



Capturing Crime at the Micro-place: A Spatial Approach to Inform Buffer Size

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Abstract

Objectives The current study develops a methodology to identify spatially relevant buffer sizes for micro-place evaluation research. It applies this methodology in an examination of the causal impact of demolitions on crime in Detroit, Michigan.

Methods We utilize Ripley's bivariate K-function to guide our choice of buffer size. We select a buffer size as the distance at which the examined spatial features exhibit significant sustained attraction prior to the introduction of the intervention. We argue that buffers that are identified in this way capture the spatial relationship between environmental features and are therefore better-suited to capture the actual impact of the treatment on crime. We apply this knowledge in a synthetic control design that estimates the citywide effect of demolitions on disaggregated crime outcomes.

Results With the exception of burglaries, we find fairly limited evidence of a strong, consistent effect of demolitions on crime. The largest negative effects were observed in the immediate months following demolition. Overall, the considerable uncertainty of our estimates suggests that the effect of demolitions may not be consistent across all neighborhoods.

Conclusions At the very least, demolition programs may help temporarily reduce burglaries in areas immediately around demolition sites. However, additional crime reductions gains may be possible if demolition efforts are coupled with complementary crime prevention approaches that focus on the restoration of vacant land. We hope future micro-place evaluation research will use and expand upon our buffer size selection protocol to help improve how places are understood and captured.

Keywords Micro-place · Buffers · Ripley's bivariate K-function · Evaluation research · Abandoned properties · Demolitions

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Introduction

In evaluation research, micro-place studies often measure the effect of various interventions on crime by defining a catchment area that surrounds an intervention site, often referred to as a “buffer.” The size selected for a buffer is not a benign decision. Buffer size selections may directly impact study results by affecting the magnitude and variety of captured variables (Groff 2011, 2014; Ratcliffe 2012). Given this possibility, researchers need to give serious consideration to their selections. While the size of a buffer may be reasonably justified by practical considerations related to the intervention at-hand, it is more commonly treated as an arbitrary decision with little, if any, satisfactory justification provided. Our study showcases a well-known spatial tool to inform buffer size selections in evaluation research: Ripley’s K-function. We extend the use of this function to a multi-event context, referred to as Ripley’s bivariate K-function, and constrain evaluations to the street network.

To showcase our approach, we apply this spatial tool in an evaluation of the effect of demolishing abandoned residential properties on crime in Detroit, Michigan.¹ Although there have been several recent evaluations on the effect of demolitions on crime conducted at the micro-place level, none have focused on the selection process of buffer sizes in their evaluations and how it may affect the identification of treatment effects. Consistent with our micro-place focus, we identify a spatially relevant buffer size utilizing a bivariate K analysis. We apply this knowledge in a synthetic control design that estimates the citywide effect of demolitions on disaggregated crime outcomes. Our results indicate that demolitions have an observable negative effect on some crimes, but little to no effect on others. We also note considerable uncertainty in these estimates, suggesting that the effect of demolitions may not be consistent across all demolition sites and surrounding areas, or neighborhoods. In the following section, we provide a summary of the use and implications of buffers in micro-place evaluation research, as well as a description of the bivariate K-function and the assumptions guiding its use to identify a spatially relevant buffer size.

Literature Review

The Use and Implications of Buffers

Many studies have used spatially articulated buffers to capture relationships and assess treatment effects at the micro-place level (e.g., Mazerolle et al. 2002; Taylor et al. 2011; Wooditch and Weisburd 2016; Ariel and Partridge 2017). These studies highlight a key challenge that micro-place criminology is tackling: there is no agreed upon definition of “place” within the literature. A micro-place may include particular street segments, addresses, or intersections, or groupings of street addresses or street blocks (see Weisburd et al. 2009). Regardless, places are commonly understood to capture the “broader environmental context” (Tita and Radil 2010, pp. 473) that provide behavior settings within which individuals interact, and behavior patterns and routine activities develop. As such, researchers draw from a large reservoir of criminology and place theories to help explain phenomena at the micro-place level. In an ideal scenario, this knowledge should be used to identify the geographic scope or reach of the studied phenomena, which in turn informs

¹ We define an abandoned property as any physical structure that is permanently unoccupied.

the scale of analysis. Unfortunately, the usefulness of criminology and place theories to inform the selection of spatially defined places in *all* scenarios is limited, and can lead to inappropriate scale selections.² Given its implication for making valid inferences, the question of an appropriate scale of analysis has generated much scholarly attention both within and outside of Criminology (e.g., Green and Flowerdew 1996; Flowerdew et al. 2008; Kruger 2008; Brantingham et al. 2009; Dray et al. 2012; Hipp and Boessen 2013; Boessen and Hipp 2015; Taylor 2015; Steenbeek and Weisburd 2016; Schnell et al. 2017; Legewie 2018; Malleon et al. 2019).

Ultimately, researchers engaged in micro-place research should consider additional approaches outside of criminology and place theories to support their choice of scale. While considerable progress has been made in this regard in the form of multi-scale validation techniques (see Malleon et al. 2019), little attention has been given to the use and implications of buffer size selections for evaluation purposes.³ To this point, researchers who utilize buffers to define or bound a place have overwhelmingly relied upon three approaches to inform their buffer size selections. Among the most common, researchers have adopted an exploratory approach in which multiple buffer sizes are selected in an effort to discover the reach of spatial relationships (e.g., Mazerolle et al. 2002; Ratcliffe 2012; Lee et al. 2014; Zhao et al. 2015; Zahnow et al. 2017). For example, Lee et al. (2014) utilized 500 feet, 1000 feet, 2000 feet, and 3000 feet buffers around locations at which police force was used to investigate the impact of neighborhood contextual features on the level of police force applied during police-citizen encounters. Compared to previous studies, they claim that their buffer size selections are able to capture contextual effects at lower levels of aggregation (Lee et al. 2014). Beyond this explanation, however, Lee et al. (2014) do not provide any insight regarding their exact selections. Their approach characterizes buffer size selections used for exploratory purposes; that is, they are typically created based on distances of convenience, with little to no explanation provided as to why one buffer size was selected over another.

In addition, researchers have relied on previous research findings (e.g., Groff 2011; Haberman et al. 2013; Groff and Lockwood 2014; Oh et al. 2019; Taylor et al. 2019; Tillyer and Walter 2019). Insofar as multiple buffer sizes are evaluated, this approach can take on an exploratory nature. Unlike the previous approach, however, the selection of each buffer size is supported by prior research. In other words, selections are less arbitrary. For example, Haberman et al. (2013) utilized 50 feet, 450 feet, and 850 feet buffers to investigate the proximity effects of public housing communities on robbery in Philadelphia, Pennsylvania. The 50 feet buffer captured the site itself and surrounding areas to account for the

² For example, consider micro-place research that uses the buffer zone or distance decay hypotheses to spatially capture the geographic scope of offenders' criminal behaviors. The buffer zone hypothesis claims that offenders avoid committing crimes close to home. Two primary arguments support this hypothesis. Proponents of the buffer zone hypothesis argue that offenders avoid committing crimes close to home due to lack of suitable targets (Rengert et al. 1999; O'Leary 2011). Offenders may also avoid committing crimes close to home out of fear of being recognized (Brantingham and Brantingham 1981; Cromwell et al. 1991; Wright and Decker 1994). Alternatively, the distance decay hypothesis claims that offenders are more likely to commit crimes in areas close to their homes of which they readily frequent and are familiar. These hypotheses are in opposition with one another, yet draw from the same reservoir of crime and place theories to explain offending behavior. Furthermore, they lead to different scale selections to capture the geographic scope of the phenomenon of interest (see Bernasco and van Dijke 2020).

³ For example, the multi-scale error analysis method, developed by Malleon et al. (2019), represents a serious advancement in spatial criminology. However, this method is not appropriate when the aim is to understand the global impact of an intervention on crime at treatment sites and surrounding areas.

finding that geocoding is not an exact process. The subsequent buffer sizes are a function of the average length of a Philadelphia street block, reflecting an earlier Philadelphia-based study which found that the spatial influence exerted by criminogenic features only extends a short distance (see Groff 2011).

Lastly, researchers have used plain logic (e.g., Taylor et al. 2011; Hur and Nasar 2014; Piza et al. 2014; Ariel and Partridge 2017; Ratcliffe et al. 2019; Warner and Konkel 2019). For example, Ariel and Partridge (2017) support their selection of a 50 meter buffer to evaluate the effect of police presence at bus stops “based on the ability of the eye to detect objects” (pp. 815). It was reasoned that within 50 meters of a bus stop suspicious and/or criminal activity would be within eyesight of the police. Likewise, the police would be within eyesight of offenders.

While we recognize that these common approaches may be appropriate for any given research study, we argue that it is more likely the case that they result in inappropriate buffer size selections. To elaborate, the use of a buffer around an intervention site implicitly assumes that characteristics of the captured area influence crime (Taniguchi et al. 2011; Ratcliffe 2012). Common approaches to buffer size selection lose sight of this function of a buffer. As a result, they may misrepresent the spatial relationship between environmental features which can obscure the identification of treatment effects. For example, a buffer size that is too small could exclude crimes that may be associated with the environmental feature of interest, while a buffer size that is too large could include crimes that may be associated with processes related to larger spatial scales (see Massey 1997; Hipp 2007). In this way, buffer size selections are affected by the modifiable areal unit problem (MAUP) (or aggregation bias). The MAUP arises when artificial boundaries are used to contain continuous geographical data and can result in variation in analytical results (Openshaw and Taylor 1979). Related to our focus on buffer size, one way in which analytical results can be affected is by changes in scale, known as the scale effect.⁴ Examples of the scale effect typically showcase extreme variations in results produced by large changes in scope: from state level to county level, from county level to city level, from city level to census tract level, and from census tract level to census block-group level. However, the ramifications of scale are still applicable to changes in aggregation made at very small spatial scales (see Hipp 2007; Tita and Radil 2010).

Ultimately, we argue a data-driven approach that identifies the distance scales at which environmental features influence crime is needed to identify spatially relevant buffer sizes. Such an approach can also aid in the identification of theoretically-informed spatial parameters (see Ratcliffe 2010, 2012; Groff and Lockwood 2014; Xu and Griffiths 2017). Aside from the bivariate K-function, several analytical techniques have emerged to guide buffer size selections (e.g., Andresen’s spatial point pattern test, changepoint regression, intensity value analysis, and risk terrain modeling). Xu and Griffiths (2017) identified three criteria by which to judge these techniques in order to precisely calculate the distances at which environmental features exert an influence on crime. First, the nature of the relationship between environmental features should be examined over continuous measures of distance. Second, examinations of the spatial relationship between environmental features and crime must be constrained to the street network on which they are most likely to occur. Third, the

⁴ While not the focus of our discussion, it should also be known that the MAUP can result in variation in analytical results due to the zoning effect. To elaborate, the zoning effect arises when analytical results are affected as a result of alterations to the structure of spatial units, but not their number (Openshaw and Taylor 1979). As it relates to buffers, concerns regarding the zoning effect heighten when buffers vary in shape.

spatial influence exerted by environmental features should be assessed from point locations which serve as the unit of analysis. In light of these criteria, Xu and Griffiths (2017) utilized the network bivariate K-function to explore the spatial relationship between various environmental features and gun violence in Newark, New Jersey. The current study expands upon this approach by applying a variant of the bivariate K-function to inform buffer size selections for evaluation research.

The Bivariate K-Function

The bivariate K-function is part of a family of analyses used to summarize the general properties of a point pattern. In its most basic form, the empirical K-function, proposed by Ripley (1976), evaluates the global or cumulative distribution of pairwise distances between points at a range of distance values which yields an estimate of the overall intensity of the point pattern: $K(t)$. The observed relationship is then compared against a distribution under some “null” hypothesis (such as, complete spatial randomness or another simulated point pattern) to determine evidence of attraction, repulsion, or randomness. An extension of this method considers the bivariate relationship between two different point patterns. Here, the test evaluates the co-occurrence of a point pattern i and a point pattern j at varying distance scales which yields an estimate of the pairwise relationship: $K_{ij}(t)$. Thus, this comparison allows the identification of distance scales at which the relationship between pairs of two spatial features is one of attraction, repulsion, or randomness.

Ripley’s K-function has been used to explore scale (and zone) effects (e.g., Kiskowski et al. 2009; Marcon and Puech 2017) and applied to a variety of contexts (e.g., Wheeler 2007; Baddeley et al. 2015; Amgad et al. 2015). Within Criminology, the bivariate K-function has been primarily used to provide information regarding whether two spatial features are driven by common underlying processes, as well as the distance scales at which these processes are operating (Weisburd et al. 2012; Wooditch and Weisburd 2016; Xu and Griffiths 2017). For example, a finding of attraction between two spatial features indicates that they are found near each other and driven by common underlying processes. This finding is indicative of crime generators and/or crime attractors (see Wooditch and Weisburd 2016; Xu and Griffiths 2017). Conversely, a finding of repulsion indicates that the two spatial features are not found near each other and driven by opposing underlying processes. This finding is indicative of crime detractors. Lastly, a finding of randomness indicates that there is no relationship between the locations of the two spatial features. This finding may indicate that the spatial features are driven by unrelated underlying processes. Alternatively, it is possible that the spatial features are driven by *similar* underlying processes. In this scenario, the underlying processes are not similar enough for a spatial relationship to emerge.⁵

⁵ A high degree of similarity between the underlying processes of the spatial features influences the observed spatial patterns towards attraction.

Assumptions Guiding the Use of the Bivariate K Analysis to Inform Buffer Size in Micro-place Evaluation Research

Micro-place evaluation research traditionally seeks to identify the *crime reduction effects* of an intervention. In such cases, two assumptions are instrumental in guiding the selection of a spatially relevant buffer size facilitated by a bivariate K analysis:

Assumption 1 The nature of the relationship between two spatial features is one of attraction.

Assumption 2 The intervention will disrupt the spatial relationship.

These assumptions can be partially evaluated by the bivariate K-function. For example, a bivariate K analysis can be conducted prior to the introduction of an intervention to identify the distance scales at which two spatial features exhibit attraction. A detailed description of this process is provided later. These results can then be compared to a second bivariate K analysis conducted *after* the introduction of the intervention to determine whether it disrupted the spatial relationship between the examined spatial features. Here, disruption resembles the elimination of attraction at the distance scales previously identified by the pre-intervention bivariate K analysis. Importantly, these data-driven analyses should not be utilized to the exclusion of crime and place theories. To this point, in the discussion that follows we consider the causal mechanisms through which abandoned properties influence crime, as well as related research. This review provides evidence to support the two assumptions that guide the selection of a spatially relevant buffer size and showcases how our approach can be used to the benefit of criminological theory.

Assumption 1 Abandoned properties and crime arise from shared underlying processes anchored in socioeconomic disadvantage (Accordino and Johnson 2000). While important, more relevant to our understanding of the nature of the relationship between these spatial features is the role of abandoned properties to directly influence crime as crime generators and crime attractors, and indirectly influence crime by emitting cues that encourage offending.

Abandoned properties have routinely been found to be positively associated with crime even when other factors are considered (Branas et al. 2012; Raleigh and Glaster 2015; Boessen and Chamberlain 2017; Chen and Rafail 2019; Lardier et al. 2019). Several criminological theories shed light on this relationship. Consistent with routine activity and rational choice theories, abandoned properties are ideal locations for the commission of crime because they are unguarded and have few, if any, real (e.g., locks, alarms, or gates) or symbolic (e.g., lighting, signage, or indicators of care) barriers to deter crime (Becker 1968; Cohen and Felson 1979). Consequently, minimal effort is needed on the part of offenders to gain entrance into abandoned properties, a criminal act in itself, and use the coverage they provide to commit and conceal their crimes. In fact, Spelman (1993) identified abandoned properties as hot spots of crime, with 83% showing evidence of illegal use, such as prostitution and drug dealing. Additionally, Skogan (1990) found that abandoned properties are attractive targets for vandalism and the removal of valuable materials, such as copper. Thus, abandoned properties may function as crime attractors by providing

well-known criminogenic opportunities that attract motivated offenders (Brantingham and Brantingham 1995).

Abandoned properties may also draw individuals who do not have any intentions of committing crime, but who may nonetheless end up committing an offense or becoming a victim of one. To this point, research has found that abandoned properties are commonly used as teen hang outs, or meeting places and staging areas for crime committed elsewhere (see Spelman 1993; Teixeira 2015; Porter et al. 2018). Interactions at abandoned properties may lead to crime in several ways. For example, crime is likely to occur when motivated offenders cross paths with unassuming targets, or when individuals inadvertently become entangled in altercations or enticed by illicit activities, such as drug use.

In addition, abandoned properties may not only directly influence crime by providing criminogenic opportunities, but they may also *indirectly* invite crime into surrounding areas by acting as environmental cues. As environmental cues, abandoned properties convey information to individuals regarding expectations governing behavior (Lindenberg 2018). In particular, abandoned properties are suggestive of a breakdown in residents' investments in their neighborhoods, which in turn facilitates crime by signaling to offenders that criminal acts will likely go unpunished. Furthermore, this perspective complements broken windows theory which contends that physical disorder—defined as environmental features that reflect neighborhood decay—influences offenders' expectations regarding residents' willingness to intervene to maintain order (Wilson and Kelling 1982; Skogan 1990). In order to elicit such an effect, broken windows theory contends that abandoned properties must be visible. Strictly interpreted, crime should only be affected in the visible areas surrounding abandoned properties. That being said, awareness of the locations of abandoned properties may be enough to influence criminal behavior outside of these areas. To this point, interviews with offenders reveal that they select crime targets based upon their proximity to abandoned housing, which are used as escape options (Porter et al. 2018). In other words, crime is committed elsewhere, but offenders' awareness of the locations of abandoned properties influence their selection of crime targets.

Beyond Spelman's (1993) and Skogan's (1990) early analyses, research on abandoned properties and crime that is conducted at the micro-place level is severely limited. In the only other micro-place study conducted to-date, Wallace and Schalliol (2015) evaluated the relationship between the physical characteristics of abandoned properties and social disorder—captured as loitering, drug use or sales, public drinking/drunkenness, prostitution, and individuals fighting or arguing—in Chicago, Illinois. They recorded the environments of abandoned properties and their face blocks utilizing longitudinal photographic and systematic social observations over the course of several weeks (Wallace and Schalliol 2015). In a series of fixed effects linear regression models, Wallace and Schalliol (2015) found that as abandoned buildings decayed, social disorder significantly increased around them. This finding complements research on abandoned properties and crime conducted at the aggregate level, which is largely supportive of a positive association between abandoned properties and crime (see Branas et al. 2012; Raleigh and Glaster 2015; Boessen and Chamberlain 2017; Chen and Rafail 2019; Lardier et al. 2019).

Ultimately, criminological theory and research on the relationship between abandoned properties and crime suggest that abandoned properties influence crime at their exact locations by acting as crime generators and/or crime attractors, as well as at larger scales by acting as environmental cues and influencing the selection of crime targets. However, these realizations are only partially helpful in selecting an appropriate buffer size to capture the effect of demolitions. At the very least, our review suggests that an appropriate buffer size should include the visible areas around abandoned properties. Ideally, we would like more

detail on the distance scales at which abandoned properties influence crime beyond these areas. This information can be provided by a bivariate K analysis.

Assumption 2 Efforts to mitigate the criminogenic consequences of abandoned properties have largely centered on demolitions. As of our submission, 21,110 residential properties have been demolished in Detroit since the start of its demolition program in 2014, with an additional 269 slated for demolition (see City of Detroit 2020). By removing criminogenic opportunities and cues that encourage offending, demolition strategies are expected to reduce crime by spatially disrupting the relationship between abandoned properties and crime.

Research on the effect of demolishing abandoned properties on crime conducted at the aggregate level is generally supportive of a crime reduction effect (Stacy 2017; Wheeler et al. 2018), especially in Detroit (Jay et al. 2019; Larson et al. 2019). Only a small handful of studies have explored this relationship at the micro-place level. Compared to aggregate level studies, micro-place studies have found more variable effects (Spader et al. 2016; Wheeler et al. 2018; Porter et al. 2018). Of these studies, all but one utilizes buffers centered at the location of abandoned properties. For example, Wheeler et al. (2018) evaluated the effect of demolitions on crime in Buffalo, New York using the exact parcel locations of abandoned properties, as well as 100 feet, 500 feet, and 1000 feet buffers. In particular, they utilized a propensity score approach to match parcels that experienced a demolition to comparable control parcels that did not experience a demolition (Wheeler et al. 2018). Wheeler et al. (2018) then used a difference-in-difference approach to capture the effect of demolitions on crime. Overall, they found significant crime reduction effects for violent and non-violent crime at the exact parcel location, and smaller but significant effects for all evaluated buffer sizes except for the 100 feet buffer (Wheeler et al. 2018).

In addition, Spader et al. (2016) examined the effect of demolitions on crime from 2008 to 2013 in Cleveland, Ohio and Chicago, Illinois by comparing 250 feet buffers around treated abandoned properties relative to 354 feet control buffers. They found that demolitions had a statistically significant negative effect on burglaries and thefts in Cleveland, but had no effect on crime in Chicago (Spader et al. 2016). As suggested by Wheeler et al. (2018), these results may be affected by the number of demolitions that were examined in each city, with a notably smaller sample size used in Chicago than in Cleveland, as well as by the use of control buffers that are spatially adjacent to treatment buffers.

These evaluations showcase two of the previously discussed common approaches to inform buffer size selections. Wheeler et al.'s (2018) selections characterize a clearly exploratory approach. They utilized multiple buffers to explore the spatial influence of demolitions on crime, providing little justification for their selections (Wheeler et al. 2018). In comparison, Spader et al. (2016) do not explore a range of buffer sizes. Rather, they selected treatment and control buffers based upon prior research which suggests that demolitions will have the largest impact at demolition sites and surrounding areas. Nonetheless, there is an element of subjectivity in their buffer size selections, as the meaning of “surrounding areas” could be interpreted in many different ways (Spader et al. 2016).

Ultimately, both Wheeler et al.'s (2018) and Spader et al.'s (2016) selections are subject to the critical flaws previously discussed. In particular, their selections may obscure the identification of treatment effects since they are uniformed by the spatial influence exerted by abandoned properties on crime. This knowledge sets reasonable expectations for the impact of demolitions on crime. If abandoned properties and crime exhibit attraction

at a distance of 200 feet, for example, then we would expect demolitions to disrupt this spatial relationship at this distance (and potentially at larger distance scales). As a result, we would also expect crime to decrease in the area captured by 200 feet buffers. Whether significant or not, our findings from such an assessment would hold more meaning since they were identified utilizing buffer sizes that were informed by the spatial relationship between abandoned properties and crime. In other words, our findings should better reflect the actual impact of the treatment on crime. The same cannot be said from findings identified utilizing the common approaches to buffer size selection.

Current Study

Our current study contributes to micro-place evaluation research in two specific ways. First, we propose a methodology to identify spatially relevant buffer sizes using the bivariate K-function. The K-function represents an intuitive and widely-used tool to characterize spatial point patterns. Second, we apply this methodology in an examination of the causal effect of demolitions on crime in Detroit. In particular, our study expands the pool of micro-place studies on the effect of demolitions on crime by utilizing a regularized synthetic control design to estimate treatment effects (Amjad et al. 2018; Athey et al. 2018; Kinn 2018). This area represents a relatively new avenue in the development of synthetic control methods and addresses a significant issue in our data (namely, the challenge of choosing an optimal subset of predictors that comprise a single weighted synthetic comparison unit).

Data and Methods

Data

Crime data were obtained from the Detroit Police Department's internal records management system. Among the information available was the crime type, date the incident occurred, incident address, and x–y coordinates. We focused on three broad categories of incidents: disorder crimes, property crimes, and violent crimes. Within these categories, however, crime types might be subject to substantial spatial variation (for example, while murders and robberies are both violent crimes different spatial patterns may nonetheless exist). Therefore, we considered a disaggregated set of 8 crime types. Disorder crimes were separated into (1) drug and alcohol crimes (possession, open container, and public intoxication) and (2) misdemeanor assaults. Property crimes were separated into (3) burglary, (4) damage to property, (5) larceny, and (6) motor vehicle theft. Violent crimes were categorized as (7) aggravated assault and homicide, and (8) robbery.⁶ For analysis, each of these crime categories was separately examined. In total, utilizing all incidents from 2014 and 2015 equaled approximately 140,000 incidents across the entire city.

Data on residential demolitions were retrieved from the City of Detroit's open-data portal. These data are maintained by the Detroit Land Bank Authority (DLBA) as part

⁶ We further omitted crimes that were irrelevant to the study, such as retail fraud for larcenies or telephone harassment for assaults.

of the City of Detroit's ongoing demolition program. The DLBA data provides xy-coordinates, the date of the demolition, the contractor enlisted, and cost of the demolition for each demolished residential property.⁷ Before a property can be demolished, its title must first be transferred to the DLBA. There are three primary ways a transfer of property title may be achieved. First, the DLBA may take ownership of a property as a result of a tax-foreclosure. In fact, the majority of properties owned by the DLBA are acquired in this way. Second, property ownership may be transferred to DLBA as a consequence of legal action taken against owners of "open and dangerous" properties. Third, in rare cases the DLBA accepts donated properties. Once the title of a property is transferred to the DLBA, it is either put on track for auction or demolition depending on the quality of the structure. If determined to be unsuitable for auction, the property enters into the DLBA's "demolition pipeline," a publically-accessible, online tracking system that is routinely updated. This system contains information on all properties that have been demolished, as well as properties that have secured demolition contracts or are awaiting demolition contracts.⁸ For the purpose of this study, we focused our analysis on completed residential demolitions.⁹

Using these data, we constructed our observational units in several steps. First, buffers corresponding to the size specified by the K-function $K_{ij}(t)$ were drawn around the

⁷ Given its public nature, it is possible that the DLBA data could influence the behaviors of individuals or groups that utilize abandoned properties for illicit activities. To this point, a helpful reviewer raised the possibility of an anticipation effect, whereby individuals or groups leverage the DLBA data to determine whether the abandoned properties they utilize should be deserted for alternatives not slated for demolition. We do not believe this scenario to be very likely. Properties that are slated for demolition are often not assigned demolition contracts in a timely manner. Individuals or groups that would utilize the DLBA data in the manner described would quickly become wise to Detroit's untimely demolition process and would likely lose motivation to find alternative abandoned properties. In the unlikely scenario in which individuals or groups select alternative abandoned properties, we suspect—drawing from well-established research on offender decision-making – that they would target properties in spatially proximal areas (e.g., Rengert and Wasilchick 2000; Bernasco 2010; Brantingham et al. 2017). If alternative abandoned properties are not identified within these areas, individuals or groups will likely forgo their search. In addition, the same helpful reviewer raised the possibility of non-interference, whereby knowledge of Detroit's demolition program motivates individuals or groups to conceal illicit activities in abandoned properties in an attempt to avoid demolition. While some abandoned properties are slated for demolition as a consequence of known illicit activities that occur within them, the vast majority are slated for demolition based upon their condition alone. Thus, we do not believe non-interference poses a significant issue for our analytical strategy.

⁸ Contractors are invited to bid on packages of houses—ranging from 1 house to over 100—released by the DLBA. Bidders are scored on several criteria, including whether the company is based or headquartered in Detroit, the price per bid, and the company's ability to handle the workload (City of Detroit 2020). Once a contract has been awarded, the contractor has 120 days to complete the demolition. Importantly, not all properties that are slated for demolition are assigned demolition contracts in a timely manner. If a property has not been assigned a contract it may not be demolished for years. The time to demolition largely depends on whether a property is located within a federally designated zone. These zones were selected based upon the marketability potential of Detroit neighborhoods for redevelopment investment and cover approximately 43% of the total area of the city (City of Detroit 2020). Furthermore, demolitions that occur within these zones are supported by the Hardest Hit Fund, a federally-allocated fund for the purpose of spurring economic development and reducing blight. Given access to these funds to finance demolitions, abandoned properties that are located within federally designated zones are demolished more quickly than abandoned properties located elsewhere. That being said, demolitions occurred both within and outside of federally designated zones during the time period of our study.

⁹ We considered the entire city of Detroit for analysis, with the exception of the Downtown and Midtown region of the city. These areas represent a significantly gentrified and more urban environment than the outlying neighborhoods. In addition, the number of residential demolitions was too small to reliably estimate an effect. Therefore, this portion of the city was not included in our analysis. We should note, even with this area included the results were unchanged.

Table 1 Descriptive statistics

Variable	N
Demolitions	
<i>Year</i>	
2015 Demolitions	3778
2016 Demolitions	2949
Crimes	
<i>Violent Crimes</i>	
Aggravated Assault & Homicide	6314
Robbery	4738
<i>Property Crimes</i>	
Burglary	4162
Larceny	8592
Stolen Vehicle	6369
<i>Disorder Crimes</i>	
Assault	7611
Drugs, Liquor	1061
Damage to Property	12,634

addresses of abandoned residential properties in 2014 that were subsequently demolished in 2015. Buffers of the same size were also drawn around properties that were demolished in 2016 but were abandoned between 2014 and 2015, serving as the ‘untreated’ donor pool for our synthetic control comparison group. Crimes falling within these buffers were merged to the address, and subsequently tabulated for each address and month. At the end of this process we had data on the monthly number of crime incidents falling within a designated buffer for each of the 8 crime types at all abandoned residential properties demolished between 2015 and 2016. Therefore, our unit of analysis is the buffer around each address, where the monthly number of each crime is the outcome variable of interest. Below, Table 1 shows the descriptive statistics for the total number of demolished houses and crimes separated by crime type. The N for crime types reflect the total number of crimes falling within the specified buffer. For larcenies and stolen vehicles, the specified buffer is 1 city block (approximately 400 feet), however these crimes are not utilized in the final analysis.

Analytical Strategy

Buffer Size

To determine a guideline for buffer size, we estimated the observed concentration of points— $K_{ij}(t)$ —for crime incidents i and vacant homes j in 2014 against a null distribution using the bivariate K-function. We utilized an isotropic correction to adjust for possible edge effects (essentially, sampling bias due to the omission of points lying outside the bounding window of the study area). The isotropic correction weights each pair of points as a function of the radius of the study window (Baddeley et al. 2015). In doing so, it effectively applies weights proportional to the circumference of the study window that falls within the bounding window (Wooditch and Weisburd 2016).

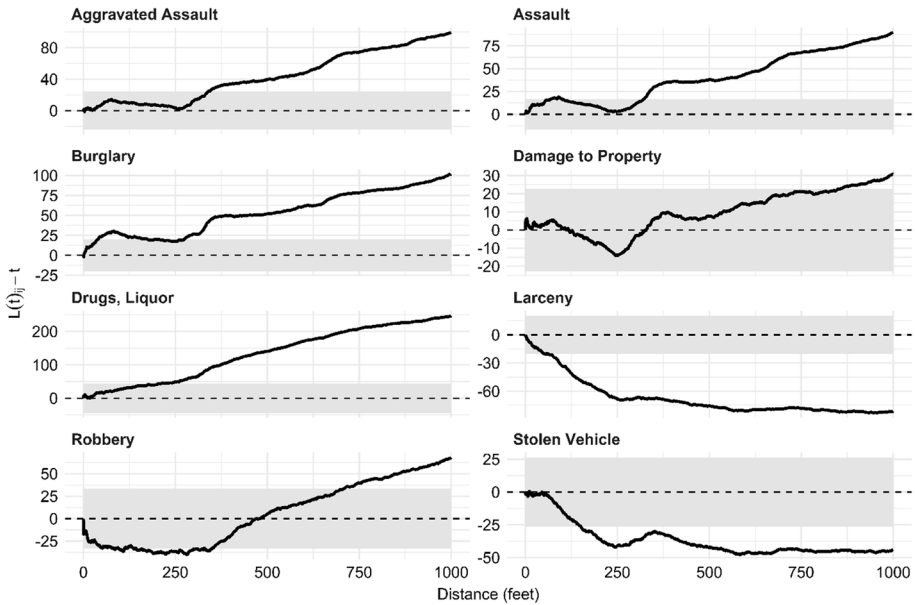


Fig. 1 Observed $L_{ij}(t) - t$ and Global CSR envelopes

Furthermore, we performed 999 simulations where the locations of crime incidents were generated based upon a stochastic point process and randomly distributed along the road network, while the locations of vacant properties were fixed in place. These simulations were then compared to the observed point concentration— $K_{ij}(t)$ —from 0 feet up to a maximum of 1,000 feet (roughly the length of two city blocks in Detroit) at one-foot increments.¹⁰ This process yielded a set of permutation envelopes, which we used to find the smallest distance at which the observed data exhibited sustained divergence from the simulated distribution. We implemented global simulation envelopes where the upper and lower limits reflect the theoretical mean value of the null distribution at all distance values (Kiskowski et al. 2009; Baddeley et al. 2015). Therefore, the test rejects the null hypothesis if the observed data lies outside of the envelope at *any* distance value. The global test statistic has an exact significance level where the test statistic α is simply the rank of the observed point concentration compared to the simulation samples:

$$\alpha = \frac{nrank}{1 + nsim}$$

As a benchmark, we chose a buffer size based on the smallest distance at which the observed spatial features exhibited significant sustained attraction within the nearest 50 feet. This method was performed separately for each crime type. It is also important to note that our approach helps lessen concerns regarding the MAUP given that our buffer sizes are not artificially selected, but rather predetermined from a process that evaluates the spatial relationship of abandoned properties and crime over continuous measures of

¹⁰ A cutoff of 1,000 feet helps lessen the risk that the studied phenomena are obscured by processes associated with competing spatial scales.

Table 2 Estimated buffer sizes

Crime	Category	Buffer Size (ft)
Drugs & Alcohol	Disorder	200
Assault	Disorder	300
Burglary	Property	250
Damage to Property	Property	800
Larceny	Property	0 ^a
Motor Vehicle Theft	Property	0 ^a
Aggravated Assault & Homicide	Violent	350
Robbery	Violent	700

^aNo evidence of spatial attraction

distance (Openshaw and Taylor 1979; Arbia 2001; Marcon and Puech 2017). Furthermore, for visualization purposes we utilized the variance stabilized $L_{ij}(t) - t$, rather than the untransformed $K_{ij}(t)$, to present our findings. Figure 1 shows the observed $L_{ij}(t) - t$ in black compared to the global CSR envelopes (shown in grey).

Table 2 displays the estimated buffer sizes relying on the bivariate K-function. For disorder crimes, significant sustained attraction was observed at distances from 200 feet for drug and alcohol crimes, and 300 feet for misdemeanor assaults. Among property crimes, burglaries exhibited significant sustained attraction at 250 feet, while significant sustained attraction was observed for damage to property at 800 feet. Both larcenies and motor vehicle thefts exhibited significant sustained *repulsion*; that is, up to 1,000 feet there were fewer incidents than would be expected under the null uniform point process. This relationship might, for instance, reflect a lack of suitable targets in the immediate vicinity of abandoned residential properties. Given their confliction Assumption 1, we excluded larcenies and motor vehicle thefts from our analysis. Furthermore, the violent crimes of aggravated assault and homicide exhibited significant sustained attraction at 350 feet, while robbery exhibited significant sustained attraction at 700 feet.

These distances were used in the evaluation stage to construct spatial buffers around each abandoned residential property. These buffer sizes correspond to roughly $\frac{1}{4}$ to $\frac{1}{2}$ of the length of a street block in Detroit, with the notable exception of damage to property and robbery which are roughly $1\frac{1}{2}$ street blocks.

Synthetic Control Design

To evaluate the impact of residential demolitions on crime, we implemented a synthetic control design in lieu of more common matching or propensity-score based methods. While matching is appealing due to its logic and simplicity, there are a number of limitations with the method that preclude their use for many practical purposes. At its core, matching assumes that balance on observable covariates will remove enough systematic differences from treatment and control units such that model dependence will be reduced and the analysis can continue as if the data arose from a randomized experiment (Imbens and Rubin 2015). Naturally, this precludes the ability of the method to match on unobserved covariates. While it can be argued that a sufficient set of observed covariates should, in theory, balance correlated unobserved covariates, this may require a large covariate set (Abadie et al. 2010). In practice, it is difficult or impossible to match on large numbers of covariates (the so-called “curse of dimensionality”), and so researchers often apply some

form of variable reduction prior to matching, most popularly by estimating a propensity score and matching on the logit of the probability of treatment (Imbens and Rubin 2015). Despite its ubiquity, this too presents issues in that good matches are not guaranteed, and the process of matching on the propensity score often worsens balance as observations are pruned (King and Nielsen 2018). In contrast to matching, difference-in-difference (DID) approaches are appealing for a number of reasons. Here, we consider the case of synthetic controls, which are a generalization of the DID approach.

The general logic of the synthetic control method is that the counterfactual outcome for the treated unit(s) can be estimated by finding a weighted subset of control units that optimizes balance in the pre-treatment time periods. This presents advantages over matching methods in that the observed differences between treatment and control cases are minimized and potentially unobserved differences as well, strengthening the assumption of equal parallel trends (Robbins et al. 2017). More importantly, in cases where there is no single untreated unit or units that best represent the treated unit, synthetic control methods can find a set of weights to satisfy this requirement. In the case of matching, arbitrary restrictions like 1–1 or 2–1 matching mean that many ‘good’ matches are often used up, leaving other units to be matched to less similar units (King and Nielsen 2018). As shown by a number of studies, synthetic controls can flexibly overcome these restrictions through a variety of weighting methods. The classic example, Abadie et al. (2010) evaluation of the impact of anti-smoking legislation on California cigarette sales (hereafter ADH), has been replicated across a diverse set of fields, including studies examining the effects of gun permit legislation on homicides (Rudolph et al. 2015) and raising the age of majority on crime (Loeffler and Chalfin 2017), among others. Many of these studies rely on the logic in the ADH method (also known as the *synth* package, used in R and Stata), wherein a subset of control units is weighted such that a convex combination of units in the untreated ‘donor’ pool minimizes pre-treatment differences in the outcome variable (Abadie et al. 2010). The *synth* method constrains weights to be positive and sum to one (Doudchenko and Imbens 2016). This necessarily sets the weight for many treatment units to zero, effectively performing variable selection (Kinn 2018).

In addition to the ADH method, other more flexible methods exist to estimate counterfactual outcomes that do not rely on the positivity and sum-to-one restrictions in *synth* (Doudchenko and Imbens 2016). In fact, it can be argued that *synth* is actually a special case of the more general elastic net or Lasso regression (Kinn 2018). As with *synth*, Lasso regression (Least Absolute Shrinkage and Selection Operator) is appealing because it has the ability to perform both variable selection and estimation (Kinn 2018). As part of the family of penalized regression methods, Lasso is often used in cases where the number of parameters is large relative to the number of observations and there exist a small subset of useful parameters, often referred to as a high “noise-to-signal” ratio (Chernozhukov et al. 2017). Lasso regression places a penalty term on the model coefficients, which has the desirable property of shrinking many of them to zero, while retaining coefficients that are strongly related to the outcome. This property of the Lasso is especially useful in the case of synthetic controls. There are often many possible candidate units in the donor pool, but a select few which actually have strong predictive qualities. The logic of the Lasso method is to treat the potential outcomes as a prediction or missing data problem, where the ‘missing’ values are essentially imputed or predicted using data from the control units (Friedman et al. 2009). The goal is to obtain a sparse set of control units that balances the tradeoffs between increased bias and variance (Kinn 2018).

For this study we implemented a Lasso synthetic control method detailed in a number of previous studies (see Kinn 2018; Chernozhukov et al. 2017) using the *glmnet* package in

R (Friedman et al. 2009). For each crime outcome variable, we aggregated the number of crimes into a single vector for the treated (demolition) group. We then used the disaggregated monthly number of crimes at each individual demolition address in the donor pool to estimate a counterfactual trend for the observed crimes at properties demolished in 2015. Our synthetic control models contain an intercept (reflecting the conditional mean of the observed pre-treatment trend) and weights for each of the individual units in the donor pool. As stated above, these weights are not constrained to sum to one, nor are they limited to strictly positive values (Doudchenko and Imbens 2016). Units which have substantial value in estimating the counterfactual trend receive correspondingly high positive or negative weights. Because many units in the donor pool have little to no value in estimating the counterfactual trend, their weights are necessarily shrunk to zero by the Lasso. This flexibility allows us to estimate a synthetic control that closely approximates the observed pre-treatment trend. The Lasso synthetic control estimator is given as:

$$(\alpha^{Lasso}, \omega^{Lasso}) = \arg \min \left\{ \frac{1}{T^0} \sum_{t=1}^{T^0} (y_{0t} - \alpha - \omega' y_t)^2 + \lambda \sum_{j=1}^j |\omega_j| \right\}$$

Here, we assume a linear relationship between the treated and control units where the model parameters are predicted by minimizing the squared loss, weighted by a model complexity parameter (also known as a ‘tuning parameter’) λ . The value of λ controls the shrinkage of the model parameters which, in practice, often reduces to a subset of only a few non-zero control units (Kinn 2018). In our case, we were specifically interested in identifying a weighted subset of control units that minimized the difference in reported crime pre-demolition. This method assumes that the common cause of crime at abandoned residences is consistent between treatment and control units, however we relax the assumption of parallel trends relied upon in the ADH method (Doudchenko and Imbens 2016; Kinn 2018).

It is important to note that we are able to construct a synthetic control with excellent balance by weighting directly onto the outcome variable for untreated units given sufficient pre-treatment time periods and control units. Indeed, this useful property allows us to estimate a counterfactual without the need for additional covariates. As it applies to many synthetic control designs (such as the ADH method), covariates often play a minor role in the estimation of the synthetic control. Rather, lagged outcomes tend to provide substantially more predictive power and generally are of more interest than covariates (Doudchenko and Imbens 2016). This finding is supported by other recent studies which argue that minimizing the difference between treated and control outcomes is often sufficient to construct synthetic controls (Athey and Imbens 2017; Kinn 2018).¹¹ Insofar that they are generally correlated, matching only on pre-treatment outcomes often approximately balances covariates as well (Botosaru and Ferman 2019).

Given the current research on synthetic control models, we argue that the inclusion of covariates is often unnecessary if good balance is obtained on pre-treatment outcomes alone. If covariates substantially improve the fit, they may be included. However, in many realistic cases they are likely to only introduce additional noise into the model if sufficient balance already exists (Athey and Imbens 2017). Thus, we would advocate for a parsimonious use of covariates (if necessary), rather than a more blanket inclusion when using

¹¹ For example, other covariates become redundant when all lagged outcomes are included in the ADH method (Kaul et al. 2015).

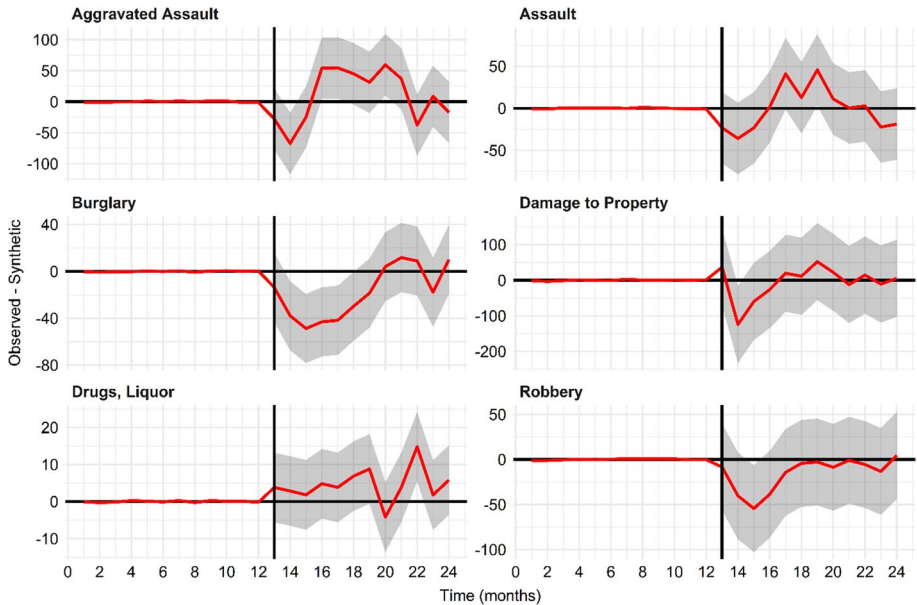


Fig. 2 Predicted and actual differences between the synthetic controls and observed data with 95% conformal inference bands

methods such as ours. In our case, we found matching on a large set of neighborhood-level covariates (using both optimal full matching and propensity score matching) was not sufficient to reduce pre-treatment covariate differences between demolished and control properties. In some cases, increasing covariate balance actually worsened pre-demolition balance on crime outcomes, a problem noted by King and Nielsen (2018). It is also possible that a sufficient set of observed covariates was not available to obtain balance across all outcomes.

Results

Below we show figures comparing the synthetic control estimates to the observed data. Each of the panels in Fig. 2 displays the difference between the observed data and the counterfactual trend, with 95% conformal inference bands highlighted in grey (Chernozhukov et al. 2017). The conformal inference bands reflect uncertainty in the synthetic control prediction based on a jackknife approach, wherein one pre-treatment time point is left out and the remaining data is used to predict that point. This process is repeated to generate a standard error of the synthetic control prediction (Lei et al. 2018). In general, across all model specifications the Lasso successfully fit a model that minimized the pre-demolition differences between the two groups. As the plots show, there are only very minor pre-treatment differences between the synthetic control units and the observed data for each of the crime outcomes.

Across all crime categories, we found generally mixed results. For violent crimes, we observed a slight initial decrease in aggravated assaults and homicides (350 feet buffer)

Table 3 Predicted and actual differences between the synthetic controls and observed data with 95% conformal inference bands

Crime	Post-demolition time	Actual	Synthetic	Difference	Upper 95%	Lower 95%
Aggravated Assault, Homicide	6 months	842	807.90	34.10	331.49	– 263.30
	12 months	883	801.02	81.98	379.37	– 215.42
Assault	6 months	1066	1092.88	– 26.88	228.36	– 282.11
	12 months	1119	1099.91	19.09	274.32	– 236.14
Burglary	6 months	446	660.73	– 214.73	– 37.82	– 391.65
	12 months	620	621.18	– 1.18	175.73	– 178.10
Damage to Property	6 months	1634	1776.37	– 142.37	507.81	– 792.55
	12 months	1936	1864.42	71.58	721.76	– 578.59
Drugs, Liquor	6 months	149	124.99	24.01	80.40	– 32.38
	12 months	152	121.15	30.85	87.25	– 25.54
Robbery	6 months	541	701.35	– 160.35	128.80	– 449.51
	12 months	675	701.67	– 26.67	262.49	– 315.83

relative to the synthetic control, which then were somewhat higher through the rest of 2015. Robberies (700 feet buffer) also observed an initial decrease following the start of the demolition program, which then remained only slightly lower than the synthetic control. In both cases, the 95% conformal inference bands indicated fairly large residual uncertainty. Among property crimes, the most substantial result was observed for burglaries (250 feet buffer), with a sharp and relatively consistent decrease (a near 33% decrease) for about the first six months of 2015. Damage to property (800 feet buffer) remained roughly the same. Lastly, among disorder crimes, both misdemeanor assaults (300 feet buffer) and drug and alcohol crimes (200 feet buffer) observed little consistent change following the start of the demolition program. Table 3 below shows the predicted and actual differences between the synthetic controls and observed data, including the 95% conformal inference bands.

The results here indicate that residential demolitions may have had a minor, if not inconsistent, impact on crime within the spatial influence of each address. In general, we found little evidence of an effect on violent or disorder crimes, with the largest and most consistent effect on burglaries within a very short distance of the address (250 feet buffer). In some respects, this is quite logical, as the demolition of an abandoned residential property effectively removes a home from the pool of possible targets. It is also possible that demolitions provide at least a transitory deterrent effect on burglaries within a short distance of a demolished property. While robberies were generally lower around addresses where demolitions took place compared to the synthetic control, the 95% conformal inference bands did not allow us to rule out that the underlying (significant) variability in the data was responsible for the difference.

Discussion and Conclusion

Buffers are commonly used in micro-place evaluation research to capture the effect of an intervention on crime. However, little critical attention has been given to the choice of a buffer size. Given the significance of this decision, this study provided a methodological

approach to quantify a spatially relevant buffer size using Ripley's bivariate K-function. In keeping with our micro-place focus, this study argued that a spatially relevant buffer size can be selected as the distance at which the relationship between two spatial features exhibits significant sustained attraction. Buffers that are identified in this way capture the spatial relationship between environmental features, which includes the behavior settings in which they occur. As a result, they are better-suited to capture the actual impact of the treatment on crime.

To this end, we provide a clear and concise example of our methodological approach in an examination of the causal impact of demolitions on crime in Detroit. However, we recognize that researchers may encounter scenarios in which its application warrants further consideration. For example, what happens if a bivariate K analysis *does not* identify significant sustained attraction between intervention sites and crime? We experienced this scenario in our evaluation. In such a scenario, the bivariate K analysis does not suggest that the spatial features are driven by common underlying processes, nor does it indicate that the intervention sites function as crime attractors and/or crime generators. For these reasons, the bivariate K analysis is unable to guide the selection of a spatially relevant buffer size. Furthermore, what happens if a bivariate K analysis *does* identify significant sustained attraction between intervention sites and crime, but at very large distance scales? Such a finding would suggest that the underlying processes driving these features may be more consistent with neighborhood-level, rather than place-based theories. For this reason, researchers should consider using a unit of analysis that reflects this larger focus to investigate the effect of the intervention on crime.

From a theoretical perspective, an analysis that considers the micro-level processes unique to each crime type should yield results that best fit the actual variation in crime (Weisburd et al. 2012). Therefore, we opted for buffer sizes that were estimated from observed spatial relationships in the data. With the exception of damage to property and robbery, the buffer sizes identified for each of our evaluated crime types corresponded to roughly 1/4 to 1/2 the length of a street block in Detroit.

Considering research on the effect of demolitions on crime, Spader et al.'s (2016) 250 feet buffer is the most comparable to those used in our study. Outside of this body of research, however, there is incredible variation in the buffer sizes selected to capture the spatial influence exerted by environmental features on crime. The majority of studies have used the length of the average street block, or some additive thereof, to guide their buffer size selections (e.g., Groff 2011, 2014; Haberman et al. 2013; Groff and Lockwood 2014; Oh et al. 2019). Our study suggests that these buffer sizes may be too coarse to accurately capture spatial relationships, especially when multiple crime types are considered. Early studies conducted by Groff (2011) and Ratcliffe (2012) provide a similar word of caution regarding the selection of an appropriate buffer size and recommend a granular approach to the study of spatial relationships. Studies which do not utilize buffers have long raised the importance of finer resolution investigations of spatial phenomena, emphasizing their ability to more accurately capture behavior settings in which crime occurs and produce more homogenous spatial units (Brantingham and Brantingham 1993, 1995; Taylor 1997; Oberwittler and Wikstrom 2009; Bernasco 2010; Groff and Lockwood 2014; Weisburd et al. 2012; O'Brien and Winship 2017; Deryol et al. 2016; Steenbeek and Weisburd 2016; Schnell et al. 2017).

Focusing on our methodological approach, three key factors may have affected our findings and deserve attention in future inquiries. First, we acknowledge a risk present in many studies that consider spatial relationships in the study domain: the stable-unit treatment value assumption (SUTVA). With any method, including our own, there is a risk that

treatment of one unit may affect a subsequent unit (for example, two adjacent demolished residential properties). In addition, we recognize a related risk that a demolished residence adjacent to a control unit might possibly affect crime at that unit. We believe the risk of this second case to be fairly low using our evaluation strategy. Indeed, by applying the Lasso method we exclude a large number of control units and retain only units that are statistically useful for estimating the counterfactual trend. Thus, we limit the risk of spillover effects and cross-contamination by keeping our pool of synthetic control units parsimonious. In contrast, if all units in the comparison group were used (hence, weighted equally in the analysis) the risk of SUTVA violations would be higher. That being said, researchers must always be cautious of these effects and recognize the possible bias introduced due to spillover and cross-contamination.

Second, another avenue of future inquiry regards a key assumption underlying Ripley's bivariate K-function: points are assumed to be homogeneously distributed throughout the study domain. However, there is considerable evidence to suggest that the underlying intensity of crime is not uniform (Brantingham and Brantingham 1993, 1995; Braga et al. 2010; Kennedy et al. 2011; Loeffler and Flaxman 2017). For example, crime generally occurs along street networks. While we accounted for this fact in our analysis, the possibility remains that along each street segment there may be unaccounted differences in the underlying intensity of crime. These differences may inflate the significance of the interaction between point patterns, potentially affecting buffer size selections and treatment effects. Without knowledge of the underlying point process that gave rise to each point pattern, researchers must settle for imperfect solutions to account for inhomogeneity, such as identifying internally homogenous areas for analysis (e.g., Couteron et al. 2003; Wiegand and Moloney 2004) or utilizing kernel density estimations as approximations of underlying intensity (e.g., Baddeley et al. 2000, 2020; Moradi and Mateu 2020). Future evaluations should explore the robustness of the bivariate K-function across these and other techniques used to address inhomogeneity.

Third, the uncertainty associated with our estimates limits our ability to make strong inferences from our findings. Our limited sample size and time frame, as well as our use of the bivariate K-function likely played key roles in this regard. In order to address the former concern, future evaluations should (if possible) utilize larger sample sizes evaluated over longer time periods both pre- and post-demolition. Regarding the latter concern, we recognize that the bivariate K-function is a *global* measure that defines the spatial relationship between two point patterns across continuous distance measures. As such, it is unable to differentiate one site from another, and how attributes of particular sites and surrounding areas affect crime. These shortcomings likely contributed to the uncertainty of our estimates in the form of unaccounted variation. To this point, research has found that some sites (e.g., bars, bus stops, convenience stores, and motels) function as crime attractors and/or crime generators, while others of the same type do not (Levine et al. 1986; Eck and Weisburd 1995; Eck et al. 2007; Wallace and Schalliol 2015; Eck 2018). Furthermore, physical and social attributes of the areas around sites have been shown to affect crime (see Groff 2014; Groff and Lockwood 2014; Deryol et al. 2016; Barnum et al. 2017; Wallace and Schalliol 2015). For these reasons, future evaluations should explore conducting separate analyses utilizing the bivariate K-function that are based upon categorizations of site attributes and/or attributes of surrounding areas that are believed to differentially affect crime in an effort to capture variation that exists across places.

Despite our methodological focus, several of our findings warrant further discussion as they relate to the demolitions and crime literature, revealing additional opportunities for future inquiries. Proceeding forward, however, these findings should be interpreted

with caution due to the uncertainty associated with our estimates. We also recognize that Detroit's history over the last 70 years, characterized by deindustrialization, drastic economic and population declines, racialized poverty, and extreme levels of blight, likely limits their generalizability (Thomas 1997; Sugrue 2005; Hollis 2018; Berglund 2019). In other words, the effect of demolitions on crime may be different in other contexts. Therefore, inferences made outside of Detroit's demolition program should be considered with caution.

To review, our findings suggest that Detroit's demolition program has the largest negative effect on crime in the areas proximal to demolition sites earliest in the demolition program (from about January to June 2015). It is likely that the most problematic residences were targeted earliest in the program, followed by the remaining demolitions. Our results are notably more skeptical than other evaluations that have used larger units of aggregation (see Stacy, 2017; Wheeler, 2018; Jay et al. 2019; Larson et al. 2019). This discrepancy is not entirely unexpected; a substantial body of research showcases how differences in scale can dramatically affect study findings (Hipp, 2007; Andresen and Malleson, 2013; Boessen and Hipp, 2015; Steenbeek and Weisburd, 2016).

At the very least, our findings suggest that demolition programs may help temporarily reduce burglaries in areas immediately around demolition sites. In general, however, we find fairly limited evidence of a strong, consistent effect of demolitions on crime. Any negative effects of demolitions observed across crime types were transitory, with crime levels eventually returning to initial or higher levels within a few months after demolition. This finding is unusual considering research on the effect of demolitions on crime generally supports a crime reduction effect, with minimal evidence of displacement (Spader et al. 2016; Stacy 2017; Porter et al. 2018; Wheeler et al. 2018; Jay et al. 2019; Larson et al. 2019; Hodgkinson et al. 2020).

On first consideration, the short-term crime reduction gains followed by a return to initial or elevated crime levels experienced in post-demolition time periods can be interpreted to suggest that abandoned properties targeted for demolition were in fact not key contributors of crime after all. In this case, the short-term crime reduction gains associated with demolitions are attributed to temporary guardianship of demolition sites during and shortly after demolitions. However, it is important to consider that demolition programs trade one problem—abandoned properties—for another—vacant land. Vacant land can quickly become disorderly without proper maintenance, as indicated by overgrown vegetation and trash, and contribute to crime through similar pathways as abandoned properties. Therefore, it stands that any crime reduction gains associated with demolition programs may be offset by the vacant land they create as their conditions worsen over time due to lack of management.¹² Indeed, a large body of research has identified vacant land to contribute to crime (Taylor et al. 1995; Kurtz et al. 1998; Branas et al. 2012; Garvin et al. 2012; Raleigh and Galster 2015). For this reason, demolition programs that also consider the care of the vacant land they create may stand to achieve greater crime reduction gains (Branas et al. 2011, 2018; Garvin et al. 2012; Kondo et al. 2016). Future evaluations should explore the coupling of demolition and vacant land management programs, such as greening and community garden programs, in an effort to determine whether crime reduction gains can be improved.

¹² Considering our analysis, this explanation is less compelling for cases in which crime is significantly elevated in post-demolition time periods because it suggests that the vacant land created by demolition efforts are more problematic than the abandoned properties that previously occupied them.

Another explanation for this unusual finding considers the influence of neighborhood contextual factors. During the time frame of our evaluation, abandoned properties were prioritized for demolition based upon whether they were located within federally designated zones determined based upon a neighborhood's redevelopment potential. It is possible that redevelopment activities within these zones may have contributed to additional criminal opportunities at and/or in areas around demolition sites in post-demolition time periods through the addition of known crime generators and/or crime attractors, such as apartment complexes and convenience stores.

Insofar as neighborhood disorder has been shown to restrict individuals' physical activities, the demolition of abandoned properties may have also contributed to elevated crime levels in post-demolition time periods by encouraging the use of public space, especially within federally designated zones where redevelopment activities promote such behavior (Ross and Mirowsky 2001; Molnar et al. 2004; Stafford et al. 2007; King 2008; Garvin et al. 2012; Wallace et al. 2019). As a result, individuals are more likely to observe and subsequently report illegal or suspicious activities in post-demolition time periods. They are also more likely to expose themselves and their properties to victimization. However, it is important to recognize that this explanation is limited to abandoned properties that are located within well-populated (or well-traversed) areas. More criminal opportunities exist around abandoned properties that are located within such areas than abandoned properties located elsewhere; there are simply more offenders (persons) and crime targets (persons or properties), and consequently more opportunities for their convergence in time and space (Sherman et al. 1989; Eck and Weisburd 1995; Raleigh and Galster 2015). Ultimately, our analysis does not consider how variation in these or other contextual factors, such as concentrated disadvantage and collective efficacy, influence our micro-place evaluation, although we suspect they contributed to the uncertainty of our estimates.¹³ Future evaluations of the effect of demolitions on crime should continue to build upon research which considers how neighborhood-level processes affect micro-place phenomena.

In conclusion, while our study contributes to research on demolitions and crime by expanding the pool of micro-place studies on this topic and applying a synthetic control design to estimate treatment effects, its primary contribution to criminological research is the development of a methodology to identify a spatially relevant buffer size for micro-place evaluation research. Outside of our micro-place evaluation, our approach can be applied to evaluate the impact of an array of other crime prevention efforts, including the installation of CCTV or enhanced police patrol at crime generators and/or crime attractors, the enforcement of building codes, as well as design alterations to environmental features. Ultimately, we hope future micro-place evaluation research will use and expand upon our buffer size selection protocol to help improve how places are understood and captured.

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¹³ Separate analyses could be performed utilizing the bivariate K-function for neighborhoods of varying typologies that are created based upon contextual factors of interest.

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