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The crime prevention effect of CCTV in public places: a propensity score analysis

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ABSTRACT

This study measures the effect of CCTV in Newark, NJ across three separate crime categories: auto theft, theft from auto, and violent crime. CCTV viewsheds, denoting camera line-of-sight, were units of analysis. Viewsheds for treatment units were created by digitizing live CCTV footage within a geographic information system (GIS). Control viewsheds were created with GIS tools and aerial imagery from Google maps. Treatment cases were matched with control cases via propensity score matching (PSM) to ensure statistical equivalency between groups. Effect was measured via odds ratios and average treatment on the treated statistics. Findings offer modest support for CCTV as a deterrent against auto theft while demonstrating no effect on the other crime types. These results suggest that CCTV appears to be a viable option for jurisdictions wishing to target auto theft. Agencies suffering from other street-level crime problems may not benefit from CCTV and may need to deploy CCTV alongside other evidence-based strategies, rather than as a stand-alone tactic, in order to achieve crime control benefits.

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Introduction

Closed-circuit television (CCTV) has become a mainstream crime prevention strategy around the world. Estimates from the United Kingdom suggest the presence of over 4.2 million cameras, a ratio of 1 per every 14 citizens (Norris and McCahill 2006). In the United States, 49% of local police departments report using CCTV, with usage increasing to 87% for agencies serving jurisdictions with populations of 250,000 or more (Reaves 2015). Complicating matters is the fact that while research designs have improved over time, the overall body of CCTV research has been classified as methodologically weak (Eck 2002; Welsh et al. 2011). CCTV evaluations have often not incorporated control areas, falling short of the minimum design needed to explore issues of causality (Cook and Campbell 1979). When controls have been used, researchers have not routinely ensured that treatment and control units are equivalent across pertinent variables.

This study is an evaluation of the 146-camera CCTV system in Newark, NJ. It extends upon the evaluation by Caplan, Kennedy, and Petrossian (2011), which focused on the first 73 cameras installed in Newark. The current study uses propensity score matching (PSM) to match treatment units to statistically equivalent controls, thus approximating the conditions of a randomized experiment (Rosenbaum and Rubin 1983, 1985). The study also incorporates the recently developed viewshed methodology (Caplan, Kennedy, and Petrossian 2011; McLean, Worden, and Kim 2013; Ratcliffe, Taniguchi, and Taylor 2009),

micro-level units of analysis denoting camera line-of-sight. CCTV effect was measured through odds ratio (OR) and average treatment on the treated (ATT) statistics. Findings provide modest support for CCTV as a deterrent to auto theft. CCTV was ineffective against other crime types.

Review of relevant literature

CCTV and crime prevention

While a range of potential crime prevention mechanisms have been theorized for CCTV (Gill and Spriggs 2005), the practical application of CCTV predominately relates to deterrence (Farrington et al. 2007; Ratcliffe 2006). From a situational crime prevention perspective, notions of deterrence are rooted in the rational choice theory of criminality (Cornish and Clarke 1986). Whereas deterministic theories view crime as an inevitable byproduct of social ills, rational choice considers crime as 'purposive behavior designed to meet the offender's commonplace needs' (Clarke 1997, 9–10). When engaging in a decision-making process, the offender considers a number of 'choice structuring properties' which include the pros, cons, and inherent risk involved the commission of a particular crime. As argued by Ratcliffe (2006, 8), the primary aim of CCTV is considered to be the triggering of a perceptual mechanism in a potential offender 'so that an offender believes if he commits a crime, he will be caught.' This is paramount in impacting the choice structuring properties of an offender in a manner that persuades them to abstain from crime.

Systematic reviews and meta-analyses conducted by Welsh and Farrington (2002, 2009a) provide overviews of the collective knowledge on CCTV. These reviews selected CCTV evaluations for inclusion according the following criteria: (1) CCTV was the main focus of the intervention; (2) the evaluation used an outcome measure of crime; (3) the research design involved, at minimum, before-and-after measures of crime in experimental and control areas, and; (4) both the treatment and control areas experienced at least 20 crimes during the pre-intervention period. Effect size was measured as an OR, indicating the proportional crime changes in the control area compared with the target area. Welsh and Farrington (2002) identified 22 evaluations for inclusion in their meta-analysis, finding that CCTV had a small, but significant, effect on vehicle crimes and no effect on violent crimes. Welsh and Farrington's most recent review (2009a) included 41 evaluations and examined CCTV effect across 4 main settings: city and town centers, public housing, public transport, and car parks. While the cumulative studies identified a 16% drop in crime, the reduction was driven by the 51% reduction in car parks, with the city and town center systems not having any significant effect. The findings of the systematic reviews (Welsh and Farrington 2002, 2009a) suggest that CCTV works better in well-defined settings (specifically car parks) than public places, most greatly impacts vehicle crime, and has little to no effect on violent crime.

Studies published since Welsh and Farrington conducted their latest review present evidence both in support of and contrary to the review findings. In their evaluation of 14 CCTV projects in England, Farrington et al. (2007) found that CCTV was effective in car parks, particularly against vehicle crime, but not in city centers or residential areas. A recent analysis of a car park system in Surrey found no evidence that CCTV influenced theft of or theft from motor vehicles (Reid and Andresen 2014), contrary to the findings of Welsh and Farrington (2002, 2009a). Recent research on public CCTV systems has produced particularly mixed results. In their study of San Francisco's CCTV system, King, Mulligan, and Raphael (2008) found no significant effect on violent crime, drug offenses, vandalism, or prostitution. Cameron et al. (2008) analyzed CCTV systems in two separate areas of Los Angeles. Neither system had any measurable effect on violent crime, property crime, or misdemeanor arrests. Lim and Wilcox (2016) found that CCTV had little effect on crime in public places of Cincinnati, though assault, robbery, and burglary showed signs of reduction effects within residential areas. Null effects have also emerged in recent studies outside of the United States, as Gerell (2016) found the implementation of actively monitored CCTV did not reduce assaults in a nightlife area of Malmö, Sweden.

While the aforementioned studies concur with the collective knowledge on CCTV, namely that cameras have limited effect in public places, recent studies have found some positive effects. Ratcliffe,

Taniguchi, and Taylor (2009) found that Philadelphia's CCTV cameras generated a 13% reduction in overall crime, a 16% reduction in disorder, but no change in serious crime. In their analysis of the first 73 cameras installed in Newark, NJ, Caplan, Kennedy, and Petrossian (2011) measured CCTV impact on 3 crime types: auto theft, theft from auto, and shootings with a significant reduction being achieved for auto theft. La Vigne et al. (2011) analyzed seven CCTV systems in three US cities: Baltimore (four systems), Chicago (two systems), and Washington, DC (one system). Significant reductions were observed in three of the Baltimore systems and one of the Chicago systems. In their study of CCTV in Schenectady, NY, McLean, Worden, and Kim (2013) found that CCTV produced a significant reduction of violent crime and disorder, but had no effect on property crime.

Evaluation methodology and CCTV control area designation

The current era of evidence-based criminology stresses the use of scientific research to guide program development and implementation (Sherman et al. 1997). Evidence-based criminology grades individual studies according to the Maryland scientific methods scale, which assigns a score of 1–5 depending on the methodology incorporated by the study (Sherman et al. 1997). The minimum interpretable design is level 3, a measure of crime before and after a program in experiment and comparable control conditions (Cook and Campbell 1979).

Reviews of research have found that CCTV evaluations often fall below level 3 of the Maryland scale. In his review of crime prevention strategies within various types of places, Eck (2002) noted that CCTV evaluations routinely did not incorporate control areas. Welsh et al. (2011) similarly reported that over 55% of studies on public surveillance used less than a comparable case-control design. The absence of control areas has important implications for the study of CCTV. In addition to compromising the internal validity of individual evaluations, such methodology hinders overall knowledge development. For example, when updating their original meta-analysis, Welsh and Farrington (2009a) excluded 23 of the 45 studies completed since their previous review due to the lack of a control area, meaning that roughly half of the new CCTV research was unable to be included in a cumulative test of CCTV effect.

It should be noted, however, that even when control areas have been incorporated, there is uncertainty regarding how well they alleviate selection bias. Random assignment provides the best avenue for reducing selection bias by creating treatment and control conditions that are statistically equivalent across pertinent variables (Cook and Campbell 1979, 341). As all documented evaluations of CCTV have occurred *post hoc*, randomization of treatment and control groups has not been possible. Randomization is incredibly challenging in respect to CCTV. Expenditures related to CCTV deployment routinely total in the millions of dollars when hardware, software, and maintenance expenses are considered (La Vigne et al. 2011). Furthermore, CCTV sites are largely permanent fixtures, with cameras hard wired to physical structures and wireless networks configured to stream footage to/from specific locations. This means that changing target areas post-experimentation would require additional expenses to remove cameras from one place and install in another. Police interventions that have more readily incorporated randomization, such as hot spots policing (Braga 2005), can much more easily change target areas post-experiment without incurring such costs.

When randomization is not feasible, researchers have incorporated matching techniques to ensure equivalency between treatments and controls. In their evaluation of the Boston Police Department's Safe Street Teams program, Braga, Hureau, and Papachristos (2012) used PSM to match target areas with comparable control areas throughout the city. While PSM has not been readily incorporated in the CCTV literature, some scholars have attempted to match target areas with similar controls. Farrington et al. (2007) matched each of the 14 systems included in the analysis with a control area with similar crime problems and socio-demographic features. La Vigne et al. (2011) selected control areas based upon their similarities to target areas in terms of land use, historical crime rates, and socioeconomic measures. However, these studies are not representative of CCTV research, with studies largely using easily accessible geographies, such as police precincts or neighborhoods without CCTV, as control areas without ensuring the balance of pertinent covariates.

Scope of current study

While CCTV studies have begun to emphasize statistically equivalent treatment and control groups, such approaches are not commonplace. PSM, which can approximate randomization by ensuring statistical equivalency of treatment and control groups, has yet to be incorporated in the study of CCTV. It is with this in mind that the current study approaches the evaluation of the full CCTV system in Newark, NJ. Units of analysis build upon the newly developed viewshed methodology, specifically the respective approaches of Caplan, Kennedy, and Petrossian (2011) and Ratcliffe, Taniguchi, and Taylor (2009). PSM (Rosenbaum and Rubin 1983, 1985) was used to select control areas that were statistically equivalent to the target areas across several key characteristics.

The current study resulted from a partnership between the Newark Police Department and a research team led by the author, which was funded by a grant from the National Institute of Justice (2010IJ-CX-0026). As part of this effort, the Newark Police Department provided the research team with all necessary crime, police activity, and CCTV data-sets. The Rutgers University (the lead agency on the grant) Institutional Review Board exempted the project from full ethics review given the absence of human subjects.

Study setting

Newark is the largest city in NJ, spanning over 26 square miles with a population of nearly 280,000 persons. The percentage of residents living below the poverty level (28%) is nearly three times that of NJ as a whole (9.9%). Ethnic minorities largely comprise Newark's population with 52.4% of the population black and 33.8% of residents identifying themselves as Hispanic or Latino (U.S. Census Bureau 2015). Newark has a long-standing reputation as a tumultuous, dangerous urban environment (Tuttle 2009). To help combat this trend, the city has made significant investments to upgrade many of its technological capabilities, including the installation of a CCTV system. Two CCTV operators under the supervision of a police sergeant monitor live footage from the cameras during all shifts from a centralized control room. Newark's camera installation occurred in five distinct phases, beginning in 2007 and ending in 2010. Alongside the CCTV system, Newark built and expanded a wireless telecommunications network that transmits video from the field to the communications center. Since network connectivity was a prerequisite for camera sites, camera installation did not immediately occur in a city-wide manner. Rather, camera installation began within the city's urban enterprise zone and then continued to other neighborhoods as the wireless network expanded.

The impact of Newark's CCTV system was first measured in the aforementioned analysis of Caplan, Kennedy, and Petrossian (2011), which found auto theft to be the only one of three crime types included in the analysis to experience a system-wide reduction during the first phase of the program. In addition to measuring the system-wide effect, Caplan, Kennedy, and Petrossian (2011) measured individual viewshed crime levels for both the 'pre' and 'post' installation periods via a location quotient (LQ), which measures crime levels in a target area compared to its occurrence over a larger control area. An LQ change toward the negative from the 'pre' to 'post' period suggested a crime reduction. Of the 73 viewsheds included in the analysis, 58 experienced reduced levels of shootings, with auto theft and theft from auto reducing in 34 and 41 viewsheds, respectively.

Piza, Caplan, and Kennedy (2014a) replicated the individual-level analysis of Caplan, Kennedy, and Petrossian (2011) when Newark's system expanded to the full 146 cameras. The analysis began by calculating a change in location quotient (Δ LQ) variable for each viewshed across six crime categories. 47.01% of viewsheds exhibited negative Δ LQ values for overall crime, 42.74% for violent crime, 49.57% for property crime, 52.14% for theft from auto, and 46.15% for both auto theft and robbery (Piza, Caplan, and Kennedy 2014a, 253). Follow-up regression models tested the effect of various factors on Δ LQ values for each crime type.

While Piza, Caplan, and Kennedy (2014a) attempted to identify correlates of Δ LQ values, the main component of Caplan, Kennedy, and Petrossian (2011), the system-wide effect of CCTV, was not

replicated. This means that there currently is no empirical measure of effect for Newark's full CCTV system. In addition, ensuring equivalency between target and control areas was beyond the scope of Caplan, Kennedy, and Petrossian (2011, 263), with the authors acknowledging that quantifying the 'environmental, social, and/or criminogenic attributes of places where cameras are installed can be a separate study in itself – to typify these places and quantify the significant similarities and differences they have with respect to all other places in Newark.' The current study directly addresses this issue.

Methodology and analytical approach

Units of analysis

Units of analysis for the current study (see Figure 1) build upon the recently developed viewshed methodology (Caplan, Kennedy, and Petrossian 2011; Ratcliffe, Taniguchi, and Taylor 2009). For the treatment areas, viewshed creation followed the approach of Ratcliffe, Taniguchi, and Taylor (2009). Researchers viewed the live feeds of the panning-mode¹ of all CCTV cameras in Newark and digitized the viewshed of each site within a GIS. A detailed GIS base map (with layers displaying streets, land parcels, building footprints, and aerial imagery) was incorporated to ensure that digitized viewsheds accurately reflected the physical landscape. For example, if the viewable area to the southeast of a camera was obstructed by a building, researchers 'snapped' the viewshed boundaries to that building in order to accurately reflect the line-of-sight.

A catchment zone was created for each viewshed to allow for a test of spatial displacement. Following the approach of Ratcliffe, Taniguchi, and Taylor (2009), catchment zones began as 291 foot buffers around each viewshed to reflect the median block size in Newark. The buffers were adjusted to reflect local road patterns. As explained by Ratcliffe, Taniguchi, and Taylor (2009, 752),

the use of actual camera viewsheds can mean that a...buffer stretches to just short of a neighboring intersection. In circumstances like this, the addition of an extra 20 ft. is sufficient to include the street intersection...and create a buffer that is a more realistic approximation of the likely displacement area.

Therefore, when a catchment zone was half a block or less from the nearest intersection, it was extended to the intersection. Otherwise, it was constricted to the buffer.

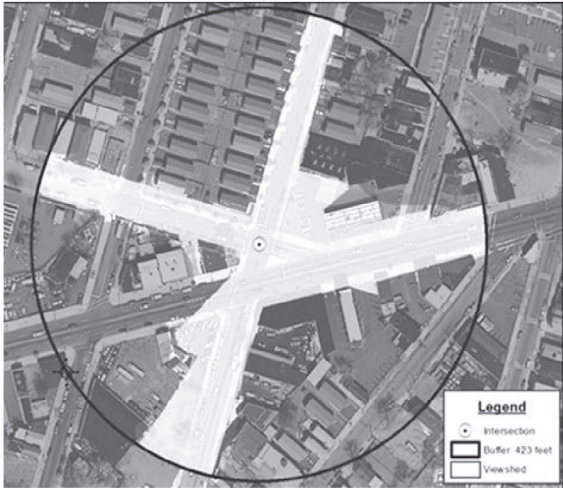
This process resulted in the creation of viewsheds and catchment zones for 141 of the system's 146 cameras. Viewsheds were not created for five cameras because they were out of service for over a year. Additionally, 13 viewsheds were excluded due to the police department having imprecise information regarding their installation dates.² Overlapping viewsheds were considered as single sites to protect against the multiple counting of individual crime incidents falling within more than one viewshed (Ratcliffe, Taniguchi, and Taylor 2009). In particular, 18 viewsheds overlapping with at least one other viewshed were merged into seven cases. After these adjustments, the analysis included 117 final viewsheds installed over four dates: 15 March 2008 (44), 31 July 2008 (50), 10 December 2009 (13), and 23 April 2010 (10).

In an effort to create control areas similar in scope to the treatment areas, pseudo-viewsheds were created for all areas of Newark falling outside of CCTV viewsheds and catchment areas. Because all cameras were placed at street intersections, the process began by first creating a GIS file of all street intersections in the study area. Following the technique developed by Braga, Papachristos, and Hureau (2010), a series of GIS geoprocessing functions generated points at every location where two or more streets intersected ($N = 2,141$).³ All intersections within a camera viewshed or catchment area were excluded, leaving a total of 961 intersections.

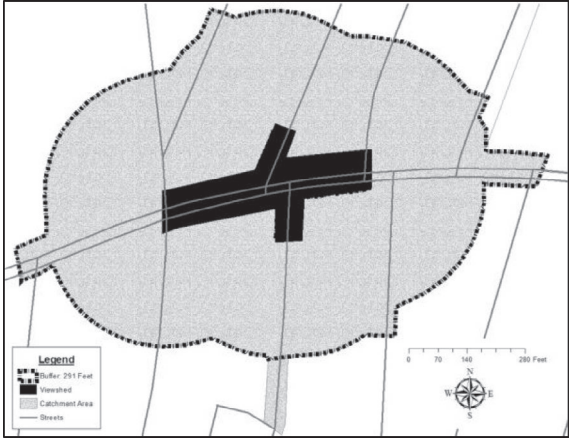
The creation of the pseudo-viewsheds followed the approach of Caplan, Kennedy, and Petrossian (2011), who first created 582 ft. buffer zones, approximately twice the average block length in Newark, around each camera location. Using imagery from Google maps and ArcGIS editing tools, they manually drew viewsheds within each buffer zone, excluding areas blocked by permanent fixtures, such as buildings. The current study utilized 423 ft. buffers around intersections to reflect the median maximum visible extent (the distance from the camera point to the furthest extent of its respective viewshed)



CCTV Viewshed



Pseudo Viewshed



Catchment Zone

Figure 1. Example CCTV viewshed, pseudo-viewshed, and catchment zone.

of the cameras included in the analysis, as measured in a GIS. Viewsheds were then manually drawn within each intersection buffer, excluding areas of obstruction as identified through aerial imagery. This approximated the visibility of hypothetical cameras at the intersections.

Outcome measures

Outcome measures were informed by the analysis of Caplan, Kennedy, and Petrossian (2011). Auto theft and theft from auto, both incorporated by Caplan, Kennedy, and Petrossian (2011), were included as outcome measures in the current study. However, Caplan, Kennedy, and Petrossian's (2011) third outcome measure, shootings, was not replicated in the current study. Instead, incidents of murder, non-fatal shootings, and robbery were summed to create an aggregate violent crime measure (aggravated assault was not included due to a high proportion of incidents occurring indoors, out of the view of CCTV: see Piza, Caplan, and Kennedy 2014a; Table 1). This was done in recognition of the sparse occurrence of shootings compared to other crimes, which could complicate the interpretation of study findings. For example, Caplan, Kennedy, and Petrossian (2011) compared shooting totals in viewsheds of cameras installed during different times of the year with randomly selected control viewsheds, finding statistically significant differences. Caplan, Kennedy, and Petrossian (2011, 264–265) argued that these findings were not 'contextually substantive in raw form' given the low number of shootings, and thus concluded that the shooting totals were not meaningfully different despite statistical significance. The use of an aggregate violent crime category effectively navigates this issue given the more frequent occurrence of the cumulative crime incidents.⁴

Propensity score matching: covariate selection

Pseudo-viewsheds were selected for the control group via PSM. Following the creation of prospective control cases (i.e., the pseudo-viewsheds) a pool of covariates was considered for inclusion in the PSM algorithm. While some scholars advocate a 'kitchen sink' approach incorporating all available variables in a data-set, others have warned that using more variables can lead to poor matches by inflating the range of propensity scores (Smith and Todd 2005). It is considered good practice to only include carefully chosen covariates that are truly related to the outcome in question (Guo and Fraser 2010). This avoids the problem of over-parameterized models, in which non-significant variables bias the estimates by substantially increasing variance (Caliendo and Kopeinig 2005).

Shadish (2013) emphasized the importance of PSM meeting the strong ignorability assumption – when assignment to the treatment group is conditionally independent from the analysis outcomes. Meeting the strong ignorability assumption is challenging because no statistical test exists to measure whether the selected covariates achieve this goal. Shadish (2013) advocated for researchers to understand the process by which cases were selected for treatment, and to select covariates in a manner that is reflective of said process. In light of these observations, the current study approached covariate selection in a manner that reflected the process of CCTV deployment in Newark. Ten covariates were included.

Each camera installation phase was preceded by an analysis of the spatial concentration of crime, to allow officials to select camera sites within high-crime places. Therefore, the first covariate included in the analysis was *the number of crime incidents that occurred during the one-year pre-installation period*. In addition, NPD officials reported that they chose camera sites in a manner that reflected their place-based policing priorities. As is the case with any police agency, the NPD has previously made numerous efforts to control crime at high crime places. Thus, the second covariate included in the PSM models was *arrest incidents that occurred during the one-year pre-installation period* to reflect the level of prior law enforcement activity within each viewshed.⁵

To reflect the piecemeal manner of camera deployment the PSM model accounted for various spatial characteristics of CCTV sites. Four dichotomous variables were created to identify whether the viewshed fell within each of Newark's four police precincts: *2nd Precinct*, *3rd Precinct*, *4th Precinct*, and *5th Precinct*. These variables ensure that each police precinct is equally represented in the treatment and control

Table 1. Propensity score matching covariate balance.

| Variable | Auto theft | | | Theft from auto | | | Violent crime | | |
|----------------------------------|----------------------------------|--------|------------------|----------------------------------|--------|------------------|----------------------------------|--------|------------------|
| | Treated (Untreated) | % Bias | % Bias Reduction | Treated (Untreated) | % Bias | % Bias Reduction | Treated (Untreated) | % Bias | % Bias Reduction |
| <i>Pre-crime count</i> | | | | | | | | | |
| Unmatched | 3.25 (3.09) | 5.50 | -199.50 | 2.69 (1.75)** | 29.00 | 45.30 | 2.50 (1.31)** | 56.20 | 71.40 |
| Matched | 3.11 (3.58) | -16.40 | | 2.33 (2.84) | -15.90 | | 2.24 (1.90) | 16.10 | |
| <i>Pre-arrests</i> | | | | | | | | | |
| Unmatched | 85.24 (23.88)** | 58.10 | 87.00 | 85.24 (23.88)** | 58.10 | 79.20 | 85.24 (23.88)** | 58.10 | 81.50 |
| Matched | 66.37 (58.42) | 7.50 | | 64.87 (52.09) | 12.10 | | 64.32 (52.95) | 10.80 | |
| <i>Ambient population</i> | | | | | | | | | |
| Unmatched | 5333.10 (4363.10)** | 43.20 | 95.80 | 5333.10 (4363.10)** | 43.20 | 95.60 | 5333.10 (463.10)** | 43.20 | 82.00 |
| Matched | 5008.90 (5049.60) | -1.80 | | 5003.30 (4960.80) | 1.90 | | 5027.40 (5201.80) | -7.80 | |
| <i>Residential Population</i> | | | | | | | | | |
| Unmatched | 1267.50 (1318.20) | -10.30 | 89.50 | 1267.50 (1318.20) | -10.30 | 83.10 | 1267.50 (1318.20) | -10.30 | 28.80 |
| Matched | 1261.10 (1266.50) | -1.10 | | 1265.60 (1257.00) | 1.70 | | 1255.00 (1291.20) | -7.30 | |
| <i>Concentrated disadvantage</i> | | | | | | | | | |
| Unmatched | 0.38 (-0.38)** | 25.40 | 51.20 | 0.38 (-0.38)** | 25.40 | 62.80 | 0.38 (-0.38)** | 25.40 | 85.40 |
| Matched | 0.46 (0.09) | 12.40 | | 0.51 (0.23) | 9.50 | | 0.46 (0.35) | 3.70 | |
| <i>Racial heterogeneity</i> | | | | | | | | | |
| Unmatched | 0.09 (0.10) ** | -27.00 | 98.20 | 0.09 (0.10)** | -27.00 | 89.50 | 0.09 (0.10)** | -27.00 | 84.40 |
| Matched | 0.09 (0.09) | 0.50 | | 0.09 (0.09) | 2.80 | | 0.09 (0.09) | 4.20 | |
| <i>2nd Precinct</i> | | | | | | | | | |
| Unmatched | 0.21 (0.39)** | -38.40 | 67.90 | 0.21 (0.39)** | -38.40 | 78.40 | 0.21 (0.39)** | -38.40 | 100.00 |
| Matched | 0.23 (0.18) | 12.30 | | 0.23 (0.20) | 8.30 | | 0.23 (0.23) | 0.00 | |
| <i>3rd Precinct</i> | | | | | | | | | |
| Unmatched | 0.21 (0.32)** | -26.30 | 75.90 | 0.21 (0.32)* | -26.30 | 91.90 | 0.21 (0.32)** | -26.30 | 83.90 |
| Matched | 0.19 (0.21) | -6.40 | | 0.19 (0.20) | -2.10 | | 0.19 (0.17) | 4.20 | |
| <i>4th Precinct</i> | | | | | | | | | |
| Unmatched | 0.32 (0.16)** | 37.90 | 76.90 | 0.32 (0.16)** | 37.90 | 41.80 | 0.32 (0.16)** | 37.90 | 82.70 |
| Matched | 0.31 (0.35) | -8.70 | | 0.31 (0.40) | -22.10 | | 0.32 (0.35) | -6.60 | |
| <i>5th Precinct</i> | | | | | | | | | |
| Unmatched | 0.26 (0.13)** | 32.70 | 92.70 | 0.26 (0.13)** | 32.70 | 48.70 | 0.26 (0.13)** | 32.70 | 92.70 |
| Matched | 0.27 (0.26) | 2.40 | | 0.27 (0.21) | 16.80 | | 0.26 (0.25) | 2.40 | |
| N | 228 (114 Treated, 114 Untreated) | | | 224 (112 Treated, 112 Untreated) | | | 226 (113 Treated, 113 Untreated) | | |
| P5M algorithm | Nearest neighbor, caliper = 0.01 | | | Nearest neighbor, caliper = 0.01 | | | Nearest neighbor, caliper = 0.01 | | |

* $p < 0.05$; ** $p < 0.01$.

group, meaning the final control areas were susceptible to the same organizational forces that influence the treatment areas (Piza, Caplan, and Kennedy 2014a; Ratcliffe, Taniguchi, and Taylor 2009). Three additional variables accounted for the population socio-demographics of the surrounding area. The first such variable was *the concentration of social disadvantage in the surrounding 2010 U.S. Census block group*. Following the approach of recent policing evaluations (Braga, Hureau, and Papachristos 2012) this covariate is an index measuring the concentrated social disadvantage (Morenoff, Sampson, and Raudenbush 2001) in the surrounding block group of each viewshed.⁶ The second socio-demographic variable was the *racial heterogeneity of the surrounding 2010 U.S. Census block group*. Racial heterogeneity was defined using a probability-based approach (Weisburd, Groff, and Yang 2012). The probabilities of different racial groups being within the same block group were averaged to form the overall racial heterogeneity index.⁷ The third socio-demographic variable was the *residential population of the surrounding 2010 U.S. Census block group*, a proxy for the number of potential victims. An additional measure of at-risk persons was included to account for the number of persons who frequent an area for work, school, or recreation but who do not reside in the area, known as the *ambient population* (Andresen 2011). The ambient population was calculated using the Oak Ridge National Laboratory's LandScan database, which provides a 24-h estimate of the expected population present at a spatial scale of about 1 km² (Andresen 2011, 195). Each viewshed was assigned the ambient population of its surrounding grid.

Following the calculation of the propensity score, the matching process accounted for the fact that cameras were installed over four different installation phases: Phase 1 (3/15/08), Phase 2 (7/31/08), Phase 3 (12/10/09), and Phase 4 (4/23/10). Crime and arrest totals for each of the prospective control areas were measured for the one-year 'pre' and 'post' periods of each installation phase. Each of the 'pseudo' viewsheds was thus represented four times in the data-set, to reflect the time frame coinciding with each of the four installation phases. To ensure that viewsheds were matched with a control incorporating the same time period, an exact match on installation phase was conducted after the calculation of the propensity score. This process follows the approach of prior research that matched treatment and control units on a single covariate of importance before other relevant variables were considered (Chen et al. 2012; Day et al. 2008; Pina-Sánchez and Linacre 2014). In the current study, the exact matching process ensures that each treatment viewshed was compared to a control with and identical 'pre' and 'post' time frame.

To achieve the exact match, each observation's propensity score was converted via the following formula:

$$[\text{Installation Phase} \times 10] + \text{propensity score}$$

This generated a value to the left of the decimal point representing the installation phase (i.e., phase 1 = 10; phase 2 = 20; phase 3 = 30; phase 4 = 40). For example, a viewshed installed during period 2 with a propensity score of 0.05 had a final propensity score of 20.05. This forced an exact match on installation phase (the left side of the decimal point) before matching based on the propensity score (the right side of the decimal point). Because viewsheds from different phases were a value of at least 10 from one another, they could not be matched together.

PSM was conducted through the PSMATCH2 program in the Stata 13.0 software package (Leuven and Sianesti 2003). Propensity scores were calculated via a logistic regression model with treatment status as the binary dependent variable to obtain predicted probabilities for all observations (Guo and Fraser 2010, 263). A nearest neighbor algorithm was used by which each treated case was matched to the untreated case with the most similar propensity score. The PSM model was refined through the use of a caliper, whereby the specified number of untreated units is selected within a maximum distance or tolerance (Apel and Sweeten 2010, 551). Calipers help to avoid bad matches by ensuring that the selected untreated units are sufficiently similar to the treated units. In the current study, a caliper distance of 0.01 was incorporated. Treated units with a propensity score greater than 0.01 from the closest untreated unit were considered outside of the region of common support and, thus, excluded from the analysis (Caliendo and Kopeinig 2005). Matching was conducted without replacement, meaning that

once a control case was matched with a treated case it was removed from the candidates for matching. Covariate balance was assessed through independent samples t-tests (Dehejia and Wahba 1999) and estimation of the standardized bias (Rosenbaum and Rubin 1985). When the t-test $p > 0.05$ and % bias < 20.0 balance is achieved (Austin, Grootendorst, and Anderson 2007). In the current study, the nearest neighbor algorithm with a caliper distance of 0.01 achieved balance for each outcome measure. This produced a final sample size of 228 (114 treated, 114 untreated) for the auto theft analysis, 224 (112 treated, 112 untreated) for the theft from auto analysis, and 226 (113 treated, 113 untreated) for violent crime analysis.⁸

Analytical approach

CCTV camera effect was measured two ways. First, the target area-wide crime change was measured via an OR:

$$OR = (a * d) / (b * c)$$

where a is the number of pre-intervention crimes in the target area, b is the number of during-intervention crimes in the target area, c is the number of pre-intervention crimes in the control area, and d is the number of during-intervention crimes in the control area. An $OR > 1$ indicates a desirable effect on crime in the target area relative to the control, while an $OR < 1$ indicates an undesirable effect. The inverse of the OR displays the crime difference within the target area. For example, an OR of 1.42 implies that target area crime reduced 30% relative to the control given that the inverted value of the OR ($1/1.42$) is 0.70 (Welsh and Farrington 2009a, 727).

Variance of the OR is calculated from the variance of the natural logarithm of OR via the below formula (Welsh and Farrington 2009b, 135):

$$VAR(LROR) = 1/a + 1/b + 1/c + 1/d$$

This estimation of variance is based on the assumption that crime follows a Poisson distribution. However, much research suggests that crime data are more accurately modeled according to a negative binomial distribution, which accounts for over-dispersion. Using the prior formula would underestimate the true variance of the data (Higginson and Mazerolle 2014, 438). Variance was calculated through an adapted formula that adds a parameter to control for over-dispersion (Farrington et al. 2007; Higginson and Mazerolle 2014; Welsh and Farrington 2009b):

$$VAR(OR) = [[(.008 * a) + 1.2] * a] / a^2 + [[(.0008 * b) + 1.2] * b] / b^2 + [[(.0008 * c) + 1.2] * c] / c^2 + [[(.0008 * d) + 1.2] * d] / d^2$$

Standard errors of $VAR(OR)$ were used to calculate confidence intervals for the observed OR (Lipsey and Wilson 2001).

Micro-level effect was measured via the ATT. Rather than consider the cumulative crime totals, the ATT measures whether individual treated units experienced a treatment effect that significantly differed from that of their matched control units. The ATT 'is defined as the expected effect of treatment for those individuals actually assigned to the treatment group' (Apel and Sweeten 2010, 545). ATTs were calculated through the PSMATCH2 program in the Stata 13.0 software package (Leuven and Sianesi 2003). The calculation of matching estimators often incorporates the bootstrap method to calculate robust standard errors. However, Abadie and Imbens (2008) found that the bootstrap method is not valid for nearest neighbor matching techniques, specifically due to significant misspecification of the asymptotic variance of matching estimators. Rather, Abadie and Imbens (2008) advocate for the use of an asymptotic variance estimators (Abadie and Imbens 2006; Abadie et al. 2004) with nearest neighbor matching. These estimators were incorporated in the current study via the NNMATCH command in STATA 13.0 (Abadie et al. 2004). Because the bootstrap method is valid for one-to-many matching techniques, bootstrapping was used in the sensitivity analysis for all radius and kernel matching algorithms.

Results

Table 1 displays the results of the PSM process. While several covariates are imbalanced across the treated and untreated units in the unmatched models, all are balanced in the matched sample, with $p > 0.05$ and % bias < 20 in each instance.

Table 2 presents the pre- and post-installation crime counts, OR, and ATT for each of the crime categories. Post-matching, treated viewsheds experienced raw crime count reductions of all crime categories except for violent crime. However, when crime changes within the untreated cases are accounted for, only auto theft experienced a statistically significant reduction of approximately 21% in the treated viewsheds as compared to the untreated viewsheds (OR = 1.26). ATT values were statistically insignificant for each crime category. This suggests that, while the cumulative target areas may have experienced a reduction of auto theft, the micro-level crime changes in the individual CCTV viewsheds did not significantly differ from that of the control viewsheds.

Table 3 presents the results of a sensitivity analysis conducted to measure whether the observed results were influenced by the PSM algorithm selection. Results are reported across a representative selection of 13 separate PSM algorithms. PSM model selection did influence the calculation and statistical significance of the OR for auto theft. For auto theft, three algorithms produced statistically significant OR values suggestive of a crime reduction (OR > 1). ORs were also above 1 for each of the other 10 algorithms, but they only approached statistical significance at $p < 0.10$. For both theft from auto and violent crime, all OR values were statistically insignificant. Findings were much more stable for the ATT values. For all three outcome measures, ATT values were statically insignificant across all 13 PSM algorithms, mirroring the main findings. However, for violent crime, ATT values for the nearest neighbor, no caliper and nearest neighborhood, caliper = 0.01 models approached statistical significance ($p < 0.10$).

Table 2. Raw crime counts, OR, and ATT.

| Crime category | N | Pre-count | Post-count | % | OR | ATT (SE) |
|-----------------|-----|-----------|------------|-------|-------|--------------|
| Auto theft | | | | | | |
| Treated | 114 | 371 | 316 | -14.8 | 1.26* | -0.36 (0.36) |
| Untreated | 114 | 383 | 410 | +7.0 | | |
| Theft from auto | | | | | | |
| Treated | 112 | 277 | 224 | -19.1 | 1.02 | 0.37 (0.33) |
| Untreated | 112 | 274 | 227 | -17.2 | | |
| Violent crime | | | | | | |
| Treated | 113 | 266 | 313 | +17.7 | 0.82 | 0.69 (0.62) |
| Untreated | 113 | 278 | 269 | -3.2 | | |

Abbreviations: OR = Odds ratio; ATT = Average treatment effect on the treated.

* $p < 0.05$.

Table 3. Sensitivity analysis.

| PSM model | Auto theft | | Theft from auto | | Violent crime | |
|-------------------------------------|--------------|-------|-----------------|------|---------------|------|
| | ATT (SE) | OR | ATT (SE) | OR | ATT (SE) | OR |
| Nearest neighbor, No caliper | -0.36 (0.36) | 1.26* | -0.37 (0.34) | 1.01 | 0.55 (0.31)^ | 0.83 |
| Nearest neighbor, caliper = .01 | -0.35 (0.37) | 1.26* | 0.37 (0.33) | 1.02 | 0.69 (0.62) | 0.82 |
| 2 Nearest neighbors, caliper = .01 | -0.28 (0.31) | 1.22^ | 0.46 (0.29) | 0.97 | 0.34 (0.42) | 0.88 |
| 3 Nearest neighbors, caliper = .01 | -0.35 (0.29) | 1.22^ | 0.32 (0.27) | 0.94 | 0.23 (0.43) | 0.91 |
| Nearest neighbor, caliper = .001 | -0.36 (0.36) | 1.29* | 0.37 (0.33) | 0.99 | 0.49 (0.25)^ | 0.76 |
| Radius, caliper = .01 | -0.31 (0.28) | 1.20^ | 0.19 (0.31) | 1.15 | 0.31 (0.34) | 0.99 |
| Radius, caliper = .001 | -0.34 (0.31) | 1.19^ | 0.16 (0.33) | 1.13 | 0.55 (0.35) | 0.93 |
| Kernel, Gaussian | -0.23 (0.28) | 1.19^ | 0.32 (0.31) | 1.14 | 0.25 (0.31) | 0.98 |
| Kernel, Gaussian, bwidth = .01 | -0.30 (0.27) | 1.20^ | 0.18 (0.31) | 1.15 | 0.28 (0.33) | 0.98 |
| Kernel, Gaussian, bwidth = .001 | -0.37 (0.31) | 1.19^ | 0.18 (0.27) | 1.13 | 0.53 (0.37) | 0.99 |
| Kernel, Epanechnikov | -0.23 (0.28) | 1.19^ | 0.33 (0.32) | 1.14 | 0.25 (0.33) | 0.98 |
| Kernel, Epanechnikov, bwidth = .01 | -0.30 (0.27) | 1.20^ | 0.18 (0.31) | 1.15 | 0.28 (0.34) | 0.99 |
| Kernel, Epanechnikov, bwidth = .001 | -0.37 (0.31) | 1.19^ | 0.18 (0.27) | 1.13 | 0.53 (0.36) | 0.93 |

* $p < 0.05$; ^ $p < 0.10$.

The cumulative findings suggest that auto theft was the only crime category to exhibit any evidence of reduction due to the CCTV system. While the main analysis found a statistically significant reduction via observed OR values, most of the alternate algorithms suggest the reduction only approached statistical significance ($p < 0.10$). However, none of the 13 OR values even approached statistical significance for theft from auto or violent crime. Therefore, while very modest, evidence of reduction is more substantial for auto theft than either theft from auto and violent crime. This largely replicates the findings of Caplan, Kennedy, and Petrossian (2011).

Given the possibility that the CCTV system in Newark reduced auto theft, the analysis concludes with a test of spatial displacement of this crime type. A weighted displacement quotient (WDQ) was calculated for auto theft, via the formula:

$$\text{WDQ} = ([Da/Ca] - [Db/Cb])/([Ra/Ca] - [Rb/Cb])$$

where D, R, and C represent the displacement (i.e., catchment), response (i.e., viewshed), and control (i.e., pseudo viewshed) areas, respectively, and b and a indicate the period before and after the intervention, respectively (Bowers and Johnson 2003). The WDQ was calculated in Ratcliffe and Breen's (2008) Spatial Evaluation of Police Tactics in Context (SEPTIC) tool. 976 auto theft incidents occurred in the catchment area in the pre-installation period compared to 659 in the post-installation period. Accounting for the incident totals in the target and control areas (see Table 2), the WDQ was 4.75, which suggests a diffusion of benefits greater than the reduction within the target area. The Phi coefficient (0.053) confirms that the WDQ is an appropriate measure of spatial changes in crime patterns (see Ratcliffe and Breen 2008).

Discussion and conclusion

Prior research suggests that CCTV works best in preventing motor vehicle crime (Welsh and Farrington 2002, 2009a). The exclusive reduction of auto theft in the current study provides additional support for this observation, though the sensitivity analysis found that most alternate calculations of the OR only approached statistical significance at $p < 0.10$. Therefore, evidence of CCTV effect on auto theft in the current study must be classified as extremely modest. However, given the consistently null effects observed in the theft from auto and violent crime analyses, CCTV seems like a more promising strategy to combat auto theft than these other crime categories in Newark.

In discussing CCTV's effect on auto theft, Caplan, Kennedy, and Petrossian (2011) stated that car thieves faced a greater risk of detection in CCTV areas because different camera viewsheds could readily identify a stolen vehicle as it travels through the city. This is unique to auto theft, as other crime types do not involve offenders traveling with conspicuous evidence of their crime upon get away. Even theft from auto, the other motor vehicle crime included in the analysis, involves the theft of small items that 'are relatively easy to hide...and, thus, make the offender less conspicuous very shortly after committing the crime' (Caplan, Kennedy, and Petrossian 2011, 270). Findings of the current study suggest that such a prevention mechanism may have been maintained when Newark's system expanded to 146-cameras. Agencies wishing to target auto theft seem to benefit much more from CCTV than jurisdictions with alternate crime priorities.

The lack of effect on the other crime categories also reflects the general trend in the literature, as CCTV has not consistently reduced street-level crime in public places. However, examples of successful public systems are not completely absent. In addition, despite the financial commitments associated with CCTV, cost-benefit analyses suggest that achieved crime reductions can offset costs and even yield longer term monetary savings (La Vigne et al. 2011), though CCTV may be more cost beneficial to society on a whole than the criminal justice system or any individual system component, including policing (Piza et al. 2016). Farrington et al. (2007) and Welsh and Farrington (2009a) found that CCTV worked best when integrated alongside other crime control strategies and when camera coverage was high. La Vigne et al. (2011) found police departments that realized crime reductions through CCTV largely incorporated proactive police activities into their operations. This suggests that police should deploy CCTV alongside other evidence-based strategies, rather than as a stand-alone tactic, in order

to achieve reductions of crime outside of motor vehicle crime. Indeed, the NPD recently experimented with such a strategy (Piza et al. 2015). An 11-week randomized controlled trial, which integrated directed police patrol alongside active CCTV monitoring, generated statistically significant reductions of violent crime and social disorder within target areas compared to control areas (the experiment occurred outside of the one-year post-installation period for all cameras and did not influence the results of the current study). Police agencies looking to combat violent and overall street-level crime should make more efforts to incorporate such proactive activities into their CCTV operations rather than deploy CCTV in the stand-alone manner that was the focus of this study (Though, see the quasi-experimental evaluation of Gerell 2016 for an example of a similar intervention generating null effects in Sweden).

The cumulative findings also have implications for criminological theory. CCTV is commonly considered a situational crime prevention strategy that seeks to increase the risk of offending by strengthening formal surveillance and place management (Clarke 1997; Cornish and Clarke 2003). From a rational choice perspective, the suggested prevention mechanism involves an offender recognizing the increased levels of formal surveillance and/or place management and, in consequence, considering the risk of offending to outweigh potential rewards. The largely null effects reported in the current study suggest that this mechanism may not be enacted by the presence of a CCTV camera. An important consideration is the difference between CCTV and other forms of formal surveillance and place management. As argued by Ratcliffe (2006, 8), 'A CCTV system is not a physical barrier. It does not limit access to certain areas, make an object harder to steal, or a person more difficult to assault and rob.' This contrasts with surveillance and place management provided by human agents, who do present such hardships to potential offenders. For example, Ratcliffe et al. (2011) considered foot patrol officers in Philadelphia as a 'certainty-communicating device' alerting potential offenders to the increased certainty of punishment within target areas. In this sense, CCTV may not significantly influence offender decision-making without ensuring the participation of capable human agents who can effectively respond to criminal behavior observed on camera, as observed in prior research (Gill and Loveday 2003; La Vigne et al. 2011; Piza et al. 2015).

Despite these implications, the current study, like most research, suffers from specific limitations that should be mentioned. While PSM can approximate the conditions of a randomized experiment, many conditions must be met for this to occur (Shadish 2013). In the current study, it is certainly possible that NPD officials considered unobserved covariates in target area selection. Therefore, even though covariates were carefully considered, it is possible that the strong ignorability assumption was not met. The reader should also be mindful of limitations inherent in certain PSM covariates. In particular, arrests were used as an indicator of police activity. However, research has shown that particular police strategies (e.g., problem-oriented policing, hot spots policing) are much more effective at generating deterrence than other strategies (e.g., reactive patrol, retroactive investigations) (Skogan and Frydl 2004). Therefore, the context in which arrests occurred may vary greatly in terms of street-level influence, which questions the validity of considering all arrests as homogenous events. Unfortunately, the NPD's arrest data files did not allow the research team to disaggregate arrests by the type of policing strategy. Viewshed creation was conducted at one period in time, under the assumption that camera line-of-sight was consistent throughout the entire one-year study period. Any change in the physical landscape (e.g., the construction or demolition of a city building) may have altered the viewshed. The use of reported Part 1 crime incidents as outcome measures may have raised some issues. Prior research has found that CCTV systems can mask crime reductions (or, generate false crime increases) when CCTV operators observe incidents that may have otherwise gone unobserved (and unreported). However, given the low levels of proactive detections of crime by the NPD's CCTV operators (see Piza, Caplan, and Kennedy 2014b), such a situation seems unlikely in Newark. Nonetheless, the use of outcome measures besides reported crime incidents may have influenced the results. Lastly, the current study was unable to measure any longitudinal deterrence decay effects of CCTV. In October 2012, Hurricane Sandy significantly damaged the CCTV system, making more than half of the cameras inoperable. The NPD did not have an active maintenance contract with the CCTV vendor, so repairs were not immediately made to the system. Cameras were repaired as discretionary funds became available, with the

precise dates of repair unknown to the research team. Given this imprecision, the study was restricted to analyzing the one-year study period.

In conclusion, the main contribution of this paper was the use of PSM to approximate the conditions of a randomized experiment. While ensuring group equivalency maximizes the rigor of quasi-experimentation, the field would greatly benefit from an actual randomized experiment testing CCTV effect. While acknowledging the challenges in conducting a true experiment with CCTV (see the discussion in the *Evaluation Methodology and CCTV Control Area Designation* section) random assignment may be possible in certain cases. In Newark, CCTV installation occurred on four separate dates over a period of three years to accommodate the creation of a wireless network to stream camera footage. Hypothetically, the NPD could have identified priority locations at the onset of the program and randomly selected a subset of locations to receive cameras during installation Phase 1. Other priority sites could have received cameras in following installation phases, after completion of the randomized experiment. Under this strategy, officials can simultaneously generate the most rigorous evidence of program effect and ensure that all priority locations receive CCTV (assuming that results of the experiment support the installation of additional cameras). Researcher–practitioner partnerships that can leverage the support for such an experimental strategy should attempt to do so.

Notes

1. When manually controlled by a user, each camera has the ability to see further than what is visible in panning mode. However, the panning mode was digitized as the viewshed for two reasons. First, all of the cameras are in panning mode more often than they are actively controlled by an operator. Secondly, constructing the viewshed based on a camera's possible view would lead to areas significant distances away from the camera being designated as part of the viewshed. For example, NPD officials once demonstrated that a camera on top of an office building was able to view airline logos on airplanes parked at Newark Liberty International Airport over a mile away. Creating viewsheds based on this capacity would lead to a grand over-estimation of CCTV coverage.
2. The NPD recorded the installation date of 11 cameras as 8 June 2007, coinciding with the official formation of the video surveillance unit. However, according to those directly involved with the camera deployment, installation of these cameras occurred during a 'test phase' spanning several months in 2006 with intermittent monitoring of the cameras beginning as early as February 2007. Two additional cameras were unable to transmit footage to the control room for over a year after their installation, likewise leading to their exclusion.
3. The GIS function placed points where street segments intersected with one another. However, in certain cases, such as a highway overpass that travels over several streets, the segments may not actually intersect in the real world. Such cases were manually identified and deleted from the file.
4. It should be noted that prior research has found that aggregate crime categories often exhibit different spatial patterns than the disaggregate categories they comprise. However, while Andresen and Linning (2012) found that the spatial patterning of aggregate and disaggregate categories significantly differed across macro- (dissemination areas) and meso- (census tracts) units of analysis, spatial patterns were not significantly different in micro units (street segments). Given that the units of analysis used in the current study (CCTV viewsheds) are similar to street segments in terms of size and scope, the use of aggregate crime categories does not threaten the validity of the findings.
5. Arrests represent only one potential police enforcement action. While the inclusion of additional metrics would have provided a more holistic measure, GIS data covering the entirety of the study period was only available for arrests. However, research has shown that arrests are a typical outcome of focused police efforts, making arrests an appropriate proxy measure for police enforcement activity.
6. For the current study, the concentrated disadvantage index included percentage of residents receiving public assistance, percentage of families living below the poverty line, the percentage of female-headed households with children under the age of 18, and the percentage of unemployed residents. While prior measures of social disadvantage have also included percentage of black residents, racial composition was addressed via a separate covariate, as discussed shortly.
7. Racial heterogeneity was calculated via the following formula: $[(\%white, non-Hispanic * \%non-white, non-Hispanic) + (\%black, non-Hispanic * \%non-black, non-Hispanic) + (\%Asian, non-Hispanic * \%non-Asian, non-Hispanic) + (\%Hispanic * \%non-Hispanic)] / 4$.
8. Another option was to use radius matching or kernel matching, where all untreated cases within the caliper (not just the nearest neighbors) are selected for the control group (Dehejia and Wahba 2002). However, for each crime type, the vast majority of untreated cases fell within the 0.01 caliper. Therefore, using these matching algorithms would have resulted in treated cases being compared to over 3,500 untreated cases, which is almost all of the

untreated cases in Newark. It was decided that the nearest neighbor method presented a more realistic comparison by creating final treatment and control groups more comparable in size. Nonetheless, as will be discussed later on, a sensitivity analysis revealed that radius and kernel matching produced similar findings as the nearest neighbor method.

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