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Faculty Research Working Paper Series

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December 2023

RWP23-033

The effects of a place-based intervention on resident reporting of crime and service needs: A frontier matching approach

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Keywords. statistical matching, matching frontier, place-based intervention, reporting behavior, 911 data, 311 data

Acknowledgments. The Bloomberg Harvard City Leadership Initiative at the Harvard Kennedy School conducted this work with funding from Bloomberg Philanthropies. We express sincere appreciation to the Division of Citizen Services in Buffalo, NY. First to Director Oswaldo Mestre Jr. for his instrumental partnership, which played an indispensable role in enabling this study. We also extend our deep gratitude to the Division of Citizen Services team, including Juweria Dahir, Ronald Dixon, Ellen Harris Harvey, Michael Smith, and the dedicated AmeriCorps Vista Volunteers including Sydney Collins, Zulmary Cruz, Cerys Hill, Opeoluwa Fadeyi, Rachel Laster, Colin Maczka, Pemba Sherpa, Desiray Slaughter, Hunter Spitzer, Jessica Starks, and Hailey Whitney. This project would not have been possible without the support of many colleagues including Kate Bischoff, Anthony Chen, Ben Clayman, Fernando Fernandez-Monge Cortazar, Tanya Dall, Elijah de la Campa, Jonathan Jay, Michelle Kondo, Jessica Lasky-Fink, Quinton Mayne, and Elizabeth Patton.

Abstract. Objectives: Prior research has found that reporting behavior of crime incidents and service needs remain low in many U.S. cities, which may be improved by place-based interventions. This study investigates whether a place-based intervention combining door-to-door education, outreach, rapid beautification, and community-oriented law enforcement can affect reporting of crime (through emergency 911 calls) and service needs (through non-emergency 311 calls).

Methods: This study employs a matching strategy using observational data from a large public repository to generate effect estimates of a place-based intervention on reporting of 911 and 311 calls. Matching is conducted using the matching frontier with an energy-distance balance metric, which allows for fine managing of the balance-precision tradeoff. Results are analyzed by overall call volume and individual sub-types.

Results: Findings indicate that treated households were 32% more likely to report a drug-related crime than untreated households within 3 months of the intervention and 42% more likely within 5.5 months compared to untreated households. In examining calls related to blight, treated households were 9% more likely to report a blight incident than untreated households within 3 months of the intervention, with similar results within 5.5 months. There was no evidence of differences in reporting of other crime or service needs between treated and untreated households.

Conclusions: This research fills an important gap in the literature by not only investigating the impact of a place-based intervention with reporting as the outcome variable, but also by assessing whether it is effective to bring together components of other interventions previously studied in isolation. The results of this study suggest that highly visible place-based interventions that build relationships between residents and institutions may have a relationship to reporting behavior, particularly for drug-related crime and blight-related service reporting.

1 Introduction

Widespread underreporting of crime and service needs has been well documented in cities across the United States (Wesley G. Skogan 1976; W. M. Skogan 1984; Levitt 1998; Goudriaan, Lynch, et al. 2004; Schnebly 2008; C. E. Kontokosta and Hong 2021). There are many reasons why a resident may choose to report (or not), including the severity of the incident, a cost-benefit calculation, context around the crime or service incident, and norms relating to the social context (Goudriaan, Lynch, et al. 2004; Felson et al. 2006; Goudriaan, Wittebrood, et al. 2006; Kwak et al. 2019). A resident's decision to report is also influenced by their community's collective experiences of systemic racism, disenfranchisement, and other forms of inequity (Levitt 1998; Schnebly 2008; Kirk and Papachristos 2011; Desmond et al. 2016; C. E. Kontokosta and Hong 2021). However, by choosing to report, residents play the role of "gatekeepers" (Greenberg et al. 1982; M. R. Gottfredson and D. M. Gottfredson 1988) in accessing public services and resolving problems (Goudriaan, Wittebrood, et al. 2006; Buell et al. 2021).

This existing theoretical work provides a solid foundation to understand the decision-making process of reporting. While the empirical base has documented promising interventions that may increase reporting (Conaway and Lohr 1994; Xie et al. 2006; Sjoberg et al. 2017; Miller and Segal 2019; Comino et al. 2020; Martínez-Schuldt and D. E. Martínez 2021; Buell et al. 2021; Jácome 2022), very few examine drug-related incidents or blight-related service needs, which are two of the most underreported incident types (Davis and Henderson 2003; Jacques, Wright, et al. 2014; O'Brien 2015; O'Brien, Sampson, et al. 2015; Theall et al. 2021). Furthermore, the interventions described in the empirical literature base can be costly and difficult to scale. A few empirical studies have examined place-based interventions (Segrave and Collins 2005; Tuffin et al. 2006; Schnebly 2008; Branas, Cheney, et al. 2011; Gill et al. 2014; O'Brien 2016), which are delivered at the community-level and often cheaper and more scalable (Yen and Syme 1999; Branas and MacDonald 2014; Blattman, D. Green, et al. 2017; Kondo et al. 2018) and this literature base has produced mixed results (Quinet and Nunn 1998; Mitchell et al. 2020). In this paper we advance this field of empirical inquiry by investigating the relationship between a place-based intervention and reporting behavior of crime (through emergency 911 calls) and service needs (through non-emergency 311 calls for service). While we do not find an association between the intervention and overall reporting of crime and service needs, we do observe an increase in reporting of drug-related crime and blight-related service needs.

The intervention, termed the Clean Sweep Initiative, is a place-based program in Buffalo, NY that brings city departments, community police officers, and local organizations together for a weekly program in high-need city blocks. The intervention consists of three components. The first is education and outreach, in which partners go door-to-door to understand resident quality-of-life issues and identify appropriate city department or service providers. The second is restoration and beautification, in which the city addresses challenges in the built environment through debris and graffiti removal, house board-ups, vacant lot maintenance, sewer repair, etc. The third is community-oriented code and law enforcement, where community police officers discuss concerns from and build relationships with residents while code enforcement officers inspect exteriors of homes and connect residents to assistance programs (Citizen Services (DCS) 2023). Prior research on the program's code enforcement arm in 2007 found that parcels that received a Clean Sweep were one-and-a-half times less likely to have a housing code violation than those which did not (Weaver and Knight 2018), suggesting the initiative to be a relevant test case for exploring whether it impacts treated households in other ways.

This study evaluates the impact of the intervention on reporting of crime and the need for city services, through emergency 911 calls and non-emergency 311 helpline calls, respectively. It relies on observational data, going back to 2009, from the city of Buffalo's public data repository. The repository contains a rich set of observational data that, like much publicly available data, incorporates features that both pose challenges for research and present opportunities for creative research design. This dataset not only includes emergency 911 calls and non-emergency 311 helpline calls, but also a rich set of covariates. This suite of variables, paired with the presence of eligible yet untreated properties, provided a unique opportunity for generating effect estimates while adjusting for confounding using matching-on-observables. When traditional matching methods—including propensity score matching, Mahalanobis distance matching, and cardinality matching—were unsuccessful in yielding comparable groups within our dataset given the extreme imbalance in the original sample, we employed a novel matching method. Our analysis was inspired by recent developments in matching, in particular, energy balancing (Huling and Mak 2020), which aims to balance the entire covariate distribution directly, and the matching frontier (King, Lucas, and R. A. Nielsen 2017), which enabled us to finely and efficiently manage the balance-precision tradeoff. This method is detailed in section 5.1.

This study contributes to the literature in three ways. First, the study explores the potential effectiveness of a place-based intervention on reporting, which combines aspects of many separate interventions into one, multi-pronged program. This speaks to an important gap in the literature that, to our knowledge, has not be filled. Second, given the limited research on the reporting of drug-related incidents and blight-related service needs, this paper makes a contribution by presenting effect estimates specific to sub-types of 911 and 311 calls. Finally, as we pursued this line of research, we had to innovate our matching technique in order to face challenges in the data. This may be useful for future researchers in confronting similar challenges in using large scale administrative data for rigorous research.

In section 2, theoretical and empirical literature on reporting behavior is presented. The setting of the Clean Sweep Initiative is described in section 3. The data and research design are detailed in section 4, the analytical strategy and results in section 5. Finally, section 6 contains a discussion of potential interpretations of the results and concludes.

2 Literature foundation

2.1 Reporting behavior: explaining the decision to report

The prevalence of underreporting has been a long-documented topic in the academic literature, with consistent findings of fewer than half of violent victimizations being reported (Wesley G. Skogan 1976; W. M. Skogan 1984; Levitt 1998; Schnebly 2008; C. E. Kontokosta and Hong 2021; Thompson and Tapp 2022). Criminal justice systems have long-undertaken research into underreporting, beginning with the United States, which began yearly national victimization surveys in 1979, with at least nine other countries following (Thompson and Tapp 2022; Davies et al. 2017; Hardy 2019). One of the more long-standing theoretical frameworks analyzing the decision to report is a two-way framework established by Goudriaan, Lynch, et al. 2004, which distinguishes first between situational and contextual processes and second between rational and normative processes, which has been widely referenced in studying reporting behavior (for example, Hoskins Haynes 2011; Torrente et al. 2017; Slocum 2018; Linning and Barnes 2022; Lee et al. 2023).

First, situational factors are physically related to the crime itself, such as whether there was violence involved, the level of harm inflicted on the victim (Goudriaan, Lynch, et al. 2004). Prior research on situational factors demonstrates a positive relationship between the severity of a crime and the likelihood that it is reported (W. M. Skogan 1984; Felson et al. 2006; Kwak et al. 2019). Second, contextual factors relate to the social context around the crime, such as the level of confidence in the police, social cohesion among neighbors, and overall level of social disorder (Goudriaan, Lynch, et al. 2004; Goudriaan, Wittebrood, et al. 2006). Contextual factors have been widely studied, particularly how confidence in the police is related to positive and negative past experiences with the police (Desmond et al. 2016; Kwak et al. 2019; Ang et al. 2021; Sheppard and Stowell 2022), while others have examined how the level of social disorder is related to collective efficacy and reporting (Sampson et al. 1997; Ross and Mirowsky 2001; Gracia and Herrero 2007; Pitner et al. 2013; O'Brien and Winship 2017).

Third, rational pathways refer to the cost-benefit considerations that a person makes when deciding to report—costs could include time in contacting the police, risk of retaliation, amount of loss or injury, while benefits could include the likelihood of police response, monetary value of a stolen item, and potential for reduced risk of future crime (Goudriaan, Lynch, et al. 2004). Extensive prior research has documented the cost-benefit calculus and its relationship to reporting behavior (Wesley G Skogan 1994; Rodríguez et al. 2001; Kaukinen 2002; Fisher et al. 2003; Tarling and Morris 2010;

Brunson and Wade 2019). Finally, normative factors refer to the customs in an individual's social context that influence their reporting behavior, including the level of attachment to physical surroundings or social networks, female empowerment, or level of police avoidance (Brunson and Wade 2019; Kwak et al. 2019; Decker et al. 2019; Hullenaar and Ruback 2021).

The theoretical literature has documented the complex factors influencing the decision to report both crime and service needs. There are additional findings within the theoretical literature that speak directly to the decision to report drug-related and blight-related service needs—two main findings of this paper—that are explored next.

2.2 Reporting of drug crime and blight-related service needs

Drug crimes are among the least likely types of crimes to be reported (Davis and Henderson 2003; Jacques, Wright, et al. 2014). As reporting a drug crime can put physical and social safety at risk, the cost-benefit calculus usually results in bystanders not reporting (Brunson and Wade 2019). The perceived costs to reporting are high for bystanders and accessories alike; dealers rely on violent retaliation when threatened—as they are unable to rely on the formal criminal justice system to resolve disputes—so the risk of retaliation is real (Jacques, Wright, et al. 2014; Jacques and Allen 2015). Not only are the perceived costs high, but the perceived benefits are low, as drug crimes often lack a distinct victim and their adverse effects are diffused throughout society. As such, those reporting drug-related incidents do not directly receive the benefits of reporting, so it is unclear whose responsibility it is to report (Jacques and Allen 2015).

As for blight-related service needs, prior research has documented why calls for service go unreported. There are disparities in 311 calling behavior across demographic and socioeconomic groups, meaning that certain groups' concerns go undocumented and unaddressed (C. Kontokosta et al. 2017). Furthermore, individuals with a greater attachment to the physical space are more likely to report 311 incidents (O'Brien et al. 2014), particularly if they are homeowners, meaning that concerns of renters and those in temporary housing are not represented (Minkoff 2016). Going beyond disparities by sub-group, 311 reporting can reflect the communities' (or individuals') level of trust in government to be responsive and just (Theall et al. 2021), as well as the extent to which residents rely on local government (White and Trump 2018). When examining blight-related calls for service, research has found that some existing 311 callers never report blight-related disorder, even when it exists (Theall et al. 2021). Furthermore, whether or not blight-related 311 calls are made is influenced by physical surroundings, level of community connectedness, and feelings of custodianship (O'Brien 2015; O'Brien, Sampson, et al. 2015).

2.3 Empirical literature on reporting behavior

Given how many varying factors play into the decision to report a drug-related crime or a blight-related service need, increasing reporting through any of these mechanisms is extremely difficult. The empirical literature base largely uses two different sets of outcome variables to study reporting—actual reporting behavior measured by 911 and 311 reports, and intent to report measured by surveys. It's worth noting that, while both can cast light on reporting behavior, they are two separate measures. Furthermore, some studies measure "actual" crime or physical disorder—rather than "reported"—through 911 and 311 calls, however the quantity of actual and reported incidents can be correlated, inversely or directly, or have no correlation at all (Wu and Frias-Martinez 2018; Theall et al. 2021). Although violent crime and property crime are the two most reliably reported types of actual incidents, reporting does not represent the true extent or distribution of crime and service needs (C. Kontokosta et al. 2017; C. E. Kontokosta and Hong 2021), particularly for drug-related crime and 311 incidents as a whole (see section 2.2).

The literature base has documented how reporting may be influenced by interventions along the lines of contextual, rational, and normative avenues.[1] First, research has found that reporting behavior can be improved by changing the contextual processes that influence the decision to report, particularly if the level of confidence in the police or social disorder among neighbors can be targeted. Residents are more willing to intervene for the common good when perceptions of the effectiveness and quality of police and institutions increase (Wells et al. 2006; Jackson and Sunshine 2007; Kochel 2012), which in turn has been found to increase reporting behavior (Conaway and Lohr 1994; Xie et al. 2006). Furthermore, when sent emails or shown pictures of government fulfilling their service request, trust and reporting of service needs has gone up (Sjoberg et al. 2017; Buell et al. 2021). Second, targeting the rational pathways of reporting—or the cost-benefit calculus—have been shown to successfully increase reporting by increasing the perceived benefits and/or decreasing the perceived costs (Comino et al. 2020; Martínez-Schuldt and D. E. Martínez 2021; Jácome 2022). Third, in examining how normative processes can be targeted to increase reporting, research found that, by changing gender norms around power and legitimacy, reporting increased (Miller and Segal 2019).

Although there is some promising research on how the decision to report can be affected, reaching broader impact is limited by cost, feasibility, and scalability. These concerns may be addressed by place-based interventions, which are delivered at the community level and are designed to target conditions within a specific geographic area. Place-based interventions target both the physical environment by reducing physical disorder (Branas, Cheney, et al. 2011; Weaver and Knight 2018; Kondo et al. 2018; Branas, South, et al. 2018; Blattman, D. P. Green, et al. 2021; Theall et al. 2021), as well

[1] Note that, as situational factors are dependent on the severity of the crime itself, these factors are excluded from the discussion (as increasing reporting via these factors would imply increasing crime severity).

as the social environment through education and outreach (Tough 2009; Martinez-Cosio and Bussell 2013; Guajardo et al. 2016). There is growing evidence that place-based interventions, rather than those designed without place in mind, may be more successful and sustainable given the changes to physical and social structures (Branas and MacDonald 2014; Kondo et al. 2018). Not only can place-based interventions be delivered to many people at once, but they are typically cheaper and easier to scale (Yen and Syme 1999; Blattman, D. Green, et al. 2017).

While place-based interventions have been used in a variety of fields, the study of their effectiveness at targeting reporting behavior—or the drivers of reporting behavior—is limited. Following a vacant lot abatement intervention, residents reported more calls of disorderly conduct, which may have been due to increased custodianship or social cohesion (Branas, Cheney, et al. 2011). Implementing community-oriented policing has been found to increase resident satisfaction, change perceptions of social and physical disorder, and improve confidence in the police (Segrave and Collins 2005; Tuffin et al. 2006; Gill et al. 2014), which in turn drives an increase in reporting (Schnebly 2008). When residents were given neighborhood-wide information about the importance of caring for physical spaces, reporting went up (O'Brien 2016). While there has been some empirical evidence showing an increase in reporting behavior following a place-based intervention, there have also been some mixed results where interventions saw both an increase and a decrease in reporting (Quinet and Nunn 1998; Mitchell et al. 2020).

This study contributes to this body of the literature in three important ways. First, by examining whether a place-based intervention—that combines individual aspects of other interventions into one—can be effective, which could be more scalable and cheaper to implement. Second, as the research base on reporting of drug-related incidents and blight-related service needs is sparse, this paper contributes to the literature by presenting effect estimates by sub-type of 911 and 311 calls. Finally, in pursuing the first two lines of inquiry, we were required to innovate methodologically to address issues in conducting matching on data from a public repository, which we hope will be useful for future researchers facing similar challenges.

3 Background and intervention description

3.1 Background

Like many cities in the United States that have faced industrial decline and a history of discriminatory housing policies, Buffalo, NY has experienced in recent years a rise in vacancy rates and blighted spaces as well as poverty rates. Redlining has since the 1930s prevented many potential homebuyers in Buffalo from acquiring property and driven down the value of existing property[2] (Blatto 2018). Once a wealthy grain and steel manufacturing hub, Buffalo's industrial decline, begun in the 1950s, resulted, as in many other rust belt cities in the United States, in reductions in jobs, increased poverty, and a high rate of resident departures (Silverman et al. 2016; Blatto 2018). By the end of the 20th century, abandoned and blighted spaces had become a serious challenge for the city. Against this backdrop, the City of Buffalo established the Clean Sweep Initiative to address some of its most urgent social challenges. With the 2008 financial crisis, the difficulty of obtaining loans in lower-income areas resulted in even more bankruptcies and a further reduction in population (Rugh and Massey 2010; Blatto 2018), making the challenges that the Clean Sweep seeks to address even more salient.

3.2 Intervention description: Clean Sweep Initiative

Established in the late 1990s to improve the wellbeing of residents in Buffalo's highest-need communities, the Clean Sweep Initiative, run by the city's Division of Citizen Services, brings more than 10 city departments, dozens of community organizations, block clubs[3], and community leaders together to engage in three main multisector collaboration activities: outreach to and education of residents; rapid service restoration and beautification of the built environment; and community-oriented code and law enforcement. Each week of the Clean Sweep season (from mid-April to October) the initiative is administered to neighborhoods of around 2-3 city blocks. Partners added over time from within and outside of City Hall reflect evolving community needs, participant feedback, and growing interest among city departments and community organizations in on-the-ground engagement. Each weekly Clean Sweep is intentionally planned to address neighborhood-specific needs related to social determinants of health[4]. The director and staff of Citizen Services work closely with the participating groups to inspire a shared mission and adapt to meet residents' needs.

During a Clean Sweep, selected blocks are closed to traffic and teams of city staff and community organization representatives go door-to-door engaging with residents and distributing information packets that highlight city and community services, such as the non-emergency help line (311), public health and safety programs, home improvement programs, fair housing laws, employment opportunities, emergency services, and youth services. Concerns surfaced in conversations with residents are either addressed immediately by appropriate on-site staff or a follow-up plan is established. If no one is home, information packets are left at the door. Public works and forestry teams remove debris,

[2] Redlining was the federally codified method of racial segregation in the United States that began in the 1930's and both restricted the flow of capital in and out of predominantly African American neighborhoods but restricted African American families from moving into predominantly White neighborhoods

[3] In the United States, block clubs are organizations formed by groups of neighbors to address common concerns by organizing collective action and advocating at the level of the local municipality.

[4] Detailed information about the Clean Sweep program was obtained through interview(s) with government official(s) and revision of program materials.

graffiti, and trash, repair signs and sewer grates, board up vacant parcels, and tend to public spaces by trimming trees and mowing grass, and community police officers introduce themselves, listen to residents' concerns, and educate them about anonymous tip lines (including the city's SafeCam[5] system), in some cases giving out their phone numbers so residents can contact them directly. Behavioral health teams are also on site. Elected officials from the neighborhood talk to residents to understand their needs and concerns and advocate for them in their elected bodies. Code inspection officers inspect the exteriors (and interiors if invited) of homes, connect residents with programs to improve infrastructure and (if needed) with fair housing representatives to address concerns. Other city staff may be present at a Clean Sweep to answer questions or gain familiarity with the neighborhood to inform upcoming programming, and volunteers including neighborhood networks, community groups, and youth organizations often participate as facilitators.

4 Data and research design

This paper investigates whether the Clean Sweep Initiative can influence reporting behavior in Buffalo, NY. The data is sourced from a large public repository from the City of Buffalo, providing both a rich set of covariates and the outcome variables. Using these datasets, the research utilizes a statistical matching strategy that combines the matching frontier with an energy-distance balance metric. This section describes both the data and research design, and begins first with the data sources.

4.1 Data sources

Data on cluster eligibility and treatment status was obtained from the City of Buffalo Management Information Systems (MIS) department. Buffalo is an Open Data city and makes non-sensitive data available for public use through an online repository. Administrative data sources contain a variety of geographic indicators including address, latitude and longitude, census block, and intersection data. Data engineers at Tolemi, a technology company that integrates data across city departments, match data from the city's OpenData Buffalo platform, called Building Blocks, to unique parcel IDs using available geographic data. All administrative data was read in real-time from Building Blocks via OpenData Buffalo, except the part two crime data, which was obtained from the Erie County Crime Analysis Center (ECCAC) and matched into Building Blocks for restricted access by the researchers. This data was treated according to the restrictions of the ECCAC, and was collapsed into counts of incidents by parcel ID, which were then anonymized and used for analysis.

The study team mapped every Clean Sweep performed during the study period (described in Section 4.2) using data provided by the MIS department on specific streets and dates. This data

[5]This system provides for Buffalo residents to register whether they have a doorbell camera. If a crime is reported nearby, the police can ask the resident to examine video recordings.

was triangulated with paper-based "walk sheets" (forms distributed to all partners who participate in a Clean Sweep that include addresses and dates) and administrative records of housing code inspections[6]. Any clusters, streets, or parcels unverified or ambiguous with respect to receiving a Clean Sweep were excluded from the sampling frame.

Each year from 2009-2020, only treated clusters were drawn; that is, no untreated clusters were drawn by the city. In 2022, we examined eligibility maps to apply city practices of assigning cluster membership to untreated units. We created overlays of the data for each year in Tolemi's Building Blocks platform and developed a "risk score" for each census block group and parcel. We then retrospectively observed the minimum score among treated clusters and used that number as the threshold of eligibility for each study year. All untreated but eligible parcels were then under consideration for inclusion in the sampling frame. The researchers examined, for each year, the total number of Clean Sweeps performed, number per council district, and cluster size in each council district. This information served as proxies for staff capability and resource constraints, which informed the researchers in drawing untreated "clusters" that adhere to feasible foot traffic patterns (described below), range of feasible cluster sizes for each year, priorities for specific council districts, and feasible cluster sizes within each council district.

Summary statistics for continuous and discrete variables used to generate the matched sample are reported in Table 1 and Table 2 (respectively).

[6]Inspections being a fundamental component of a Clean Sweep, they are reliably reflected in the data if they occur.

Table 1 Summary statistics: continuous variables

Variable		Unadjusted				Adjusted			
		Untreated		Treated		Untreated		Treated	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD
Administrative adjudication		0.06	0.44	0.09	0.52	0.09	0.59	0.09	0.54
Bedrooms		4.00	1.49	4.08	1.39	4.06	1.44	4.08	1.44
Building size		1832.20	639.08	1738.32	544.77	1759.48	568.69	1757.14	557.23
Building stories		1.74	0.37	1.70	0.36	1.71	0.37	1.71	0.36
Calls for police	All	0.23	0.92	0.25	0.94	0.29	1.03	0.28	0.93
	Part one	0.10	0.67	0.10	0.60	0.12	0.77	0.11	0.63
	Violent	0.01	0.11	0.01	0.12	0.01	0.13	0.01	0.12
	Property	0.09	0.62	0.09	0.54	0.10	0.72	0.10	0.58
	Part two	0.12	0.48	0.15	0.57	0.17	0.54	0.17	0.54
	Drug	0.01	0.12	0.02	0.18	0.02	0.16	0.02	0.18
	Other	0.11	0.46	0.13	0.53	0.15	0.50	0.15	0.49
Calls for service		0.55	1.19	0.58	1.86	0.62	1.35	0.61	1.21
Cluster size		154.41	57.36	149.33	55.14	152.39	57.55	150.23	55.94
Code inspection		0.19	0.73	0.22	0.74	0.24	0.82	0.24	0.77
DCS: American Indian or Alaskan Native	Percent change	0.16	0.78	0.13	0.77	0.08	0.69	0.08	0.71
	Percent	0.57	0.65	0.54	0.69	0.52	0.66	0.53	0.68
DCS: Asian alone	Percent change	7.39	12.10	10.27	12.03	9.28	12.09	9.33	11.10
	Percent	7.96	9.59	10.30	8.82	9.80	9.43	9.86	8.48
DCS: Black or African American	Percent change	4.40	15.10	5.21	20.46	3.37	14.56	3.72	16.59
	Percent	45.98	29.92	56.01	25.34	54.78	27.13	55.87	26.00
DCS: Hispanic or Latino	Percent change	4.51	5.99	4.80	5.99	4.45	6.16	4.46	5.87
	Percent	11.17	8.28	11.40	8.63	11.48	8.38	11.43	8.75
DCS: Non-White Hispanic or Latino	Percent change	18.84	24.09	22.61	29.35	19.33	23.17	19.72	24.74
	Percent	68.59	26.82	81.03	19.98	79.37	21.04	80.47	20.69
DCS: Population	Percent change	21.57	35.97	22.08	32.91	18.63	26.89	18.99	27.66
DCS: Residents under 18	Percent change	4.64	10.39	5.57	12.40	4.57	10.49	4.43	10.49
	Percent	24.28	6.48	28.00	5.81	27.03	5.05	27.61	5.67
DCS: Total units	Percent change	15.41	38.91	10.43	30.26	8.49	24.99	8.95	25.70
DCS: Vacancy	Percent change	13.51	5.37	15.21	5.96	14.53	5.59	15.05	5.95
	Percent	-1.46	8.52	-3.63	7.88	-3.24	6.80	-3.34	7.07
DCS: White Non-Hispanic or Latino	Percent change	2.67	21.33	-0.52	10.29	-0.77	11.62	-0.72	9.52
	Percent	31.41	26.82	18.97	19.98	20.63	21.04	19.53	20.69
First story area		1089.97	277.87	1064.12	249.82	1069.44	258.66	1066.37	252.25
Full bathrooms		1.53	0.64	1.53	0.58	1.54	0.60	1.54	0.59
Kitchens		1.47	0.54	1.50	0.53	1.50	0.54	1.51	0.53
Lot length		118.21	24.64	116.36	20.85	117.09	23.99	116.68	21.45
Lot width		36.66	15.15	33.81	10.89	34.53	11.01	34.21	11.53
Owner property count		22.52	370.05	32.18	443.89	38.15	488.56	36.67	477.32
Total livable area		1832.02	638.89	1738.35	544.86	1759.52	568.72	1757.18	557.33
Year built		1916.85	25.26	1914.38	20.48	1914.87	23.27	1914.74	21.07

Note: Means are computed by taking the mean of the year-specific means; standard deviations are computed as the square root of the mean of the year-specific variances. Standard deviations are not reported for discrete variables.

Table 2 Summary statistics: discrete variables

Variable		Unadjusted				Adjusted			
		Untreated		Treated		Untreated		Treated	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD
Business owner type	None	0.86		0.84		0.82		0.82	
	Corp.	0.04		0.05		0.06		0.06	
	LLC	0.09		0.10		0.11		0.11	
	Other	0.01		0.01		0.01		0.01	
Called police	All	0.12		0.14		0.15		0.15	
Called service		0.31		0.33		0.34		0.34	
City owned		0.00		0.00		0.00		0.00	
Council district	Delaware	0.02		0.02		0.02		0.02	
	Ellicott	0.10		0.09		0.11		0.10	
	Fillmore	0.15		0.15		0.13		0.13	
	Lovejoy	0.16		0.15		0.13		0.13	
	Masten	0.19		0.18		0.20		0.19	
	Niagara	0.10		0.10		0.10		0.10	
	North	0.08		0.08		0.09		0.09	
	South	0.05		0.05		0.04		0.06	
	University	0.16		0.17		0.18		0.18	
Fix and flip		0.01		0.01		0.01		0.01	
Lead case		0.004		0.004		0.003		0.003	
Owner occupied		0.54		0.46		0.46		0.46	
Police district	A	0.12		0.06		0.06		0.06	
	B	0.16		0.10		0.11		0.10	
	C	0.22		0.29		0.28		0.28	
	D	0.14		0.16		0.16		0.17	
	E	0.36		0.39		0.39		0.39	
Property sale		0.06		0.07		0.07		0.07	
Sewer owed		0.003		0.003		0.003		0.003	
Bathrooms	Missing	0.004		0.004		0.005		0.005	
Bedrooms	Missing	0.00		0.00		0.00		0.00	
Building size	Missing	0.03		0.02		0.03		0.03	
Building stories	Missing	0.01		0.01		0.01		0.01	
First story area	Missing	0.03		0.02		0.03		0.03	
Kitchens	Missing	0.03		0.02		0.03		0.03	
Lot length	Missing	0.03		0.02		0.03		0.03	
Lot width	Missing	0.01		0.01		0.01		0.01	
Total livable area	Missing	0.03		0.02		0.03		0.03	
Year built	Missing	0.03		0.02		0.03		0.03	

Note: Means are computed by taking the mean of the year-specific means. Standard deviations are not reported for discrete variables.

4.2 Sampling frame

The sampling frame includes treated and untreated parcels in the City of Buffalo eligible for a Clean Sweep from 2009-2013, 2015-2019, and in 2021. Although the Clean Sweep program has been in operation since the 1990s, 2009 is the earliest year for which reliable administrative data is available (2014 was excluded as data on Clean Sweep locations was not available, and 2020 due to complications from the COVID-19 pandemic). Eligibility to receive treatment is determined based on whether clusters meet the following criteria: high incidents of crime; fully contained within one council district; fully contained within one police district; high density of residential parcels; composed of 30-400 safely walkable parcels within a contiguous neighborhood. The number of completed Clean Sweeps within a given year, determined by the Division of Citizen Services based on staffing, resources, and weather, provides a suitable condition for matching, as not all eligible units can receive treatment each year.

The treatment eligibility and allocation process is as follows. Each year, the MIS department provides the City of Buffalo Division of Citizen Services with a map showing the city divided by council district and neighborhoods with an overlay of crime density, types of crime incidents, housing code violations, and suggested Clean Sweep locations. Staff ensure that candidate Clean Sweeps are within contiguous neighborhoods and examine available data related to unemployment, poverty, and other socio-economic factors. The Division of Citizen Services then conducts ground truthing whereby staff conduct in-person inspections of candidate areas to assess the level of visible physical disorder. Staff also identify vacancies as Clean Sweeps often board up abandoned or schedule demolition of deteriorated housing. Level of social cohesion is also incorporated. Staff rely on conversations with residents to gain an understanding of which city segments feel like cohesive neighborhoods and try to keep Clean Sweeps within geographic areas that make sense. Treatment allocation among the pool of eligible areas is then determined by taking into account staff constraints, resources, and weather, sometimes all at once before a Clean Sweep season begins and sometimes on a week-by-week basis.

Clean Sweep clusters are always within the same council district as resources are allocated at the council district and police district level (council districts are completely contained within the same police district in Buffalo). Clean Sweeps being performed in dense residential areas (regardless of vacancy rate), very few occur in highly commercial areas. Geographic construction of the clusters is arbitrary, based on the number of properties feasible given weather conditions and available staff and resources on a given day as well as the walkability of the blocks, usually selected in a "U," "S," or "W" pattern such that partners arrive in close proximity to their cars at the conclusion of a Clean Sweep. Being performed on foot (except by staff operating heavy machinery), Clean Sweeps rarely cross busy thoroughfares to ensure the safety of partners.

4.3 Outcomes

Outcome variables were constructed using panel administrative data from the City of Buffalo on police incidents (911 calls and police-reported incidents) and calls for service (311 calls). The 911 data was categorized according to the FBI's uniform crime reporting program (UCR). Part one offenses include violent[7] and property crime[8], part two offenses included drug crime[9] and all other offenses[10]. Researchers consulted with the Buffalo Police Department to clarify sub-types of these offenses. The City of Buffalo's 311 data had more than 220 original sub-types. In consultation with the director of the Buffalo 311 system, we cleaned this data to combine duplicate types (e.g., misspelled or outdated names and sub-types that were used inconsistently) into single variables. We grouped 311 data into call types directly and visibly addressed during the Clean Sweep [11] and those that are not. Calls that were directly and visibly addressed during Clean Sweep are those we refer to as "blight-related incidents".

To examine early and late effects of the program, we established fixed time horizons for police and service call outcomes for parcels that received the intervention. The start date for police incidents is the day after the Clean Sweep, for service incidents, five days after the intervention (because city staff often input 311 service requests to the system on behalf of residents during the remainder of the business week). Untreated parcels having no clear "start date," we used matching (described in section 5.4 below) to anchor the untreated to treated parcels and considered the start and end dates of the untreated parcels to be those of the corresponding matched treated parcels. Time horizons for the endline were 90 days and 166 days after the intervention (see limitations below), elsewhere referred to as "3 months" and "5.5 months." Data from months outside the study period was omitted and continuous outcome variables were generated to capture the number of times each outcome occurred over each time horizon. Dummy variables subsequently generated took a value of 1 if the event occurred within the timeframe and 0 if the event had not occurred in the timeframe.

4.4 Variables and sample size

Treated parcels are those located within an eligible cluster that received a Clean Sweep in the years 2009-2013, 2015-2019, and 2021, untreated parcels those located within an eligible cluster that did not

[7] Murder and nonnegligent manslaughter, rape, robbery, and aggravated assault.

[8] Burglary, larceny, motor vehicle theft, and arson.

[9] Drug abuse violations.

[10] Other assaults (simple), forgery and counterfeiting, fraud, embezzlement, stolen property, vandalism, weapons, prostitution and commercialized vice, sex offenses (except forcible rape, prostitution, and commercialized vice), gambling, offenses against family and children, driving under the influence, liquor laws, drunkenness, disorderly conduct, vagrancy, suspicion, curfew and loitering (persons under age 18), runaways (persons under age 18), and all other offenses.

[11] Abandoned vehicles, boarding, bulk trash, curb repair, fair housing, fallen tree, graffiti, holes in the road, illegal dumping, lead paint inspection, ordinance violations, pests, rodents, save streets (the name for the Clean Sweep program in the Buffalo 311 system), sewer repairs, sidewalks, street signs and signals, stump removal, abandoned trash or recycling totes, excess trash, tree request, vacant lot issues, and leaves and lawn cleanup.

receive a Clean Sweep in the year it was matched to a treated parcel. The treatment variable is binary (1 for treated parcels and 0 for untreated parcels). The intervention being directed at residential properties, non-residential properties are excluded. Each parcel being considered an independent observation for each year it is included in the sampling frame, a parcel included in an untreated cluster may appear in a treated cluster for a later year. A treated parcel may similarly appear as an untreated cluster for a later year. We addressed this potential source of bias by investigating the wash out period of the intervention. Consultation with the city and exploration of the literature suggesting that wash out times for impacts on reporting of crime and service needs would not exceed two years, parcels are excluded from the sampling frame for a given year if they had received treatment up to 2.5 years prior.

As our goal in employing matching was to reduce confounding, the variables included in the matching procedure were those strongly associated with treatment and outcomes. As treatment eligibility is determined by predictors of poverty, socio-economic status, and crime, we chose covariates predictive of these factors. For example, although the correlation between treatment and first story area might not be immediately apparent, we assume parcels with a larger area, likely being bigger and thus reflecting higher income, to be less likely to receive treatment. Similarly, we assume treatment to be associated with whether a parcel is owned by the city or a particular type of business, as these variables capture signals from predatory landlords, likelihood of resolving code violations, income level, etc. We employ similar logic for each covariate included in the matching procedure.

Before matching, the sampling frame included 248 treated clusters with 30,344 treated parcels and 1,003 untreated clusters with 126,992 untreated parcels for the years 2009-2013, 2015-2019, and 2021. Average cluster size for all 157,336 observations was 164, with a minimum cluster size of 22 and maximum of 438. For ten variables that had missing values[12], the missingness not being severe, we employed the missingness indicator approach whereby missing values are replaced by the mean and a dummy variable is included that corresponds to whether the variable is missing. These variables were included in the matching procedure. The unavailability of 2020 census data for 170 properties in Buffalo resulted in missing values for these observations—however, all of these properties were later pruned from the matched sample during the subsetting phase. Appendix A provides descriptions of the cluster-level, cross-sectional, and panel data used in the study.

4.5 Limitations

Data generated by cities and used in open data platforms pose many challenges in a research context. With respect to the entire dataset, the match rate for geographic data to unique parcel ID numbers

[12] building stories, year built, first story area, bedrooms, building size, bathrooms, kitchens, lot length, lot width, and total livable area

is 90% or higher where the unmatched data have no usable geographic data. That the unmatched data is not reflected in the dataset is a limitation of the present study. Moreover, although panel data was available for some variables, only cross-sectional data was available for others (see Appendix A). Most of the cross-sectional data is structural to the parcel and thus unlikely to change (e.g., lot width); however, in the event that the variable changed within the study timeframe, the data would only have the last-updated value with no history unavailable.

Important demographic variables at the parcel level critical to reducing confounding are also lacking. In the absence of parcel-specific demographic data, data from the 2020 census was used. Clusters are not contained within entire census block groups, and each parcel takes the value of its census block group. Due to historic redlining, it was important to address extreme imbalance observed in the census block group variables including race. This limitation is addressed in the discussion of the matching strategy below.

Treatment eligibility is not uniform across high-need areas due to feasibility constraints of the intervention. Houses on the ends of streets or on facing cross streets are serviced only if they follow the pattern of foot traffic and do not require crossing a busy street. Not all parcels in high-need areas are equally eligible to be included in a Clean Sweep cluster, for example, areas occupied by mostly commercial properties and properties that face busy commercial thoroughfares, which pose a danger to partners. Nor were Clean Sweeps occasionally performed at apartment complexes included owing to the unavailability of unit-specific information from the administrative data sources. As such, our results cannot be extrapolated to high-need parcels located in areas of Buffalo where a Clean Sweep is infeasible (we specify on our estimand in section 5.2).

In Buffalo, incident data (311 and 911) is associated only with the parcel in which the incident occurred. For privacy reasons, it does not include information on the parcel from which the call originated, which may be different from the parcel in which the incident occurred. The research is thus unable to distinguish calls concerning an issue on an individual's property from those concerning an issue on another's property. Clean Sweep staff input 311 data for up to five days after the intervention, but because that data captures signals from implementation rather than resident response, those five days are excluded. As it is not possible to identify where the call originated, this also excludes resident calls made during the five-day time period and could underestimate the number of calls. Further, although it would be interesting to investigate longer-term effects of the program, because the treatment is repeated each year, the longest time horizon possible is 166 days[13] without having the endline variables be affected by a future treatment.

[13] the latest and date of any Clean Sweep any year was October 31, and the earliest date of any Clean Sweep any year was and April 17, respectively, the difference in days is 167 between October 31 and April 17, but 166 was chosen to account for leap years

The year 2021 did not follow the pattern for treatment eligibility and allocation observed in previous years. The study team was planning to run a randomized controlled trial on the Clean Sweep Initiative to examine its impact on wellbeing and visible blight utilizing survey data. The RCT was cancelled, however, due to lack of statistical power and resource constraints incurred by the COVID-19 pandemic. In 2021, cluster eligibility determinations and randomization were done in advance of the Clean Sweep season and included more data than was previously used. For in-depth information on the original RCT, cluster eligibility determination, and randomization procedure see the AEA RCT registry (Robb et al. 2021). The present study included all randomly assigned treated and untreated clusters as well as 32 additional untreated but eligible clusters not included in the original RCT for reasons of resource constraints. Between-year differences are accounted for in the analytical strategy outlined below.

5 Analytic Strategy and Results

We estimated the treatment effects in two steps: first matching the treated and untreated units, and then estimating treatment effects using outcome regression models in the matched samples. These procedures are described in the following sections.

5.1 Matching

Matching was used to balance the distribution of covariates between treated and untreated parcels within each year and to identify the time horizon used to populate the outcomes for the untreated units. Each untreated unit was assigned the same time horizon as the treated unit to which it was matched. Other methods of adjusting for confounding that did not involve pairing units, such as weighting methods or regression methods in isolation, could not be used to satisfy this second function.

The matching specification needed to accommodate a number of unique features of the dataset including many covariates, extreme imbalance and lack of overlap between treatment groups, small hypothesized effect sizes requiring large matched samples, and within-year treatment allocation. Among the matching methods attempted were propensity score matching (Rosenbaum and Rubin 1983), Mahalanobis distance matching (Rubin 1980), and cardinality matching (Zubizarreta et al. 2014). All failed for various reasons. The extreme imbalance made propensity score matching impossible because propensity scores could not be accurately estimated in a way that yielded overlap in the propensity score distribution. Similarly, Mahalanobis distance-matched units were too far apart to yield good balance. Cardinality matching, designed for use in samples with low overlap, avoids these problems by directly optimizing matched sample size subject to balance constraints without pairing units (Visconti and Zubizarreta 2018). Cardinality matching, too, failed to balance features of the covariate distributions beyond the means, and its extreme computational intensity rendered tuning the method to manage the balance-precision tradeoff impossible.

To solve these problems, we developed a novel matching method inspired by existing methods, energy balancing and the matching frontier. This method sought to optimize a measure of covariate balance termed the "energy distance" between treatment groups. This is a nonparametric, multivariate, scalar measure of the difference between covariate distributions and a multivariate analogue of the Cramér–von Mises statistic for comparing univariate distributions (Rizzo and Székely 2016). When the energy distance is equal to 0, the covariate distributions are identical, indicating that the treatment groups are perfectly balanced. A weighted version of the energy distance was developed by Huling and Mak 2022, who developed it as the criterion in an optimization problem to find balancing weights. We sought matching solutions that optimized the energy distance as well as solutions with an energy distance near the optimum but with a greater remaining sample size.

The energy distance as defined by (Rizzo and Székely 2016) is as follows:

Let Z and V be independent random vectors with cumulative density functions G and H, respectively. Let Z' and V' be independent and identically distributed copies of Z and V, respectively.

The energy distance between distributions G and H is defined as:

$$\mathcal{E}(G,H) \equiv 2\mathbb{E}\|Z-V\|_2 - \mathbb{E}\|Z-Z'\|_2 - \mathbb{E}\|V-V'\|_2$$

where $\|\cdot\|_2$ is the Euclidean norm.

When both G and H are empirical cumulative distribution functions, the sample energy distance can be computed as follows, where n and m are the sizes of Z and V, respectively:

$$\left[\frac{2}{nm}\sum_{i=1}^{n}\sum_{j=1}^{m}\|Z_i - V_j\|_2\right] - \left[\frac{1}{n^2}\sum_{i=1}^{n}\sum_{i'=1}^{n}\|Z_i - Z_{i'}\|_2\right] - \left[\frac{1}{m^2}\sum_{j=1}^{m}\sum_{j'=1}^{m}\|V_j - V_{j'}\|_2\right]$$

The matching frontier developed by King, Lucas, and R. A. Nielsen 2017 provides a framework for managing the relationship between balance and remaining sample size. At each point along the frontier, an optimal matching solution is found, which creates a function that relates sample size to the optimal balance at that sample size. A point can be selected on the frontier to manage a researcher-specified tradeoff between balance and sample size, and the matched sample at that point is used to assess balance more finely and estimate the treatment effect. We created a matching frontier using the energy distance as the balance measure. We did so by sequentially dropping the unit that, when dropped, yielded the largest decrease in the energy distance (i.e., the greatest improvement in balance). Each point along the frontier represents an ever-shrinking matched sample with better balance than the larger matched sample before it, as measured by the energy distance[14]. We selected a sample along the frontier that yielded the largest sample size such that largest standardized mean difference averaged across years for each covariate was below 0.1, thereby ensuring adequate balance while maximizing the resulting matched sample size.

Optimizing the energy distance along the matching frontier does not involve pairing units; units are simply pruned from the original sample, with the remaining un-pruned units at each point along the frontier constituting the resulting matched sample. In anticipation of performing an additional pairing step in the matched sample (described subsequently), we added an additional constraint that only untreated units could be pruned until the ratio of untreated to treated unit was 2:1, at which point units from either group were dropped while retaining this ratio. The original sample of 157,336 observations was pruned to 77,955 observations. Once an acceptable matched sample was found, we performed an additional pairing step to pair each untreated unit in the matched sample to a treated unit using a 2:1 match without replacement. This pairing step did not discard any units, but simply arranged the selected subsample into sets, each with one treated and two untreated units. This additional step of pairing after subset selection is recommended by Zubizarreta et al. 2014 who demonstrate that pairing on prognostically important variables after an initial step of subset selection improves the precision of the resulting effect estimate and protects against certain kinds of unmeasured confounding. We followed this recommendation by choosing a set of the covariates deemed most predictive of the outcome[15] and performing 2:1 nearest neighbor matching on the Euclidean distance of the standardized covariates in the set. The entire matching analysis (i.e., creating the frontier and pairing in the selected sample) was performed separately within each year. The standardized mean differences of the adjusted and unadjusted samples averaged across years are shown in Figure 1.

[14] Eventually, dropping units starts to increase rather than decrease the energy distance; we considered only samples that were as large or larger than the sample with the lowest energy distance.

[15] Part one crime, part two crime, owner occupied, code inspection, administrative adjudication, fix and flip, property sale, full bathrooms, kitchens, DCS percent of Black or African American, DCS percent of White Non-Hispanic, DCS percent of Hispanic or Latin American, DCS Percent of residents under age 18.

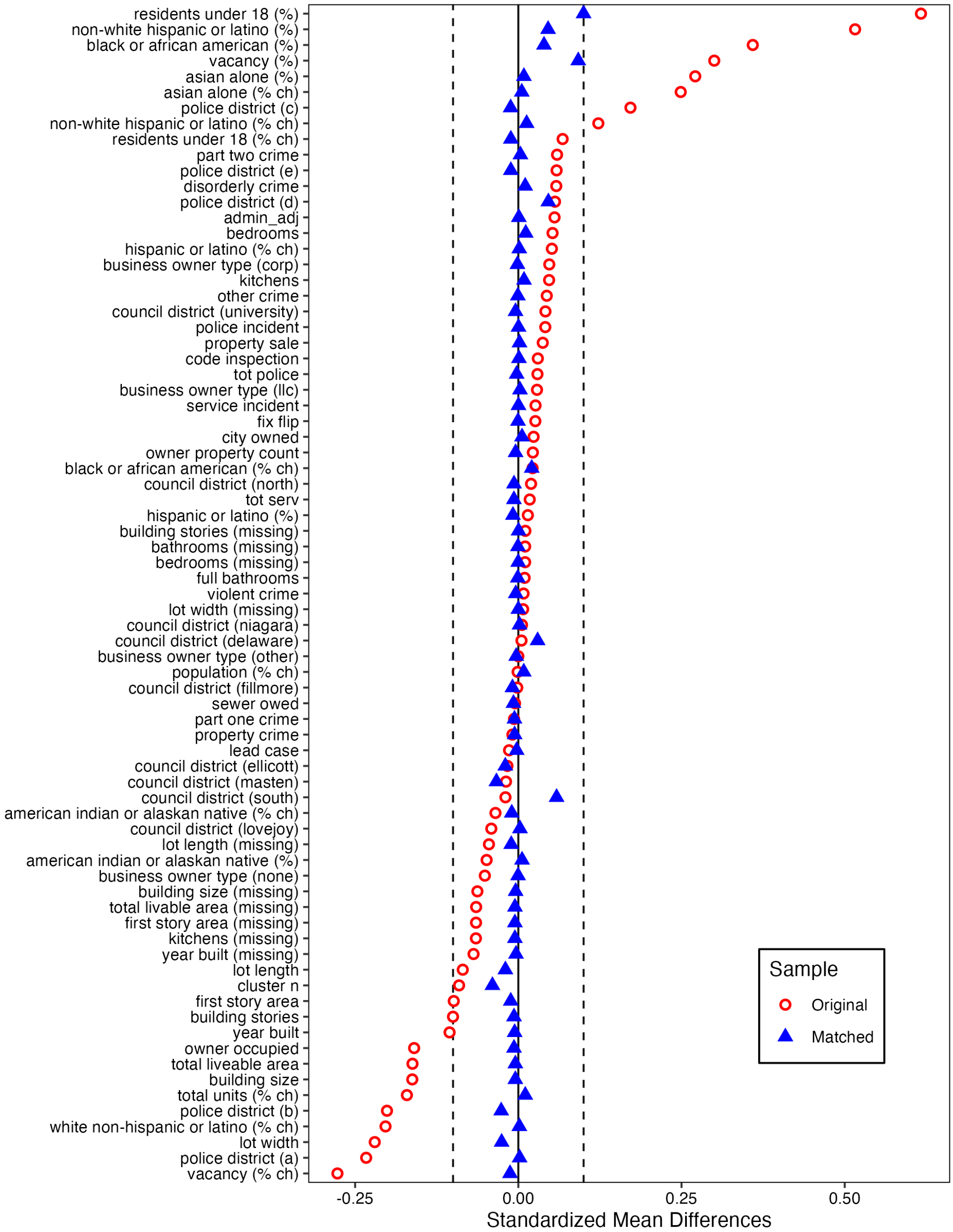

Fig. 1 Love plot: Standardized mean differences for unmatched and matched samples
Each point represents the average standardized mean difference across all years for each variable.
Thresholds (dashed vertical lines) are placed at -.1 and 1.

5.2 Estimating Effects

The estimand of interest is the average treatment effect in the matched sample, here measured as the ratio of the probabilities of each outcome between the treatment groups. We sought a single marginal effect estimate, averaging across years, as a summary of the overall effect of each treatment. Estimating the effect for each outcome proceeded as follows. First, logistic regression models were fit for each outcome with year, treatment, covariates, treatment-by-year interaction, and covariate-by-year interactions to the full matched sample[16]. The model was fit incorporating weights resulting from the matching, which were .5 for the control parcels and 1 for the treated parcels, in order to mimic a balanced randomized trial (Austin 2008). Two-way cluster-robust variance estimates were used (Cameron et al. 2011), with clustering on matched pair membership (Abadie and Spiess 2021) and on treatment cluster (Abadie, Athey, et al. 2022). The covariates included were the same as those used for the pairing step as well as a set of covariates with the largest remaining imbalances[17] (Nguyen et al. 2017).

Logistic regression models were fit for each outcome (binary, 0 if the outcome did not occur and 1 if the outcome did occur) with year, treatment, and the following covariates: Part one crime, part two crime, owner occupied, code inspection, administrative adjudication, fix and flip, property sale, full bathrooms, kitchens, DCS percent of Black or African American, DCS percent of White Non-Hispanic, DCS percent of Hispanic or Latin American, DCS Percent of residents under age 18, DCS Percent vacant, whether any service call was made, and whether any police call was made. The model also included treatment-by-year interaction and covariate-by-year interactions to the full matched sample. The model was fit incorporating weights resulting from the matching, which were .5 for the control parcels and 1 for the treated parcels. Two-way cluster-robust variance estimates were used, with clustering on pair membership and on treatment cluster.

The logistic regression equation can be expressed as:

$$\ln\left(\frac{P(Y_{ki}=1)}{1-P(Y_{ki}=1)}\right) = \sum_{y\in\text{Years}} \mathbb{I}(\text{Year}_i = y)\left(\beta_{0y} + \beta_{\tau y}\text{Treat}_{yi} + \sum_{p\in P}\beta_{py}X_{pyi} + \sum_{p\in P}\beta_{p\tau y}(X_{pyi}\times\text{Treat}_{yi})\right)$$

where $P(Y_k = 1)$ is the probability of binary outcome variable k being equal to 1, where k includes violent crime, property crime, drug crime, other crime, blight, and non-blight. Years indicate the study years, which included 2009-2013, 2015-2019, and 2021. Treat_{yi} is the binary indicator for whether an individual parcel i was treated or not in year y. X_{pyi} corresponds to each covariate p for each parcel i in year y, where the covariates P include part one crime, part two crime, owner occupied, code

[16] Including interactions between year and all other predictors is equivalent to fitting a separate model within year.

[17] Part one crime, part two crime, owner occupied, code inspection, administrative adjudication, fix and flip, property sale, full bathrooms, kitchens, DCS percent of Black or African American, DCS percent of White Non-Hispanic, DCS percent of Hispanic or Latin American, DCS Percent of residents under age 18, DCS Percent vacant, whether any service call was made, and whether any police call was made.

inspections, administrative adjudication, whether the property had been fixed and flipped, property sales, full bathrooms, kitchens, Department of Census and Statistics (DCS) estimate of the percent of Black or African American, DCS percent of White Non-Hispanic, DCS percent of Hispanic or Latin American, DCS Percent of residents under age 18, DCS Percent vacant, whether any service call was made, and whether any police call was made. The model allowed for year-specific intercepts and slopes for all predictors. The model was fit incorporating weights resulting from the matching, which were .5 for the control parcels and 1 for the treated parcels. Two-way cluster-robust variance estimates were used, with clustering on pair membership and on treatment cluster.

Next, a marginal effects procedure was used to estimate marginal probabilities under each treatment for each year, which were then used to compute risk ratios (i.e., the ratio of the marginal probability of the event under treatment to that under the absence of treatment). This involved generating outcome predictions from the model had all parcels been treated and had all parcels been untreated (i.e., setting Treat_{yi} to 1 for all units and then setting Treat_{yi} to 0 for all units) and computing the average probabilities across units for each set of predictions; this procedure is also known as g-computation (Snowden et al. 2011). Next, we averaged these averaged probabilities across years to arrive at the marginal risk under treatment and the marginal risk in the absence of treatment. Finally, we computed the natural logarithm of the ratio of these marginal risks, computed standard errors and confidence intervals using the delta method, and exponentiated the estimates and confidence interval bounds to arrive at effect estimates and their confidence intervals on the appropriate scale (i.e., risk ratios)[18]. It should be noted that the year-specific treatment effects were not estimated due to within-year covariate imbalances that were not present when averaging across years.

5.3 Software

Matching analyses were performed in R (R Core Team 2021) using the *MatchingFrontier* package (version 4.1.0[19]) (King, Lucas, R. Nielsen, et al. 2023) to create the matching frontier and the *cobalt* package (version 4.4.1) (Greifer 2022) to assess balance. After matching, outcome regression models were fit using Stata (STATA Corp LLC 2021), with the vcemway package (Gu and Yoo 2019) used to compute the two-way clustered standard errors. Treatment effect estimates were computed using Stata's `margins` and `nlcomp` commands, which implement the delta method to compute standard errors for post-estimation quantities (STATA Corp LLC 2021).

[18] An alternative approach would have been to compute a single marginal risk for the full sample under each treatment rather than averaging year-specific marginal risks. The former approach weights each parcel equally while the latter weights each year equally. The marginal risk marginalizing over year is equivalent to a weighted average of the year-specific risks where years with larger samples are given more weight; however, such years are not necessarily more representative (i.e., the sample size within each is arbitrary), so an unweighted average seemed more natural. In any case, effect estimates from the two approaches were similar.

[19] Available on Github at https://github.com/IQSS/MatchingFrontier

5.4 Results

Table 3 shows the risk ratios for receiving treatment from the multivariate logistic regression models outlined in 4.2 above for the 3-month follow-up and 5.5-month follow-up averaged across all 11 years with a binary outcome variable (0 if no call was made, 1 if one or more calls were made). Ninety-five percent CIs in parentheses are based on clustered standard errors by Clean Sweep cluster and matched pair membership. Three-month follow up represents the time horizon of 90 days, 5.5-month follow-up the time horizon of 166 days, post intervention (see section 5.2 for a description of the time horizon).

Table 3 Risk ratios for receiving treatment on key outcome variables, 3-month follow-up

	Marginal probability		Risk ratio		95% CI
	Untreated	Treated			
All police incident types	0.0645	0.0632	0.979		(0.921, 1.042)
Violent crime	0.0038	0.004	1.06		(0.816, 1.376)
Property crime	0.0244	0.0228	0.936		(0.848, 1.033)
Drug-related crime	0.0047	0.0062	1.321	*	(1.034, 1.687)
Other crime	0.0395	0.0393	0.995		(0.913, 1.085)
All service incident types	0.1286	0.1343	1.044		(0.993, 1.097)
Blight incidents	0.0401	0.0437	1.09	*	(1.007, 1.180)
Non-blight incidents	0.0989	0.1025	1.037		(0.978, 1.099)

Coefficients presented are risk ratio estimates for receiving treatment from multivariate logistic regression models averaged across all 11 years. *** p ¡ 0.001, **p¡0.01, *p¡0.05.

Controlling for the most important covariates used in matching, their interactions with year, and interactions between year and treatment, we see that households who received a Clean Sweep were 32% more likely to have one or more drug crimes reported than households who did not receive a Clean Sweep within 3 months of the intervention (risk ratio = 1.321, 95% CI [1.034, 1.687], p=0.026). In the same time period, households who received a Clean Sweep were 9% more likely to have a blight-related 311 incident reported than those who did not receive a Clean Sweep (risk ratio = 1.090, 95% CI [1.007, 1.180], p =0.034). We also present the marginal probabilities for both the treated and untreated groups, recognizing that these incident types rarely occur. The marginal probability of an untreated household having a call related to drug-related crime in the 3 months following the intervention is 4.7 per 1000 untreated households while the marginal probability for a treated household is 6.2 per 1000. The marginal probability of an untreated household having a call made to their household related to blight in the 3 months following the intervention is 40.1 per 1000 untreated households while the marginal probability for a treated household is 43.7 per 1000 households.

Next, using the same covariates, we see that within the 5.5 month time horizon, households who received a Clean Sweep were 42% more likely to have one or more drug-related incidents reported than households who did not receive a Clean Sweep (risk ratio = 1.415, 95% CI [1.162, 1.723], p=0.0005). In the same period, households who received a Clean Sweep were 9% more likely have one or more blight-related incidents reported than households who did not receive a Clean Sweep (risk ratio =

Table 4 Risk ratios for receiving treatment on key outcome variables, 5.5-month follow-up

	Marginal probability		Risk ratio		95% CI
	Untreated	Treated			
All police incident types	0.0978	0.0965	0.986		(0.939, 1.036)
Violent crime	0.0066	0.0067	1.021		(0.834, 1.249)
Property crime	0.0361	0.0335	0.927		(0.858, 1.002)
Drug crime	0.0077	0.0109	1.415	***	(1.162, 1.723)
Other crime	0.0641	0.0634	0.989		(0.926, 1.056)
All service incident types	0.2019	0.205	1.015		(0.975, 1.058)
Blight incidents	0.061	0.0664	1.088	*	(1.017, 1.164)
Non-blight incidents	0.1617	0.1626	1.006		(0.959, 1.055)

Coefficients presented are risk ratio estimates for receiving treatment from multivariate logistic regression models averaged across all 11 years. *** p ¡ 0.001, **p¡0.01, *p¡0.05.

1.088, 95% CI [0.959, 1.055], p=0.014). Under the 5.5 month time horizon, the marginal probability for an untreated household having a call related to drug-related crime is 7.7 per 1000 untreated household while the marginal probability for a treated household is 10.9 per 1000 treated households. The marginal probability of an untreated household having a call made to their household related to blight incidents in the same time period is 60.9 per 1000 untreated household while the marginal probability for a treated household is 66.4 per 1000 household.

6 Discussion and conclusion

The main objectives of this research were to, first, understand whether a place-based intervention can be effective in changing reporting behavior, given the cost and scaleability implications compared to more traditional interventions in the field. This fills a gap in the literature as it not only examines the effect of a place-based intervention on reporting, but also it examines whether a multi-pronged intervention, that brings together components of other separately-studied interventions, can be effective. The second objective was to present effect estimates categorized by sub-types of 911 and 311 calls, contributing to the literature for how reporting behavior differs by sub-type. In doing so, we show how innovative methods can be used to study large-scale administrative datasets, such as 911 and 311 data, to answer questions important to the design of policies and programs. We discuss here how the present research addresses these objectives, offer potential interpretations of our findings, and suggest areas for future research.

Our first objective was to explore whether a place-based intervention can be a useful tool in influencing reporting behavior. Our findings include effect estimates of the impact on residents' reporting behavior attributable to a place-based intervention that combines three components, including community-oriented law enforcement, education and outreach, and high visibility restoration and beautification efforts. Given prior research of these three components on their own, there is reason to believe that they might have played a role in influencing our outcomes of interest. This study goes beyond the current literature, as it brings together these three distinct literature bases into one study which, to our knowledge, has not been done before. To begin with the first component, community-oriented law enforcement may have contributed to our overall findings, as these activities serve to build relationships with residents, connect them to resources to anonymously report concerns, and discreetly follow up on reported challenges. This component point towards similar findings in the literature that law enforcement with a community focus may change the cost-benefit calculation of reporting by increasing trust, which in turn increases reporting (Anderson 2000; Goudriaan, Wittebrood, et al. 2006; Schnebly 2008). As to the second component of the intervention, the education and outreach conducted by the volunteers and city staff each week may have contributed to the observed effects. Connecting residents to much-needed resources on the spot—such as employment assistance, grants for home repair, and legal assistance for predatory landlords—may have signaled the government as a trustworthy partner with whom to engage. This would be consistent with existing findings showing that residents are more willing to engage with government if they observe them responding in a fair way with positive results (T. R. Tyler 2005; T. Tyler and Fagan 2006; Van Ryzin 2011). The third component of the intervention, restoration and beautification, may also have played a role in the results, as this component is largely responsible for the "awe factor" of Clean Sweeps. Areas with high degrees of physical disorder (suffering from illegal dumping, graffiti, damaged infrastructure, etc.) are suddenly clean and repaired in a matter of hours. This component would nod to a separate body of literature showing that residents who see visible signs of government fulfilling requests see an increase in both trust and reporting (Campa 2020; Sjoberg et al. 2017). Multi-pronged place-based interventions may be effective tools to influence reporting behavior, however more research is needed to see which components drive each effect.

Our second objective was to analyze 911 and 311 data by sub-type and present their effect estimates to contribute to the discussion around reporting behavior of different types of incidents. Starting with 911 data, we found that receipt of a Clean Sweep was associated with an increase in the likelihood of drug-related incidents being reported among treated household compared to untreated households. There was no difference in reporting behavior for violent, property, or "all other" crime between treated and untreated properties. These findings may seem nonsensical at first glance, however they are consistent with findings from other streams of research indicating the sensitivity of some types of crime to be reported compared to others. For example, the gap between how many drug crimes take place compared to how many drug crimes are reported is large (Davis and Henderson 2003; Jacques, Wright, et al. 2014), while the gap between the number of crimes taking place versus being reported is much smaller, however, for violent and property crimes (Goudriaan, Lynch, et al. 2004; R. Martínez et al. 2008; Contreras and Hipp 2020). The null results for the category of "all other crimes", ranging from embezzlement to cyberbullying, is also consistent with the literature indicating that these incidents are unlikely to have been affected by this intervention given the lack of influence the social or physical environment has over them (Goudriaan, Wittebrood, et al. 2006; O'Brien 2016; Braga et al. 2019). Furthermore, the unique drivers of underreporting of drug crime may be influenced specifically by components of this intervention; existing findings suggest that reducing the perceived threat of retaliation and improving the perceived effectiveness and trustworthiness of government may increase reporting behavior among the most underreported crime types (Davis and Henderson 2003; Brunson and Wade 2019). Residents' decision as to whether or not to report may have been re-calibrated by the community-oriented nature of the police officers present, as well as an increased trust in and knowledge of the government as effective partners with whom to engage.

Furthermore, we did not see a significant difference in 311 calls overall between treated and untreated households–but there was a significant increase for blight-related incidents among treated compared to untreated households. One potential reason for these results may be hinted by prior research nodding to the concept of unveiling government work that often goes unseen, where it has been found that visible signs of government responsiveness increased reporting (Campa 2020; Buell et al. 2021). To exemplify this, note first that blight-related incidents include graffiti removal, sewer repairs, sidewalk repair, excess trash, vacant lot issues, etc., while non-blight related incidents range from telecommunications to licenses and registration assistance. The critical distinguishing factor is that the intervention visibly addresses a large number of blight-related incidents on the spot. Non-blight related incidents, while potentially being addressed in one-to-one conversations, are not visible because they are not manifested through physical changes to the environment. Local government work is often done gradually on a case-by-case basis, and is thus less apparent to residents when it gets done in the background (Mettler 2011). However, when done on a large scale, the intervention may have both shown residents what the government can do to remediate blight, and also increased confidence in their effectiveness to do so. Our null findings for non-blight related incidents may have been because residents did not observe them being resolved outside their household. Another potential avenue for our results may have been driven by a strengthened sense of connectedness within the neighborhood, as physical disorder was resolved during the intervention alongside trusted volunteers and block clubs. This would point to a separate body of literature, finding that blight-related 311 calls are underreported in areas with low levels of community connectedness (O'Brien 2015; O'Brien, Sampson, et al. 2015). These findings harmonize distinct segments of research—one relating to unseen government work and the other relating to community connectedness—into one study. This research is not, however, an investigation of the mechanisms that may have driven our results, so more research is needed to understand how drug-related incidents and blight-related service needs may be influenced by a place-based intervention.

In pursuit of meeting these objectives, we had to innovate methodologically in order to address challenges in the dataset. Datasets sourced from public repositories present opportunities to study policies and programs for which collecting primary data would be expensive or infeasible. However, the data is often messy and incomplete, so creative solutions are required to enable their use. This dataset, like most data sourced from public repositories, lacked individual-level data on demographics such as race, ethnicity, age. It is critical to adjust for confounding using demographic variables, so we employ census variables as a proxy and assign each parcel the value for its census block. Census variables, however, are coarse measures of demographic variables, so their use exacerbated imbalance within a dataset that was already severely imbalanced (observe the Census variables in 1). The analysis was further complicated by the large number of observations and covariates, as these are computationally intensive to match on. We tried several matching methods—including propensity score matching, Mahalanobis distance matching, and cardinality matching—however none were able to yield a matched sample with standardized mean differences less than 0.1 for all covariates. We developed a novel matching method that used the matching frontier (King, Lucas, and R. A. Nielsen 2017) with the energy distance (Huling and Mak 2020) to directly optimize balance.

Using the energy distance alongside the matching frontier allowed us to efficiently optimize the pruning process to find the largest possible matched sample with all standardized mean differences below 0.1, which was critical due to the large sample needed to obtain enough statistical power for our small hypothesized effect sizes. In addition to the strategies in the matching procedure, we employed several other tactics throughout the analysis process to account for the messiness of the data, including conducting an additional 2:1 matching step to mimic a block randomized trial, using a two-way clustered standard error to adjust inference for pairing and cluster membership, and controlling for a set of covariates in the outcome model to adjust for remaining imbalances and increase precision. In doing so, we showed how 911 data and 311 data can be paired with administrative data on property-level characteristics to explore policy-relevant questions, which is an approach has been under-explored so far in the literature base. The fact that we were able to yield a matched sample using these methods suggests that they might be useful in other contexts and would be a relevant area of future research. We were able through these strategies to generate effect estimates despite severe, albeit common, challenges posed by administrative data sourced from a public repository.

Taking these findings into consideration, we encourage further research on which components of place-based multi-pronged interventions are the most important in improving reporting behavior. Furthermore, it would be relevant for future research to explore the mechanisms driving reporting behavior, particularly reporting of drug-related crime and blight-related service needs. Finally, we hope that an energy-based frontier matching strategy can be useful in other contexts with similar data challenges, and encourage additional research on how these challenges can be addressed in other ways.

Appendix A Description of cluster-level, cross-sectional, and panel data

Cluster-level data includes cluster membership and size. Although clusters are not contained within entire census block groups, in the absence of parcel-specific demographic data, data obtained from the 2020 census was used. Each parcel takes the value of its census block group; 2020 census variables include percent composition and percent change (since the 2010 census) of race, ethnicity, age, vacancy, total population, and total units.

The cross-sectional data used to generate the continuous parcel-level characteristics includes area of first story, owner property count, number of bedrooms, building size, number of full bathrooms, number of kitchens, building stories, lot length, lot width, and total livable area. Cross-sectional data used to generate categorical datasets includes dates and locations of all Clean Sweeps since 2009, current parcel land use, city ownership, census block group, police district, council district, year built, whether the city owns the parcel, and whether the owner is a business (and if so, the type).

The panel data used to generate the continuous parcel-level variables during the baseline time period includes code inspections, administrative adjudication (i.e., housing court), 311 calls for service, and police incidents (both 911 calls and police reported data) for violent, property, drug, and other crime (see definitions in section 4.3). Panel data used to generate parcel-level categorical variables during the baseline time period includes whether the parcel had a lead case, owed a sewer bill, underwent a fix-and-flip, was owned by the city, had a police incident reported, and had a service call made.

Baseline counts of all panel variables were generated from June 1 of the previous year through April 1 of the study year, which is the same timeframe used by the city to determine eligibility each year. The earliest day, regardless of year, that a Clean Sweep in our sample took place being April 17, data used in the matching was not affected by treatment. As administrative data is not available before January 2009, the timeline for baseline counts for the 2009 study year are from January 1, 2009 – April 1, 2009. The difference in baseline timeframe between years is addressed by the matching procedure, exact matching being conducted on year.

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