The Effects of Street Cleaning on Serious Crime and Gun Violence: A Block Randomized Controlled Trial

John M. MacDonald[a] | Wilson Hernandez[a] | Anthony A. Braga[a] | Charles C. Branas[b]

[a]Department of Criminology, 566 McNeil Building, University of Pennsylvania, Philadelphia, PA 19104, USA. Email: johnmm@upenn.edu

[b]Department of Epidemiology, Columbia University, New York, NY

ABSTRACT

This paper presents results from a block randomized controlled trial (RCT) in Philadelphia, Pennsylvania, evaluating the impact of street cleaning interventions on gun violence and street crimes. The intervention, implemented on 150 high gun-violence street segments, utilized the Pennsylvania Horticultural Society's LandCare Block Cleanup (LCBC) program, providing biweekly and monthly trash removal, weeding, and sidewalk sweeping. A subset of segments received enhanced services, including free trash and recycling bins. While the intervention significantly reduced visible litter by 16%, it yielded statistically insignificant effects on gun violence and street crimes. These findings suggest that basic environmental cleanups alone may be insufficient to reduce violence, emphasizing the need for more comprehensive place-based crime prevention strategies. Interventions addressing broader criminogenic factors, such as abandoned structures and open-air drug markets, are likely necessary. This research provides actionable insights for policymakers and urban planners seeking scalable, non-policing approaches to crime reduction.

Keywords: Place-based crime prevention, gun violence, disorder, litter.
JEL Codes: K42, R11, I18.

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1. Introduction

It is, by now, a criminological axiom that crime is geographically concentrated at small places in American cities (Sherman, Gartin, & Buerger, 1989; Weisburd, 2015). In Philadelphia, for example, it is estimated that only 3% of street segments accounted for 50 percent of all shootings between 2019 and 2023.[1] Within the Kensington neighborhood of Philadelphia, just two street segments[2] experienced 43 shootings during this period. The spatial concentration of gun violence in these select places also appears to be relatively stable over time across major U.S. cities, even during the epidemic rise in shootings during the COVID-19 pandemic (MacDonald et al., 2023). The policy implication of this empirical result is straightforward - if cities are to better control violent crime, policy makers need to focus resources on these places of concentrated crime (or "hot spots").

The uneven distribution of crime at small places within and across neighborhoods is well supported by prominent criminological theories. Crime opportunity theories, such as routine activity (Cohen & Felson, 1979), rational choice (Cornish & Clarke, 1986), and environmental criminology (Brantingham & Brantingham 1991), suggest place characteristics, situations, and dynamics cause criminal events to concentrate at specific locations. While the causal connections between disorder and subsequent crime remain murky (e.g., see Weisburd et al., 2015), broken windows theory has been applied to understand how increasing social disorder (e.g., public drinking, loitering, etc.) and physical disorder (abandoned buildings, vacant lots, trash, etc.) reduces informal social control, attracts serious offenders, and escalates crime in specific places (Wilson & Kelling, 1982; Skogan, 1990). Social disorganization theories, typically applied to

[1] Authors' calculations from linking shootings to street blocks for years 2019-2023.

[2] These street segments were the 800 Block of Allegheny Ave (n=24) and the 700 Block of Clearfield Ave (n=19).

understand crime variations at the city-level (Baumer, 1994) and neighborhood-level (Sampson, Raudenbush, & Earls, 1997), have been used to show how physical disorder, unsupervised juveniles, and diminished collective efficacy in local settings influence persistent block-to-block crime variations within neighborhoods (Weisburd, Groff, & Yang, 2012).

Randomized controlled trials and rigorous quasi-experimental evaluations support hot spots policing as an effective crime prevention strategy (National Academies, 2018), with mounting evidence that problem-oriented policing interventions, done in conjunction with communities and residents and designed to remediate underlying place-based conditions, generate larger preventive impacts than enforcement interventions alone (Braga & Bond, 2008; Braga et al., 2019). Social scientists, policy makers, and community activists have long expressed concerns that oversue of policing strategies risks generating an erosion in trust of municipal institutions and leadership, unnecessary abuse and violence, and increased exposure to the criminal justice system in structurally disadvantaged minority communities (see e.g., Alexander, 2010; Hinton, 2016). Community demand to reduce the harm generated by overly aggressive policing approaches coupled with deep concerns about the trajectory of the post-COVID gun violence epidemic fuelled a resurgence of interest in non-law-enforcement alternatives to prevention. As a result, public health and urban planning efforts to reduce gun violence through place-based change were prominently featured amongst community violence interventions recommended by the U.S. Department of Justice, American Public Health Association, and the Centers for Disease Control and Prevention.[3]

[3] http://bja.ojp.gov/program/community-violence-intervention/overview (accessed December 14, 2024); https://www.apha.org/topics-and-issues/gun-violence (accessed January 2, 2025). https://www.cdc.gov/firearm-violence/prevention/index.html (accessed January 13, 2025)

Recent experimental studies have demonstrated that trash pick-up, housing and vacant lot remediation, and additional streetlights can reduce violence and serious crime in places where crime is concentrated (MacDonald, Branas & Stokes, 2019; MacDonald et al., 2024). A randomized experiment of cleaning and greening vacant lots in high-poverty areas, along with biweekly maintenance including trash pickup found it caused reductions in overall crime, gun violence, burglary, and shootings (Branas et al., 2018; Moyer et al., 2019). A randomized experiment of installing working windows and doors on abandoned houses in Philadelphia found the basic repairs were associated with significant reductions in gun violence, an effect that appears to be mediated by the observed physical repairs on homes (MacDonald et al., 2023; South et al., 2023). On the other hand, the study found that only trash cleanup around vacant and abandoned houses produced no reductions in gun violence.

Cleaning up litter and trash on the streets in high gun violence areas could theoretically impact gun violence and street crimes by shifting routine activities (Clarke & Felson, 1979) or the situational dynamics that lead to gun violence (Clarke, 1995). Litter and trash cleanups could also signal that a block is being cared for by residents, reduce incivilities, and boost a block's ability to engage in shared expectations of social control (Lanfear, Matsueda & Beach, 2020). From a policy perspective, if cleaning up trash and litter on streets helps reduce serious crime, it could generate more support as a non-policing intervention. Street cleaning and litter removal are basic services routinely provided by local governments and nonprofits in business districts, making them easy to scale with additional funding. In fact, research shows that business improvement districts focused on litter removal and private security experience significant reductions in serious crime and violence (Cook & MacDonald, 2010).

This experimental evaluation seeks to determine whether basic street cleaning (trash/litter removal, weeding, and sidewalk sweeping) impacts gun violence and street crime on high gun violence street segments in Philadelphia, PA, and whether the effects vary by frequency of cleanups. A block randomized controlled trial (RCT) was used to evaluate the effects the Pennsylvania Horticultural Society's (PHS) LandCare Block Cleanup (LCBC) program on gun violence and street crime. By providing new evidence on physical disorder and crime, this study makes three contributions. First, it is the first RCT to focus specifically on estimating the causal relationship between a standalone street cleaning / trash removal program and gun violence and street crimes. Second, this study assesses, for the first time, whether the causal relationship between litter/trash and serious crime varies based on treatment dosage, that is, the frequency of cleanups. Finally, the study estimates if the causal impact between cleaning up litter and trash on serious crime is impacted by blocks receiving enhanced cleanup services such as trash and recycling bins, alley cleanings, and limited tree plantings and vacant lot cleanups.

2. Methods

2.1. Research Design

A block RCT with two treatment arms was used to test the effects of the PHS LCBC program on gun violence hot spots in Philadelphia.[4] Following best practices in designing place-based trials with relatively small sample sizes (Weisburd & Gill, 2014), block randomization was used to increase the statistical power of the RCT. Street segments (defined as the block faces between two intersections; see Weisburd, Groff, & Yang, 2012) were the units of analysis in the block RCT. With funding from the Neubauer Family Foundation, PHS hired and managed five

[4] The study protocol was approved by the University of Pennsylvania Institutional Review Board and the analysis plan was pre-registered prior to the start of the intervention (https://doi.org/10.17605/OSF.IO/Q46XW).

contractor teams to conduct cleanups at 150 street segments for a six-month intervention period. These contractors could conduct cleanups that included picking up all trash from the sidewalk and curbline, removing weeds and vegetation, maintaining sidewalk tree pits, and cleaning the street gutters. They performed these tasks biweekly for 75 street segments (the first treatment group) and monthly for another 75 street segments (the second treatment group). PHS was also able to deliver enhanced cleanup services (such as providing free trash bins to residents and vacant lot remediation) for an additional 25 street segments in the biweekly group.

All city street segments were ranked ordered by total gun crime incidents[5] reported to the Philadelphia Police Department (PPD) between January 2020 and January 2023. The top 360 street segments were initially selected for possible inclusion in the block RCT. We chose to cut-off at the top 360th segment to ensure that after exclusion criteria we included street segments that were in the top of the city for gun violence. One street segment was immediately excluded due to the presence of the Penn Presbyterian Medical Center, a trauma center that included PPD reports of gun violence that occurred elsewhere in the city. Between April 2023 and May 2023, PHS reviewed the other street segments to determine whether these units were eligible for inclusion in the study. This review led to the exclusion of ten additional segments due to pre-existing daily street cleaning (n=2) or the presence of a police station (n=1), a hospital (n=2), a shopping center (n=1), a Philadelphia Housing Authority parking lot (n=1), a transit center (n=1), a railyard (n=1), or a short one-way street (n=1).

[5] Gun crime incidents included shootings, gun assaults, robberies committed with guns, and firearm violations. Shooting incidents involve victims who suffered a fatal or nonfatal gunshot wound. Gun assaults include shootings as well as incidents involving victims who reported that they were fired upon and not wounded or threatened with a gun that was not fired. Firearm violation incidents include illegal gun carrying, illegal gun possession, illegal firearm storage, and other violations. All PPD crime data are available at https://opendataphilly.org/datasets/shooting-victims/; https://opendataphilly.org/datasets/crime-incidents/. PPD analysts confirmed that these data represented the universe of reported incidents of gun violence in Philadelphia. Over 98% of incidents have an x-y coordinate.

After these exclusions, the remaining street segments were once again ranked order based on total gun crime counts from 1 (highest) to 349 (lowest), excluding segments ranked from 301 to 349 from the experimental design. Fifteen strata of 20 street segments each (i.e., the statistical blocks) were then created based on rank order from the highest (1) to lowest rank (300). A randomized uniform variable was then generated for each segment, and segments were sorted within each stratum into experimental groups by this randomized variable. Street segments were assigned to biweekly cleaning based on being in sequence 1 to 5, monthly cleaning based on being in sequence 6 to 10, and control based on being in sequence 11 to 20. The result was the random assignment of 75 segments to biweekly cleaning, 75 segments to monthly cleaning, and 150 segments to the no-treatment control group. Study street segments were then randomly selected for enhanced PHS services within the three experimental groups. Another randomized uniform variable was generated, and segments in the three groups were sorted on this variable. Segments were assigned to be eligible to receive enhanced PHS services in the biweekly group if they were in sequence 1 to 25 (n=25), in the monthly group if they were in sequence 1 to 25 (n=25), and in the control group if they were in sequence 1 to 50 (n=50). Only the 25 street segments in the biweekly services treatment arm received enhanced PHS services.

Residents were informed of the LCBC cleanup program prior to the July 2023 start of the six-month intervention period. Biweekly and monthly street cleanups followed a specific schedule with timing at specific locations coordinated by the research team. The five PHS contractors reported that more than 2,700 40-gallon trash bags were collected from the treatment street segments during this six-month intervention. All households from the 25 enhanced services biweekly cleanup street segments received fliers inviting them to sign up for free trash and recycling bins. Residents living in 110 households from 19 different biweekly cleaning street

segments signed up, resulting in the delivery of 125 35-gallon trash bins with wheels and 125 23-gallon recycling bins with lids. PHS also cleaned up 17 alleyways on ten street segments that were overgrown with vegetation and trees, remediated five vacant lots on three street segments, and pruned 28 street trees and mulched the tree pits on two street segments. Figure A1 in Appendix A shows the locations of treatment and control street segments in the Philadelphia LCBC block RCT.

2.2. Key Variables and Balance Assessment

The primary dependent variables were total gun crime, fatal and nonfatal shooting, gun assault, gun robbery, and firearm weapon offense incident counts per month on each street segment during the six-month pre-intervention period (January 2023 – June 2023), six-month intervention period (July 2023 – December 2023), and six-month post-intervention period (January 2024 – June 2024). Additional exploratory dependent variables to measure potential broader cleanup intervention impacts on street crime included total street crime (non-gun) aggravated assault (non-gun), robbery (non-gun), simple assault, theft from vehicle, and vandalism / criminal mischief incident counts per month on each street segment during the six-month pre-intervention, intervention, and post-intervention periods.

The LCBC implementation was measured through monthly observations of litter on each experimental street segment starting one month before the intervention commenced (June 2023 baseline) and during the six intervention months (July 2023 – December 2023). Observer schedules and routes for scoring street segments were created independently and without knowledge of the segments' experimental status and cleanup schedule. Nineteen trained observers used the official Philadelphia Parks and Recreation scoring matrix that captured litter levels on the street segments based on a previously validated 4-point Likert scale (1 = *little to no*

litter, 2 = *litter in the amount that can be collected by a single person*, 3 = *litter in the amount that would require collection by a team of people*, 4 = *litter in the amount that would require collection by a large team of people and/or heavy machinery*).[6] Four segments were dropped from the scoring list after being visited at baseline it was determined they were next to police training facilities and hospitals. A total of 16 segments could not be scored during scheduled times due to exogenous reasons, such as temporary closures due to police responses to emergency events or public works repairs to roads. A supervised machine learning classification and regression trees was used to impute litter scores for these missing segments (Berk, 2016), which performed slightly better than mode imputation.[7]

Table A1 in Appendix B shows the baseline means and standard deviations for each outcome across the treatment and control condition study arms. Cohen's *d* represents the absolute standardized mean difference between each experimental treatment and the control group; whereas Eta-squared (η2) measures the proportion of variance explained in the outcome per experimental arm relative to the control arm.[8] Cohen's *d* and η2 showed that balance was achieved in the baseline outcomes between treated and control groups. All Cohen's *d* values were below a small effect size of .20 and Eta-squared coefficients were small (<0.02), meaning the treatment explains a negligible proportion of the outcome variance. Overall, this suggests that

[6] In 2018, the City of Philadelphia adopted the Keep America Beautiful Community Appearance Index and refined this litter index over several years of implementation (see https://www.phila.gov/media/20190821135505/Litter-Index-Report-2019.pdf). Additional research suggests that the litter index is an acceptable measure of Philadelphia resident perceptions of trash in their neighborhoods (Hausman et al., 2019). The research team further assessed the reliability of the litter index by having two raters conduct independent ratings of the study street segments. The average interrater reliability was 0.92.

[7] The stability of results was tested after excluding the street segments with imputed values and the results were substantively the same.

[8] η2= .01 is considered a small effect size; .06 a medium effect size, and .14 or higher a large effect size.

the block randomization produced baseline balance, and the treatment and control groups were comparable on observable factors.

2.3. Model Estimation

A series of regression models were used to estimate the impact of the LCBC intervention on the dependent variables via an intent-to-treat (ITT) using a difference-in-differences framework. The main model used to estimate the ITT regression models was specified as follows:[9]

Eq. (1):

$$Y_{i(t)} = \beta_0 + \beta_1 \text{Biweekly}_i + \beta_2 \text{Monthly}_i + \beta_3 \text{Post}_i + \beta_4 \text{Adjacent}_i + \beta_5 \text{Biweekly}_i * \text{Post}_t + \beta_6 \text{Monthly}_i * \text{Post}_t + \beta_8 \text{Adjacent}_i * \text{Post}_t + \beta_9 \text{Biweekly} * \text{Adjacent}_i * \text{Post}_t + \beta_{10} \text{Monthly} * \text{Adjacent} * \text{Post}_t + \delta_{s(i)} + u_{i(t)}$$

In each regression model, the biweekly and monthly street segment cleaning interventions effects in the post-intervention period (relative to pre-period) were estimated with coefficients β_5 and β_6, respectively compared to the no-intervention control study arm. Spatial displacement and diffusion impact of the LCBC intervention were estimated by calculating the monthly counts of outcomes in street segments that were adjacent (i.e., intersected within ten feet) to street segments included in the experiment. If the outcome counts in the adjacent street segments near the treatment arms rise, relative to adjacent segments near no-treatment control condition, the positive estimates for coefficients β_9 and β_{10} would suggest displacement. If street segments adjacent to treatment study arm experienced reductions in outcomes, this pattern suggests

[9] We also estimated the ITT on street segments that were part of the experiment in the following form: $Y_{it} = \beta_0 + \beta_1 \text{Biweekly}_i + \beta_2 \text{Monthly}_i + \beta_3 \text{Post}_i + \beta_4 \text{Biweekly}_i * \text{Post}_t + \beta_5 \text{Monthly}_i * \text{Post}_t + \delta_{s(i)} + u_{i(t)}$. However, the expanded model (1) by design provides identical estimates of βMonthly* Post and βBiweekly*Post.

spillover benefits diffused to the surrounding street segments. Robust standard errors were clustered by strata (1 to 15) level to match the block randomization design. OLS regression was used to estimate the monthly change in litter index since the variable scale was continuous. For litter index changes we do not include measures of adjacent street segments, as those were not observed during the experiment. Poisson regression models were used to estimate the other count-based dependent variables.[10]

The ITT estimates from the experiment assume that all treated street segments are impacted in the same (monotonic) manner. It is possible that a subset of street segments may generate impacts on outcomes by engendering more neighborly involvement. To test the possible influence of heterogeneous effects by street segments, the primary model was re-estimated by substituting segment-level fixed effects model where treatment is identified based on the post-period for any street segment that received the biweekly or monthly cleanup. Finally, as a sensitivity test of the primary model estimates, the results for all dependent variables were re-estimated after removing two street segments that were part of the Taking Care of Business program relaunched by the City of Philadelphia after the experiment,[11] two street segments that were next to hospital emergency rooms, and sixteen street segments whose litter index values were imputed. Since the study tests multiple hypotheses with the same data across multiple outcomes family-wise error rate (FWER, Bonferroni) *p*-values are reported to address the issue of false positives (Type I errors).

[10] Postestimation likelihood ratio tests confirmed that the overdispersion parameters were not equal to zero and overdispersion adjustments were required. As such, robust standard errors were used to adjust for overdispersion in the Poisson regressions (Berk & MacDonald, 2008). The results are substantively similar and standard errors only slightly different at the 100th decimals when using Negative binomial variant of the Poisson model.

[11] Taking Care of Business is a City of Philadelphia program that funds community-based nonprofits to sweep sidewalks and remove litter within neighborhood commercial corridors, with the goals of having cleaner environments, create job opportunities for disadvantaged populations, and improve economic activity.

Appendix C presents details of additional analyses estimating the direct effects of litter reductions caused by the LCBC intervention on gun violence and street crimes using a contamination-adjusted ITT (CA-ITT) model.

Statistical power was assessed by estimating the minimum detectable effect size (MDE) for a sample of 300 blocks with 80% power to reject the null hypothesis of no difference between study arms. The MDE was estimated using monthly shooting incident counts from January 1, 2020, to August 31, 2022, in a block randomization simulation. The top 300 segments were divided into 15 strata and randomly assigned to one of three study arms (75 monthly cleanup, 75 biweekly cleanup, 150 control). The treatment assignment was shuffled 1,000 times, and a Poisson regression model was used to estimate monthly shooting counts, as part of a permuted block randomization (Matts & Lachin, 1988). This process yielded an MDE of 9.5 percentage points, indicating the study could detect differences of nearly 10% between arms.

3. Results

Table 1 presents the results for the estimates of the effect of the LCBC interventions on the litter index at treated street segments relative to control street segments. Biweekly cleanup arm reduced litter index scores by -0.298 (p<.01, FWER), while the monthly cleanup reduced litter index scores by -0.313 (p<.05, FWER). The results show that street segments were observed to be significantly cleaner whether it was a biweekly or monthly cleanup relative to control condition of no cleanup. The reduction is equivalent to a third point in the litter score or approximately a 16% improvement relative to the baseline litter index score. Table 1 also shows that these results remain robust when dropping segments as described above.

Table 2 shows the results from the effect of the LCBC intervention on gun violence and street crimes. Estimates are presented from the exponentiated coefficients from the Poisson

models as incident rate ratios (IRR). Biweekly cleanups were associated with a non-significant reduction in overall gun violence by 27% compared to control blocks and consistent reductions in each gun violence outcome. While the implied reduction is large enough to be a meaningful reduction in gun violence, the standard errors are large and indicate a null effect. Similarly, there were implied reductions for street crimes, but the estimates were not precise. After considering there are 11 outcomes across two treatment arms, the FWER *p*-value needed to reject the null hypothesis would be .002. None of the estimates approach this *p*-value. The effects for the monthly treatment arm on gun violence and street crimes were inconsistent in direction and all null effects.

Table 2 also presents the results that test for displacement or spillovers from the experimental street segments to adjacent ones on gun violence outcomes. All estimates are null consistent with the fact that the main intervention did not produce a statistically significant impact on gun violence or street crimes.

Table A2 in Appendix C presents the results 25 randomly selected biweekly street segments that had extra efforts from PHS including the provision of free trash and recycling bins and other enhancement activities. Results show a nonsignificant -28.2% reduction in gun violence and nonsignificant reductions for all but one offense category. These results suggest a meaningful effect on gun violence but are not statistically significant, especially after considering multiple hypothesis testing. For street crimes the results from the biweekly treatment arm are positive overall but not in a consistent direction across subcategories.

Table A3 in Appendix D presents the results of the CA-ITT models that test the causal effect of litter changes induced by the experiment on changes in gun violence and street crimes. All results are null and not in a consistent direction, suggesting that the estimated litter reduction

gains were insufficient to significantly impact these outcomes. These results imply that the estimated incremental reduction in litter between the baseline and treatment period for the 150 street segments was not sufficient to explain the variation in gun violence and street crimes.[12]

4. Discussion

Given the optimism for place-based interventions in reducing violence and their advantage of not requiring the involvement of the criminal justice system, street cleaning stands out as a promising intervention to curb gun violence that aligns with basic services that local governments and business districts already implement and could likely scale-up. Our findings show that the intervention reduced litter, but there were null effects on gun violence and street crimes. While the LCBC program largely showed null effects on these outcomes, the consistent non-significant reductions in the biweekly treatment arm provide suggestive evidence that street cleaning could be a contributor to gun violence and street crime reduction strategy. For cities looking to implement place-based community violence interventions, there is a critical policy lesson that can be gleaned from this experience.

The LCBC was a one-dimensional intervention applied to heterogenous set of street segments. While all included street segments suffered from high levels of litter, gun violence problems at these places may have stemmed from varying problems such as gun robberies of commuters, gang conflicts, and drug market violence. Situational crime prevention and problem-oriented policing interventions encourage practitioners to identify the specific underlying conditions that drive violence at crime hot spots. These approaches focus on implementing tailored responses that address the criminogenic dynamics contributing to recurring crime in these areas (Clarke, 1995; Braga, 2008). Removing trash may have ameliorated certain

[12] The null effects reported do not appear to be the result of statistical power. See Appendix E for simulation.

environmental features that contributed to crime problems in the treated street segments, but these cleanups may not have sufficiently addressed other longstanding structures and dynamics (e.g., abandoned buildings, vacant lots, and open-air drug markets) more directly related to high levels of gun violence in these places.

Other "one-size-fits-all" place-based interventions, however, have been successful in controlling gun violence and serious crime. These interventions involved more dramatic transformations that tipped high crime places into safe environments. For instance, the greening vacant lots can transform trash-filled vacant lots into well-lit green spaces with benches and gardens that can stimulate informal social control by residents using these places for prosocial activities (Branas et al., 2018). Although the litter was significantly reduced at treated street segments, criminal offenders may not have perceived these improvements as substantive enough to change routine activities and the use of spaces to curb violence and street crimes. Place-based interventions may have to reach a "tipping point" in terms of perceptions of disorder that triggers cognizant expectations about behavioral norms in an area or the level of informal social control (Gladwell, 2000). Reductions in litter above that tipping point would induce improvements in feelings of insecurity that would drive neighbors to spend more time on the streets and engage in efforts to regain control, informal surveillance, and social ties, prompting a sense of place ownership (Ferraro & LaGrange, 1987; Hipp, 2010),

There are important normative and public health reasons to clean trash from city streets (Thacher, 2004; WHO, 2015). In fact, focus groups and in-depth interviews that were conducted with residents and cleanup crews as part of the experiment revealed the negative impacts of litter and neglect on community well-being. Residents commented on frequently encountering trash and litter, including mattresses, tires, furniture, and abandoned vehicles, and how this made them

frustrated with the lack of city response. Beyond community wellbeing benefits, policy makers should view street cleaning interventions as a tactic of a larger strategic framework to transform dangerous public places into safe environments. The results of this RCT and other scientific evidence suggest that community violence interventions focused on changing the environment of high crime places need to more directly impact the longstanding and persistent built structures and underlying dynamics that cause criminal activity at these locations.

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Table 1. Effects of street cleaning on litter score

	Litter Index	TCOB	Imputed	ER
Biweekly	-0.298**	-0.303**	-0.279**	-0.298**
	(0.0703)	(0.0743)	(0.0704)	(0.0702)
Monthly	-0.313*	-0.313*	-0.322*	-0.313*
	(0.101)	(0.101)	(0.107)	(0.101)
Ave. Dep	1.849	1.854	1.841	1.854
N	2115	2101	2017	2101

Standard errors in parentheses. Estimates also include fixed effects for regions of the city (Northwest, Northeast, North, Kensington, West/Southwest). TCOB=2 Taking Care of Business blocks removed. Imputed=14 blocks imputed observations removed. ER=2 ED/Emergency Department blocks removed. Litter index varies from 1 to 4. N=2,115 (298 blocks*7 months).

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$ (FWER, Bonferroni)

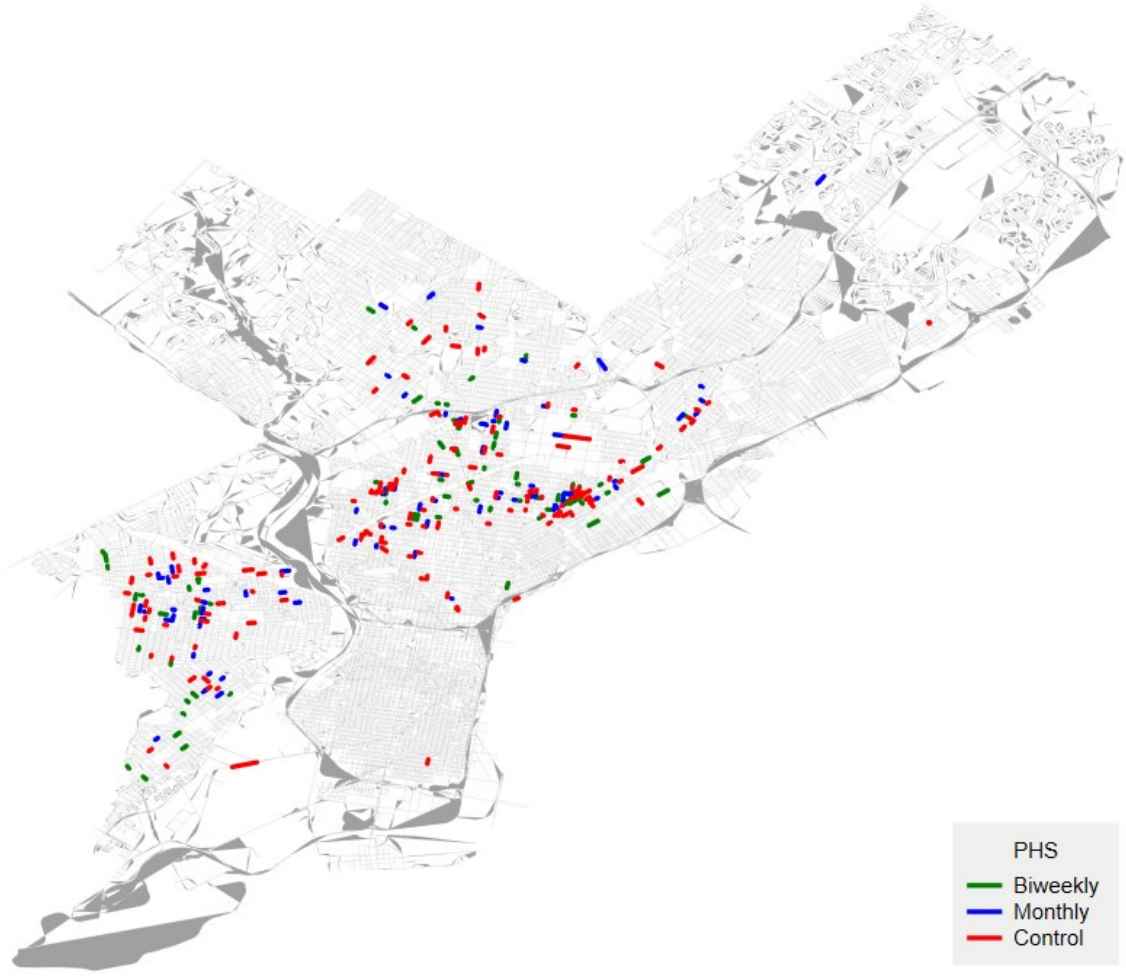
Table 2. Effects of street cleaning on treated and adjacent blocks

	Gun Violence	Gun assault	Robbery Gun	Weapons violation	Shooting	Street	Vandalism	Other Assault	Aggravated Assault No Gun	Robbery No Gun	Theft from Vehicle
Biweekly	0.730	0.887	0.857	0.609	0.741	0.923	0.760	1.076	1.074	0.947	0.440
	(0.210)	(0.340)	(0.501)	(0.204)	(0.384)	(0.158)	(0.224)	(0.152)	(0.401)	(0.393)	(0.205)
Monthly	0.957	1.747	2.000	0.700	0.528	1.287	0.829	1.338	3.536	2.684	0.754
	(0.374)	(0.956)	(1.136)	(0.287)	(0.350)	(0.183)	(0.123)	(0.333)	(1.783)	(1.523)	(0.480)
ABiweekly	1.436	1.434	1.234	1.381	1.645	1.129	1.134	1.012	1.029	1.635	2.079
	(0.643)	(0.713)	(0.678)	(0.623)	(1.435)	(0.234)	(0.363)	(0.184)	(0.406)	(1.029)	(0.862)
AMonthly	1.279	0.535	1.290	1.412	2.819	0.792	0.965	0.858	0.297	0.435	1.205
	(0.614)	(0.344)	(0.779)	(0.777)	(2.096)	(0.140)	(0.255)	(0.233)	(0.163)	(0.259)	(0.776)
Ave. Dep	0.0802	0.0222	0.0149	0.0277	0.0155	0.258	0.0598	0.122	0.0317	0.0205	0.0236
N	29970	29970	29970	29970	29970	29970	29970	29970	29970	29970	29970

Standard errors in parentheses are clustered by block strata. City region level fixed effects included. N=2,9970 (1,665 blocks * 18 months). A=Adjacent

APPENDIX A.

Figure A1. PHS Landcare Block Cleanup Experiment



Source: Philadelphia Street Center Lines

APPENDIX B.

Table A1. Baseline balance in outcomes between treated and control groups

	(1)		(2)		(3)		Cohen's *d*		
	Biweekly Mean	SD	Monthly Mean	SD	Control Mean	SD	(1 v. 3)	(2 v. 3)	η2
Total Gun	0.129	0.413	0.124	0.470	0.100	0.385	0.073	0.058	0.001
Gun Assault	0.033	0.180	0.020	0.140	0.029	0.192	0.023	0.050	<0.001
Robbery Gun	0.031	0.186	0.013	0.115	0.022	0.155	0.053	0.062	0.001
Weapon	0.051	0.249	0.056	0.346	0.031	0.180	0.097	0.098	0.002
Shooting	0.013	0.115	0.036	0.228	0.018	0.182	0.027	0.089	0.002
Total Street	0.313	0.675	0.213	0.524	0.287	0.687	0.039	0.115	0.003
Vandalism	0.067	0.275	0.062	0.260	0.061	0.274	0.020	0.004	<0.001
Other Assault	0.138	0.410	0.118	0.403	0.150	0.461	0.027	0.072	<0.001
Aggravated Ass.	0.044	0.217	0.009	0.094	0.037	0.205	0.037	0.157	<0.001
Robbery No Gun	0.036	0.197	0.009	0.094	0.027	0.161	0.051	0.124	0.003
Theft from Veh.	0.029	0.180	0.016	0.141	0.012	0.110	0.121	0.027	0.002
N=	450		450		900		1350	1350	1800
Litter Index	2.081	0.749	2.02	0.766	2.204	0.819	0 .155	0 .221	0.009
N=	74		75		149				298

APPENDIX C.

Table A2. Effects of street cleaning on enhanced blocks

Outcomes	Enhanced		Ave. Dep	N=
All Gun Crimes	0.708	(0.225)	0.091	1800
Gun assault	1.100	(0.809)	0.026	1800
Robbery Gun	0.375	(0.335)	0.015	1800
Weapons violation	0.625	(0.285)	0.030	1800
Shooting	0.708	(0.710)	0.018	1800
Street Crimes				
All Street Crimes	1.092	(0.233)	0.322	1800
Vandalism	0.462	(0.256)	0.073	1800
Other Assault	1.520	(0.439)	0.162	1800
Aggravated Assault	1.714	(0.941)	0.043	1800
Robbery No Gun	0.750	(0.565)	0.020	1800
Theft from Vehicle	0.400	(0.340)	0.021	1800

Exponentiated coefficients; Standard errors in parentheses are clustered by block strata. City region level fixed effects included. Sample consists of the biweekly group (n=25) that receive the enhanced relative to monthly (n=25) and control (n=50) that did not receive any enhancements. N=1,800 (100 blocks*18 months). Control group is reference.

APPENDIX D.

Contamination-Adjusted ITT Model Specification Details

The theory of change upon which the intervention lies implies a reduction in the outcomes of gun violence and street crimes occurs through reductions in litter on street segments. To test the causal effect of changes in litter induced by the experiment on outcomes, a contamination-adjusted ITT (CA-ITT) model was estimated. Specifically, an instrumental variable Poisson regression model using general methods of moments was estimated.[13] This model estimates the effects of changes in visible signs of litter on street segments induced by the experiment on gun violence and street crimes. CA-ITT models use the random assignment to a treatment condition as an instrumental variable (Sussman & Hayward, 2010). In this analysis, the effects of changes in levels of litter in the pre and post (t={0,1}) periods ($\Delta\text{Litter}_i = \text{Litter}_i 1 - \text{Litter}_i 0$) in a segment (*i*) induced by the street cleaning interventions (z={biweekly, monthly}) were estimated relative to the no-treatment control condition on outcomes (Y_{it}) according to the following form:

Eq. (3):

$$Q(\beta,\gamma,\delta) = \left(\frac{1}{N}\sum_{i=1}^{N}\left(z'_{\text{biweekly},i}(y_i - \exp(\beta'\Delta\text{Litter}_i + \gamma' Y_0 + \delta' s_i)) + z'_{\text{monthly},i}(y_i - \exp(\beta'\Delta\text{Litter}_i + \gamma' Y_0 + \delta' s_i))\right)\right)' W \left(\frac{1}{N}\sum_{i=1}^{N}\left(z'_{\text{biweekly},i}(y_i - \exp(\beta'\Delta\text{Litter}_i + \gamma' Y_0 + \delta' s_i)) + z'_{\text{monthly},i}(y_i - \exp(\beta'\Delta\text{Litter}_i + \gamma' Y_0 + \delta' s_i))\right)\right)$$

The parameter of interest is β which estimates how the pre-post change in the litter score on a segment (*i*) induced by the biweekly or monthly (z) treatments impacts the gun violence and street crimes. The means of the litter index and the total counts of gun violence and street crimes

[13] Using a two-stage least squares as an alternative does not substantively change the conclusion. The *F*-tests for the first-stage equation are all significant a p<.05 for all models, ranging from a minimum value of 4.11 for litter change in total street crimes model to a maximum value of 8.90 for the gun assaults model.

for the pre and post periods were used because there was only one monthly observation for litter index in the pre-period. Eq. (3) includes fixed effects for the region of the ($\delta_{s(i)}$) and the baseline values of the outcome (Y_{i0}). Standard errors were clustered by strata level.

The causal estimate from the CA-ITT assumes that the random assignment to biweekly or monthly street segment cleanups explains a meaningful amount of the variation in changes in litter and that there are no other activities occurring on treatment segments that are not occurring on control segments. We are not aware of any special activities in treatment segments that were not part of the experiment. The CA-ITT also assumes that the biweekly and monthly treatments predictions of change in litter only impact outcomes through changing the levels of observed litter. This is an assumption that cannot be directly tested, since it is plausible that street segment cleanups could impact things aside from the observed level of litter on a street segment.

Table A3. Effects of street cleaning on gun violence (Poisson Instrumental Variable)

	Change in litter		N
Gun Violence	2.091	(2.614)	298
Gun assault	0.301	(0.602)	298
Robbery Gun	3.606	(8.601)	298
Weapons violation	0.426	(2.492)	298
Shooting	0.035	(0.107)	298
Street	1.636	(2.082)	298
Vandalism	16.26	(27.85)	298
Other Assault	1.428	(2.125)	298
Aggravated Assault No Gun	0.682	(9.163)	298
Robbery No Gun	0.540	(2.112)	298
Theft from Vehicle	3.510	(10.790)	298

Exponentiated coefficients; Standard errors in parentheses are clustered by block strata. City region level fixed effects and pre-treatment outcomes included in model. Random assignment to biweekly and monthly exclusion restrictions in IV model. N=298 blocks.

APPENDIX E.

Statistical Power of Experiment

The citywide reduction in gun violence observed in Philadelphia following the LCBC pilot experiment influences the size of the reduction required to reject the null hypothesis. In a post-hoc power calculation, data is simulated over 100 iterations with the three study arms using the estimated incidence rate ratios (IRRs) from the experiment. The outcome variable is generated based on Poisson distributions, with adjustments for group-specific IRRs. A Poisson regression model is fitted in each iteration to compare the treatment groups with the control group, followed by a hypothesis test for significance using the average rate of gun violence of 0.102. The power of the test is assessed by the proportion of significant results. The simulation indicates that the study has 72% power to reject the null hypothesis of no difference between the treatment and control study arms on gun violence at an alpha level of 0.05 (72 out of 100 iterations showed statistically significant results). If the average gun violence remained at the same level as the year prior to the experiment, or 30% higher (0.132), the study would have 87% power to reject the null hypothesis. After adjusting for family-wise error rate (FWER), however, in either scenario, the effect sizes would need to be considerably larger to reject the null hypothesis.

STATA code for the calculation

```
**power calculation for post-hoc comparison
* Set the total number of observations (5400) and set seed for reproducibility for 100 *iterations
clear
set seed 12345
set obs 5400  // Set the total sample size

* Define parameters
local n_group1 1800  // Number of observations in treatment group 1 (1800)
local n_group2 1800  // Number of observations in treatment group 2 (1800)
```

```
local n_group3 3600  // Number of observations in control group (3600, twice the size of treatment groups)
local irr_group1 0.730  // IRR for treatment group 1
local irr_group2 0.957  // IRR for treatment group 2
local alpha 0.05  // Significance level (95% confidence)
local avg_y 0.102  // Average dependent variable (dependent variable's mean)

* Initialize the power result variable (set to 0 initially)
gen power_result = 0

* Set up a loop to simulate data for 100 simulations
forval i = 1/100{
   * Drop the 'group' and 'depvar' variables to avoid conflicts from previous iterations
   capture drop group depvar

   * Simulate group assignment (1 = treatment 1, 2 = treatment 2, 3 = control)
   gen group = .
   replace group = 1 if _n <= `n_group1'  // Group 1 (1800 observations)
   replace group = 2 if _n > `n_group1' & _n <= `n_group1' + `n_group2'  // Group 2 (1800 observations)
   replace group = 3 if _n > `n_group1' + `n_group2'  // Group 3 (3600 observations, twice the size of groups 1 and 2)

   * Simulate outcome variable based on group assignment and IRR (Poisson model)
   gen depvar = .
   replace depvar = rpoisson(`avg_y') if group == 3  // Control group
   replace depvar = rpoisson(`avg_y' * `irr_group1') if group == 1  // Treatment group 1
   replace depvar = rpoisson(`avg_y' * `irr_group2') if group == 2  // Treatment group 2

   * Fit the Poisson regression model to assess the effect of treatment groups
   poisson depvar ib3.group

   * Test if the treatment groups differ from the control group
   test 1.group 2.group  // Test if treatment groups differ from control group

   * Store the result of the hypothesis test (if p-value < alpha)
   if r(p) < `alpha' {
      replace power_result = power_result + 1  // Increment the result
   }
```

```
}

* Calculate power (proportion of significant results)
summarize power_result
di "Power to detect an effect is: " r(mean)
```