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Urban building demolitions, firearm violence and drug crime

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Abstract Although multiple interventions to remediate physical blight have been found to reduce urban firearm violence, there is limited evidence for demolishing vacant buildings as a violence reduction strategy. Starting in 2014, Detroit, MI launched a large-scale program that demolished over 10,000 buildings in its first 3 years. We analyzed the pre-post effects of this program on fatal and nonfatal firearm assaults and illegal drug violations at the U.S. Census block group level, using propensity score matching and negative binomial regression. Receiving over 5 demolitions was associated with a 11% reduction in firearm assaults, relative to comparable control locations, 95% CI [7%, 15%], $p = 0.01$. The program was associated with larger reductions in firearm assaults for the locations receiving moderate numbers of demolitions (between 6 and 12) than for locations receiving high numbers of demolitions (13 and over). No effects were observed for illegal drug violations and no evidence of spatial crime displacement was detected. These findings suggest that vacant building demolitions may affect gun violence.

Keywords Firearm · Violence · Drugs · Demolitions · Blight remediation

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Introduction

Urban firearm violence is a major public health concern and the single largest driver of firearm-related injury among young people. Among 15–19 year olds, the hospitalization rate from firearm assaults is 7.8 times higher in urban areas than rural areas and more than twice the rates of hospitalization from unintentional and self-inflicted firearm injuries combined (Herrin et al., 2018). Firearm violence disproportionately burdens African American children, who are 10 times more likely than White children to die by firearm assault (Fowler et al., 2017).

Addressing physically blighted spaces has gained attention as a strategy to reduce urban firearm violence. Several recent studies have found safety benefits associated with securing abandoned houses (Kondo et al., 2015, 2018) and improving the condition of vacant lots and the introduction of green spaces (Branas et al., 2018; Heinze et al., 2018; Kondo et al., 2016; Kuo, 2001). Deteriorated spaces are not only typical of the de-industrialized, disinvested, and underserved neighborhoods where interpersonal firearm violence is most endemic, but may play a causal role in the commission of violence. The physical disorder observed at these sites signals a lack of social control, potentially encouraging violence perpetration, according to *broken windows theory* (Kelling & Wilson, 1982). Vacant houses and overgrown lots can also provide *situational opportunities* for activities associated with firearm violence, such as those involving illegal guns and drugs (Spelman, 1993; Garvin et al., 2013), by hiding these activities from view.

By contrast, interventions that improve physical condition may foster *busy streets* where positive social processes emerge (Heinze et al., 2018). These environment-focused interventions are not intended to supplant long-term institutionalized efforts to respond to violence in disadvantaged and marginalized communities; however, addressing violence through environmental modification may offer benefits over resource-intensive alternatives with potentially short-lived effects, such as police patrols (Draca, Machin, & Witt, 2011).

A little-understood, but widely implemented strategy for addressing physical blight is simply to demolish buildings that are vacant and/or hazardous. Such buildings are particularly widespread in Northern U.S. industrial manufacturing cities that experienced substantial population loss in the second half of the 20th century. Although crime prevention is often cited as a rationale for demolitions programs, researchers have found limited evidence that demolishing vacant houses reduces violence (Spader, Schuetz, & Cortes, 2016; Stacy, 2018; Wheeler, Kim, & Phillips, 2018).

In Cleveland and Chicago, Spader et al. (2016) found no difference in violence in the areas immediately surrounding demolitions, compared to areas slightly farther away. In Buffalo, Wheeler et al. (2018) found significant violence reductions in the areas immediately surrounding demolitions, compared to locations with similar previous crime levels and no demolition, but these effects were not significant in a neighborhood-level analysis. In Saginaw, MI, Stacy (2018) found significant neighborhood-level violence reduction, but only from demolitions conducted in the previous month, and demolitions were associated with *increased* violence at a 4-month lag. None of these studies considered firearm-specific violence as an outcome. It remains uncertain, therefore, whether demolitions generate lasting reductions in neighborhood-level violence, and their effects on firearm violence are unknown.

We study Detroit, MI, which has demolished an unprecedented number of vacant buildings since 2014 (*The Economist*, 2017). The scale of Detroit's demolitions program provides an unusual opportunity to test the neighborhood effects of different levels of demolitions activity. In the present study, we use a quasi-experimental design to assess the effects of those demolitions on fatal and nonfatal firearm assaults and drug-related crimes during a 14-month follow-up period. We build on existing research by examining firearm-specific outcomes and by analyzing treatment effects at differing dosage levels, using treatment and control neighborhoods that are matched on social, demographic and physical characteristics. Our design goes further than previous studies to (a) consider how the quantity of demolitions affects neighborhood-level crime, including firearm violence, and (b) identify and control for systematic differences in the places that receive more versus fewer demolitions.

Methods

Vacant buildings and demolition in Detroit

Detroit represents an extreme example of economic decline associated with deindustrialization. From 1950 to 2016, the city's population dropped from 1.8 million to under 700,000. In 2014, it was estimated that at least 43,000 residential buildings were vacant and that approximately 78,000 buildings met the city's criteria for blight (Detroit Blight Task Force, 2014). This estimate was derived, in part, from a 2013-14 citywide survey in which volunteers catalogued the physical condition and apparent occupancy status of every property (Data Driven Detroit, 2014). A similar survey, conducted in 2009, had identified over 30,000 residential properties that appeared to be vacant (Data Driven Detroit, 2010).

Given this excessive volume of blighted structures, Detroit's large-scale demolitions program began in 2014, drawing information from the prior property surveys. By the end of 2016, the period we study here, the city had demolished over 10,000 buildings. The program cost approximately $130 million during this timeframe, mostly from federal economic recovery funds (Cwiek, 2016).

Researchers have previously called Detroit's demolitions program "targeted and rapid" (Dynamo Metrics, 2015, p. 6). Initially, the program was targeted toward six neighborhoods. According to a city report, these neighborhoods were among "the strongest areas with marketability for redevelopment investments" (Detroit Land Bank Authority, 2013, p. 2). As implemented, however, the program was not limited to these areas. While the strategy for allocating demolitions has not been clearly documented, it has been reported that officials first prioritized neighborhoods with higher residential occupancy, in order to stabilize home values in those places, and then proceeded to higher-vacancy neighborhoods (*The Economist*, 2017). This approach is consistent with *right-sizing* strategies, which encourage higher population density in a smaller number of places. Some Detroiters, however, have viewed the city's economic recovery strategies through a racial lens, contending that recent investments have disproportionately benefited the city's White minority (Bach, 2017).

Data and analysis

Our unit of analysis was U.S. Census block groups in Detroit (n = 879). While researchers have previously estimated effects of blight remediation on crime at the level of individual properties, we studied remediation at the block group level because we did not expect that demolishing a single building would necessarily influence crime outcomes in a city with 78,000 blighted structures. Rather, we hypothesized that remediation would begin to affect violence when residents observed transformation at a slightly larger ecological level. Thus, we examined causal effects at the micro-neighborhood level represented by block groups. Analyzing Detroit block groups yielded a dataset with more than 10 times as many spatial units as previous neighborhood-level analyses of crime and demolitions (Wheeler et al., 2018; Stacy, 2018).

Outcome variables were obtained from Detroit Police Department (DPD) crime incident data from January 2009 through November 2016 (DPD, 2018), for a total of 31 complete yearly quarters and one incomplete yearly quarter. These variables were firearm assaults and illegal drug violations. The firearm assaults variable combined firearm homicides (i.e. fatal shootings) and aggravated assaults (i.e. assaults involving serious injury) in which firearms were the weapons used. Firearm involvement was a classification DPD applied retrospectively based on a review of incident records, prior to release of the incident data. Even though firearm violence was the main outcome of interest, we analyzed drug violations because reducing illegal drug-related activity was considered a possible mechanism by which demolitions could reduce firearm violence. We aggregated crime records geographically by block group and temporally by quarter-years.

Block groups were considered treated (i.e. as having received the demolitions intervention) once they had received more than 5 cumulative demolitions by the end of the previous quarter. To allow a sufficient post-treatment observation period, only block groups reaching the threshold by the end of Q3 2015 (n = 343) were considered treated. The 5-demolition threshold in the main analysis was chosen based on an exploratory review of the distribution of demolitions in each block group. Because the typical Detroit block group contained 15 occupied census blocks, locations reaching the threshold had approximately one demolition on every third block. The effect of differing dosages was tested in a secondary analysis.

Since only 19% of block groups received zero demolitions and these untreated units were generally the least similar to higher-demolition units, we did not restrict the control group to locations with zero demolitions. Instead, any prospective control unit was eligible for inclusion so long as its demolitions count did not exceed the relevant threshold. In other words, our analysis compared treated units to similar units that received *less treatment*, not necessarily *no treatment*.

We matched the sample 1:1 based on a linear propensity score and optimal matching (Rosenbaum, 2002). In particular, for each unit meeting the demolitions threshold before the Q3 2015 cutoff, we identified a similar control unit, to implement a *treatment on the treated* analysis. Matching retained the block groups most similar to the treated blocks groups on eight potential confounders at the outset of the demolitions program. Using matching as a pre-processing step reduces model dependence in observational studies (Ho et al., 2007). One concern was that treatment assignment might have been associated with unequal patterns of social and economic change that occurred during the pre-treatment period. Matching on variables that predicted treatment assignment, therefore, strengthened the assumption that *common trends* would hold between treated and control units throughout the study period (Wing, Simon, & Bello-Gomez, 2018).

Matching variables were derived from the property surveys discussed above and obtained from the five-year U.S. Census American Community Survey ending in 2014. These variables included physical conditions (the number of buildings in early 2014, the proportion of residential structures that were unoccupied in early 2014 and the proportion of residences that went vacant between 2009 and 2014) and social and demographic variables (total population, log-normalized median household income, male population aged 15–34, share of residents who are non-Hispanic Whites, and a composite variable for concentrated disadvantage). These variables were selected based on their expected association with receipt of treatment and/or their potential to bias estimated treatment effects. We performed multiple logistic regression on the full sample to produce the propensity score matches and also to analyze the factors influencing receipt of treatment.

We identified the period from the start of large-scale demolitions (Q2 2014) through the quarter when all treated units had reached treatment status (Q3 2015) as the treatment period. We omitted observations during this period from our regression model, allowing us to compare pre-treatment and post-treatment outcomes using a standard difference-in-difference framework. We explored differences in treatment effects within the treatment group in a secondary analysis that stratified observations based on the number of demolitions received.

We used negative binomial regression on the matched data to estimate post-treatment effects on the crime outcomes, both of which were counts. Our model included two-way fixed effects, i.e. separate fixed effects for each block group and for each time period. The treatment indicator was set to 1 for treated units during the post-treatment period, and was otherwise set to 0.[1] This difference-in-difference approach represents a standard framework for estimating average between-group effects in longitudinal quasi-experimental studies (Wing et al., 2018). Unit fixed effects controlled for any time-invariant attributes of the block groups, while time period fixed effects controlled for seasonality and any other group-invariant time trends.

We accounted for intraclass correlation by clustering standard errors by block group. More specifically, since typical cluster-robust standard errors do not reliably prevent Type I error when the number of clusters is small, we applied small-sample t test corrections proposed by Bell and McCaffrey (2002), as operationalized by Pustejovsky and Tipton (2018). To determine whether un-modeled spatial dependencies in the data had affected our estimates, we applied Moran's I test to the model's deviance residuals at each time point, with neighbors determined by queen's contiguity of the block group polygons.

To assess whether treatment effects depended on the number of properties demolished, we stratified the treatment group into lower- and higher-demolitions halves based on the number of demolitions conducted by the end of the treatment period. We analyzed each stratum using the same two-way fixed effects regression model as in the main analysis, comparing the treated units in each stratum with their corresponding matched controls from the previous step.

A major concern for blight remediation programs is that crime might simply relocate from remediated areas to non-remediated areas. Like the majority of studies examining crime displacement (and its inverse, the diffusion of benefits) we examined the immediate spatial effects, specifically whether treatment was associated with crime increases in nearby untreated areas (Johnson, Guerette, & Bowers, 2014). Using our modeling framework from the previous steps, we assessed for spatial displacement by examining whether treatment changed the association in crime outcomes between neighboring block groups. In particular, for each of our crime models, we added a spatial lag term (the average of neighboring units' outcomes at a given time period) as a fixed effect, as well as its interaction with the treatment term. The interaction term would detect spatial spillover specifically associated with treatment. This approach is conceptually similar to previous studies' use of untreated buffer areas as controls (Johnson et al., 2014) except that spillover between adjacent treated units could also be detected.

Analysis was conducted in R software. Institutional review board review was waived because the study involved no human subjects.

Results

The matching process reduced treatment–control group imbalance on the matching variables in the main analysis (Table 1). Treatment units received substantially more demolitions by the end of the treatment period ($M = 16.6$) than control units did ($M = 2.4$). Of the 343 matched pairs, only 2 were geographical neighbors.

In the propensity score regression (see Supplemental Table 1), receipt of treatment was positively associated with the proportion of non-Hispanic White residents, concentrated disadvantage, the number of buildings in the block group, and the residential vacancy percentage, and was negatively associated with the proportion of residential properties that went vacant from 2009 to 2014.

The matched groups showed similar trends for each crime outcome prior to treatment and the overall trend in both groups was downward for each outcome (Fig. 1). In the pre-treatment period, the treatment group experienced more firearm assaults ($M = 1.16$) and drug violations ($M = 1.29$) per block group-quarter than the control group (firearm assaults, $M = 1.07$; drug violations, $M = 1.18$). In the post-treatment period, the treatment group had fewer firearm assaults ($M = 0.93$) than the control group ($M = 0.96$) but more drug violations (treatment group, $M = 0.94$; control group, $M = 0.89$).

Our regression model estimates (Table 2) indicated that treatment was associated with an 11% reduction in firearm assaults, compared with control units, 95% CI [7%, 15%], $p = 0.01$. We found no association with illegal drug violations. In the stratified analysis (Table 3), treatment was associated with a 14% reduction in firearm assaults in the

[1] We used a negative binomial model to model outcome $Y_{it,}$ the number of crimes at unit i at time t, as a function of the unit fixed effect $a_{i,}$ the time fixed effect $b_{t,}$ and a constant treatment effect δ. Letting D_{it} refer to i's treatment status at time t we have (including the log link for the negative binomial):

$$\log E[Y_{it}] = a_i + b_t + \delta D_{it} + \varepsilon_{it}$$

We used a negative binomial model to allow for overdispersion of the Y due to unobserved heterogeneity. The δ term is the average effect of treatment on the treated.

Table 1 Mean values of study variables, before and after statistical matching procedure, by group

Variable	Treated (n = 343)	Pre-match Untreated (n = 536)	Post-match Control (n = 343)
Matching variables			
Residential vacancy (%)	24	15	21
Became vacant since 2009 (%)	13	8	11
Parcels with structures (n)	321	286	313
Block population (n)	764	818	778
White, non-Hispanic population (%)	8	8	8
Males ages 15–34 (n)	110	113	109
Median income (USD)	22,572	26,790	24,341
Concentrated disadvantage[a]	0.24	− 0.15	0.09
Treatment variable[b]			
Demolitions received, Q2 2014–Q3 2015	16.6	1.8	2.4
Outcome variables[b]			
Firearm assaults/qtr, pre-treatment	1.16	0.95	1.07
Firearm assaults/qtr, post-treatment	0.93	0.85	0.96
Illegal drug violations/qtr, pre-treatment	1.29	0.99	1.18
Illegal drug violations/qtr, post-treatment	0.94	0.76	0.92

Treated status based on a threshold of > 5 total demolitions from Q2 2014–Q3 2015. Vacancy and parcel data were obtained or derived from 2009 and 2014 residential parcel surveys (Data Driven Detroit, 2010; Data Driven Detroit, 2014). All other matching variables were obtained or derived from 2014 5-year American Community Survey

[a]Composite factor of population below federal poverty line, population with high school education, single mother-headed households, and unemployment rate

[b]All treatment and outcome variables are reported as counts

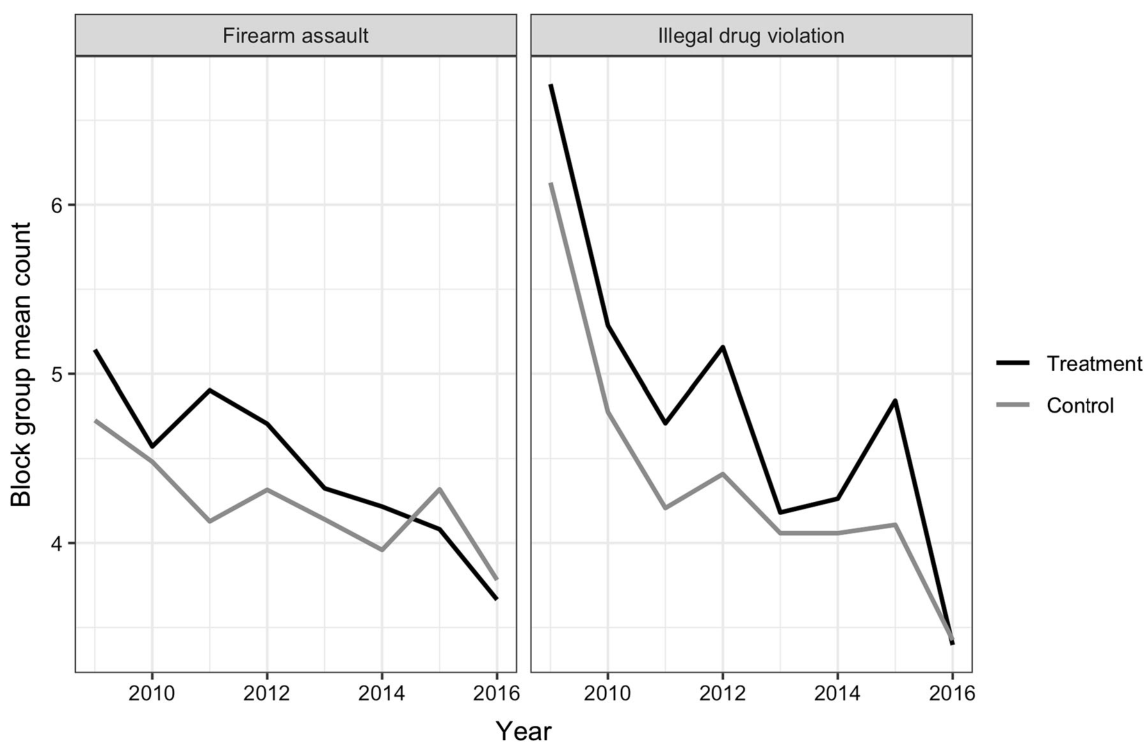

Fig. 1 Annual mean crime outcomes for treatment and control groups

Table 2 Estimated treatment effects from demolitions program

Outcome	Estimated treatment effect	
	IRR[a] (95% CI)	p
Firearm assaults	0.89 (0.85, 0.93)	0.01
Illegal drug violations	0.95 (0.89, 1.01)	0.98

[a]Incident rate ratio (IRR) using negative binomial regression with two-way fixed effects and matched sample. For model specifications, see "Methods"

lower-demolitions stratum, 95% CI [9%, 18%], $p = 0.01$, and with a similar-magnitude, but non-significant, estimated reduction in illegal drug violations for the same group. No treatment effects were observed in the higher-demolitions group.

Moran's I tests did not find spatial autocorrelation in the deviance residuals for the firearms model at any time period. In the drug violations model, 3 out of 26 time periods showed residual spatial autocorrelation at $p < 0.05$, but none was during the post-treatment period.

Our spatial lag regressions, used to test for spatial displacement of crime, found no significant associations with the spatial lag terms or their interactions with the treatment indicator (Supplemental Table 2). Adding the spatial lag terms did not substantially alter the main effects estimates.

Discussion

Our results support the proposition that removing abandoned buildings from a neighborhood can reduce firearm violence. In Detroit, we found that the neighborhoods that received a modest number of demolitions early in Detroit's large-scale demolitions program experienced significantly fewer firearm assaults in the 14-month period following these demolitions, relative to comparable neighborhoods that received fewer demolitions. The reduction in firearm violence was more evident in the subgroup of neighborhoods that received a moderate number of demolitions (6–12) rather than a large number (13 or more). This finding indicates that for reducing firearm violence, a moderate number of demolitions may suffice.

The finding also may suggest that high numbers of demolitions may overwhelm municipal capacity to follow up on the full inventory of newly cleared properties, leaving a certain number of newly demolished properties with a similar level of disorder as the original abandoned structures. This incomplete planning in terms of what to do *after* a structure has been demolished has occurred in other cities and may have stymied the impact of Detroit's demolitions (Branas et al., 2011; Garvin et al., 2013; Clark, 2016). More complete follow-up and planning after demolitions could range from policies to quickly remove construction and demolition debris to policies for community reuse of the newly cleared space, such as rapid greening of newly created vacant lots or the construction of affordable housing.

Importantly, we also did not find evidence that assaults were simply displaced to adjacent neighborhoods. This finding is consistent with prior research, particularly on large-scale crime prevention activities, which typically have found no displacement (Telep et al., 2014). Despite the balance of evidence, however, the possibility of crime displacement is often cited as a reason not to pursue crime prevention projects (Johnson et al., 2014). Such an a priori concern might seem particularly salient for demolitions in a context such as Detroit, where the large number of vacant and abandoned properties would allow crime simply to "move" from demolished properties to the remaining abandoned properties, negating the intervention's benefits at the population level. It appears that this phenomenon did not occur.

Our results indicate, additionally, that demolitions do not necessarily reduce firearm violence by reducing illegal drug-related activity. Since vacant houses can be used to process, store, sell and/or consume drugs (Spelman, 1993; Garvin et al., 2013), eliminating these physical spaces through demolitions might be expected to reduce illegal drug activity in a given neighborhood through *opportunity reduction* (Spelman, 1993). Nearby firearm assaults might be expected to decline in turn, since drug selling and

Table 3 Estimated treatment effects by dosage level

Outcome	Dosage level			
	Lower (6–12 demolitions, $n = 177$)		Higher (13–90 demolitions, $n = 166$)	
	IRR[a] (95% CI)	p	IRR (95% CI)	p
Firearm assaults	0.86 (0.82, 0.91)	0.01	0.93 (0.87, 0.99)	0.24
Illegal drug violations	0.87 (0.79, 0.95)	0.35	1.03 (0.94, 1.13)	0.72

Block groups in each dosage stratum were compared 1:1 with matched controls and analyzed independently

[a]Incident rate ratio (IRR) using two-way fixed effects with matched sample. For model specifications, see "Methods"

consumption are associated with violence (Hohl et al., 2017). However, we did not observe clear evidence of this phenomenon, either at the main treatment threshold or in the dose–response analyses.

It appears more likely, therefore, that demolitions reduced firearm violence through other mechanisms, such as by changing perceptions of safety and guardianship. A moderate number of demolitions might be perceived as improving neighborhood conditions and larger numbers of demolitions did not enhance this perception. This explanation is consistent with theories such as *broken windows theory* (Kelling & Wilson, 1982) and *busy streets theory* (Heinze et al., 2018), which emphasize how physical condition can influence crime by signaling whether a location is safe and well cared-for. If demolitions reduce crime through this mechanism, it is possible that the first dozen demolitions are more symbolically valuable than subsequent demolitions. On this account, locations would receive similar benefits whether they received many demolitions or only a few, and thus our model could show non-significant effects at higher dosages, as we observed here.

In addition to the effects of demolitions on crime, our findings provide new information about how Detroit's demolitions were allocated. As expected, neighborhoods with more total buildings and more vacant residences received more demolitions, according to our propensity score regressions. Controlling for the other covariates, neighborhoods experiencing greater ongoing population loss and abandonment, as measured by residential vacancy change since 2009, received *fewer* demolitions. This finding is consistent with the program's stated goal to bolster neighborhoods already considered desirable. We also found, however, that neighborhoods with a larger proportion of non-Hispanic White residents received more demolitions, even after controlling for the other covariates. Such an association was not one of the program's stated goals and has not been noted in news accounts such as *The Economist* (2017). This problematic finding could be interpreted in terms of racial biases influencing which neighborhoods are considered desirable (Krysan et al., 2009) or large-scale housing discrimination that Detroit's Black residents have experienced from Great Migration-era "redlining" through the present (Smith, Lafond, & Moehlman, 2018).

Limitations

While we took steps to minimize confounding, unobserved, time-varying factors could have influenced the outcomes we observed. Police response times, for example, are reported to have declined substantially since 2014 (Wilkinson, 2017) and some vacant houses have been boarded for security (Stafford, 2017). The same neighborhoods chosen to receive more demolitions might have also been chosen to receive these other services, which could be partly responsible for the observed reductions. Moreover, the comparison of lower- and higher-dosage treatment effects could be suspect if treatment assignment was dynamic, i.e. if larger numbers of demolitions were assigned to locations based on whether neighborhood conditions appeared to be improving during the treatment period.

Police department crime data are an imperfect measure of the outcomes we studied here. These data can reflect differential reporting rates by neighborhood or patterns in police enforcement. For example, if police focused their enforcement efforts in the same areas receiving demolitions and paid less attention to the control locations, our approach could fail to detect true declines in illegal drug activity. Further research would be necessary to reject the possibility that demolitions influence either resident reporting or police behaviors, independent of true incidence. Moreover, there has been controversy over the accuracy of Detroit's crime data in past decades (Ashenfelter, 2001).

We did not conduct direct observation of the demolition program or collect data from residents related to neighborhood physical and social changes. These intermediate observations could help validate and explain the phenomena we observed in our models. Moreover, we only assessed the effects of treatment during the post-treatment period. It is possible that treatment also had effects during the 15-month treatment period, perhaps related to the demolitions activity itself rather than the post-demolition physical landscape. Future research could examine these effects.

Finally, our displacement analyses only tested for spatial displacement of each outcome among adjacent units. Therefore, we cannot conclude that no displacement occurred. Other forms of displacement could include more distal spatial effects (e.g., spillover into suburbs) or displacement of crime types, such as a transition from firearm assaults to non-firearm assaults. However, we employed the most commonly used approach to testing for crime displacement, whereas many prior studies of crime prevention have not analyzed displacement at all (Johnson et al., 2014).

Conclusions

Understanding how demolitions affect crime could influence how cities respond to physically blighted buildings. We found that Detroit's demolitions program may have reduced subsequent violence in the neighborhoods that received more than 5 demolitions during the first 15 months of the demolitions program. Receiving substantially more demolitions did not appear to improve outcomes. Non-Hispanic White residents may have benefited disproportionately from this program. In light of racial disparities in exposure to firearm violence, our findings would support racial equity as an explicit consideration when cities allocate blight remediation resources.

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Compliance with ethical standards

Conflict of interest Jonathan Jay, Luke W. Miratrix, Charles C. Branas, Marc A. Zimmerman, David Hemenway declare that they have no conflict of interest.

Human and animal rights and Informed Consent This article does not contain any studies with human participants or animals performed by any of the authors. The Harvard T.H. Chan School of Public Health institutional review board waived review of this study as non-human subjects research.

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