Table 1: Weights assigned to control cities in analyses of firearm homicides and assaults in health data.

Place name	Weight
Pittsburg city	1.139
Hawthorne city	1.109
Ceres city	0.925
East Rancho Dominguez	-0.844
Riverside city	-0.757
Rialto city	-0.643
Lakewood city	0.572
Lancaster city	-0.550
San Bernardino city	-0.372
San Diego city	0.342
Modesto city	0.340
Huntington Park city	0.320
San Francisco city	0.274
Anaheim city	-0.261
Highland city	-0.257
Lynwood city	-0.246
Fresno city	-0.227
Oakland city	-0.189
Corona city	0.182
Oxnard city	-0.174
Westmont	0.162
East Los Angeles	0.095
Bakersfield city	0.094
Vallejo city	0.065
Long Beach city	0.038
Inglewood city	-0.034
Fairfield city	0.021
Salinas city	-0.017
Los Angeles city	-0.012
Sacramento city	-0.012
Gardena city	-0.002
Union City city	0.001

Appendix Table 2: Weights assigned to control cities in analyses of nonfirearm homicides and assaults in health data.

Place name	Weight
Santa Clarita city	-0.367
Fairfield city	0.296
Lancaster city	0.238
Bakersfield city	-0.205
Madera city	0.200
Apple Valley town	-0.171
Vallejo city	0.161
Chula Vista city	0.154
Roseville city	0.154
Citrus Heights city	0.141
Fresno city	-0.133
Pittsburg city	0.105
Oakland city	0.100
Rancho Cucamonga city	0.093
Sacramento city	0.064
San Diego city	-0.062
San Jose city	-0.053
Stockton city	-0.047
Long Beach city	0.042
Hayward city	0.038
Los Angeles city	0.028
Chico city	-0.026
San Francisco city	0.016
Ontario city	-0.010
Norwalk city	0.005

Appendix Table 3: Weights assigned to control jurisdictions in analyses of firearm homicides and assaults in crime data.

Jurisdiction	**Weight**
Houston, TX	0.045
Charlotte Mecklenburg Police Department, NC	0.022
Milwaukee, WI	0.013
Phoenix, AZ	0.005
Las Vegas Metro Police Jurisdiction, NV	0.004
Los Angeles, CA	0.002
Detroit, MI	0.001

Appendix Table 4: Weights assigned to control jurisdictions in analyses of nonfirearm homicides and assaults in crime data.

Jurisdiction	Weight
Portland, OR	0.054
Harris, TX	0.045
Los Angeles County, CA	0.031
Omaha, NB	0.024
Newark, NJ	0.020
Philadelphia, PA	-0.014
Springfield, MA	0.009
Dallas, TX	-0.007
Jersey City, NJ	0.006
Memphis, TN	0.005
Milwaukee, WI	0.005
Los Angeles, CA	0.001

Results for the association of the Operation Peacemaker Fellowship with nonfirearm homicides and assault in crime data, for different post-intervention periods, Richmond, California

In the post-intervention period, comparing observed outcomes to those predicted in the absence of the program indicated that Operation Peacemaker was associated with increases in nonfirearm homicides and assaults. Specifically, the program was associated with 167 more nonfirearm homicide/assaults in crime data, corresponding to an average increase of 30 cases per year (main text Table 1). However, placebo tests (main text Table 1; Appendix Figures 1-4) indicated that this association was likely to be due to chance. Although the program was associated with increases in nonfirearm homicides/assaults in crime data in the first 3.5 post-intervention years and reductions in these outcomes thereafter (main text Figure 2), analyses dividing the post-period in two (July 2010-December 2013 and January 2014-December 2015; below) still suggested these associations were likely due to chance.

Appendix Table 5: Summary of generalized synthetic control results for the association of the Operation Peacemaker Fellowship with nonfirearm homicide and assault in crime data, for different post-intervention periods, Richmond, California.

	Nonfirearm homicides and assaults in crime data		
	Post-intervention period: July 2010 – December 2015	**Post-intervention period: July 2010 – December 2013**	**Post-intervention period: January 2014 – December 2015**
Observed cases post-intervention	4,950	3,387	1,563
Predicted cases post-intervention without Operation Peacemaker	4,783	2,999	1,784
Average annual post-intervention change in cases associated with Operation Peacemaker	+30	+111	-111
	Proportion of control place/jurisdiction-quarters more extreme, average, post-intervention		

All control units	452/753 (0.60)	210/753 (0.28)	143/753 (0.19)
< 20x MSPE	452/753 (0.60)	210/753 (0.28)	143/753 (0.19)
< 5x MSPE	452/753 (0.60)	210/753 (0.28)	143/753 (0.19)
< 2x MSPE	451/752 (0.60)	209/752 (0.28)	142/752 (0.19)

MSPE: Mean squared prediction error in pre-intervention period models.

Sensitivity analyses with post-intervention period restricted to July 2010 – December 2011

Because other violence prevention efforts (e.g. Ceasefire) scaled up beginning in 2012, we tested the sensitivity of our results to restricting the post-intervention period to July 2010 – December 2011. Results (below) suggest this restriction did not meaningfully alter the results.

Appendix Table 6: Summary of generalized synthetic control results for the association of the Operation Peacemaker Fellowship with firearm and nonfirearm homicides and assaults, with post-intervention period restricted to July 2010 – December 2011, Richmond, California.

	Homicides and assaults in health data		Homicides and assaults in crime data	
	Firearm	Nonfirearm	Firearm	Nonfirearm
Observed cases post-intervention	165	1,360	245	1,394
Predicted cases post-intervention without Operation Peacemaker	224	1,197	371	1,256
Average annual post-intervention change in cases associated with Operation Peacemaker	-40	+109	-84	+92
	Proportion of control place/jurisdiction-quarters more extreme, average, post-intervention			
All control units	4/622 (0.01)	6/630 (0.01)	41/641 (0.06)	367/753 (0.49)
< 20x MSPE	2/529 (0.00)	4/624 (0.01)	41/641 (0.06)	367/753 (0.49)
< 5x MSPE	2/529 (0.00)	4/615 (0.01)	38/638 (0.06)	367/753 (0.49)
< 2x MSPE	2/529 (0.00)	4/612 (0.01)	30/622 (0.05)*	366/752 (0.49)

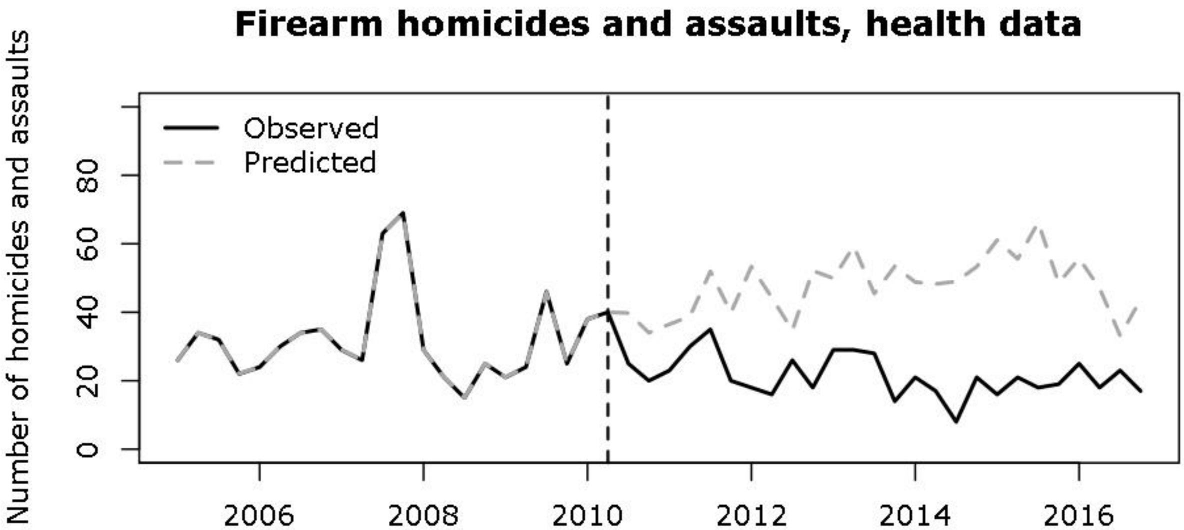
MSPE: Mean squared prediction error in pre-intervention period models.
*Proportion is <0.05 but rounded.

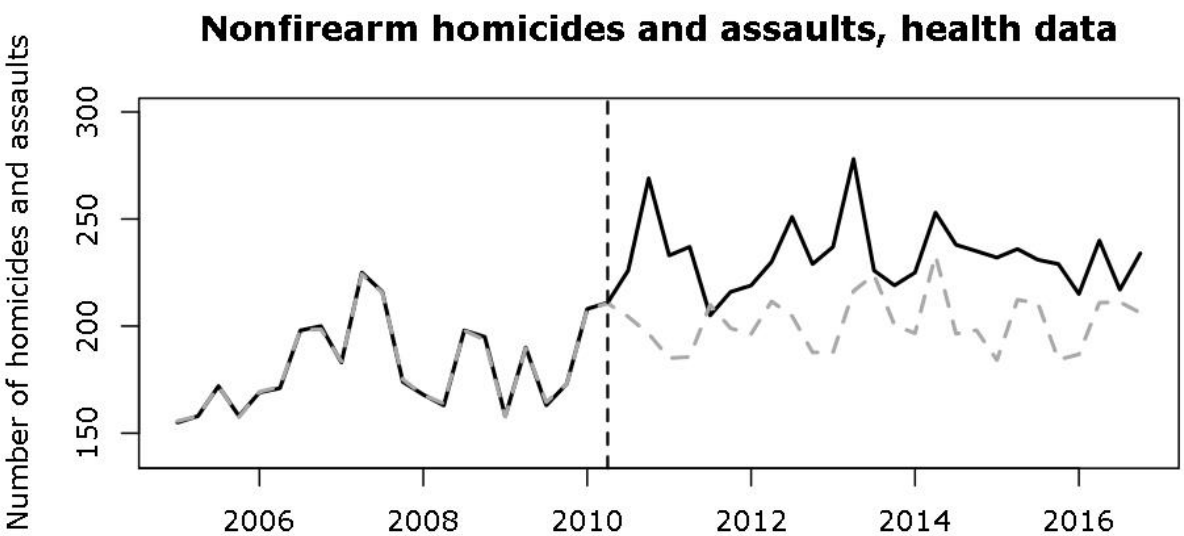
Sensitivity analyses using health data with covariates

The generalized synthetic control approach can include covariates, by first modeling the outcome as a function of measured covariates in each place and time, and then applying the synthetic control procedure to the intervention and control unit outcome residuals from this first-stage model. However, the geographic boundaries of law enforcement agencies are not always well-defined, making it difficult to assign appropriate place-level covariates (e.g. from Census data). Additionally, previous research has found that excellent and often superior pre-intervention fit is achieved without covariates (5–7). We tested the sensitivity of our results to inclusion of sociodemographic covariates predictive of violence in models of health outcomes, for which covariates can be directly assigned. Covariates were drawn from California Department of Public Health's Healthy Communities Data and Indicators Project and were yearly measures of: concentrated poverty rate, overall poverty rate, GINI coefficient for income inequality, percent of persons aged 25 and older with at least a high school degree or equivalent, percent of rented households spending more than 50% of monthly income (HUD-adjusted) on housing costs, percent of households that are female-headed with children under 18 and no husband, percent of households that were individuals living alone, percent of households that are overcrowded, percent registered and voted voters, ratio of total jobs to total housing, modified retail food environment index, 10-year change in number of households, and unemployment rate. Covariates that were highly correlated with others in this set were excluded (e.g. racial composition).

Adding sociodemographic covariates in analyses of health data (results below) made the associations with both firearm homicides/assaults and nonfirearm homicides/assaults somewhat stronger, but did not substantively alter the results.

Appendix Figure 5: Observed and predicted firearm and nonfirearm homicides and assaults, by quarter, from analyses of health data using covariates, before and after implementation of the Operation Peacemaker Fellowship, Richmond, California, 2005-2016.





The vertical dotted line indicates the initiation of the Operation Peacemaker Fellowship in June 2010.

Appendix Table 7: Summary of generalized synthetic control results for the association of the Operation Peacemaker Fellowship with firearm and nonfirearm homicides and assaults, in health data analyses with covariates, Richmond, California.

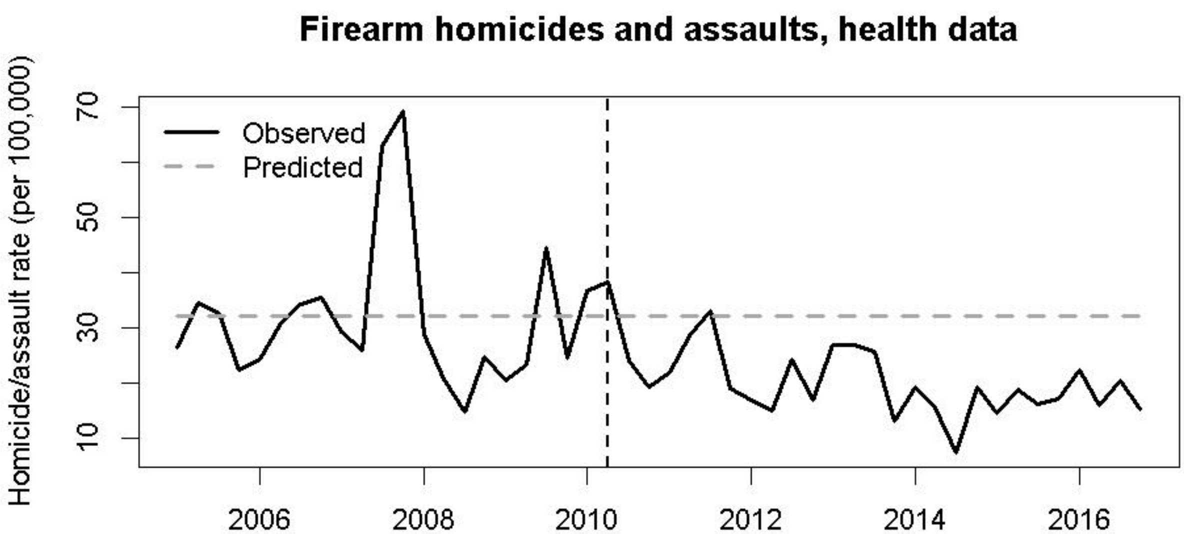
	Homicides and assaults in health data	
	Firearm	**Nonfirearm**
Observed cases post-intervention	555	6,060
Predicted cases post-intervention without Operation Peacemaker	1,244	5,240
Average annual post-intervention change in cases associated with Operation Peacemaker	-106	+126
	Proportion of control place/jurisdiction-quarters	
All control units	1/624 (0.00)	14/630 (0.02)
< 20x MSPE	0/622 (0.00)	14/630 (0.02)
< 5x MSPE	0/622 (0.00)	10/625 (0.02)
< 2x MSPE	0/622 (0.00)	9/619 (0.01)

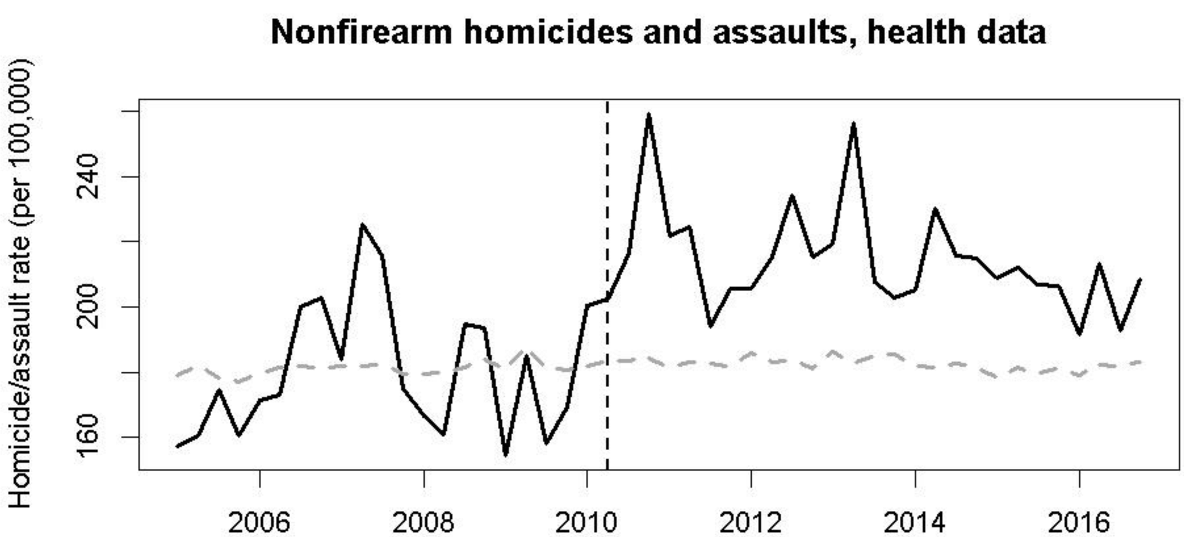
Post-intervention period: July 2010 – December 2016.
MSPE: Mean squared prediction error in pre-intervention period models.

__Sensitivity analyses of health data using rate outcomes__

We modeled outcomes as counts rather than rates, because for crime date, the geographic boundaries of law enforcement agencies do not always correspond to clear populations at risk, and for health data, models of counts achieved better pre-intervention fit than rates. Nevertheless, we present results for health data analyzed using rates as a sensitivity analysis. Results from this analysis were consistent with the main analysis results.

Appendix Figure 6: Observed and predicted firearm and nonfirearm homicides and assaults, by quarter, from health data analyses using rate outcomes, before and after implementation of the Operation Peacemaker Fellowship, Richmond, California, 2005-2016.





Appendix Table 8: Summary of generalized synthetic control results for the association of the Operation Peacemaker Fellowship with firearm and nonfirearm homicides and assaults, in health data analyses using rate outcomes, Richmond, California.

	Homicides and assaults in health data	
	Firearm	**Nonfirearm**
Observed rate post-intervention (per 100,000 person-quarters)	19.7	214.8
Predicted rate post-intervention without Operation Peacemaker (per 100,000 person-quarters)	32.0	182.5
	Proportion of control place/jurisdiction-quarters more extreme, average, post-intervention	
All control units	5/625 (0.01)	27/630 (0.04)
< 20x MSPE	5/580 (0.01)	27/630 (0.04)
< 5x MSPE	3/577 (0.01)	27/630 (0.04)
< 2x MSPE	2/574 (0.00)	14/543 (0.03)

Post-intervention period: July 2010 – December 2016.
MSPE: Mean squared prediction error in pre-intervention period models.

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