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Fewer Vacants, Fewer Crimes?
Impacts of Neighborhood Revitalization Policies on Crime

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Abstract

The relationship between neighborhood physical environment and social disorder, particularly crime, is of critical interest to urban economists and sociologists, as well as local governments. Over the past 50 years, various policy interventions to improve physical conditions in distressed neighborhoods have also been heralded for their potential to reduce crime. Urban renewal programs in the mid-20th century and public housing redevelopment in the 1990s both subscribed to the idea that signs of physical disorder invite social disorder. More recently, the federal Neighborhood Stabilization Program (NSP) provided funding for local policymakers to rehabilitate or demolish foreclosed and vacant properties, in order to mitigate negative spillovers—including crime—on surrounding neighborhoods. In this paper, we investigate the impact of NSP investments on localized crime patterns in Cleveland, Chicago and Denver. Results suggest that demolition activity in Cleveland decreased burglary and theft, but do not find measurable impacts of property rehabilitation investments—although the precision of these estimates are limited by the number of rehabilitation activities.

Keywords: Crime; foreclosures; broken windows; neighborhood revitalization

JEL codes: R1; R3; H4; H7

1) Introduction

In the fourth season of the television show, The Wire, an emerging gang leader (Marlo Stanfield) murders two dozen rival gang members and hides the bodies in vacant rowhouses throughout West Baltimore. The police do not find the hidden bodies for several months, and thus miss an opportunity to intervene in the escalating gang war. In this fictional example, viewers are left to wonder: if Baltimore had had fewer vacant buildings in which to hide the evidence, could Marlo's nefarious activities have escaped detection for so long? This example mirrors a more general policy question faced by policymakers in many U.S. cities troubled by distressed neighborhoods: can interventions to demolish or rehabilitate vacant buildings decrease nearby crime rates?

The hypothesis that neighborhood physical environment can affect the incidence of crime and other types of social disorder has been explored in several strands of academic research. Becker's classic (1968) theory models an individual's decision to engage in criminal activity as a function of various costs and benefits, including the income available from legal activities, the expected gains from crime, the probability of being caught and the disutility of punishment.

Within this framework, the local physical environment is most likely to affect the expected gains from crime and the probability of getting caught. For instance, a neighborhood with high-value houses or cars will offer more potential theft targets, while the presence of vacant buildings provides locations in which to carry out crimes unobserved. The important deterrent effect of observation by non-criminal bystanders has been emphasized by urban planners and criminologists. Jacobs (1961) discusses "eyes on the street" as a component of neighborhood safety, and routine activity theory posits the absence of "capable guardians" as a necessary condition for crime (Cohen and Felson 1979). More generally, the "broken windows" hypothesis argues that signs of physical disorder send visible signals that an area lacks the social infrastructure to deter or catch criminals, thereby inviting more criminal activity (Wilson and Kelling 1982). The intuition behind the "broken windows" theory underlies several important urban policies , including large-scale urban renewal programs in the mid-20th century and HOPE VI redevelopments of troubled public housing projects in the 1990s (Popkin et al 2012).

In this paper, we examine the impact of a recent neighborhood revitalization policy, the Neighborhood Stabilization Program (NSP), on localized crime. NSP is a series of three related programs that provided federal funding to state and local governments and non-profit organizations, in order to mitigate negative spillovers from concentrated foreclosures onto surrounding neighborhoods. Totaling nearly $7 billion across three rounds of funding, NSP was by far the largest public policy intervention aimed at helping local areas hard hit by the housing crisis, and represented a substantial influx of funds for many localities.[1] NSP offers an interesting institutional setting to test the impacts of different neighborhood revitalization strategies, because local grantees could use funds both for demolition and rehabilitation of foreclosed and vacant properties. As discussed in more detail below, these activities have the potential to alter nearby crime through several mechanisms. Using data on NSP investments in Chicago, Cleveland, and Denver, we examine how the incidence of crime changed over time in the immediate vicinity of properties that were demolished or rehabilitated under NSP. We explore variation in impacts across types of crime, particularly between violent, property and public disorder crimes, and discuss how variation in each city's underlying physical conditions and approach to NSP might change the effectiveness of revitalization activity.

Because NSP is still quite new, evidence on its effectiveness is only beginning to emerge. A small literature has begun to examine the impact of NSP activities on housing market outcomes, including prices, future foreclosures, and vacancy rates (Bak 2015; Ergungor and Nelson 2012; Schuetz et al 2015; Schuetz, Spader and Cortes 2015). Most of these studies find little evidence that NSP impacted local housing markets, and attribute the lack of impacts in part to the small scale and spatial diffusion of most NSP investments. In the only related study of crime outcomes, Plerhoples (2012) uses quasi-experimental variation in the timing of demolitions in Saginaw, Michigan—including both demolitions prior to the start of NSP and demolitions conducted during the first months of the NSP program—to estimate short-term impacts on crime. The results are mixed, finding that demolitions are associated with

[1] The first round of funding provided $3.9 billion as part of the Housing and Economic Recovery Act of 2008. The second round, totaling $2 billion, was part of the 2009 American Recovery and Reinvestment Act. The third round of $1 billion was issued under the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act (HUD 2010).

reductions in property crime at the block level and increases in violent crime at the block group level. A larger literature exists regarding previous housing and neighborhood revitalization policies that were primarily aimed at improving the physical environment of distressed neighborhoods, with a secondary goal of improving safety and other social conditions (Joice 2011, Newman 1972, Popkin et al 2012). Aliprantis and Hartley (2015) demonstrate that HOPE VI demolitions of large, highly concentrated public housing projects in Chicago caused significant and substantial decreases in violent crime, notably homicide, shots fired, vice and prostitution. Freedman and Owens (2011) conclude that new LIHTC developments produced county-level reductions in violent crime. By contrast, Lens (2013) does not find consistent evidence that affordable housing rehabilitation and redevelopment in New York City impacted crime. Each of these studies focuses on the impacts of large-scale multi-family properties, while most properties treated through NSP were single-family or small multi-family buildings.

A broader literature also exists regarding the relationship between local crime patterns and several dimensions of neighborhood physical environments. Phillips and Sandler (2015) find that the availability of public transportation increases the ability of criminals to access targets in other parts of the city. Teh (2007) analyzes the impacts of openings and closings of liquor stores in Los Angeles, and finds that new alcohol outlets in low SES neighborhoods increase both violent and property crimes nearby. Similarly, Twinam (2015) concludes that commercial activity is associated with lower crime levels, controlling for density, although liquor stores and late-night bars contribute to assaults and robberies. Most immediately relevant to our analysis, there is a growing empirical literature examining the relationship between foreclosed properties and crime. Clark and Teasdale (2005) and Immergluck and Smith (2006) provided initial evidence of the association between foreclosures and increased crime using cross-sectional data. Multiple subsequent studies have similarly examined data aggregated to the block group, tract, county, or metropolitan area (Williams, Galster, and Verma 2013; Baumer et.al. 2012; Jones and Pridemore 2012; Kirk and Hyra 2012; Wallace, Hedberg, and Katz 2012; Goodstein and Lee 2010). Four additional studies have used research designs with data aggregated to geographies smaller than a census tract (Cui and Walsh 2015; Ellen, Lacoe, and Sharygin 2011; Lacoe and Ellen 2014; Stucky, Ottensman, and Payton 2012).

With the exception of Kirk and Hyra (2012), each of these studies finds that a relationship exists between foreclosures and increased crime.[2]

In this analysis, we use geographically and chronologically precise data on crime and NSP investments in three cities to examine whether crime levels changed within close proximity to demolitions or rehabilitations of formerly vacant and distressed properties. We construct treatment and control areas at small geographic proximity to NSP investments, and apply a difference-in-difference approach to compare changes in crime levels before and after NSP activity, defined by quarter-year from 2008 to 2013. The results provide robust evidence that property demolitions in Cleveland reduced the incidence of burglary and theft, with the impact persisting for four quarters following the demolition before dissipating. Conversely, we do not find consistent evidence that the property demolitions in Chicago or the rehabilitation activities in Cleveland, Chicago, or Denver impacted crime outcomes on or near the NSP properties—although the precision of these estimates are limited by the number of NSP properties in each category.

The remainder of this article is organized as follows. Section 2 provides some background on NSP implementation in the three study cities and develops specific hypotheses for how NSP activities may impact crime. Section 3 discusses the data and empirical approach. Section 4 presents results, and Section 5 concludes.

2) Context: NSP Investments and Crime

NSP offers an interesting institutional setting to examine the impact of revitalization activities on crime, because of several key features that distinguish NSP from previous policies. Although NSP funds had to be spent in distressed areas, most investments were made in relatively low density, single-family neighborhoods, quite different from the concentrated public housing developments targeted by HOPE VI. Similarly, most properties treated by NSP were single-family houses or small multifamily buildings, scattered throughout eligible

[2] An important caveat is that foreclosure-related vacancy may drive this relationship. For example, Cui and Walsh (2015) do not find a significant impact of foreclosures on crime among all foreclosures in their sample, finding impacts only when isolating periods of foreclosure-related vacancy.

neighborhoods. NSP grantees had considerable flexibility in how they designed and implemented neighborhood stabilization strategies that responded to the needs of their local communities, including the choice of activity, location and concentration of investments, type of properties targeted, and more nuanced measures such as the quality of rehabilitation work. Therefore neighborhood "treatment" from NSP is fairly heterogeneous. Each of these programmatic elements raises implications for NSP's potential effects on crime. Additionally, the prevalence and composition of crime in NSP neighborhoods differed substantially across the three study cities, which may also affect the likelihood that revitalization activities will impact crime.

2.1) NSP background and implementation in study cities

NSP was intended to help stabilize neighborhoods with high concentrations of foreclosed and vacant properties, through targeted demolition and rehabilitation.[3] By intent, all census tracts that received NSP investments were economically, socially, and physically distressed prior to the program.[4] In general, NSP investments were targeted to neighborhoods with low household incomes and educational attainment, large black and Latino population shares, low property values, and high rates of vacant and financially distressed properties (Schuetz et al 2015; Schuetz, Spader and Cortes 2015). However, the degree of distress within NSP-targeted areas—both in absolute levels and relative to non-NSP neighborhoods—varied substantially across cities, including the three cities in this study. Table 1 summarizes NSP activities, crime, and relevant social and economic conditions for NSP-targeted areas across all three cities. The number and types of NSP investments also varied across cities, because of underlying housing market conditions, the amount of NSP allocations, and grantees' strategic goals and capacities. The three cities studied in this article—Chicago, Cleveland and Denver—were chosen based on the availability of point-specific crime data and detailed data on NSP

[3] NSP provided financing for five eligible activities: rehabilitation, redevelopment, demolition, land-banking and financing. In this paper, we concentrate on rehabilitation and demolition, because they comprise the overwhelming majority of properties treated.

[4] Eligibility for NSP funding was determined at the census tract level, based on "risk scores" developed by HUD that were intended to capture the prevalence of current and predicted future foreclosures, vacancy rates, and other measures of housing market distress. See Reid (2011) for more discussion of risk scores.

outputs. Although the cities are not representative of all NSP grantees, they illustrate a range of approaches to NSP and had varied public safety environments. [5]

Cleveland was among several Rust Belt cities that were particularly hard hit by the housing crisis. Many neighborhoods in the city had experienced declining population and housing values for decades prior to the crisis, and so had high vacancy rates even before recent foreclosures (Whitaker and Fitzpatrick 2013). More than half the census tracts in Cuyahoga County were eligible for NSP funding. Of the three study cities, Cleveland treated by far the largest number of properties (more than 1000) with NSP funds. The local NSP grantee, the Cuyahoga County Land Reutilization Corporation (CCLRC), developed a strategy that prioritized demolitions in areas of the city with high concentrations of abandoned properties, along with rehabilitation of foreclosed single-family properties in a few neighborhoods (Figure 1). Demolition and land banking activities accounted for about 90 percent of NSP2 properties in Cuyahoga County—although the lower costs of demolitions meant that these properties only accounted for 24.5 percent of expenditures from the NSP2 grant (Schuetz, Spader and Cortes 2015). NSP tracts in Cleveland had only slightly higher crime rates than the city overall, substantially lower crime than NSP tracts in Chicago but substantially higher crime than in Denver (Table 1). NSP tracts in Cleveland were, on average, only slightly more disadvantaged than the city overall on measures such as median household income and prevalence of financially distressed and vacant properties.

Chicago's housing market was in general less affected by the foreclosures crisis than Cleveland's, but the NSP neighborhoods in Chicago also had high vacancy rates and quite high crime levels. In Chicago fewer than 300 properties were treated with NSP funds, mostly on the South and West Sides of the city (Figure 2). NSP activities were implemented by the City of Chicago and three national non-profit organizations: Chicanos Por La Causa (CPLC), The Community Builders, and the Center for Community Self-Help. The City of Chicago and CPLC account for the majority of NSP investments, split between rehabilitation (55 percent of

[5] These cities were selected from among 19 counties included in a HUD-sponsored evaluation of NSP . As discussed in Section 3, detailed data on the location, type and timing of NSP investments was collected during this evaluation. Point-specific crime data was available for only a small number of the 19 counties. For additional information about NSP2 activity in each county, see Appendix B of Spader et.al. (2015).

properties) and demolition. The rehabilitation activities include a mix of single-family and multi-family properties, with increased prioritization of multi-family properties over time. Demolitions were used to supplement these rehabilitation activities rather than as a standalone strategy for removing abandoned properties. The demolition activities frequently cleared properties for future redevelopment by the city or, in a few cases, demolished properties where hazards or rehabilitation needs discovered after acquisition made rehabilitation financially infeasible. NSP investments were clustered in relatively high-crime neighborhoods (Figure 2). The average NSP tract had 164 crimes per quarter during the study period, or roughly 1.2 crimes per square mile per quarter, more than double the city average (Table 1).[6] NSP tracts in Chicago had much higher population (and housing) densities than the other two cities, which may alter the geographic range of impact from NSP activities. Targeted tracts had initially quite high vacancy rates, although relatively low incidence of foreclosed and real estate owned (REO) properties.

Of the three study cities, Denver had the lowest crime levels and least distressed neighborhoods—and consequently received much less NSP activity than either Cleveland or Chicago. The two NSP grantees, the City of Denver and CPLC, primarily focused on rehabilitation of foreclosed properties in two relatively low-crime neighborhoods (Figure 3). This strategy was typical of NSP grantees in cities that had had strong housing markets, prior to the 2007-2009 crisis, rather than those working in areas of long-standing distress. In a neighborhood west of downtown with older housing stock, the grantees sought to rehabilitate and resell foreclosed properties to new homeowners, maintaining property quality and preventing transition of the properties to rental occupancy. The second neighborhood (near the airport) contained newer housing stock in a suburban development affected by high rates of recent foreclosures. In this neighborhood, the grantees similarly sought to use rehabilitation and downpayment assistance to transition foreclosed properties to new owners. Crime rates in Denver's NSP tracts—and the city of Denver overall—were much lower than in either Chicago or Cleveland, and more heavily weighted towards property crimes than public disorder. Denver's NSP tracts were also more

[6] There is some debate in prior literature about whether normalized crime rates should be expressed as a function of population or land area; as shown in Table 1, the variation in population density between Chicago and Cleveland changes the size of the gap between these cities depending on metric, although both are consistently and substantially higher than Denver.

affluent than those in Chicago and Cleveland, with lower initial vacancy rates but higher rates of foreclosed and REO properties.

2.2) Hypothesized impacts of NSP activities on different crime types

As discussed in Section 2.1, the type, quantity and concentration of neighborhood revitalization activity carried out under NSP varied across and within cities, as did the prevalence and composition of crime. In this section, we lay out more specific hypotheses for how demolition and rehabilitation of distressed properties might affect various types of crime. We also discuss how other city and neighborhood characteristics, such as population density and land use patterns, might alter the relationship between revitalization activities and crime.

Previous studies have hypothesized a number of mechanisms by which vacant and foreclosed properties could increase crime (Cui and Walsh 2015; Ellen et.al. 2013; Immergluck 2008). Turnover of neighborhood residents may decrease monitoring by reducing population size or weakening the relationships among neighborhood members. The presence of foreclosures, along with any associated reductions in maintenance, may attract crime to the extent that it creates a source of visual blight or alters perceptions about the risk of arrest. Because dropping property values cause local revenues to drop, foreclosures and vacant properties may also decrease local government resources for crime prevention or prosecution. These same basic mechanisms are likely also relevant to the processes through which the rehabilitation and demolition of foreclosed and abandoned properties will affect nearby crime.

The expected impacts of demolition on crime are fairly straightforward. Vacant and abandoned buildings may create opportunities for property crimes such as burglary, theft and arson, as well as public disorder crimes such as squatting, drug use and vandalism. Broken windows, graffiti and trash are common problems among foreclosed and vacant properties (NFHA 2014). Demolition of vacant properties, by removing potential targets, should therefore lower the incidence of these crimes. Vacant properties could also be used to commit violent crimes like homicide, rape, and assault, although these offenses generally occur more

frequently in occupied and populated areas.[7] The impact of demolitions on offenders' perceptions of being caught is less clear. Removing vacant buildings may improve visibility in a neighborhood for police or other capable guardians, but it may also signal weak expectations for the neighborhood's future prospects.

The expected net impacts of property rehabilitation on crime are not unambiguously positive or negative. Rehabilitation and reoccupancy of a foreclosed building may reduce opportunities for property crimes or public disorder offenses. The arrival of new residents may also increase potential offenders' perceptions of being caught. However, the rehabilitated buildings may create new targets for property crime, and the new occupants may themselves be targets for violent or property offenses. NSP grantees generally screened prospective buyers or renters for income and other eligibility criteria, so we assume that new occupants of NSP properties are not engaged in criminal activity themselves. However, they or their property may introduce new targets for robbery, auto theft, or other offenses to the neighborhood. One further complication in predicting the impact of rehabilitation investments on crime is that, in practice, rehab work under NSP was heterogeneous: local grantees set different targets for the quality of rehabbed properties and differed in the visibility of work to the surrounding neighborhood. Therefore it may be harder to estimate an "average" treatment effect for rehabilitation, relative to demolition.

Both demolition and rehabilitation activities may also have short-term implications for crime during the period when work is carried out. While the grantee's presence may increase offenders' perceptions of being caught, the construction sites may attract crimes such as robberies of construction workers or thefts of building materials, tool, or vehicles. For example, several NSP2 grantees reported issues with theft of appliances, tools, and other items during the construction period. One grantee also reported having a contractor withdraw due to concerns about crew safety in a high-crime neighborhood. While demolition work was generally completed within two weeks, rehabilitation work typically lasted about 9-10 months,

[7] Aliprantis and Hartley (2015) point out that the large public housing properties that were demolished in Chicago were known as sites of violent crime, often gang-related, so their demolition was expected to disperse or reduce the crime that previously occurred on the properties.

so those work sites presented extended opportunities for crime before the properties received new occupants (Spader et.al. 2015).

3) Methodology and Data

The empirical approach measures the impact of NSP investments on crime using a difference-in-differences research design. We use GIS to plot the point locations of crime incident reports and NSP activities, constructing localized counterfactual areas for comparison with the areas receiving NSP investment. Intuitively, the counterfactual areas reflect the need to balance two potentially conflicting objectives: (1) they should be close enough to NSP properties to share all of the unobserved neighborhood attributes that will affect crime outcomes and (2) they should be far enough from NSP properties that the NSP activities will not directly affect the incidence of crime. As discussed in the previous section, NSP activities were targeted to economically and physically distressed neighborhoods, and in Chicago NSP tracts were among the highest-crime neighborhoods in the city. Concentrating on very small geographic areas near NSP activities is therefore desirable in constructing a plausible counterfactual.

3.1) Difference-in-Differences Methodology

The difference-in-differences methodology focuses on the areas immediately proximate to NSP properties, identifying the set of crimes that occur within 250 feet (the "treatment ring").[8] The hypothesis is that demolition and rehabilitation activities will have the largest impacts on the treated property itself and in the streets and alleyways immediately adjacent to the property. The analysis compares the crime incidence in the treatment ring to a control ring defined by a concentric circle with equal area that falls just outside the treatment ring. That is, the control ring includes the area between 250 feet and 354 feet from the property. The analysis period follows these areas for the 20 quarters from March 2008, one year before the

[8] This is a fairly common approach to event history methods (see, for instance, Cui and Walsh 2015, Ellen et al 2011). Some studies define control areas using the same distance as the treatment radius (i.e. treatment within 250 feet, control from 250-500 feet), which yields treatment and control groups of different land area. We follow Cui and Walsh (2015) in defining control rings with the same area as the treatment ring, thus a smaller radius.

first round of NSP activities began, through February 2013, the deadline for expending all funds from the second round of NSP.

The regression to be estimated is:

$$(Eq.\ 1)\ \ Crime_{itc} = \alpha + \beta_1 NSP_{it} + \beta_2 T_i + \beta_3 T_i * NSP_{it} + \beta_4 Q_t + \beta_5 P_i + \varepsilon_{itc}$$

In this equation there is an observation for each ring, *c*, associated with NSP property *i* in quarter *t*. The outcome, $Crime_{itc}$, is the number of crimes observed in quarter *t* for ring *c* of property *i*. The NSP property fixed effects, P_i, remove time-invariant factors associated with each property that are constant across treatment and control rings. The inclusion of these fixed effects means that the model compares the crime outcomes in the treatment ring to the control ring for the same NSP property, isolating variation across quarters and between the treatment and control rings. The model also includes quarter fixed effects, Q_t, to control for the trend in crime outcomes across time.

The measure of NSP investment, NSP_{it}, is an indicator variable that identifies whether NSP activity has begun on the property. The period of NSP activity is defined to begin with the earliest observed quarter of acquisition or activity by the NSP grantee, whichever occurs first. The NSP period then includes all subsequent quarters through the end of the analysis period in order to determine whether the relative incidence of crime in the treatment and control rings changes following NSP investment. In equation 1, the interaction of the measure of NSP investment with the treatment ring, $T_i * NSP_{it,}$ produces coefficient estimates that measure the impact of NSP activities on crime. These estimates identify whether, after NSP activity begins, the number of crimes is significantly different in the treatment ring than the control ring relative to the differences between rings in prior quarters. Supplemental analyses discussed in the results section also examine specifications that separate the period when work is present on the property from the periods following work completion. These analyses test whether the impact of NSP activities are sustained through the end of the observation period or limited to the periods when NSP activities are physically occurring on the property.

As discussed in Section 2, crime may be affected by neighborhood environmental conditions such as the proximity to commercial corridors, volume of pedestrian and vehicle traffic, and the presence of police and other capable guardians. By defining the control area as

a concentric ring immediately adjacent to the treatment ring, our research design minimizes the potential for the crime trajectories of the treatment and control rings to differ due to these and other unobserved factors. A further aid to identifying the effect of NSP activity on crime is that, although grantees selected census tracts to receive NSP funds based on pre-existing measures of distress, they consistently indicated in interviews that there were quite constrained in their ability to target specific properties for investment. This was particularly true for properties acquired for rehabilitation: many banks with large REO inventories chose to release only a few properties at a time, to avoid flooding the market, and NSP grantees in some areas had to compete with private investors also purchasing foreclosed properties (Fraser and Oakley 2015; Newburger 2010; Reid 2011; Spader et al 2015). The difficulties experienced by grantees in acquiring strategically important properties introduces a quasi-random nature into the location of NSP properties which aids in identification (although it may have hindered the effectiveness of the program overall).

One tradeoff of designating very small treatment and control rings is that this design imposes 250 feet as the maximum distance for the impact of NSP activities to affect crime. To the extent that the impact of NSP activities extends beyond 250 feet, equation 1 identifies the average difference between the impact on the treatment ring relative to the control ring—underestimating the true impact on crime in the treatment ring.[9] To examine this possibility, supplemental analyses discussed in the results section add a "third ring"—defined by a concentric circle of equal area that falls between 354 and 433 feet from the NSP properties—to determine whether the impact of NSP activities extends further than 250 feet.

Figure 4 shows a stylized illustration of the rings imposed on the underlying street grid. This image illustrates the proximity of the treatment and control areas within blocks that are likely to share the same underlying neighborhood attributes. It also highlights that it is possible for NSP properties to be located less than 250 feet apart, causing treatment and control rings to overlap with the location of other NSP properties.[10] While such overlap occurs within the

[9] See Cui and Walsh (2015) for a more complete discussion of this issue.

[10] A limitation of this overlap is that individual crimes can contribute to the crime counts for several properties. This overlap results in correlation between the error terms of different observations, which

analysis samples for each city, the patterns of overlap maintain treatment-control contrast between the treatment and control rings. In Cleveland, the treatment rings contain, on average, 2.21 NSP properties, compared to 0.91 NSP properties in the control rings. In Chicago and Denver, the treatment rings contain, on average, 1.69 and 1.36 NSP properties, compared to 0.21 and 0.16 NSP properties in the control rings, respectively.

Because NSP activity may have different impacts on different types of crimes, as discussed in Section 2, we estimate separate regressions for total crime, three general crime types—violent, property, and other—and for eight finer categories of crime (robbery, aggravated assault, burglary, theft, auto theft, simple assault, drug offenses and vandalism). In each case, the outcome measure, $Crime_{itc}$, is a count of the number of observed crimes. Equation 1 is therefore estimated with poisson regression with bootstrapped standard errors.[11] Appendix A presents results when the analyses are replicated using OLS estimation.

One limitation of the rings design is that the analysis cannot distinguish whether the identified impact of NSP activities on crime represents a reduction in crime or the displacement of crime from one location to another. Previous literature has shown that highly localized crime prevention strategies may cause displacement rather than deterrence (see, for instance, Helsley and Strange 1999, Marceau 1997). For example, the demolition of a property may displace crime from the demolished property to other properties on nearby blocks or to other areas of the city. If crime is displaced to locations in the control rings, the estimated impact of NSP activities will overstate the magnitude of the true impact on crime. Alternatively, if the crime is displaced to locations outside the study area, the estimated impact of NSP activities

violates the independence of error terms assumption. This issue does not bias the estimated coefficients. However, traditional heteroskedaticity-robust standard errors may be smaller than the standard errors that would occur in a sample of equal size where the rings for each property did not overlap. We therefore use bootstrapped standard errors with 400 replications for all poisson regressions and standard errors clustered at the block group level for all OLS regressions. These approaches produce more conservative standard error estimates and may better address issues related to serial correlation in difference-in-differences analyses (see Cameron and Miller 2015; Bertrand, Duflo, and Mullainathan 2004). As an additional precaution, we also conducted robustness analyses using the design proposed by Stucky, Ottensman, and Payton (2012), which separates the analysis area into 1000 foot by 1000 foot grid cells that do not overlap one another. The association of NSP activities with grid cell crime counts produced by that design is substantively similar to the findings discussed in the results section.

[11] Cluster-robust standard errors are bootstrapped with 400 replications.

represents the impact on the treatment areas relative to the control areas. In either case, the estimates in this paper are used to measure the local effects of NSP activities and not the cumulative effect for the city as a whole.

3.2) Data sources and variable construction

Property-level information on the location, activity type, and timing of NSP activities is collected from HUD administrative data for the first and second rounds of NSP (referred to as NSP1 and NSP2). For NSP2 properties, information on the start and end dates for each activity was collected from grantees, along with the acquisition date if the property was acquired by the grantee.[12] For NSP1 properties, dates are taken from HUD's centralized reporting system, which provides the approximate completion date but not the start date. Because demolition activities typically require less than one week to complete, the reporting date is used to identify the quarter when the demolition occurred. For properties that were rehabilitated under NSP1, we define the rehabilitation start date to occur three quarters prior to the reporting date—the median length of observed rehabilitation activity among NSP2 properties.[13]

Crime data for the areas surrounding each NSP property is collected from the local police department in each city. The coverage of the available data reflects the jurisdiction of the city's police department, which approximates the boundaries of each county's primary city—Cleveland, Chicago, and Denver. The coverage areas include all properties with NSP activities in Denver County (Denver) but exclude a small number of properties with NSP activities in Cook County (Chicago) and Cuyahoga County (Cleveland). The crime data includes extensive information on the crime incident reports recorded by each city's police department, including a point location and date for each crime incident report. Incident reports document incoming reports of crime to the police department, regardless of whether the crime is confirmed or an arrest is made. Incident reports may be less likely than arrest data to reflect changes in policing

[12] Many demolitions – at least those originally intended for demolition – did not involve acquisition of the property by grantees. Some were already city-owned due to prior tax foreclosures, while some were owned by absentee landlords.

[13] As a robustness check, we replicate the analyses using alternative measures that reflect short (5th percentile; 1 quarter prior) and long (95th percentile; 5 quarters prior) rehabilitation periods. The findings are similar using these alternative measures, as well as when the rehabilitation properties are limited to NSP2 properties for which precise timing data is available.

strategy over time in longitudinal data, although there may be changes over time in residents' likelihood of reporting problems to police. Moreover, there may be neighborhood-level differences in the likelihood that an incident is reported to the police. This limitation is most relevant for producing population estimates and examining cross-sectional variation across neighborhoods.

The analyses aggregate the crime incident reports using the FBI's Uniform Crime Reporting (UCR) definitions. The crime categories and component offense types include the following: [14]

- Violent Crime: Violent crime includes all incident reports related to homicide, rape, robbery, and aggravated assault or battery.
- Property Crime: Property crime includes all incident reports related to burglary, theft, and auto theft.
- Other Crime: Other crime include all incident reports related to simple assault, drug offenses, weapons offenses, and vandalism.

The UCR definitions include arson as a fourth measure of property crime. It is excluded from the aggregated measure of property crime in this article because of the potential for a mechanical relationship between arson and demolition (i.e. some cities elevate properties that experience arson to the top of their list of properties for demolition).

The use of UCR definitions increases the reliability of the offense categories and the comparability of the data across cities. However, the crime data provided by the cities is subject to two limitations. First, an incident with multiple offenses is categorized according to the most severe offense committed. As a result, the set of crime incident reports observed in each city may omit some property and other crimes that were committed during the same incident as a violent crime. Second, the crime data omits a small number of incident reports that might reveal sensitive information (e.g. offenses involving children).

[14] The Denver crime data categorizes crimes using the National Incident Based Reporting System (NIBRS). For the analysis in this paper, the Denver data has been reclassified to UCR categories.

4) Results

We begin by showing a set of descriptive statistics on the number and type of crimes in the study areas, including balance tests to evaluate the quality of the counterfactual areas defined by the control rings. We then present a series of regression results, comparing crime frequency in treatment and control areas, before and after NSP activity, for each city and type of crime. Results suggest that NSP demolitions in Cleveland decreased theft and burglary in the immediate vicinity, but find no evidence that demolitions reduced crime in Chicago. Rehabilitation activities do not show measurable impacts on crime in any of the three cities.

Table 2 displays the total number and type of crime incident reports that occurred in census tracts with NSP investment over the course of the entire study period. These figures provide insight into the most common types of crimes in each UCR category.[15] In all three cities, violent crime is by far the least frequent type, making up 8-10 percent of all crimes. The distribution of crimes by type show that robbery and aggravated assault account for the 91-94 percent of violent crimes in each city and that burglary and theft account for 79-84 percent of property crimes. The mechanisms presented in Section 2 suggest that burglary, theft, and vandalism are the types of crime most likely to be impacted by demolition and rehabilitation activity; collectively, these form a substantial share of total crime in all three cities. As previously discussed, arson is excluded from the aggregated measure of property crime used in the analyses because of the close relationship between arson and property demolition in many cities. The counts in Table 2 show that arson accounts for less than 1 percent of all crime incident reports in each city.

Figure 5 presents the percent of NSP properties with activities underway or completed in each quarter. These figures show that no NSP activities had yet begun during the first four quarters of the study period. Table 3 therefore uses the crime data from these four quarters prior to the start of NSP investment—March 2008 to February 2009—to conduct balance tests that compare the relative levels of crime between the treatment and control rings. The results

[15] The category definitions and reporting procedures may differ slightly across cities, so differences in the crime counts of each city may reflect reporting procedures in addition to differences in the relative incidence of crime.

suggest that the treatment and control rings are well-balanced, showing that the average number of crimes per quarter is not significantly different in the treatment versus control rings for any crime type or city. Table 3 additionally includes balance tests that compare the number of crimes in the third ring to the number of crimes in the treatment ring. While the relative incidence of crime is similar between the third ring and the treatment ring for most measures, the results suggest that the similarities with the treatment ring may begin to deteriorate as the distance from the treatment ring increases.

The crime counts in Table 3 also illustrate the differences across cities in the density of crime surrounding NSP properties. In Chicago, the average number of crimes per quarter in the treatment rings is more than double the value for Cleveland. This difference reflects the location of Chicago NSP properties in neighborhoods with denser populations and higher baseline levels of crime than the neighborhoods targeted for NSP activities in Cleveland. In contrast, the treatment rings surrounding NSP properties in Denver contain only 0.6 crimes per quarter, on average. This relative safety of NSP neighborhoods in Denver (and indeed the whole city) influenced the grantees' strategies, which emphasized reoccupancy of foreclosed properties and did not include any demolition of abandoned properties.

The remainder of this section examines the impact of the NSP investments on crime outcomes. Table 4 presents the coefficients for poisson estimation of the rings model. The results for property crimes (column II) show that NSP demolitions in Cleveland are associated with significant reductions in property crime. The average marginal effect for the coefficient estimate of -.072 implies a reduction of .08 property crimes per quarter following NSP activity. Table 4 also separates the measure of property crime into its component offense types (columns III-V), showing that the impact of Cleveland demolitions reflects reductions in burglary and theft. These results are consistent with the changes to the physical environment created by demolitions, which remove vacant properties that might be targets for burglary and theft. Conversely, the estimates do not find any evidence that demolitions affect offense types like auto theft, which are less closely related to the demolition of an abandoned structure. Estimating separate regressions for individual types of violent crime and other crime shows no significant impacts for any one type of crime (results available from authors upon request).

Unlike in Cleveland, the coefficients on NSP properties treated with demolition in Chicago are not significantly different from zero, and the signs are mixed across different crime types. The estimated relationships with violent crime and auto theft are negative, but the coefficients on property crime, burglary, theft, and other crime are actually positive, contrary to expectations. These estimates may reflect greater heterogeneity in the purpose of Chicago demolitions. The NSP grantee in Cleveland designed an intentional strategy for using demolitions to remove a large number of abandoned structures in neighborhoods with high levels of vacancy. In Chicago, the NSP grantees conducted some demolitions with this purpose, but also used demolitions to prepare sites for redevelopment and to clear structures in cases where rehabilitation costs would have been prohibitively high. Alternatively, because the initial crime levels in Chicago's NSP neighborhoods were higher than in Cleveland, the demolition crews may have been more likely to become crime targets.

Table 4 also shows that the estimated impacts of rehabilitation activities in all three cities do not have consistent or significant effects on crime. In Cleveland and Denver, the signs on NSP rehabbed properties are positive for nearly all crime categories (except other crime in Denver (Column VI)), and not close to zero in magnitude, although imprecisely estimated. In Chicago, the signs are mixed: negative for violent crime (Column 1), burglary and theft (Columns 3-4), positive for the other categories, and quite varied in magnitude. One possible explanation for this finding is that the smaller sample sizes for these activities limit the statistical power of the estimates. Alternatively, the estimates for rehabilitation activities may reflect the ambiguous relationship between property rehabilitation activities and nearby crime. For example, while the presence of the NSP grantee and new occupants may deter crime by increasing perceptions that the blockface is being monitored, the work site and rehabilitated property may also create opportunities for burglary and theft. These contradictory effects may partially offset one another and/or produce heterogeneous impacts that vary across different types of neighborhoods and that may not be picked up by an average effect.

To further explore the impact of Cleveland demolitions on property crime, we also conduct two extensions of the rings model. These extensions find that the impact of NSP demolitions in Cleveland is limited to the 250 foot distance of the treatment rings and

dissipates approximately one year after the demolition is completed. Table 5 presents results when the estimation approach is replicated after adding the "third ring" as the baseline for analysis. The estimates therefore treat both the treatment ring and the control ring as potential treatment areas, comparing the trajectories of crime in these rings against the newly-added third ring. The results for the treatment ring are nearly identical to Table 4 with respect to demolitions ('T x Demo') and rehabilitations ('T x Rehab'). Conversely, the estimates that compare the original control ring to the third ring ('C x Demo' and 'C x Rehab') do find any evidence that the incidence of property crime was significantly reduced in the control ring relative to the third ring following NSP investment. These results suggest that the impact of NSP demolitions in Cleveland is limited to the 250 foot distance defined by the treatment rings. The scale of the underlying street grid varies across neighborhoods in all three cities, but in general, 250 feet corresponds roughly to the length of a city block, which seems a reasonable distance to observe impacts from demolition of a single-family house.

Table 6 explores the duration of the estimated impacts on burglary and theft by separating the NSP period into four periods: the period of NSP activity, the first four quarters following completion of the NSP activity, the subsequent 4 quarters, and the remaining quarters prior to February 2013. These measures allow the analysis to explore whether the impacts of NSP investment are sustained through the end of the observation period or limited to the periods when NSP activities are physically occurring on the property. For demolitions, the period of NSP activity is typically a single quarter, with multiple quarters only in cases where the grantee acquired the property prior to demolition. For rehabilitation properties, the NSP activity period begins when the property is acquired or rehabilitation work begins and ends when rehabilitation is completed and the property is reoccupied.

The results in Table 6 show that the impact of NSP demolitions is concentrated in the NSP activity period ('T x Demo Work') and the subsequent four quarters ('T x Demo Year 1'), dissipating thereafter ('T x Demo Year 2' and 'T x Demo Year 3').[16] This pattern suggests that the

[16] Because some NSP properties do not begin work until less than 5 quarters before the end of the study period, it is possible that this pattern is due to composition effects if the impact of NSP activities is larger for properties that began work later in the period. This is not the case. The estimates are similar if when

impacts of NSP demolitions on crime are temporary. The average marginal effects for the work period and the subsequent four quarters are each approximately -0.12 property crimes per quarter. Applying these estimates to a duration of just over five quarters implies that NSP demolitions in Cleveland produced an average reduction of approximately 0.6 property crime incident reports per demolition. While this figure represents the average impact implied by the analyses, it should be treated as a rough approximation due to the wide confidence intervals associated with the impact estimates.

5) Conclusions

This article examines the extent to which neighborhood revitalization investments affect nearby crime using data from the Neighborhood Stabilization Program in Cleveland, Chicago, and Denver. While studies of prior neighborhood revitalization initiatives focus on the construction, rehabilitation, or demolition of large multi-family properties, NSP investments primarily supported rehabilitation and demolition of single-family and small multi-family properties. These investments therefore provide a unique opportunity to learn about the effects of smaller-scale neighborhood revitalization interventions, which might be feasible to local governments in the absence of a larger federal program. They also provide insight into the extent to which NSP investments may have helped to offset the impact of foreclosures on crime in neighborhoods hard hit by the foreclosure crisis.

The results suggest that NSP demolitions in Cleveland reduced the incidence of burglary and theft within 250 feet of the demolished properties. The effect is present during the quarter in which the demolition takes place and persists for an additional four quarters thereafter before dissipating. Additional analyses do not find evidence that the impact extends to areas more than 250 feet from the demolished property or to types of crime that are less closely tied to the residential property itself—e.g. assault, drug offenses, etc. Instead, these effects are consistent with demolitions reducing the incidence of burglary and theft on the demolished property itself or in areas immediately adjacent to the property.

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the models are replicated using the sample of properties that begin work more than 5 quarters prior to the end of the study period.

Conversely, the results do not find measurable impacts of rehabilitation activities in any of the three cities or of demolition activities in Chicago. Each of these activities included fewer than 200 properties, so it is possible that this result is due to weak statistical power for measurement. Alternatively, it may reflect heterogeneity in the nature of these activities, which may not be picked up in an estimate of the average effect. The NSP grantees in each city had substantial discretion to design strategies that were tailored to the specific needs of their communities and varied in their objectives for rehabilitation and demolition investments. These findings should therefore be interpreted within the context of the NSP objectives, strategies, and targeted neighborhoods in each city. The difference in initial crime levels across the three cities may also have played a role in the effectiveness of NSP at reducing localized crime. Denver's crime level was quite low—about one crime every two quarters in the study areas—so there was less room for improvement than in the other two cities. Conversely, it is possible that the scale of NSP investments in Chicago was too small to be effective, given the neighborhoods' substantially higher crime rates. Cleveland not only had lower initial crime rates than Chicago, it also treated a much larger share of distressed housing, perhaps reaching a critical mass in affected neighborhoods.

The relationship between NSP demolitions and property crime in Cleveland adds to the literature describing the implications of the built environment for the incidence and spatial patterns of crime. While some portion of the reduction in property crime may reflect crime displacement to other areas of the city, the local effects described by this article may be useful to policymakers considering the use of demolition as part of a targeted strategy for specific neighborhoods. The caveat to this conclusion is that the magnitude of the impacts are relatively modest and limited to the case of Cleveland. Any application of these findings to future neighborhood revitalization strategies should therefore consider the potential impacts of crime in combination with the full set of objectives for the neighborhood revitalization effort. In assessing the broader implications for the Neighborhood Stabilization Program, these objectives include the program's intended effects on home prices, sales volumes, foreclosures, vacancies, and tenure change.

Acknowledgements

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Figure 1: NSP Investments and Crime: Cleveland

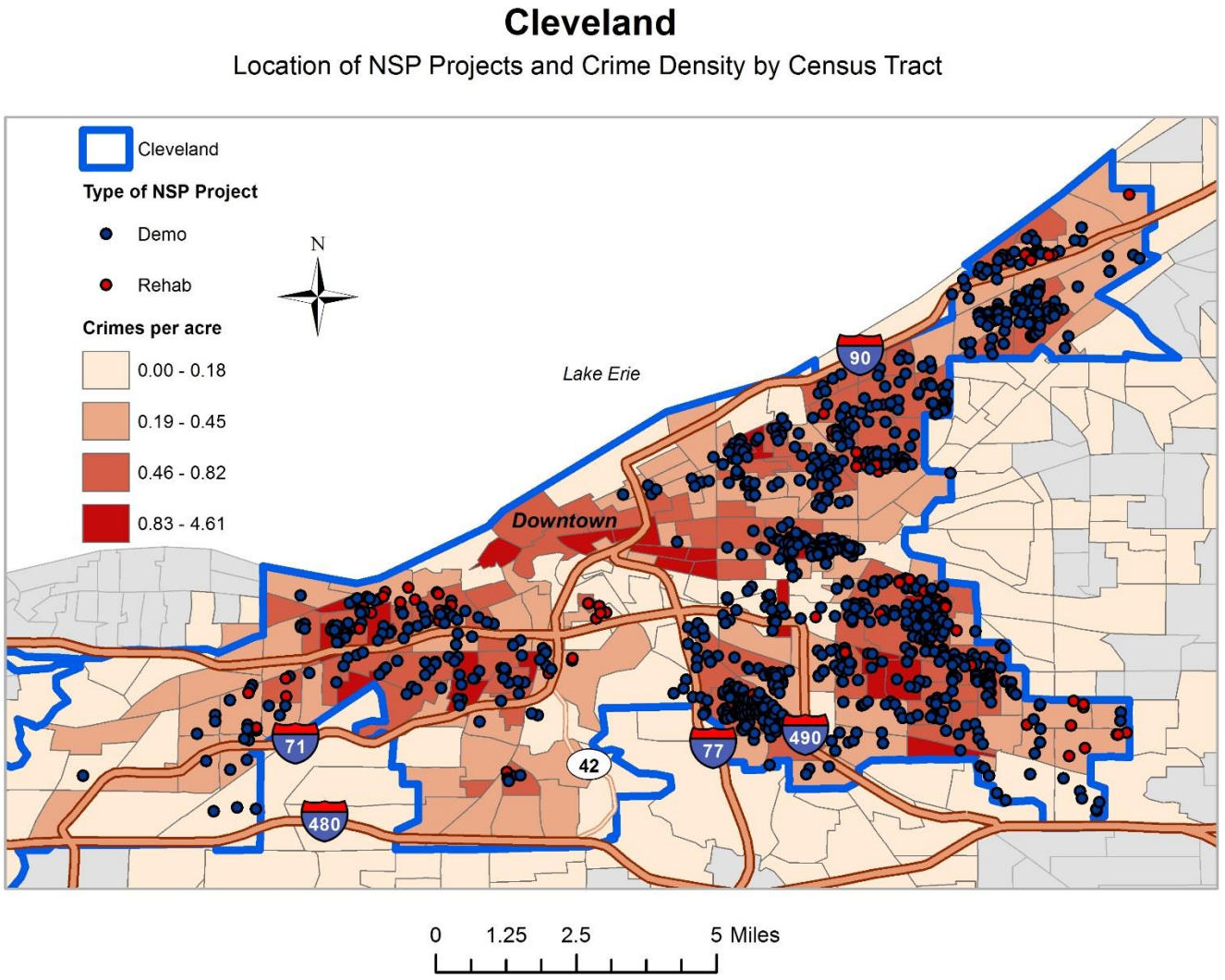

Figure 2: NSP Investments and Crime: Chicago

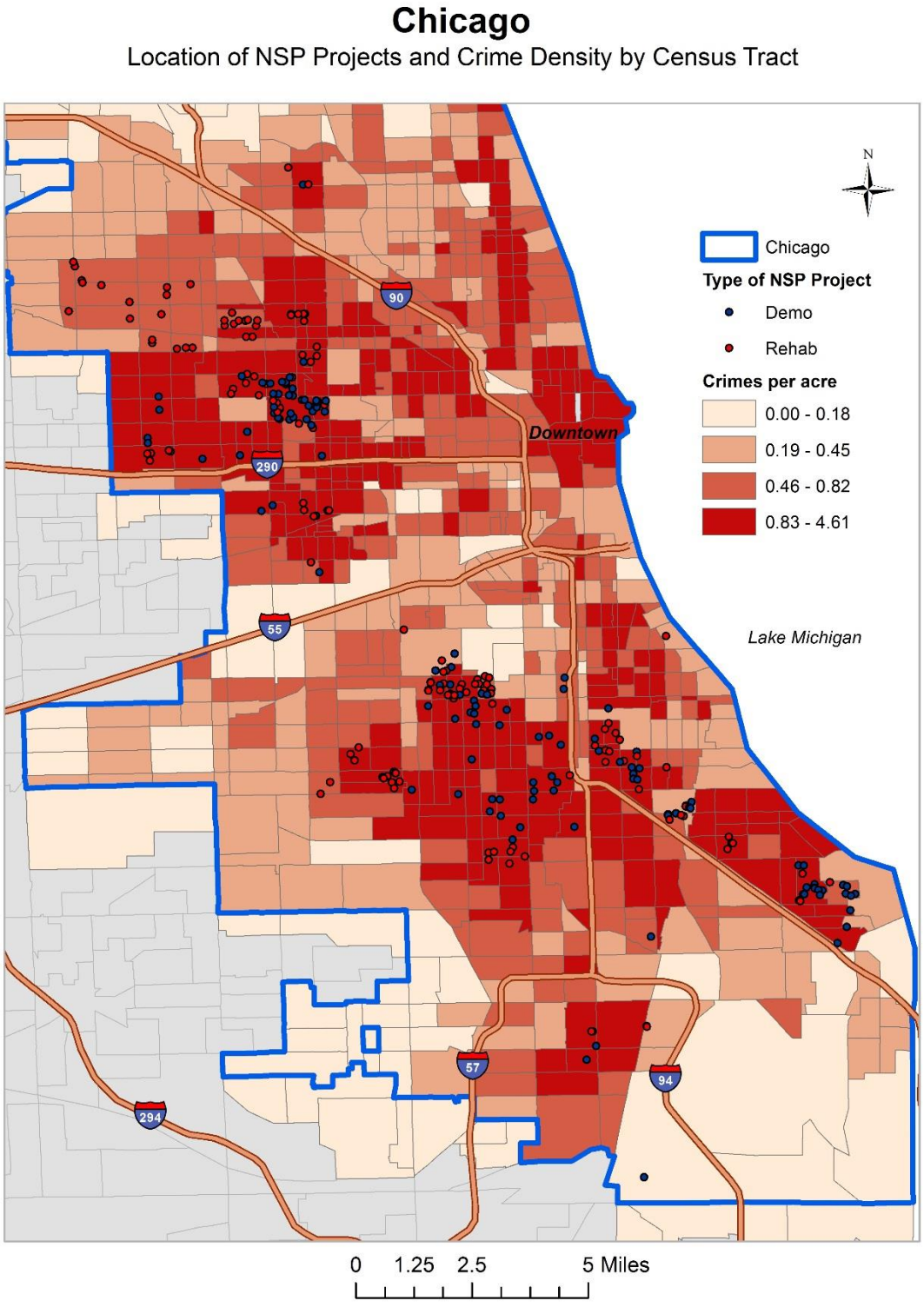

Figure 3: NSP Investments and Crime: Denver

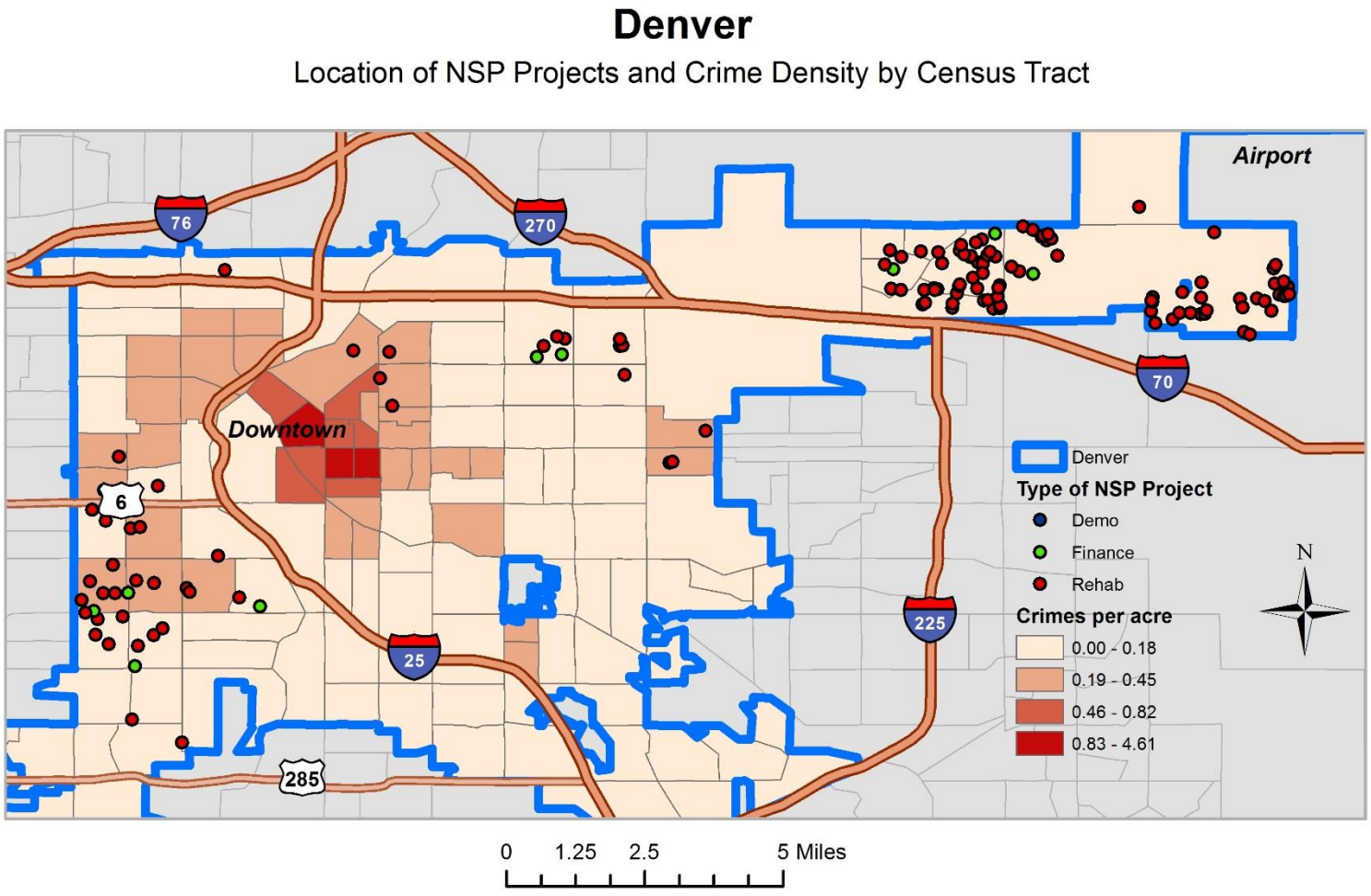

Figure 4: Stylized Illustration of Rings Methodology

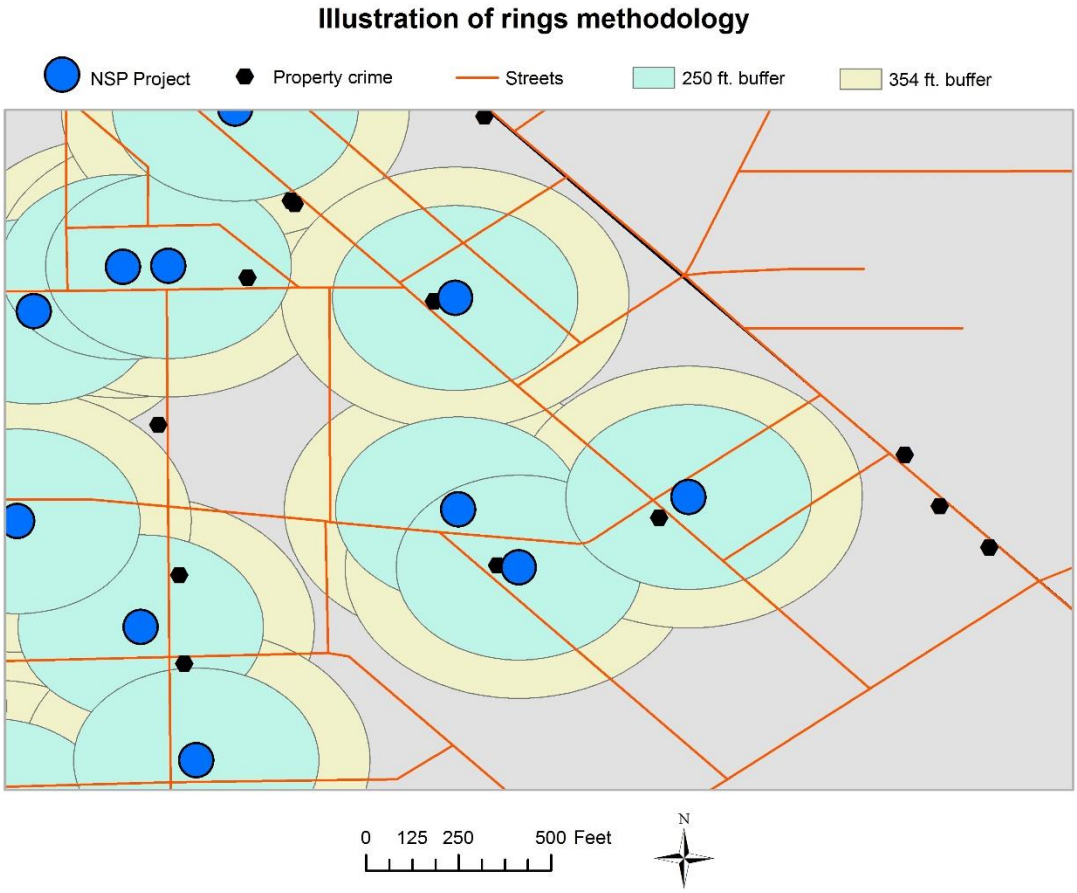

Figure 5: Cumulative Percent of NSP Properties with Activities Underway or Completed

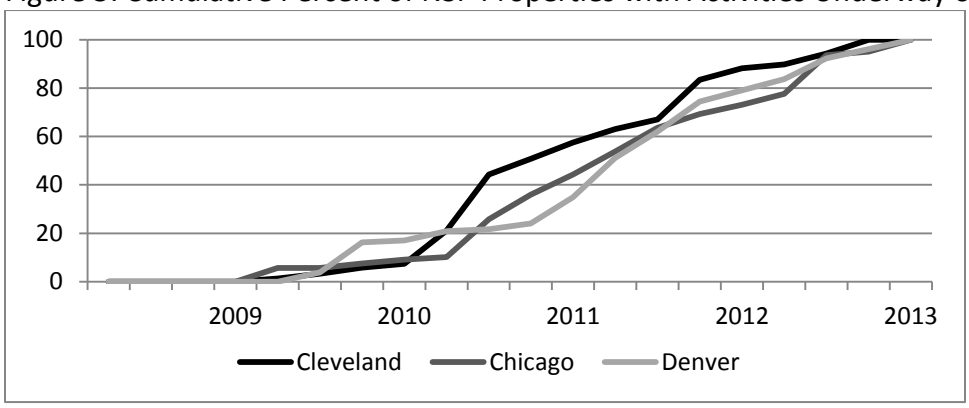

Table 1: Comparison of NSP Activities, Crime, and Social Conditions by City

	Cleveland	Chicago	Denver
NSP investments			
Total NSP properties	1054	275	139
Rehabilitation (%)	8.4%	54.9%	92.1%
Redevelopment (%)	0.4%	0.4%	0.0%
Demolition (%)	89.0%	41.1%	0.0%
Land Banking (%)	2.2%	3.6%	0.7%
Financing (%)	0.0%	0.0%	7.2%
Crime			
Total crimes per quarter	75.48	164.18	75.52
Total crime/pop	44.45	59.14	15.19
Total crime/sq mi	0.47	1.22	0.15
Other characteristics			
Population density	7,982	20,215	7,531
Median HH income	29,930	34,156	43,256
Hispanic (%)	8.7%	38.3%	61.1%
Black (%)	61.9%	53.3%	16.4
Distress/1000 properties	39.2	18.8	96.2
Vacancy/1000 properties	169.1	223.0	145.4

Note: NSP activities include the total for each city. Crime figures are the average per quarter for all quarters in the study period, for NSP tracts. Other characteristics are census tract averages for tracts containing NSP activities. Distress indicates the number of properties in foreclosure or REO, taken from Core Logic. Data on population characteristics comes from the American Community Survey (2005-2009).

Table 2: Distribution of Crimes by Offense Type in Census Tracts with NSP Investments (Mar 2008 – Feb 2013)

	Cleveland	Chicago	Denver
# Total Crimes	358,823	472,418	50,327
Violent Crimes			
Homicide	0.1%	0.2%	0.2%
Rape	0.7%	0.5%	0.7%
Robbery	5.0%	4.8%	3.6%
Aggravated Assault	3.5%	5.9%	5.3%
Property Crimes			
Burglary	15.6%	7.1%	17.0%
Theft	25.2%	15.2%	28.9%
Auto Theft	7.2%	5.2%	11.4%
Arson	0.7%	0.2%	0.5%
Other Crimes			
Simple Assault	21.9%	26.8%	5.6%
Drug Offenses	6.7%	19.8%	6.7%
Weapons Offenses	2.2%	1.6%	1.8%
Vandalism	11.3%	12.6%	18.3%

Table 3: Balance Tests Comparing the Average # Crimes Per Quarter during Quarters 1-4 (Mar 2008 – Feb 2009)

	Cleveland			Chicago			Denver		
	Treat Ring	Control Ring	Third Ring	Treat Ring	Control Ring	Third Ring	Treat Ring	Control Ring	Third Ring
Total Crime	2.92	2.88	3.02	6.38	6.29	6.65	0.61	0.61	0.55
Violent Crime	0.24	0.25	0.29**	0.77	0.79	0.81	0.05	0.03	0.04
Property Crime	1.40	1.37	1.40	1.55	1.54	1.71*	0.36	0.34	0.29
Other Crime	1.27	1.26	1.33	4.06	3.97	4.13	0.21	0.24	0.22
N	4108	4108	4108	1056	1056	1056	516	516	516

** p<.01; *p<.05

Table 4: Poisson Estimates of the Impact of NSP Investments by Crime Type

Column:	I	II	III	IV	V	VI
Outcome:	Violent Crime	Property Crime	Burglary	Theft	Auto Theft	Other Crime
	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)
Cleveland						
T x Demo	.009	-.072**	-.097**	-.077**	0.042	.030
	(.057)	(.027)	(.036)	(.030)	(.060)	(.029)
T x Rehab	.083	.112	.088	.105	0.215	.069
	(.252)	(.097)	(.109)	(.103)	(.153)	(.115)
Demo	.016	.030	.020	.042	0.030	-.065
	(.057)	(.032)	(.041)	(.035)	(.066)	(.031)
Rehab	-.204	.097	.161	.078	0.005	-.088
	(.157)	(.080)	(.116)	(.085)	(.134)	(.079)
T Ring	.030	.026	.054	.006	0.015	-.001
	(.052)	(.026)	(.030)	(.028)	(.049)	(.032)
Chicago						
T x Demo	-.088	.016	.048	.073	-0.133	.045
	(.087)	(.069)	(.115)	(.099)	(.122)	(.078)
T x Rehab	-.075	.001	-.011	-.015	0.097	.088
	(.081)	(.048)	(.081)	(.066)	(.091)	(.089)
Demo	.002	-.026	-.128	-.030	0.140	-.116
	(.077)	(.064)	(.111)	(.084)	(.128)	(.067)
Rehab	.044	-.020	-.086	.063	-0.115	-.034
	(.073)	(.044)	(.079)	(.065)	(.098)	(.064)
T Ring	.061	.050	.141*	-.034	0.073	.023
	(.057)	(.034)	(.057)	(.047)	(.060)	(.045)
Denver						
T x Rehab	.069	.109	.193	.006	0.154	-.092
	(.343)	(.114)	(.183)	(.181)	(.307)	(.145)
Rehab	-.230	.053	.018	.002	0.254	.267
	(.430)	(.139)	(.207)	(.209)	(.296)	(.164)
T Ring	.288	-.057	-.124	.016	-0.077	.170
	(.285)	(.108)	(.152)	(.137)	(.217)	(.125)

Separate models are estimated for each city and crime outcome. All models also include quarter fixed effects (results omitted). Standard errors are bootstrapped with 400 replications. Non-significant results are omitted for the other offense types with sufficient sample sizes for estimation: robbery, aggravated assault, simple assault, drug offenses, and vandalism.

N=34082 observations (1027 properties) in Cleveland; N=8144 observations (264 properties) in Chicago; N=3974 observations (129 properties) in Denver.

** p<.01; *p<0.5; T = treatment ring.

Table 5: Poisson Regression Estimates with 3 Rings
Selected results shown for Cleveland.

Column:	I		II		III	
Outcome:	Property Crime		Burglary		Theft	
	Coef	S.E.	Coef.	S.E.	Coef.	S.E.
T x Demo	-.077**	.029	-.119**	.036	-.065*	.031
C x Demo	-.005	.029	-.022	.037	.012	.033
T x Rehab	.061	.106	-.027	.116	.062	.114
C x Rehab	-.051	.124	-.115	.139	-.043	.133
Demo	.032	.029	.028	.038	.030	.030
Rehab	.099	.083	.184	.095	.088	.092
T Ring	.005	.027	.066*	.029	-.026	.030
C Ring	-.021	.028	.012	.030	-.031	.030

All models also include quarter fixed effects (results omitted). Standard errors are bootstrapped with 400 replications. T = treatment ring; C = control ring; Omitted baseline category is the third ring.
N=34082 observations (1027 properties) in Cleveland.
** p<.01; *p<.05

Table 6: Poisson Regression Estimates by Time Period
Selected results shown for Cleveland.

Column:	I		II		III	
Outcome:	Property Crime		Burglary		Theft	
	Coef	S.E.	Coef.	S.E.	Coef.	S.E.
T x Demo Work	-.121*	.050	-.129*	.060	-.164**	.052
T x Demo Year 1	-.126**	.032	-.159**	.047	-.117**	.036
T x Demo Year 2	-.001	.044	-.013	.054	-.024	.053
T x Demo Year 3	.030	.060	-.014	.076	.069	.068
T x Rehab Work	.074	.109	.073	.145	.074	.116
T x Rehab Year 1	.011	.126	-.095	.150	.005	.135
T x Rehab Year 2	.196	.181	.191	.185	.168	.223
T x Rehab Year 3	.462	.271	.621*	.309	.461	.358

All models include dummies for interacted variables (T Ring & NSP activity periods). Results omitted. Standard errors are bootstrapped with 400 replications.
N=34082 observations (1027 properties) in Cleveland.
** p<.01; *p<.0; T = treatment ring.

Appendices

Appendix A: Replication of Estimated Models using OLS

Exhibit A.1: Replication of Table 4 – OLS Regression Estimates for the Rings Model by Crime Type

	Violent Crime	Robbery	Aggravated Assault	Property Crime	Burglary	Theft	Auto Theft	Other Crime	Simple Assault	Drug Offenses	Vandalism
	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)	Coef (S.E.)
Cleveland											
T x Demo	0.002	0.008	-0.003	-0.099**	-0.052**	-0.053**	0.006	0.033	0.016	0.011	0.000
	0.013	0.010	0.007	0.037	0.019	0.020	0.009	0.037	0.027	0.012	0.014
T x Rehab	0.011	0.012	-0.011	0.143	0.038	0.070	0.035	0.064	0.009	0.008	0.036
	0.047	0.028	0.018	0.118	0.047	0.063	0.024	0.112	0.082	0.022	0.034
Demo	0.004	-0.002	0.005	0.037	0.004	0.026	0.007	-0.085*	-0.038	-0.024*	-0.016
	0.012	0.010	0.008	0.047	0.025	0.026	0.011	0.040	0.029	0.011	0.016
Rehab	-0.030	-0.025	0.005	0.120	0.070	0.047	0.003	-0.073	-0.032	-0.015	-0.017
	0.026	0.015	0.018	0.108	0.054	0.055	0.020	0.082	0.071	0.018	0.029
T Ring	0.007	0.003	0.002	0.037	0.030	0.004	0.002	-0.001	-0.010	0.006	0.001
	0.013	0.008	0.006	0.041	0.019	0.020	0.008	0.045	0.026	0.014	0.012
Chicago											
T x Demo	-0.056	0.004	-0.075	0.015	0.008	0.047	-0.040	0.156	0.015	0.051	0.079
	0.056	0.029	0.042	0.096	0.054	0.060	0.032	0.293	0.165	0.141	0.059
T x Rehab	-0.049	-0.005	-0.048	-0.001	-0.013	-0.012	0.024	0.314	-0.103	0.408	-0.035
	0.057	0.030	0.043	0.063	0.037	0.043	0.026	0.371	0.112	0.323	0.048
Demo	0.011	0.010	0.027	-0.022	-0.049	-0.010	0.037	-0.402	-0.033	-0.229	-0.126*
	0.054	0.032	0.041	0.087	0.051	0.052	0.035	0.258	0.120	0.175	0.058
Rehab	0.041	0.011	0.043	-0.030	-0.041	0.050	-0.039	0.022	0.158	-0.111	-0.004
	0.053	0.034	0.034	0.063	0.041	0.047	0.033	0.215	0.086	0.195	0.055
T Ring	0.040	-0.012	0.055	0.075	0.074*	-0.022	0.023	0.086	0.030	0.031	0.032

	0.042	0.020	0.030	0.055	0.032	0.033	0.019	0.164	0.093	0.098	0.037
Denver											
T x Rehab	0.005	0.006	0.000	0.035	0.026	0.001	0.008	-0.015	-0.017	-0.009	0.009
	0.012	0.004	0.012	0.032	0.025	0.017	0.014	0.028	0.012	0.011	0.024
Rehab	-0.009	0.000	-0.003	0.017	0.000	0.005	0.013	0.046	0.020	0.011	0.007
	0.015	0.007	0.013	0.037	0.026	0.026	0.011	0.025	0.014	0.013	0.021
T Ring	0.010	0.000	0.008	-0.017	-0.015	0.002	-0.004	0.029	0.012	0.008	0.009
	0.009	0.004	0.008	0.031	0.019	0.016	0.010	0.024	0.009	0.008	0.017

All models include quarter fixed effects. Standard errors are clustered at the block group level.
N=34082 observations (1027 properties) in Cleveland; N=8144 observations (264 properties) in Chicago; N=3974 observations (129 properties) in Denver.
** p<.01; *p<.0; T = treatment ring.

Appendix B: Balance Tests Comparing Trends in Treatment vs. Control Areas between Year 1 & Year 2.

Table 3 presents balance tests that compare the average number of crimes per quarter in the treatment and control rings. While these comparisons are useful for examining balance between the treatment and control rings, differences in the average level of crime between the treatment and control areas do not necessarily imply that the model will produce biased estimates. Instead, the necessary assumption is that the relative change in crime between the treatment and control rings across quarters is not correlated with unobserved factors. A limited test of this assumption is possible using data for the period prior to NSP investment. The tables below presents the results of models that estimate equation 1 after limiting the sample to the first eight quarters of the study period—Year 1 (quarters 1-4) and Year 2 (quarters 5-8)—and dropping observations for properties where NSP investment begins prior to quarter 9. The estimates treat quarters 5 through 9 as the treatment period, replicating the difference-in-differences estimator.

The resulting estimates provide an empirical test of whether any differences in the relative trend of crime between the treatment and control areas are measurable between the first and second years of the study period. In the results below, the Year 2 dummy variables show significant changes between Year 1 and Year 2 for several crime outcomes—i.e. that the trend in crime changed between years in ways that occurred in both the treatment and control rings. However, none of the 18 impact estimates are significant at the 5 percent level.

Exhibit B.1: Poisson Estimates for the Rings Model Comparing Y2 (Q5-Q9) to Y1 (Q1-Q4).

	Violent Crime	Property Crime	Burglary	Theft	Auto Theft	Other Crime
	Coef (S.E.)	Coef (S.E.)	Coef (S.E)	Coef (S.E)	Coef (S.E)	Coef (S.E.)
Cleveland						
T x Year 2	.060	.046	.012	.060	.088	.035
	.073	.036	.046	.044	.081	.037
Year 2	-.160**	.007	.045	.015	-.135*	-.023
	.051	.027	.039	.032	.055	.027
T Ring	-.014	.013	.069	-.009	-.065	-.010
	.065	.031	.039	.036	.061	.039
Chicago						
T x Year 2	.062	-.004	-.037	.020	-.044	.001
	.079	.056	.103	.076	.125	.048
Year 2	-.114*	-.064	.086	-.129*	-.148	-.005
	.057	.036	.067	.056	.089	.037
T Ring	.015	.028	.165*	-.039	-.027	.037
	.069	.046	.078	.074	.088	.049
Denver						
T x Year 2	-.749	-.292	-.311	-.211	-.412	.308
	.485	.182	.288	.269	.341	.217
Year 2	.431	.000	.018	-.071	.102	-.352
	.339	.130	.208	.194	.265	.182
T Ring	.526	.095	.154	.050	.069	-.093
	.425	.189	.266	.270	.281	.279

Standard errors are bootstrapped with 400 replications.
N=15168 observations (948 properties) in Cleveland; N=3,776 observations (236 properties) in Chicago; N=1,664 observations (104 properties) in Denver.
** p<.01; *p<.05; T = treatment ring.

Appendix C: Placebo Tests of Difference-In-Differences Model on Out-of Sample Data

As a basic specification test of the difference-in-differences model, we estimated the rings model on crime data for other areas of each city using a placebo measure of NSP treatment. These estimates provide a simple test of whether the spatial patterns of crime across rings, and time produce spurious relationships in the estimation models.

The rings samples for the placebo tests include non-NSP properties that are located in tracts with NSP activity. We randomly select a subset of non-NSP properties to achieve the same sample size as the set of NSP properties used in the impact analyses. The placebo measure of NSP investment randomly determines a start date for the NSP investment.

Consistent with the intended interpretation of each model, the results do not systematically identify relationships between the placebo measures of NSP investment and crime. None of the 18 estimated placebo coefficients is significant at the 5 percent level.

Exhibit C.1: Placebo Estimates for the Rings Model using Poisson Regression.

	Violent Crime	Property Crime	Burglary	Theft	Auto Theft	Other Crime
	Coef (S.E.)	Coef (S.E.)	Coef (S.E)	Coef (S.E)	Coef (S.E)	Coef (S.E.)
Cleveland						
T x Year 2	0.009	-0.009	-0.042	0.003	0.034	0.006
	0.050	0.026	0.032	0.032	0.056	0.027
Year 2	0.017	-0.024	-0.003	-0.016	-0.120*	-0.022
	0.053	0.023	0.031	0.026	0.051	0.029
T Ring	-0.003	0.060	0.066*	0.057	0.055	0.038
	0.126	0.031	0.031	0.036	0.050	0.063
Chicago						
T x Year 2	0.033	0.040	0.078	0.021	0.031	0.102
	0.056	0.043	0.065	0.066	0.077	0.028
Year 2	-0.048	-0.052	-0.101	-0.054	0.030	-0.046
	0.045	0.035	0.057	0.051	0.070	0.027
T Ring	0.008	0.018	-0.021	0.049	0.005	-0.028
	0.046	0.037	0.046	0.061	0.056	0.044
Denver						
T x Year 2	-0.227	0.094	-0.039	0.128	0.240	0.087
	0.264	0.132	0.173	0.187	0.213	0.118
Year 2	0.179	0.126	0.182	0.035	0.289	-0.167
	0.167	0.090	0.152	0.123	0.185	0.085
T Ring	-0.206	-0.090	0.050	-0.152	-0.169	0.005
	0.203	0.090	0.105	0.138	0.165	0.089

Standard errors are bootstrapped with 400 replications.
N=34082 observations (1027 properties) in Cleveland; N=8144 observations (264 properties) in Chicago; N=3974 observations (129 properties) in Denver.
** p<.01; *p<.05; T = treatment ring.