Quasi-experimental study finding no localised gun crime or call reduction after gun buybacks in Philadelphia

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ABSTRACT

Introduction Gun buyback programmes have been popular in the USA since the 1970s. Studies show that they have no effect on citywide gun crime rates, but more microlevel examinations around gun buyback locations have not been conducted. This study tests for local effects of 34 Philadelphia, PA buyback events at 30 locations between 2019 and 2021.

Methods We analysed all gun-related crime events and gun-related calls for service attended by the police from 2019 to 2021. Multilevel models with an autoregressive residual structure were estimated on weekly gun crime and call event intensity (inverse distance weighted) totals across a range of distances (4000–8000 feet). Impacts of a gun buyback event were estimated for 1–4 weeks postevent.

Results Statistically significant weekly increases in gun event intensity are associated with seasonality and after the murder of George Floyd. Gun event intensity was not significantly affected by gun buybacks. Across 20 sensitivity tests of different distances and time periods (4000–8000 feet and between 1 and 4 weeks), gun buybacks were not statistically associated with any localised reduction in the intensity of gun crimes and calls.

Conclusions Extant research has failed to uncover any effect of gun buybacks on citywide gun crime rates. The current results now contribute a lack of evidence at the local level to this literature. While gun buybacks remain popular with politicians and the public, this study adds to the ongoing question of whether buyback funds could be better spent more effectively.

WHAT IS ALREADY KNOWN ON THIS TOPIC

⇒ Gun buyback programmes have been popular since the 1970s, but scholarly research has found that they have no citywide impact on gun crime. Prior studies have not examined calls-for-service to the police or explored these data at more localised scales than citywide.

WHAT THIS STUDY ADDS

⇒ The study adds gun-related calls for service to gun crime data and examines the intensity of events in the immediate local aftermath of gun buybacks across a range of spatial and temporal scales.

HOW THIS STUDY MIGHT AFFECT RESEARCH, PRACTICE OR POLICY

⇒ The results confirm that the absence of a citywide effect does not mask any positive local spatial and temporal effects. It reinforces that gun buybacks are not an effective crime prevention tool at the neighbourhood or citywide level.

INTRODUCTION

Gun buyback programmes are popular with politicians and frequently supported by police departments. The earliest gun buyback programme occurred in Baltimore, Maryland, USA in 1974[1] and the state of knowledge, as well as the arguments both for and against gun buybacks, has changed little since Plotkin's[2] book on the topic. Buyback programmes are theoretically structured around two hypotheses. First is the general principle that communities that have more guns will have more gun violence. There is little dispute in the scientific literature regarding this hypothesis.[3] The second hypothesis is that 'offering cash for guns in a city will reduce the number of incidents in which guns are used in crime in that city' (3–30).[4] Finding evidence to support this second hypothesis has proved more difficult. Braga and Wintemute[5] add a third possibility, that gun buyback events raise the awareness of such issues, resulting in positive consequences.

Callahan *et al*[6] found no significant decrease in violent crime or firearm-related events in relation to a Seattle buyback programme as measured by police reports and medical records. Rosenfeld[7] examined the effects of two St. Louis buybacks in 1991 and 1994, also finding no significant effect on violent crime. And in a recent review, Ferrazares *et al*[8] found more than 300 events in 277 US cities between 1991 and 2015, recovering on average 397 firearms per buyback, but concluded there was no evidence of a significant decrease in gun crimes at the city level. This all echoes the conclusion of Sherman *et al*[4] (3–30) who similarly concluded that while the 'scientific rigour of the buyback evaluations is not great', the four studies they reviewed provided 'moderate evidence of no effect'.

Sherman *et al*[4] propose three general reasons why we have not seen evidence of citywide benefits to gun buyback programmes. First, a buyback can attract weapons from beyond the target city.[9] Second, surrendered firearms may be weapons that have been stored at home and not carried in public. And third, there is the potential unintended consequence that potential offenders may use the buyback money to upgrade to a more lethal weapon.

Guns surrendered in buybacks are not likely to be the crime guns related to shootings and homicides. Kuhn *et al*[10] examined the specifics of guns brought to a Milwaukee buyback, finding that the characteristics of the guns brought in did not match the characteristics of weapons typically used in crime or suicide. Baumann *et al*[11] found that while the demographics of those turning in weapons were similar to those most at risk to commit suicide, they were not similar to those who were most at risk to commit homicide.

The extant crime reduction research has focused on citywide impacts, but those studies may mask effects at smaller spatial scales. In this study, we explore whether a citywide exploration is too coarse to detect effects that may be occurring at a more microlevel. We analyse gun buybacks at distances up to 8000 feet within Philadelphia from 2019 to 2021, using a series of Thiessen polygons to connect gun related events to their closest buyback location.

DATA

Philadelphia, Pennsylvania, is the sixth-largest city in the USA by population. As of 1 July 2021, the 5-year estimates from the American Community Survey estimated the city had 1 576 251 residents; Of those residents, 40.8% were black non-Hispanic, 38.5% were white non-Hispanic and 15.4% were Hispanic. The median household income of US$52 649 is one of the lowest for large US cities and 23% live in poverty.[12]

Crime and call for service data, covering 2019 to the end of 2021, were accessed through a collaborative agreement with the Philadelphia Police Department. Crime data included all gun-related murders, robberies and aggravated assaults. The call-for-service database was searched for calls related to person with a gun, person with a weapon and gunshots being heard or observed. The recorded final disposition of these calls was checked to remove events where the officer attending found no evidence of a gun being used or present. Additionally, any incidents that originated from police were removed. This is because the use of hotspots policing[13–15] will focus police resources in high crime areas, and their discovery of gun crime would potentially skew data recording. Citywide counts for gun crime and call for service data show a gradual increase over time, with a spike for half a year in the months following the murder of George Floyd (figure 1).

Gun buyback dates and details were provided by the Philadelphia Police Department's Office of Forensic Science and cross-checked against social media and newspaper reports. Twice, we identified two locations that were less than 800 feet from each other, and so a central point between them was used. This eventual dataset contained 34 buyback events coded to 30 locations. The total number of guns surrendered across all 34 events was 1059 firearms, comprising 502 revolvers, 270 semiautomatic handguns, 134 shotguns, 152 rifles and 1 AR-15. These guns were purchased at a total cost of US$93 350. An average gun buyback event resulted in the surrender of approximately 32 firearms per event at a gun recovery cost of US$2745 (15 revolvers, 8 semiautomatic handguns and 4 shotguns per event), though two events did not result in the recovery of any firearms. This figure does not include costs associated with police time, facilities or weapon disposal.

A weekly dataset of gun buybacks was binary coded depending on whether a gun buyback occurred in that week. If a gun buyback occurred on a Saturday, that week (from 6:00 hour on that Saturday to 5:59 hour on the subsequent Saturday) was coded 1. To explore temporal sensitivity, we also examined if there was any effect of gun buybacks on subsequent periods, extending our analysis to include up to 4 weeks after a buyback.

Recorded gun events were assigned their nearest buyback location by a point-in-polygon operation linking events to a gun buyback Thiessen polygon (also called a Voronoi diagram).[16 17] This spatial constraint meant that each gun event could contribute a score to only one buyback site, thus retaining the independence

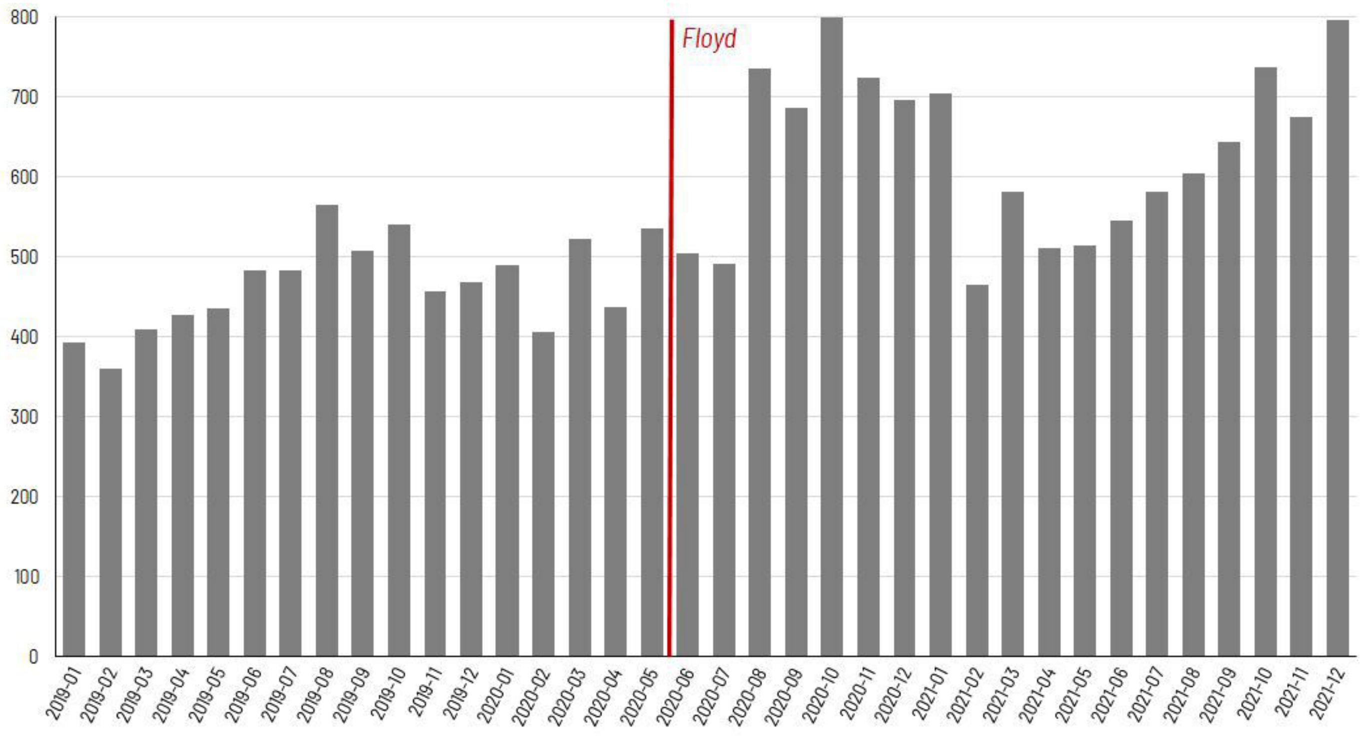

Figure 1 Monthly frequency of gun crime and calls for service, Philadelphia, Pennsylvania, USA, 2019–2021.

of observation that is important to regression analysis. Each event was weighted with a linear inverse distance metric based on a maximum distance (bandwidth) from the buyback location (range 0–1) such that the intensity of gun events each week at a location was:

$$\lambda_i = \sum_{d_j<k} 1 - \left(\frac{d_j}{k}\right) \quad (1)$$

Where λ_i is the gun event intensity at location i; d_j is the Euclidean distance from location i to a gun event j; and k is the bandwidth of the search parameter in feet. In this way, events closer to the gun buyback are assigned a higher value than events farther away. To explore spatial sensitivity to the choice of bandwidth, five different bandwidth distances from 4000 feet to 8000 feet (in 1000 foot increments) were used. Distances less than 4000 feet were explored (in preliminary analyses not reported), but ruled out because they tended to have excessive numbers of zero counts due to a lack of events in such short distance/time bands. The models were severely underpowered (see power calculations later in the article), and as such, statistical models subsequently struggled to converge.

All gun buyback events, except for one, were held on Saturdays. As such, events were assigned to the week (starting at 6:00 hours on the Saturday) in which they occurred. Four additional fields were explored. Linear and quadratic dummy variables were added to control for changing trends over time. The linear variable captured the linear trend of crime in the city, while the quadratic variable captured any curvilinear temporal deviation in this trend. A system 'shock' occurred in spring of 2020 when the combined effects of the pandemic and civil unrest in the wake of the murder of George Floyd in Minneapolis, Minnesota, USA saw a subsequent increase in violence in many cities, including Philadelphia.[18 19] Consequently, a dichotomous variable (0=premurder of George Floyd, 1=post) was added to compensate for these events. Lastly, because of consistent evidence of increased violence in seasonably warmer periods[20] the average weekly temperature was included as a seasonality control from climate data available online from the National Oceanic and Atmospheric Administration (ncei.noaa.gov/cdo-web/). The temporal trend and seasonal controls were mean centred.

ANALYTICAL STRATEGY

Hierarchical linear modelling recognises nested data structures,[21] such as repeated observations across individuals or neighbourhoods.[22] In this example, we have gun crime intensity scores per week in the vicinity of gun buyback locations. Because the sites serve other purposes (many are religious institutions), they do not have buyback events most weeks. At these times, the sites act as control locations both for themselves as well as other sites that have a gun buyback event during that week or weeks.

An initial model was estimated absent any predictors and compared with an unconditional model. Then variables were added to the model building process in steps of increasing complexity, using Bayesian information criterion (BIC) values and Akaike information criterion (AIC) values to confirm model improvement of fit. When the AIC and BIC disagreed, BIC was chosen on the grounds that, as Shmueli[23] (300) observes, 'AIC measures predictive accuracy while BIC measures goodness-of-fit', and goodness-of-fit is the preferred goal of this article.

Because time periods in panel data are not independent of previous time points,[24] the residual errors were estimated with an autoregressive (AR1) autocorrelation coefficient[25] to compensate for expected autocorrelation. A φ value of between 0.1 and 0.13 was consistent across models, suggesting modest serial autocorrelation in the residuals. The linear and quadratic coefficients were not significant in any model, indicating an absence of long-term trend evident at the local spatial level around buyback sites. Given figure 1 suggests an increasing linear trend citywide over time, and considering the model specification discussion from Berk *et al*,[26] we retained the linear variable in the model. The significant temporal characteristics of the analysis were the impact of the murder of George Floyd, temperature (reflective of seasonality), and the serial autocorrelation mentioned above.

With small-area spatial analyses conducted across small time windows of a week, it was not surprising to discover that smaller spatial bandwidths (the distance within which events were considered associated with a buyback) generated substantial zero value observations. Even when events were detected, this resulted in generally low intensity scores. Statistical power to detect a change was therefore a consideration. Models were estimated across different spatial bandwidths (4000–8000 feet) and temporal scales from 1 to 4 weeks in duration. In this way, we examined variations of different spatial and temporal dimensions to reduce the possibility that any results were sensitive to certain space-time parameters. As an example of descriptive values, table 1 shows mean (and SD) weekly incident counts within 7000 feet of each of the gun buyback sites across 156 weeks. Mean weekly incident counts (out to 7000 feet) range from 0.891 at site 2 to 7.481 at site 30. When gun event values are converted to an intensity score and summed for the week, the descriptive statistics across 156 weeks are shown in table 2.

Each iteration (by spatial and temporal bandwidth) consisted of a crime intensity file with 156 weeks across 30 buyback locations (n=4680 observations). One value was winsorised as an extreme outlier related to a single, high-profile incident where a barricaded suspect fired hundreds of rounds at police during a 7-hour standoff on 14 August 2019, injuring six police officers.[27]

The model that converged across all weeks and bandwidths is shown in equation (2):

$$\begin{aligned} Y_{itk} = \; & \beta_{0i} + \beta_1 \left(floyd\right)_{it} + \beta_2 \left(linear\right)_t \\ & + \beta_3 \left(temp\right)_t + \beta_4 \left(buyback\right)_t + u_{0i} + \varepsilon_{it} \end{aligned} \quad (2)$$

Where Y_{itk} is the intensity score of gun events occurring in week t within k feet of gun buyback location i; β_0 is the intercept comprising the mean gun incident intensity score around buyback site i; β_1 is the slope coefficient for the binary time-varying parameter for the murder of George Floyd at buyback site i; β_2 a mean-centred fixed slope coefficient absorbing any linear trend; β_3 a fixed slope coefficient adjusting for the average weekly temperature in Philadelphia; β_4 the coefficient representing the fixed effect of a buyback event; u_{0i} represents any unexplained systematic differences between gun buyback locations; and, ε_{it} any remaining residual deviation in intensity score totals within each buyback site. This means that the model allows each buyback location to have its own random intercept, and each site has its own random slope modelling effects stemming from the murder of George Floyd. The merit of the Floyd variable being specified as a random effect is supported by evidence that postpandemic/George Floyd violence impacts have differentially and more negatively affected minority communities.[19 28 29] Finally, there are three fixed effects: the centred mean average temperature, centred mean linear trend, and the effect of having a buyback programme. The choice of a fixed effect for the treatment is due to the desire to identify any overall policy relevance of gun buybacks at the local spatial level, rather than isolate specific site characteristics.

Table 1 Mean and SD for weekly gun event counts within 7000 feet of each of 30 gun buyback sites

Buyback site	Mean	SD	Minimum	Maximum
1	4.051	2.659	0	18
2	0.891	1.221	0	6
3	2.026	1.541	0	7
4	5.776	3.081	0	15
5	2.282	5.552	0	69
6	2.064	1.718	0	8
7	7.096	4.091	0	20
8	1.103	1.159	0	4
9	2.724	1.992	0	9
10	1.071	1.176	0	5
11	1.776	1.361	0	7
12	6.449	3.312	1	19
13	5.500	2.919	0	18
14	2.878	2.168	0	13
15	2.340	1.976	0	11
16	4.212	2.588	0	15
17	6.372	3.551	0	20
18	5.724	2.955	0	21
19	3.212	2.223	0	11
20	1.083	1.147	0	5
21	4.218	2.515	0	13
22	1.917	1.549	0	9
23	2.147	1.532	0	7
24	5.590	2.929	0	15
25	2.968	1.871	0	9
26	1.660	1.488	0	8
27	6.923	3.609	0	22
28	1.026	1.124	0	5
29	2.250	1.848	0	11
30	7.481	3.838	0	21

Note: The large maximum value for site 5 is an outlier event. It is left as the maximum here to show the scale of the event; however, in table 2, the outlier high intensity value was winsorised to 13.851.

Table 2 Mean and SD for weekly event intensity within 7000 feet of each of 30 gun buyback sites

Buyback site	Mean	SD	Minimum	Maximum
1	1.548	1.177	0.000	8.285
2	0.649	0.892	0.000	4.236
3	1.126	0.997	0.000	5.241
4	3.108	1.864	0.000	9.793
5	1.745	5.055	0.000	13.851
6	1.107	0.984	0.000	5.014
7	4.203	2.635	0.000	12.781
8	0.654	0.763	0.000	3.050
9	1.791	1.318	0.000	6.421
10	0.760	0.889	0.000	4.155
11	1.094	0.893	0.000	4.196
12	3.758	2.050	0.474	11.317
13	3.689	1.997	0.000	11.937
14	1.746	1.354	0.000	8.500
15	1.483	1.297	0.000	6.805
16	2.341	1.680	0.000	10.142
17	3.873	2.224	0.000	12.849
18	3.707	1.974	0.000	13.851
19	1.809	1.395	0.000	9.282
20	0.837	0.916	0.000	4.297
21	2.638	1.619	0.000	7.788
22	1.236	1.031	0.000	6.136
23	1.193	0.934	0.000	4.331
24	3.317	1.969	0.000	10.800
25	1.915	1.246	0.000	6.889
26	1.103	1.058	0.000	4.994
27	3.427	1.961	0.000	11.117
28	0.351	0.460	0.000	2.291
29	1.132	1.030	0.000	6.459
30	3.191	1.770	0.000	8.961

Note: The maximum value for gun buyback site 5 has been winsorised.

RESULTS

As an example of the model building process, table 3 shows the results when looking at event intensity within 7000 feet and testing the possibility of an effect in the 3 weeks after a gun buyback event. Model 1 estimates the unconditional model, indicating the location variation in weekly intensity value in the immediate neighbourhood around buyback sites that is evident from table 2. Model 2 adds the Floyd dichotomous variable as a random effect. There is a statistically significant Floyd effect of a weekly increase in gun event intensity around sites of 0.549 on average, though substantial variance between sites (0.45). The AR1 φ parameter indicates there is a modest component of temporal autocorrelation from week to week.

Model 3 adds the centred average weekly temperatures and linear trend. Rising temperatures are associated with increases in local gun violence intensity, and the linear trend is retained due to its theoretical significance to model specificity.[26] Finally, the effect of the buyback events are added in model 4. In the case of this example, for up to 3 weeks after the event. In this example, the coefficient is small, and the result is not statistically significant ($p=0.831$).

Table 3 shows the example when examining the potential effect of a gun buyback over 3 weeks, on gun event data with a maximum bandwidth of 7000 feet. As stated earlier, to examine the possibility of the modifiable areal unit problem,[30 31] as well as variations due to sensitivity in the choice of temporal effects,[32] we explored the data across four time periods (1–4 weeks) and five bandwidth distances (4000–8000 feet). Table 4 shows that none of the beta coefficients from equation 2 are anywhere close to statistical significance.

Study power

While the p values in table 4 are clearly indicative of a lack of effect of gun buybacks in localised gun event intensity at any distance between 1 and 4 weeks, one note of caution should be mentioned. At the immediate local level of within a few thousand feet of a location during any week, there is not as much gun-related crime or calls for service as one might expect for a high crime city. With this in mind, power calculations calibrated to detect a 20% reduction in violence intensity show increasing experimental power as the number of weeks continues, but it should be recognised that the low number of incidents in any particular single week certainly limit the capacity of this study to detect any measurable effect. Conventional experimental power (80%) is only available for longer time frames and greater distances (table 5, based on 100 simulations).

Table 3 Multilevel models estimating weekly gun event intensity score around gun buyback sites with buyback effects predicted to last 3 weeks out to 7000 feet

	Model 1		Model 2		Model 3		Model 4	
	β	SE	β	SE	β	SE	β	SE
Fixed effects								
Intercept	2.007	0.210***	1.716	0.169***	1.800	0.174***	1.799	0.174***
Floyd			0.548	0.095***	0.389	0.126**	0.390	0.128**
Linear					0.002	0.001	0.001	0.001
Temperature					0.007	0.002***	0.007	0.002***
Gun buyback							0.033	0.156
Random effects (SD)								
Neighbourhood	1.142		0.904		0.904		0.904	
Floyd			0.450		0.451		0.451	
Variance components								
φ			0.121		0.117		0.118	
Log likelihood	8557		8387		8376		8376	
AIC	17 120		16 788		16 770		16 772	
BIC	17 140		16 834		16 828		16 837	

Notes: N=4680 (156 weeks across 30 groups).
*p<0.05, **p<0.01, ***p<0.001 (two-tailed tests).
AIC, Akaike information criterion; BIC, Bayesian information criterion.

DISCUSSION

This is, to the best of our knowledge, the first study to explore an immediate neighbourhood level effect of gun buyback programmes on the number of gun-related crimes *and* calls for service to the police in the aftermath of gun buyback events. Regardless of the spatial and temporal range examined, we find that buybacks for community firearms do not have any localised effect on gun crimes and calls. Our research, therefore, adds microlevel confirmation to the existing literature that finds the absence of an effect at the citywide level.

As stated in the previous section, we recognise the caveat that the relative scarcity of gun-related events at the microspatial and temporal levels used in this study resulted in some of the tests having low to moderate power; however, given the p values detailed in table 4, we find it unlikely that the tests at shorter spatial frames would have found any effect. This lack of effect would be exacerbated if the results were interpreted even more conservatively with a Bonferroni-type correction for the experiment-wise error rate.

We also recognise that with an average of 32 guns surrendered at gun buyback events, the numbers of weapons recovered pales in relation to the number of guns in America. As such, this constitutes a low dosage exploration of gun buyback events. We should also point out that we are not examining injuries or fatalities resulting from suicide or accidental gun use, but simply focusing on the primary goal of most advertised gun buyback events which is the reduction in criminal activity.

So why did we do this research, instead of following the advice of Sherman *et al*[4] (3–30) that 'there seems little reason to invest in further testing of the idea'? First, while not optimistic (given the extant literature) we were genuinely open to the possibility that there might be a localised benefit. Second, regardless of the scientific research, gun buyback programmes remain popular. A Google Search for 'gun buyback' in early April 2023 returned more than 4 million hits, and in May 2021, H.R.3143 was introduced to the 117th Congress designed to direct 'the Office of Justice Programmes within the Department of Justice to establish a gun buyback grant programme for state, local and tribal law enforcement agencies'.[33]

Table 4 Buyback beta coefficient (β_4), SE and p value for all equation (2) models

Distance	Weeks	β_4	SE	P value
4000	1	0.056	0.171	0.744
	2	0.050	0.128	0.697
	3	0.012	0.107	0.911
	4	0.066	0.094	0.483
5000	1	0.070	0.200	0.727
	2	0.027	0.151	0.858
	3	0.019	0.127	0.883
	4	0.059	0.112	0.595
6000	1	0.086	0.225	0.703
	2	0.017	0.170	0.918
	3	0.032	0.143	0.822
	4	0.064	0.126	0.610
7000	1	0.104	0.245	0.672
	2	0.016	0.185	0.932
	3	0.033	0.156	0.831
	4	0.080	0.138	0.563
8000	1	0.123	0.261	0.637
	2	0.020	0.198	0.919
	3	0.031	0.167	0.853
	4	0.098	0.148	0.508

Table 5 Power estimates to detect a 20% reduction in violence intensity

Distance (feet)	1 week	2 weeks	3 weeks	4 weeks
4000	35	48	74	75
5000	26	53	69	81
6000	32	61	77	87
7000	29	64	83	91
8000	31	72	81	87

While gun buybacks are locally feasible and often perceived as relatively low cost with strong public support,[34] it remains important to reiterate the lack of scientific validity for this public policy. In the USA at least, gun buybacks do not reduce gun crime. As Mullin[35] argues, from an economic perspective gun buybacks that occur regularly can reduce the cost of gun ownership by raising the liquidation value of a firearm and thus reduce the cost of ownership. Reduced ownership costs could stimulate increased gun purchases. This would have an iatrogenic effect counter to the goals of policy-makers. Given this article is yet another voice in the chorus of research pointing out the ineffectiveness of gun buybacks for crime prevention, we reiterate the conclusion of Charbonneau[34] (1) who stated 'policy-makers and community groups should consider whether the scarce resources allocated to gun buybacks—even if these resources are minimal—might be better spent on more-promising violence prevention efforts'. The evidence remains unequivocal in that, at least in the USA, gun buybacks are ineffective and a distraction from more effective crime prevention policies.

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