Impact Evaluation of the LAPD Community Safety Partnership*

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This version: July 10, 2020

Abstract

In 2011, the Los Angeles Police Department (LAPD), in conjunction with other governmental and nonprofit groups, launched the Community Safety Partnership (CSP) in several communities long impacted by multi-generational gangs, violent crime and a heavy-handed approach to crime suppression. Following a relationship-based policing model, officers were assigned to work collaboratively with community members to reduce crime and build trust. However, evaluating the causal impact of this policy intervention is difficult given the unique nature of the units and time period where CSP was implemented. In this paper, we use a novel data set based on the LAPD's reported crime incidents and calls-for-service data to evaluate the effectiveness of this program via augmented synthetic control models, a cutting-edge method for policy evaluation. We do rigorous testing using numerous falsification analyses, to evaluate the robustness of the results. In the public housing developments where it was first deployed, CSP reduced reported violent crime incidents and shots fired and violent crime calls, starting roughly three years post-implementation. We do not find evidence of crime displacement from CSP regions to neighboring control regions. These results are promising for policy-makers interested in policing reform.

Keywords: gang violence, community policing, time-series cross-sectional data, augmented synthetic control method, policy evaluation

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*We thank Chad Hazlett and the UCLA Causality Reading Group for thoughtful comments and discussion. We thank LAPD Chief of Police Michel Moore, the CSP Research and Evaluation Advisory Committee, and the community residents and institutional partners that make CSP possible. This research was funded by the Balmer Group, The California Endowment, Caruso, Cindy Miscikowski, The Smidt Foundation, the Weingart Foundation, and an anonymous donor. We acknowledge Connie Rice as a co-creator of the CSP public safety model and a visionary regarding relationship-based policing.

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1 Introduction

In 2011, the Los Angeles Police Department (LAPD) partnered with the Housing Authority of the City of Los Angeles (HACLA), and the Urban Peace Institute to launch the Community Safety Partnership (CSP) (Rice and Lee, 2015). CSP was designed to address the high levels of violent crime and corrosive effects of multi-generational violent gangs entrenched in several Los Angeles public housing developments (PHDs). In Jordan Downs and Nickerson Gardens, located in the Watts neighborhood of South Los Angeles, gangs dominated the physical spaces and controlled the activities of daily life. Long-standing rivalries between gangs in the PHDs brought near daily conflict to the communities and allowed violent and property crime to thrive. Residents in PHDs in Los Angeles had largely resigned themselves to a life of insecurity and instability, vulnerabilities reinforced by high levels of concentrated disadvantage.

At its core, CSP was a rethinking of how to approach the problems of violent crime and gangs built around a relationship-based policing model. Two decades of heavy-handed crime suppression had succeeded in cultivating widespread distrust of police, but appeared to do little to blunt the control of gangs. CSP saw arrest-based crime suppression as a last resort and rather sought to have "police officers and residents work in mutually respectful partnership to identify and prevent crime" (LA Office of the Mayor, 2017). Funded by the LAPD and the Housing Authority of the City of Los Angeles (HACLA), CSP recruited and trained a special group of officers for five-year assignments in the PHDs. CSP officers were tasked with "support[ing] community and youth programs, address[ing] quality of life issues and develop[ing] programs to address and reduce violent crimes" (LAPD News Release, 2015). In effect, CSP officers were to become invested members of the community, rather than simply an intervening force that shows up when there are problems.

The communities participating in CSP exhibited reduced violent crime and improved relations with the police almost immediately, a fact that attracted considerable media attention (e.g. Siegler, 2013; Blackstone, 2014; Streeter, 2014). Though encouraging, it is critical to recognize that inferring causality is challenging under the circumstances. CSP was developed at a time when Los Angeles as a whole was experiencing an unprecedented crime decline and implemented in highly targeted communities. Crime peaked in Los Angeles in 1992, a year which saw nearly 1,100 murders citywide. Los Angeles then experienced nearly two decades of falling violent and property crimes, while simultaneously adding a half-million new residents. At its lowest point in 2013, there were 251 homicides citywide, a 335% reduction. Given the overall decrease in crime throughout the city, it is possible that crime would have fallen in CSP areas, even if the program had not existed. Given the significance surrounding policing reform in America, it is important to carefully consider the causal impact of community-policing programs such as the CSP.

To determine whether CSP actually reduced crime, we require knowledge of what crime and disorder *would have been like* in these communities had CSP not been implemented, i.e., the counterfactual. For example, in Jordan Downs and Nickerson Gardens, both PHDs located in South Los Angeles, average violent crime dropped from roughly 34 incidents to roughly 23 per PHD per 6 month period. In order to attribute this whole reduction to CSP, we would have to assume that crime would have remained constant throughout the study time period had CSP not been implemented, an assumption almost certainly violated given trends seen in non-CSP communities. Therefore, we wish to know how much of that reduction is likely due to the impact of CSP versus how much is attributable to the overall reduction of crime in Los Angeles during this period.

One common approach, the difference-in-differences method, assumes that crime reduction trends are similar to other communities in the surrounding area which did not participate in CSP. Average violent crime in census tracts in Los Angeles, excluding those eligible for CSP, saw a reduction from roughly 9 to roughly 8 incidents per census tract per 6 month period, indicating the counterfactual reduction, had CSP not been implemented would have been about 1 violent crime per housing development per 6 month period. Under this strong "parallel trends" assumption—that crime would have had a similar trajectory in the CSP units as in the greater South Los Angeles area—the impact of CSP was a reduction of roughly 10 violent crimes per housing development per 6 month period.

However, the ecological uniqueness of the public housing developments in which CSP was implemented, for example, in terms of housing density, and the wide range of observable and unobservable characteristics that describe these neighborhoods, strains the credibility of the parallel trends assumption. It also does not account for the temporal and spatial richness of the data available for analyzing the impact of CSP. Using the daily crime data for South Los Angeles, an alternative to the difference-in-differences approach is to use a synthetic control method (Abadie, Diamond, and Hainmueller, 2010). In lieu of finding naturally-occurring comparable control units, which are unlikely to exist for PHDs, the synthetic control method (SCM) constructs a "synthetic" control unit by weighting the observed control units to match the observed pre-treatment outcome among the treated units. This combination of control units, the synthetic control, "often does a better job of reproducing the characteristics of the unit or units representing the case of interest than any single comparison unit alone" (Abadie, Diamond, and Hainmueller, 2015, pg. 496). By projecting out this model over the post-treatment period, we can estimate a counterfactual for the treated unit for which we wish to estimate the the treatment effect. Due to concerns about in-exact balance and extrapolation, we apply a derivative of the SCM method, the augmented synthetic control method (ASCM Ben-Michael, Feller, and Rothstein, 2019), to evaluate the impact of the CSP. This method is at the leading edge of the literature, and provides a unique avenue for estimating a the impact of CSP given the challenges for causal identification in this data set. We test observable implications of the identifying assumptions necessary for ASCM by conducting a series of placebo tests, including both spatial and temporal tests, that give us confidence in the credibility of the resulting estimates.

We evaluate the impact of CSP on crime in Nickerson Gardens and Jordan Downs, two public housing developments in South Los Angeles, for the six-year period between 2012-2017 using a novel data set based on the LAPD's reported crime incidents and calls-for-service data. Specifically, we estimate the average number of violent crime incidents and violent crime calls-for-service prevented per six-month period (semester) per public housing development. We find that, on average, CSP prevented 9.21 violent crimes and reduced violent crime calls-for-service by 8.60 per semester per housing development between January 1, 2012 and December 31, 2017. The effects of CSP were not immediate. The first three years following CSP deployment (2012-2014) showed little difference between the treated and synthetic control units. Treatment effects appear in late 2014 and continued through 2017. The city as a whole experienced an increase in violent crime staring in late

2014. This increase is not apparent in Jordan Downs and Nickerson Gardens.

The remainder of this paper proceeds as follows. In Section 2, we describe CSP and how it corrects for some of the well-know deficiencies of so-called "community policing." In Section 3, we describe the data used in model balancing and testing. Section 4 introduces augmented synthetic control methods and the underlying assumptions. Section 5 turns to model evaluation using placebo tests. Section 6 presents results on the impact of CSP in Jordan Downs and Nickerson Gardens PHDs. Section 7 discusses the implications of the results for CSP and relationship-based policing more broadly.

2 Background

The Community Safety Partnership (CSP) was launched in four public housing developments (PHDs) in Los Angeles in late 2011 (Leap, 2020). It is one part of a comprehensive approach to violence reduction in places that have long suffered under the control of powerful street gangs and the corrosive effects of crime suppression tactics, persistent neglect by city officials and concentrated social and economic disadvantage. Gang prevention and intervention efforts, as well as broad community engagement projects, are spearheaded by the Mayor's Office of Gang Reduction and Youth Development (Tremblay et al., 2020). Infrastructure improvement in PHDs is the responsibility of the Housing Authority of the City of Los Angeles (HACLA). CSP is responsible for establishing and sustaining basic security and safety in the targeted communities. The approach of CSP is to concentrate on building long-lasting relationships between police and members of the community and leveraging those relationships for collaborative problem solving (Leap, 2020; Rice and Lee, 2015).

CSP extends a long history of community policing efforts in both the United States and United Kingdom, while seeking to correct for deficiencies in these past efforts. The Anglo-American model of policing places service to the public and crime prevention at its core (Reisig, 2010). Though recognizable as early as the founding of the London Metropolitan Police Service in 1829, these core principles have often been set aside in the drive for modernization and professionalization of police organizations around a paramilitary model. The paramilitary model of policing was envisioned as a means for strictly regulating the legitimate use of force, limiting police discretion, and preventing corruption within the ranks and political influence from without. However, the quest for efficiency also encouraged the impersonal delivery of police services and enforcement tactics decoupled from the social contexts in which crime and disorder occur. In essence, citation, arrest and use of force, even if legitimate in a legal sense and necessary to ensure safety, can appear arbitrary and capricious if police have neither the time, inclination or incentive to understand the context in which such actions are taken. A lack of trust in police is an understandable outcome.

The first serious moves towards community policing in the late 1960s sought to reestablish the importance of context, while continuing along the path of police modernization and professionalization.[1] The President's Commission on Law Enforcement and Administration of Justice laid bare the many failings of policing in mid-Century America (Blumstein, 2018; Law Enforcement, Justice, and Katzenbach, 1967). The Commission's report recommended a series of reforms designed to improve police-community relationships including geographic-based commands to deal with local (especially minority) community needs, citizen-advisory groups to convey what those needs are, and attention to recruiting and promoting minority officers. Team policing initiatives in the early 1970s, though short-lived, were a direct response to these recommendations (Sherman et al., 1973).

In spite of promising early steps, community policing today is perhaps best described as a general orientation adopted by policing organizations. Indeed, the US Department of Justice defines community policing as "a philosophy that promotes organizational strategies, which support the systematic use of partnerships and problem-solving techniques, to proactively address the immediate conditions that give rise to public safety issues such as crime, social disorder, and fear of crime" (DOJ, 2009, pg.1). The most persistent criticism of community policing is that it is too amorphous (Cordner, 1997). A wide range of policing strategies and tactics may qualify as community policing. Thus, evaluation and generalization of individual community policing efforts is challenging.

CSP in Los Angeles was designed with these weaknesses of past community policing efforts in mind. It is a deliberate model of police recruitment, training, deployment, strategic and tactical orientation, and command oversight (Leap, 2020). CSP started with a

[1] Incidents of excessive use of force and corruption continue to arise with unfortunate regularity (Fryer Jr, 2016), indicating that there is still room for significant police reform.

year of planning prior to launch in 2011. A detailed selection process was established to recruit officers with existing orientations towards problem solving. Selected officers underwent training aimed at building understanding of the interrelated cultural, demographic, and economic factors that impact public safety in CSP sites. Training was designed and delivered by the Urban Peace Institute, a community-based civilian organization. Officers were trained on techniques for defusing community-wide dangers without over-relying on traditional suppression tactics such as arrest.

The CSP model also recognized that alternatives to suppression require trust and a network of community relationships that could be called on to solve immediate, local problems. Since building reliable social networks requires both time and stability of effort, CSP established long-term deployments for officers, lasting five-years in each community. The deployments also had a separate command structure allowing for greater autonomy and discretion of officers. Officers were provided unique incentives (promotion and pay) to reward community-engaged behaviors not captured by traditional metrics such as crime and arrest statistics. The ultimate goal of CSP was not only to build trust in policing, but also to provide the basic security and safety necessary for normal social and economic activity. While it is clear that CSP has not been immune to many of the well-known challenges facing community policing, such as ambiguity about how to balance law enforcement against relationship building (Leap, 2020), even partial adherence to the CSP model may be expected to have an impact. Our purpose is to evaluate whether CSP succeeded in providing basic security and safety in the PHDs where it was deployed.

3 Data

Outcomes of interest for evaluating CSP come from two data sources: LAPD reported crime incidents data and LAPD calls-for-service data. Reported crimes typically originate from a call to the police by a member of the public. However, because reported crimes also undergo a verification process, they filter out much of the noise associated with calls (Klinger and Bridges, 1997). Calls-for-service are thus viewed as an aggregate indicator of police demand, fear of crime and victimization (Porter et al., 2019). Reported crimes are viewed primarily as an indicator of victimization. The observed reported crime incidents occurred between January 1, 2006 and December 31, 2017, while observed calls for service incidents occurred between June 5, 2007 to May 31, 2019. We only include observations with valid geospatial coordinates so the location corresponding to treatment status (CSP or non-CSP) can be attributed. We exclude crime incidents and calls recorded with geospatial coordinates corresponding to police stations.[2] The final dataset is restricted to reported incidents and calls-for-service from July 1, 2007 to December 31, 2017, the period of overlap for the two data sources.

Six PHDs received treatment under CSP within LAPD's South Bureau: Jordan Downs, Nickerson Gardens, Imperial Courts, Avalon Gardens, Gonzaque Village, and Harvard Park[3] (listed in order of implementation, earliest to latest). CSP was implemented in the first three developments in November 2011, the next two in July 2016, and the last in October 2017. CSP was also deployed in the Ramona Gardens PHD in East Los Angeles in late 2011. Ramona Gardens was not considered in our analyses. The remainder of this paper is focused on Jordan Downs and Nickerson Gardens, where enough time has passed since CSPs initial implementation to evaluate the impact.[4] Analyses for the latter three developments are not included in the main body of this paper as they are preliminary in nature due to treatment implementations just before or just after our period of study. See Sections of S5 and S6 the Supplementary Materials for these preliminary results.

3.1 Control Units

As PHDs are unique in their community structure, we aim to find naturally-occurring control units that are of similar size and lie within a similar geographic region. We use Census boundaries, and their associated shapefiles, to construct control units for analysis.

To construct control units, we aggregate raw reported crime incident and calls for service

[2]It is common practice to use a police station address when the location of the crime is unknown. This exclusion removes approximately two % of the data.

[3]Harvard Park is the only treated unit which is not a PHD. Instead, Harvard Park is a traditional residential neighborhood. Therefore, CSP may act differently in this region due to confounding factors as compared to CSP PHDs (e.g. strength of community ties, building composition, local housing association/ governance structures).

[4]The results are qualitatively very similar when the Imperial Courts PHD is included, as shown in the Supplemental Material.

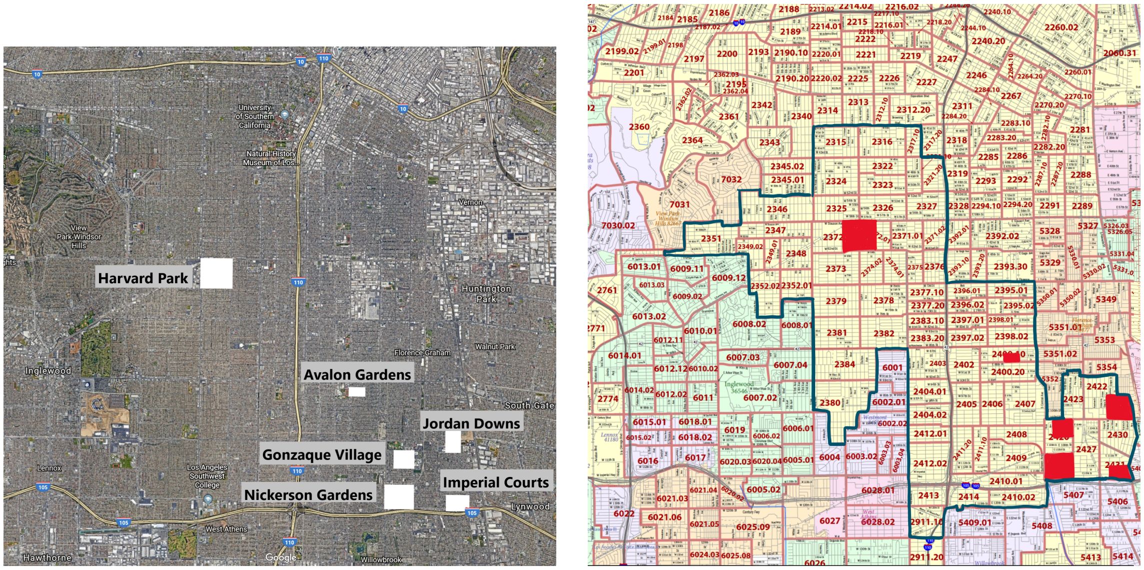

Figure 1: Left: a Google Earth view of the region of interest with treated PHDs labelled. Right: South Bureau in terms of Census Tract boundaries to the southernmost cut-point of Tract 2911.10. Treated public housing developments in red. The region of interest is outlined in blue, this is a subset of South Bureau including most of the LAPD's Southeast and 77th Street Divisions.

observations across space and time using Census geographies. Spatially, events are grouped by Census boundaries by *block group*. Temporally, events are grouped by fiscal *semester* beginning on July 1, 2007.

We focus on *block group*-level control units, rather than *block*-level control units (the smallest Census geography unit) as the former is of a similar size to public housing units. All CSP PHDs are comprised of multiple Census blocks. [5]

To spatially define the control units, we first restrict the data to tracts within the 77th Street and Southeast Divisions of LAPD's South Bureau. Southeast Division is then reduced further, with the southernmost tract in the region being Tract 2911.10. We aim to avoid interpolation bias by restricting the pool of control units to a small geography around

[5]Data from the Census Bureau's Decennial Census, which records various information on demographics, socioeconomic status, etc., are only available for *blocks* and *block groups*. Therefore, defining the region of interest in terms of Census boundaries allows us to use Census population data to consider the effect of CSP in terms of per capita outcomes. These analyses are included in Section S3 of the Supplemental Material.

the treated units, rather than using the whole of the LAPD area(Abadie, Diamond, and Hainmueller, 2015). A Google Earth view of the final region and the resulting breakdown into Census tracts is shown in Figure 1. In total, the pool of control units consists of 234 *block groups* where all units that eventually receive treatment are excluded.

Temporally, *semesters* are defined as events from January 1 to June 30 and from July 1 to December 31 within a given year. *Semester* is chosen as the smallest time-level specification as it is better able to demonstrate seasonal trends than *year* and is less noisy than *quarter*-level data.[6]

3.2 Treated Units

The PHDs that receive CSP define our treated units. While bigger than a census *block*, they do not always consist of an entire *block group* region. Therefore, all *blocks* within a PHD are aggregated and recoded as a single, new *block group* unit per PHD. *Blocks* assigned to a PHD are then removed from the set of control *block groups*. Census *block group* boundaries across the region of study are shown in Figure 1. For figures demonstrating the allocation of Census units to PHD boundaries for each PHD, see Section S1 of the Supplemental Material. In our analyses, Jordan Downs encompasses seven *blocks* and Nickerson Gardens encompasses 13 *blocks*. Jordan Downs and Nickerson Gardens appear of similar geographical size to the average control unit in our study: the average number of *blocks* within a control *block group* in our study is 10.41.

CSP was implemented in Jordan Downs and Nickerson Gardens in November 2011.[7] Therefore, at the semester level, treatment implementation is approximated with the beginning of 2012. Under this approximation, the final pre-treatment semester contains approximately two months of CSP conditions.[8]

[6] We discuss the robustness of our results to the *quarter* time specification in Section S3 of the Supplemental Material. From a simple in-time placebo check, we are less confident in our ability to fit *quarter*-level ASCMs than *semester*-level ASCMs. This may be due to the rare nature of some of our outcomes of interest, which makes the semester aggregation of events less noisy.

[7] CSP was implemented in Imperial Courts in 2011; Avalon Gardens and Gonzaque Village in July 2016, and the Harvard Park in October 2017.

[8] If we reassign the treatment periods to be exact (i.e. recode incidents in November and December 2011 to have occurred during the first semester of 2012), our results are comparable. For *violent crime*,

3.3 Outcomes

In the body of this paper, we are primarily interested in crime and disorder in Jordan Downs and Nickerson Gardens. We focus on *violent crime* incidents and *shots fired and violent crime* calls. The outcomes are defined as follows (LAPD Consolidated Crime Analysis Database (CCAD) code in parentheses): *violent crime* is defined as homicide (110), assault with a deadly weapon/ attempted homicide (230, 231), and robbery (210, 220); *shots fired and violent crime* is defined as shots fired (246), robbery (211), assault with a deadly weapon (245), and murder (187) calls. [9]

4 Methodology

In this section we introduce the augmented synthetic control method (ASCM) (Ben-Michael, Feller, and Rothstein, 2019). We also introduce the required identification assumptions for this method with special attention given to possible spillover effect violations related to the Stable Unit Treatment Value Assumption (SUTVA) under the potential outcomes framework (Rubin, 1974; Neyman, 1923). We do not find evidence of spillover effects.

4.1 Notation and Estimands

Define a treatment indicator, D_i, such that units receiving treatment are denoted by $D_i = 1$ and units who do not receive treatment, i.e. those in control, have $D_i = 0$. To be consistent with the literature, we use notation for a single treated unit. Time periods are denoted by $t \in (1, \ldots, T)$, and treatment is implemented at time T_0. Consider a case with N total units where the first unit, $i = 1$, are treated at time T_0 and the rest, $i = 2 \ldots N$, are never exposed to treatment. Under the potential outcomes framework (Rubin, 1974), define unit

the ATT estimates are -9.21 and -9.15 under approximated treatment assignment and recoded treatment assignment, respectively. For *shots fired and violent crime*, the ATT estimates are -8.60 and -8.60 under approximated treatment assignment and recoded treatment assignment, respectively.

[9] Additional outcomes (reported crime incidents: burglary, total incidents; calls for service: shots fired, quality of life, total calls) were considered and found to have poor synthetic control fit. Results available by request.

i's potential outcomes at time t as $\{Y_{it}(0), Y_{it}(1)\}$ under control and treatment, respectively, where $Y_{it}(1)$ is only defined for $t \geq T_0$. Similarly, the treatment effect for unit i at time t is defined as

$$\tau_{it} = Y_{it}(1) - Y_{it}(0) \tag{1}$$

. Define Y_i as the observed outcome for unit i at time t with treatment status d. The fundamental problem of causal inference is that the potential outcomes for unit i and time t are never jointly observable, making τ_{it} unobservable (Holland, 1986). Yet, this quantity is of primary interest in policy problems such as the CSP evaluation, where we wish to know the impact of CSP for, specifically, the units who received treatment. To evaluate the CSP policy, our primary quantity of interest, then, is the average treatment effect on the treated (ATT), a policy relevant estimand that captures the impact of the CSP for the PHDs that participated in the program. We can also evaluate average treatment effect for the treated units at certain time period, called the ATT_t.

$$ATT_t = E[Y_{it}(1) - Y_{it}(0)|D_i = 1, T = t] \tag{2}$$

$$ATT = E[ATT_t], T_0 < t < T \tag{3}$$

Substantively, the ATT_t (Equation (2)) is the average difference between the crime rates in treated regions post-treatment and what the crime rate would have been in those regions during the post-treatment years had CSP not been implemented for a given time period t for $t > T_0$. Equation (3), which is the average of the ATT_t over all post-treatment time periods, captures the overall impact of CSP.

Under the potential outcomes framework, we require the SUTVA (Rubin, 1974) that there be no interference between units, meaning the treatment status of unit i cannot affect the potential outcomes of unit j, for $j \neq i$. While the credibility of SUTVA for this application is more thoroughly discussed in Section S2.3 of the Supplemental Material, the concern is there may be interference between treated units and their adjacent control units. Crime displays patterns of local contagion (Mohler et al., 2011) and hot spot policing experiments have shown diffusion of benefits over relatively short spatial distances (Bowers et al., 2011). Therefore we evaluate potential crime displacement by estimating the effect of CSP using pseudo-treatment units neighboring the Jordan Downs PHD, and we do not find evidence of crime displacement. Section S2.3.1 of the Supplemental Material describes our procedure and the relevant displacement result.

4.2 Synthetic Control Method (SCM)

We seek to know the impact of the CSP on the units that actually participated in the program, a quantity captured by the ATT. The ATT is composed of an observable quantity, the average crime rate at time t among the treated units, and an unobservable quantity, the average crime rate at time t for treated units *had they not participated in CSP*. The goal of synthetic control methods is to find weights w_i^* for each control unit, $i > 1$, to construct an estimate for the counterfactual (unobserved) quantity $Y_{it}(0)$ for treated units (Abadie, Diamond, and Hainmueller, 2010). This can be used to estimate the treatment effect for treated unit $i = 1$ at time t with:

$$\widehat{ATT}_t = Y_{1t}(1) - \sum_{i>1} w_i^* Y_{it}(0) \tag{4}$$

Each weight w_i^* is determined so that the sum of the weighted pre-treatment control units "balances", i.e. makes summary statistics such as the mean match for, the treated unit and weighted control units across the pre-treatment outcomes for each time period t. Weights are constrained to sum to one. We can test an observable implication of the assumptions by comparing the weighted pre-treatment control outcomes to the observed treated outcomes in the pre-treatment period. In the post-treatment period, we estimate the counterfactual potential outcome for treated units as:

$$\sum_{i\neq 1} w_i Y_{it}(0) \approx Y_{1t}(0) := \widehat{Y_{1t}^{SCM}} \tag{5}$$

where we define $\widehat{Y_{1t}^{SCM}}$ as the synthetic control estimate at each time t. Using SCM, the ATT can be estimated as:

$$\widehat{ATT} = \overline{Y_1} - \overline{\widehat{Y_1^{SCM}}} \tag{6}$$

where the average outcomes are estimated over the post-treatment time period $t > T_0$. SCM is only able to construct a valid synthetic control if the set of treated units is contained within the convex hull defined by the pre-treatment outcomes of the control units. There has to exist a weighted sum of control units that resemble the treated units on pre-treatment outcomes to reduce the potential of large pre-treatment imbalances under the model. As discussed in Section S2.1 of the Supplemental Material, the raw count *violent crime* outcomes do not fit the convex hull criterion while the per capita outcome does meet this constraint. Therefore, the augmented synthetic control model which adjusts for such pre-treatment imbalances is a natural choice for analysis in the case of a likely convex hull violation.

When considering crime and disorder outcomes, SCM approaches have been previously used to investigate the effect of policy interventions (i.e. right-to-carry laws (Donohue, Aneja, and Weber, 2019); permit-to-purchase laws (Rudolph et al., 2015)) and also to investigate causal relationships between crime and ongoing or naturally-occurring phenomena i.e. drought (Goin, Rudolph, and Ahern, 2017); sporting events (Pyun, 2019); organized crime (Pinotti, 2011; Becker and Klößner, 2017). Many of these applications focus on state or city-level effects. Our work on the CSP intervention is more closely related to neighborhood-based crime policy interventions (Saunders et al., 2015; Robbins, Saunders, and Kilmer, 2017; Rydberg et al., 2018).

4.3 Augmented Synthetic Control Method (ASCM)

There is a robust and developing time-series cross-sectional literature that include methods that relax the assumptions required for the SCM (eg: Arkhangelsky et al., 2019; Hazlett and Xu, 2018; Athey et al., 2018; Imai, Kim, and Wang, 2018; Brodersen et al., 2015). We use the augmented synthetic control method (ASCM Ben-Michael, Feller, and Rothstein, 2019), a derivative of the SCM that uses a model-based adjustment to account for bias introduced by inexact balance between the treated and control units and for extrapolation outside the convex hull. As discussed in Ben-Michael, Feller, and Rothstein (2019), the bias between the pre-treatment fit of the treated unit's observed pre-treatment state and the estimated synthetic control is expressed as: $Y_{1t}(0) - \sum_i w_i^* Y_{it}(0)$ for each time t. Because any potential outcome $Y_{it}(0)$ can be decomposed into the sum of an outcome model, m, and an error term, ϵ, then the bias under imbalanced SCM can be re-written as:

$$\text{Bias from SCM model} = [m(X_1) - \sum_{i>1} w_i m(X_i)] + E[\epsilon_1 - \sum_{i>1} w_i \epsilon_i] \tag{7}$$

for covariates X_i and weights w_i^*. This bias is estimated using an outcome model, $\hat{m}$. Therefore, ASCM adds a bias term to the traditional SCM estimator:

$$Y_{1t}^{\widehat{ASCM}} = \sum_i w_i^* Y_{it}(0) + \left(\hat{m}(X_1) - \sum_i w_i^* \hat{m}(X_i)\right) \tag{8}$$

$$Y_{1t}^{\widehat{ASCM}} = Y_{1t}^{\widehat{SCM}} + \left(\hat{m}(X_1) - \sum_i w_i^* \hat{m}(X_i)\right) \tag{9}$$

In our analyses we use the Generalized Synthetic Control Method (GSCM, Xu, 2017) as the outcome model $m(X)$. This model accounts for both unobserved unit and time heterogeneity that is correlated with treatment through the use of unit-specific intercepts interacted with time-varying coefficients, making this a flexible model to estimate the imbalance leftover from regular SCM.

Defining X as the lagged outcome, the SCM weighting criterion is adjusted so that weights on the controls, w_i, are constrained to be non-negative and sum to one:

$$min_w ||X_1 - X_0' w||_2^2 + \zeta \sum_{D_i=0} f(w_i) \tag{10}$$

This re-formulation, Equation 10, allows for imperfect pre-treatment fit between the lagged outcomes while penalizing the dispersion of the weights via some penalizing function f and the hyperparameter, ζ. This dispersion penalty regulates the eventual ASCM weights which allow for extrapolation in the case of imperfect pre-treatment fit.

Standard errors for ASCM estimates are estimated using the jackknife approach, proposed for synthetic control methods in Arkhangelsky et al. (2019), a variation of the classic bootstrap where samples are taken from leave-on-out estimation. These standard errors estimate a range of effect estimates based upon a modified, leave-one-out control group.

SCM assumes the outcome can be re-written as a linear factor model of the pre-treatment outcomes, time-related terms, and error term (Abadie, Diamond, and Hainmueller, 2010). A violation of the linear factor model assumption may lead to large interpolation biases (Abadie, Diamond, and Hainmueller, 2010). Under this assumption, in ASCM, the model estimation error can be broken down into bias from underfitting the model due to imbalance and bias from overfitting the model to noise (Ben-Michael, Feller, and Rothstein, 2019). The augmentation step aims to reduce the bias from imbalanced fit without overfitting. In Section S2.2 of the Supplemental Material we present the ASCM's estimated bias component and discuss the potential bias-reduction from the augmentation step of the ASCM.

5 Model Evaluation

Evaluating the credibility of SCM methods is difficult, particularly in light of the fact that we are estimating the treatment effect for a very small number of units, in this case only two PHDs. Traditionally, the most common way to evaluate the fit of synthetic control methods has been to use placebo tests (Abadie, Diamond, and Hainmueller, 2015), which rely on the idea that we should not find evidence of treatment effects where none should exist, such as before treatment has been implemented. Estimation of nonzero placebo effects would undermine the credibility of the final results. The more evidence that passes the scrutiny of the placebo tests, the more credible the resulting analysis.

These placebo tests, suggested by the literature (Heckman and Hotz, 1989; Abadie, Diamond, and Hainmueller, 2010; Abadie, Diamond, and Hainmueller, 2015), serve as the primary method of evaluating the appropriateness of the method. In particular, we asses the fit of the ASCM for each outcome separately, using the following approaches: (1) estimating placebo (i.e. null) effects for the pre-treatment period to assess the model fit; (2) assess potential confounding events or anticipation effects by running models with pseudo pre-treatment implementation dates; (3) evaluate the range of placebo treatment effects by estimating the placebo effect for each control unit (Section S4 of the Supplemental Material).

Taken holistically, these checks are used to evaluate the credibility and confidence in each outcome model. *Violent crime* incidents and *shots fired and violent crime* calls outcomes had strong results across the suite of evaluation approaches and are discussed in depth.

5.1 Model Specification

Before evaluating the impact of CSP in the post-treatment periods, we assess the ability of the ASCM to balance the trajectory of the pre-treatment outcome for the treated units and the synthetic control. During the pre-treatment period, CSP was not implemented and therefore the ASCM models should not detect a treatment effect for any outcome. Non-zero effects would indicate possible remaining confounding between the treated units and synthetic controls, calling into question causal inferences on that outcome. Any observed imbalance could also indicate the potential scale of bias in the estimated impact of CSP.

To ensure the placebo ASCM models are only evaluated on the pre-treatment period, we split this period into training and testing sets using a 2/3 : 1/3 rule. Under this rule, the placebo treatment implementation dates are assigned at the 2/3 marker of the pre-treatment period. For Jordan Downs and Nickerson Gardens, where treatment was implemented in 2012, the psuedo-implementation date is 2010.5 [10] and therefore three periods (those starting 2010.5, 2011, 2011.5) are considered post-treatment for evaluation of the placebo models.

While the results of this paper focus on the violent crime incidents and calls outcomes for Jordan Downs and Nickerson Gardens, the model specification approach described here severely limits the time frame for these regions to 2007-2011. Besides containing very few temporal units at the semester level, this period is unusual due to the economic strain of the 2008 stock market crash and resulting recession through 2009. Therefore, the placebo tests for other PHDs are included in the Supplementary Material to evaluate our overall ability to find a suitable synthetic control for these outcomes. Increasingly good model fit as the length of the pre-treatment period increases, see results in Sections S5 and S6, bolsters our confidence in the final models for Jordan Downs and Nickerson Gardens which use the full pre-treatment period.

The results for this simple placebo in-time are shown in Table 1. The placebo effect

[10] 0.5 years denotes treatment start at the second semester of the given year

for *violent crime* is insignificant with an estimate [95% confidence interval] of -2.83 [-13.09, 7.43]. For reference, the pre-treatment semester average for this outcome in Jordan Downs and Nickerson Gardens is 34.11 incidents. Therefore the ratio of the placebo estimate to the pre-treatment average is approximately eight %. This suggests confidence in our ability to fit a synthetic control model for this outcome and these PHDs, as the placebo effect is small relative to the scale of the outcome. The point estimates are roughly 30% and 40% of the estimated impact of CSP for violent crimes and calls for service, respectively.

Outcome	Pre-Treatment Average	Placebo Estimate
Violent Crime Calls	34.11	-2.83 [-13.09, 7.43]
Shots Fired and Violent Crime Incidents	43.06	-3.44 [-5.96, -0.92]

Table 1: For Jordan Downs and Nickerson Gardens, we provide the pre-treatment semester average for the treated PHDs and the placebo estimate with 95% confidence interval.

The confidence interval for *shots fired and violent crime* is close to, but does not include, zero. The estimate [95% confidence interval] is -3.44 [-5.96, -0.92]. Relative to the pre-treatment semester average for this outcome in this location, 43.06, the placebo effect is also roughly eight % of the observed pre-treatment average of *shots fired and violent crime* calls. We consider the proportionally small estimated bias a good indicator of our ability to construct an appropriate synthetic control for this outcome.

5.2 Confounding Across Time

To assess potential anticipation effects or confounding events in the pre-treatment period we conduct a placebo check in time. More specifically, potential anticipation effects for each PHD are evaluated by pseudo-"assigning" the treatment start date as two years, one year, and one semester earlier than the actual implementation date. If there are no other factors influencing crime outcomes during the pre-treatment period, the resulting ATT_t estimates using the pseudo-implementation date should follow a similar trend as the true ATT_t estimates.

As demonstrated in Figure 2, count results for Jordan Downs and Nickerson Gardens largely follow a consistent trend: the psuedo-treatment ATT_t (red) follows the true estimated ATT_t (dashed with a shaded 95 % confidence interval using jackknife standard errors). Taking both the model specification and temporal results into consideration, we are most confident in the synthetic control for the count outcomes of *violent crime* and *shots fired and violent crime* for Jordan Downs and Nickerson Gardens.

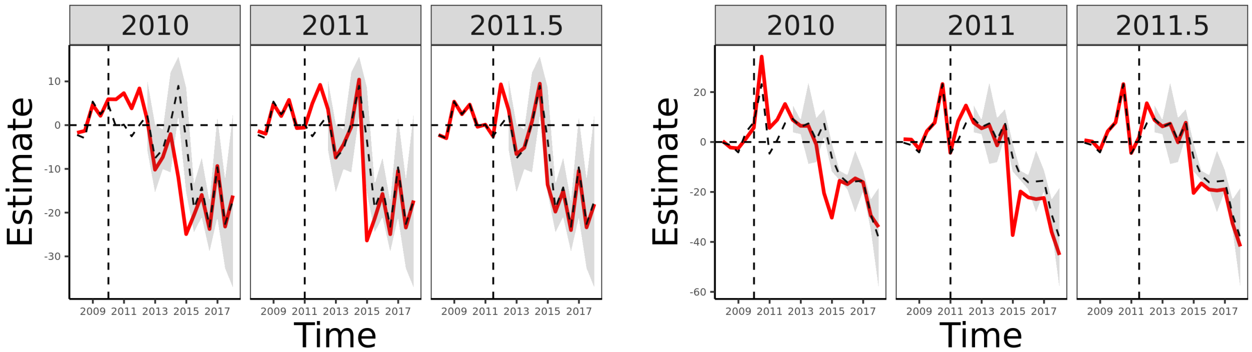

Figure 2: We evaluate robustness across time for Jordan Downs and Nickerson Gardens for violent crime incidents (A) and shots fired and violent crime calls (B) by comparing psuedo-treatment ATT_ts (red) to the estimated ATT_t for the true implementation of 2012 (dashed line with shaded standard errors). Each panel contrasts a psuedo-model to the estimated model. For each panel, the psuedo-implementation date is both marked in the panel title and by a vertical dashed line within the panel. In the absence of confounding events during the pre-treatment period, we would expect to see the pseudo-implementation ATT_t estimates closely follow the ATT_t estimated from the true treatment period.

6 Results

In this section, we evaluate the treatment effect of CSP on violent crime outcomes at Jordan Downs and Nickerson Gardens, the outcomes for which the placebo tests from Section 5 are strongest. Results are robust to including the Imperial Courts PHD, a small and unique PHD which also received a CSP intervention in November 2011. These results can be found in the Supplementary Materials S5.

6.1 CSP Reduced Violent Crime and Disorder in Deployed Areas

As CSP was largely motivated by a desire to reduce violent crime in PHDs, we are substantively interested in the effect of CSP on the reported crime incidents *violent crime* outcome and the calls for service *shots fired and violent crime* outcomes. From the previous section, we find evidence that these results pass numerous placebo tests, and we are confident the fit of the ASCM models for these outcomes of interest. As presented in Table 2, CSP has an estimated impact of 9.21 (95% CI: [7.73, 10.69]) fewer violent crimes per semester per housing development during the post-treatment period and 8.60 (95% CI: [6.52, 10.68]) fewer violent crime calls-for-service per semester per housing development. For Jordan Downs and Nickerson Gardens combined, these correspond to 18.42 fewer violent crimes and 17.2 fewer shots-fired and violent-crime calls-for-service per semester over the entire post-treatment period.

Outcome	Est. (C.I.)
Violent Crime Incidents	-9.21 [-10.69, -7.73]
Shots Fired and Violent Crime Calls	-8.60 [-10.68, -6.52] height

Table 2: Estimated impact of CSP for Jordan Downs and Nickerson Gardens with 95% confidence interval using jackknife standard errors.

Figure 3 plots the trajectories of the reported violent crime incidents in Jordan Downs and Nickerson Gardens and the synthetic control model across the period of study. The lines in this figure allow comparison of the observed averages for the two treated units and the estimated synthetic control for each time point. One final model evaluation measure is evident, in the pre-treatment period, the observed reported crime incidents in the treated units and the estimated synthetic controls are very similar, as evidenced by similar trends in left panel and ATT_t estimates near zero in the right panel of Figure 3.

Figure 3 visualizes the changing *violent crime* trend across time while contextualizing the scale of the effect estimate. Three regions of interest are flagged. During the pre-treatment period (Region 1, in Figure 3), Jordan Downs and Nickerson Gardens (solid line) each experienced on average approximately 35 violent crimes per semester. The synthetic control model (dashed line) closely tracks the mean trend in Jordan Downs and Nickerson

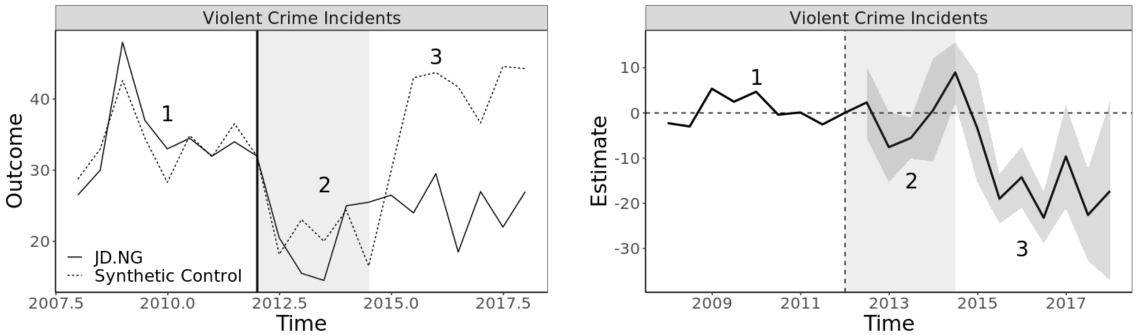

Figure 3: Reported violent crime incidents (A) trajectory for the observed treated units (solid) versus the estimated synthetic control units (dashed) and (B) ATT_t estimate across time with shaded bounds for two jackknife standard errors. 2012 treatment implementation is denoted with vertical line.

Gardens. The solid vertical line shows that CSP is implemented at the end of 2011. In the post-treatment period, following the implementation of CSP, there are two phases of behavior. In the immediate post-treatment period (Region 2), from the beginning of 2012 to the middle of 2014, the mean violent crime trend in Jordan Downs and Nickerson Gardens continues to track or mirror the synthetic control. Beginning in the second-half of 2014 (Region 3), the treatment and synthetic control units diverge. Comparing the minimum and maximum average count outcomes in Figure 3, violent crime in the synthetic control units increases by a factor of 2.8, while in the CSP treatment units, the mean increases from its lowest point by no more than a factor of 2.

Though observed *violent crime* counts in Jordan Downs and Nickerson Gardens drop to their lowest levels in approximately the first two years after implementation, the model estimates there is roughly no effect of CSP during this time frame as the estimated synthetic control reflects this trend. Starting in 2015, the synthetic control begins to separate from the observed crime counts.

The trajectory plot for *shots fired and violent crime*, Figure 4, shows a training period for the first three years post-implementation. From the beginning of 2012 to the middle of 2014 (Region 2), the observed outcome from Jordan Downs and Nickerson Gardens approaches the estimated synthetic control. From the latter half of 2014 through our period of study (Region 3), the two trajectories begin to diverge with the synthetic control exceeding the

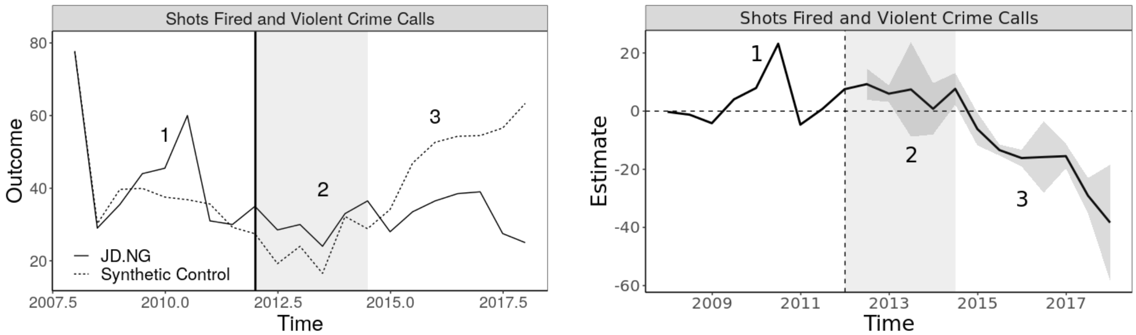

Figure 4: Reported shots fired and violent crime calls (A) trajectory for the observed treated units (solid) versus the estimated synthetic control units (dashed) and (B) ATT_t estimates across time with shaded bounds for two jackknife standard errors. 2012 treatment implementation is denoted with vertical line.

observed outcome from Jordan Downs and Nickerson Gardens. The ATT_t decreases from a slighly positive effect (substantively negative, increased crime as a result of CSP) in Region 2, to an increasingly large negative effect (substantively positive, reduced crime as a result of CSP).

7 Discussion and Conclusions

This paper seeks to quantify the effect of the Community Safety Partnership (CSP) on crime and calls-for-service in Jordan Downs and Nickerson Gardens, two public housing developments in South Los Angeles. Both housing developments suffered from high levels of violent crime and entrenched multi-generational gangs before 2011 treatment implementation. Using the augmented synthetic control method, which allows us to construct counterfactuals for communities in which CSP was implemented; we found CSP led to an average reduction of 9.21 fewer violent crimes and 8.60 fewer violent crime calls-for-service per semester per housing development between January 1, 2012 and December 31, 2017. The estimates reflect reductions of -27.0% and -20.0% in crimes and calls-for-service, respectively, compared to pre-intervention means. Furthermore, preliminary analyses suggest CSP did not simply displace crime from Jordan Downs to neighboring regions, as discussed in Sections S2.3 and S2.3.1.

We report results using raw counts for outcome data. However, the population density of Jordan Downs and Nickerson Gardens is above that of the average control unit. The average population for the control units is 1278 in terms of 2010 Census population counts.[11] For comparison, the combined Jordan Downs and Nickerson Gardens region was reported to have 6719 residents: 2714 residents from Jordan Downs and 4005 residents from Nickerson Gardens. This increased density may contribute to a violation of the convex hull assumption, and place more emphasis on the modeling to alleviate potential bias. To investigate sensitivity to this assumption, we run the analyses for per-capita outcomes defined as crime counts per 1000 residents. The Census population vector is recorded by *block* so we can construct an exact estimate of population, in terms of perfectly matched spatial boundaries, for both the treated and control units. Ultimately, as seen in Section S3 of the Supplementary Materials, the per-capita results are substantively similar to our analyses. In the per-capita analysis, the pre-treatment trends are almost exactly matched, and the convex hull assumption appears sound.

While Jordan Downs and Nickerson Gardens both observed a decrease in violent crime immediately post treatment, this result is insignificant and consistent with the overall crime patterns in Los Angeles at the time. However, after approximately three years of treatment, the ASCMs found a significant decrease in violent crime in these two developments as a result of CSP. During this time, Los Angeles experienced a citywide rise in violent crime that was not reflected in the CSP regions.

These results offer several benchmarks for cities considering developing relationship-based policing programs like CSP. First, relationship-based policing does appear to have a significant impact on violent crime above and beyond other factors. This is important to know given the commitment in time and resources needed to mount a relationship-based policing program. CSP officers are dedicated to small areas, which removes them from general patrol or other investigative duties in the community at large. That this commitment yields a reduction demand for service and crime more than compensates for these costs.

The results also suggest that crime is not simply "pushed around the corner," which

[11] US Census Bureau: 2010 Census, Summary File 1, Table P1: Total Population.

suggests that adjacent areas are not simply forced to absorb the costs of crime displacement. It is possible, though not yet certain that the benefits of CSP diffuse beyond the boundaries of the public housing developments. However, more work will be needed to examine these effects.

While these are all positive indicators, it is also clear that mounting a relationship-based policing program requires patience. Here we see that the main effects are not observed for the first three years after deployment. This lag in impact could be explained in several ways. First, it seems obvious that relationships between police and communities cannot be willed into existence overnight, particularly where those communities have a long list of grievances. It is possible that three years is a minimum amount of time needed before real progress can be made in restoring trust and building effective relationships that counter crime. Second, it is also possible initial effects of CSP were drowned out by noise. Recall that CSP was implemented towards the end of a two-decade long period of crime decline in the city. Perhaps crime was about as low as it could go when CSP started. It was only when crime started to rise in 2014, above some friction point, that the real effects of CSP could be detected. More work will be needed to confirm this hypothesis.

Finally, it is important to recognize that our results here do not speak to the other major component of CSP, which was to restore trust and build lasting relationships Leap, 2020; Rice and Lee, 2015. Clearly ensuring the safety and security of community members is a necessary component of such a process, but there is more more involved than simply low crime numbers. Future work will need to integrate evidence from the qualitative impact of CSP on peoples lives as the counterpart to this quantitative story.

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Supplementary Material
Impact Evaluation of the LAPD Community Safety Partnership

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July 10, 2020

1 Census Units to PHD allocation

In allocating Census tracts, block groups, and blocks to PHD units, there are cases where Census boundaries do not exactly reflect natural PHD boundaries. Figure 1 shows the block allocations for each treated unit, regions that do not contain public housing are highlighted in red. As shown in the figure, we err on the side of over-estimation of the region of interest for conservative effect estimates. Of note for our main analyses, Nickerson Gardens (Tract 2426, top middle) is a total of 13 blocks where three of those contain some control regions. Jordan Downs (Tract 2421, top right) includes the entire tract, seven blocks, where three blocks contain some control regions. The large untreated portion of Jordan Downs in Block 1003 is comprised of a high school, charter school, and several companies.

2 Assumption Evaluation

In this section, we more thoroughly evaluate the identifying assumptions required under this framework and 1) demonstrate the need to use a synthetic control method that accounts for the likely violations of the convex hull assumption, 2) discuss the linear factor model assumption and the potential for bias under the ASCM framework, and 3) fail to find evidence of displacement by failing to find evidence that SUTVA does not hold.

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Figure 1: This paper uses block allocations for treated units as outlined in blue. The red highlighted regions do not contain public housing but are included under these boundary definitions. By including the control regions, we may slightly over-estimate the overall effect of CSP. Top, left to right: Imperial Courts, Nickerson Gardens, Jordan Downs. Bottom, left to right: Avalon Gardens, Gonzaque Village, Harvard Park.

2.1 Assumptions: Convex Hull

Because the synthetic control is usually constructed using a convex combination of the control units, SCM is only able to construct a valid synthetic control if the set of treated units is contained within the convex hull defined by the pre-treatment outcomes of the control units. There has to exist a weighted sum of control units that resemble the treated units on pre-treatment outcomes. If the treated units are too different from the controls, then SCM will not be able to find a set of weights that satisfies the weighting constraints and achieves balance between the pre-treatment outcomes for the treated units and the synthetic control. As shown in Figure 2, the raw count data does not fit the convex hull assumption, as PHDs tend to experience a higher volume of crime than our control units, likely partially due to the higher population density of PHDs. The treated outcomes tend to exceed the control outcomes across all time periods and all outcomes at the chosen *block group* unit specification. As a consequence, the traditional synthetic control estimator would be a biased estimator for our data. Crime rate outcomes calculated per capita do lie within the convex hull of pre-treatment control outcomes. For this reason, we check per capita results in Section 3 of the Supplementary Materials and show the results are robust to this alternative specification.

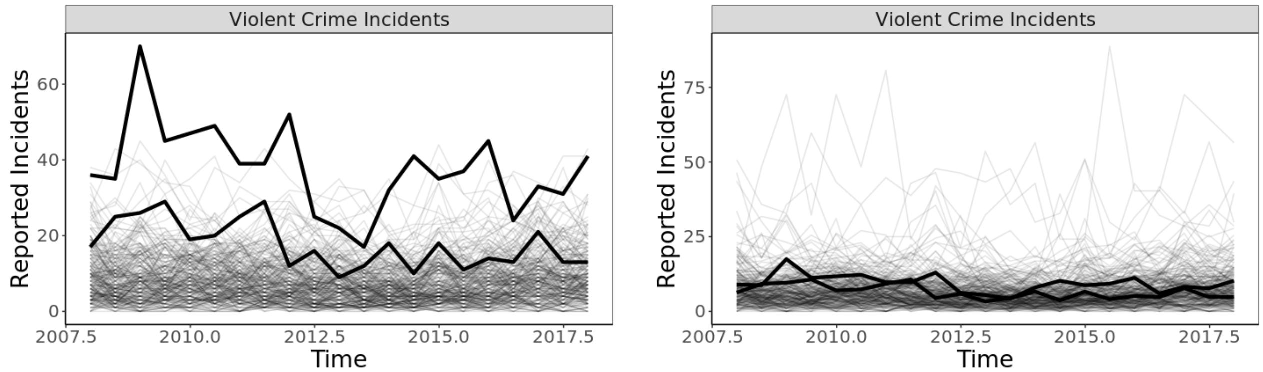

Figure 2: Violent crime outcomes for the treated (bolded) versus control units for raw counts (left) vs per capita counts (right). The violent crime count outcomes show a convex hull violation that is resolved when considering the rate of crime per 1000 persons.

2.2 Assumptions: Linear Factor Model

SCM assumes the outcome can be re-written as a linear factor model of the pre-treatment outcomes, time-related terms, and error term (Abadie, Diamond, and Hainmueller, 2010). A violation of the linear factor model assumption may lead to large interpolation biases (Abadie, Diamond, and Hainmueller, 2010). Under this assumption, in ASCM, the model estimation error can be broken down into bias from underfitting the model due to imbalance and bias from overfitting the model to noise (Ben-Michael, Feller, and Rothstein, 2019). Ideally, the augmentation step would reduce the bias from imbalanced fit without overfitting.

Table 1 presents the estimated bias component to determine the possible bias-reduction from the modeling step in ASCM. First, we provide the average amount that ASCM's augmentation (bias-correction) step changes the original SCM estimate (i.e. estimated bias). To grasp the scale of the bias relative to the final effect estimates, we provide the ratio of the average bias to the final point estimate, denoted as a %age, in parentheses. Assuming the ASCM augmentation is improving model fit as opposed to fitting to noise, the imbalances from traditional SCM are substantial relative to the size of the estimated effects.

	Violent Crime Incidents	Shots Fired and Violent Crime Calls
JD.NG	1.54 (16.7 %)	3.80 (44.2 %)

Table 1: Estimated average imbalance between the SCM and ASCM models for the violent crime and shots fired and violent crime outcomes, from left to right. For reference, the ratio of the imbalance to the ASCM effect estimate is provided as a %age in parentheses.

2.3 Assumption: Stable Unit Treatment Value Assumption (SUTVA)

Under the potential outcomes framework, we require the SUTVA (Rubin, 1974) that there be no interference between units, meaning the treatment status of unit i cannot affect the potential outcomes of unit j, for $j \neq i$. In the present case, no interference between treated units and non-adjacent control units is reasonable to assume Ridgeway et al., 2019. Contagious spread of the benefits of deterrence, or of crime events (e.g., via displacement) are thought to be very local processes Weisburd et al., 2006; Loeffler and Flaxman, 2017. We cannot exclude, however, interference between treated units, or between treated units and their adjacent controls. First, despite the spatial separation of the PHDs, CSP deployments in Nickerson Gardens and Jordan Downs shared a common LAPD command structure. Officers and staff could have discussed operations across PHDs, causing joint treatment effects across both units. We must assume this effect is negligible.

Second, and of primary concern, treated units may interfere with neighboring, non-CSP regions. Crime displays patterns of local contagion (Mohler et al., 2011) and hot spot policing experiments have shown diffusion of benefits over relatively short spatial distances (Bowers et al., 2011). Therefore, we evaluate potential crime displacement by estimating the effect of CSP on control units neighboring the Jordan Downs PHD.

These displacement analyses are restricted by the available data. First, some neighbors of Jordan Downs and Nickerson Gardens are under LA Sheriff's Department jurisdiction. Our analyses are limited to only those spillover regions within LAPD jurisdiction. Second, Nickerson Gardens is a short distance from Gonzaque Village and therefore the two PHDs share potential spillover regions. Due to these shared potential spillover regions, we would not be able to attribute any observed effect to Nickerson Gardens as opposed to Gonzaque Village. To address this, the investigation is solely focused on Jordan Downs and should be considered exploratory in regards to the overall displacement effect of CSP.

To test displacement from Jordan Downs, we consider neighboring regions of Jordan Downs that are within two distance thresholds, approximately 890 ft (270 m) and approximately 1,640 ft (500 m), from the borders of the PHD. For each distance, we redefine neighborhoods within the threshold as a new, psuedo-treated *block group* unit. True treated regions are removed from consideration as either treated or control units. For a map of the Jordan Downs displacement regions, see Figure 3. We then estimate the treatment effect of these pseudo-treatment regions, which neighbor the treated PHD, but were not, themselves, directly treated. Evidence of an effect in these buffer regions would indicate possible spillover effects of the CSP.

2.3.1 CSP Does Not Displace Crime to Neighboring Regions

In terms of displacement, we are interested in evaluating whether the observed reduction of violent crime and disorder in Jordan Downs and Nickerson Gardens is simply transferred to neighboring regions with a less intense police presence.

Figures 4 and 5 provide the estimated crime and calls trajectories for the 270 m and 500 m distance thresholds, respectively. Using the available LAPD data, we do not find evidence of crime displacement from Jordan Downs to neighboring regions within 270 and 500 m. Preliminary evidence suggests that crime incidents and calls-for-service declined slightly, compared to synthetic controls, in the areas immediately surrounding Jordan Downs. There were 4.27 fewer violent crimes and 2.28 fewer violent crime calls-for-service in the displacement region 890 feet from Jordan Downs. The evidence is mixed at 1,640 feet from Jordan

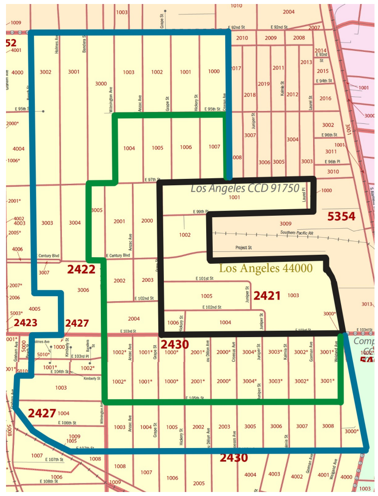

Figure 3: Displacement regions for Jordan Downs (black) are considered for Census regions within approximately 270 m (green) and approximately 500 m (blue). The orange and green shaded regions to the right of Jordan Downs are not within LAPD jurisdiction.

Outcome Type	Displacement Distance	VC	SF-VC
Raw	270 m from JD	-4.27 (-5.19, -3.35)	-2.28 (-4.6, -0.04)
Per Capita*	270 m from JD	-2.65 (-4.01, -1.29)	9.23 (-24.77, 43.27)
Raw	500 m from JD	-4.25 (-5.73, -2.77)	26.79 (NA)
Per Capita*	500 m from JD	-0.95 (-1.77, -0.13)	-1.92 (-36.08, 32.34)

Table 2: Estimated ATT for regions within each distance threshold (270, 500 m) from Jordan Downs. Jackknife standard errors are in parentheses. If there are too few non-zero weighted units to estimate the standard error, the standard error is denoted as NA. To compare the per capita results with the count results, we approximate the count estimates with row "Per Capita*" where we multiply the crime per capita (by 1000 residents) by 2.714.

Downs. There were 4.25 fewer violent crimes, but an estimated 26.79 more violent crime calls-for-service in the 1,640-foot buffer region around Jordan Downs. However, the latter figure is likely the result of poor model fit. Overall, we conclude that there is no evidence for crime displacement and tentative evidence for a small diffusion of benefits to surrounding areas.

Instead of finding practically negative effects of crime displacement (increase in crime in neighboring areas), these results could suggest no effect of CSP in neighboring regions, a good indication for the SUTVA interference assumption. ATT estimates for the effect of CSP in these regions are provided in Table 2.

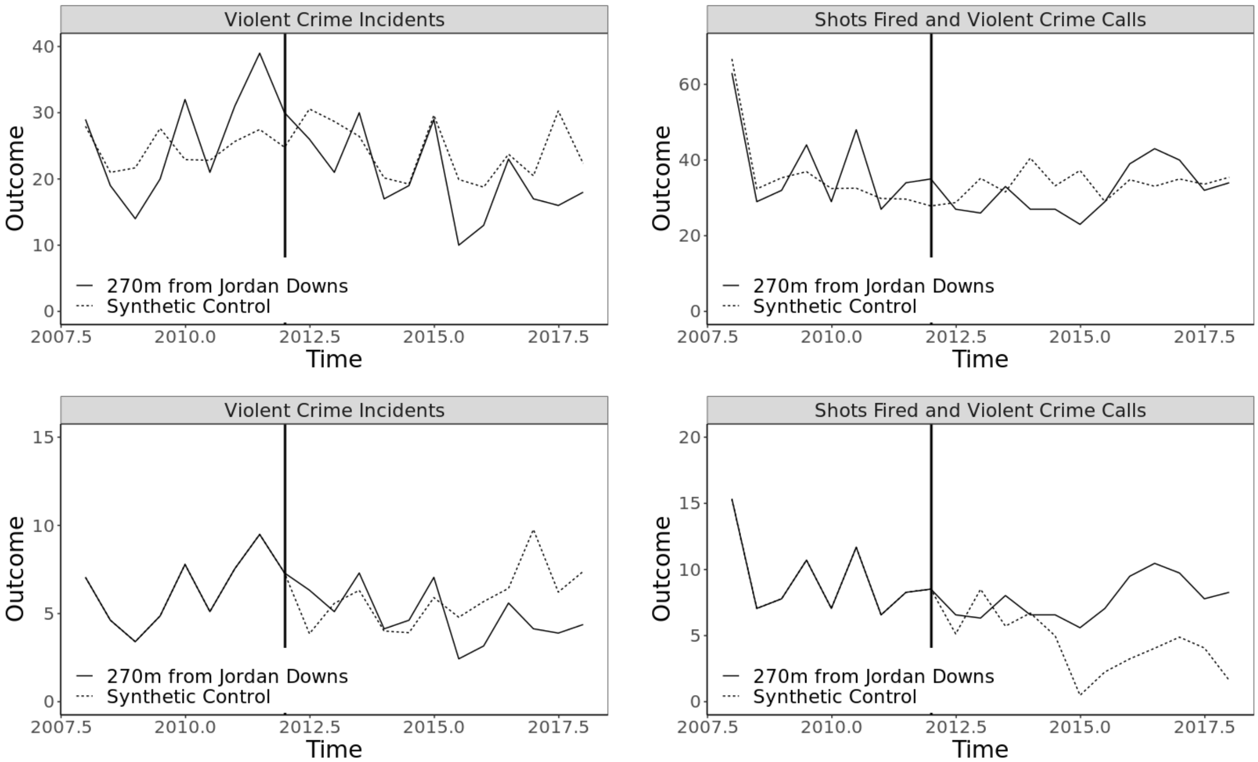

Figure 4: Crime outcomes trajectories for Jordan Downs (solid) versus the estimated synthetic control (dashed) do not indicate spillover effects between Jordan Downs and adjacent controls within the 270 m distance threshold. Top: count outcomes, bottom: per capita outcomes.

3 Robustness to Alternative Specifications

In this section, we evaluate the robustness of our results to per capita outcomes (per 1000 persons in 2010 population counts) and the *quarter* time specification. This approach is compared to the raw count outcomes and *semester* time specification chosen in the main paper body. For ease of comparison, we adjust the *quarter*-level to the *semester* scale and the per capita by 1000 persons to the raw population. These adjustments are denoted with the "*" in Table 3 and the following discussion.

As shown in Table 3, the results from these two robustness checks are largely consistent with our original estimates. The *violent crime* incidents raw count estimates for semester and quarter* roughly indicate a reduction in violent crime between -11 to -8 units from the 95 % confidence intervals. Conversely, the quarter* confidence interval for *shots fired and violent crime* calls includes zero. Placebo checks for *shots fired and violent crime* calls indicate the synthetic control fit may be substandard for the noisier quarter specification.

The per capita estimates indicate a substantively positive effect, a reduction in crime, in these outcomes as a result of CSP. The scale of the effects vary. For *violent crime* incidents, the per capita* quarter* analyses indicate a stronger effect of -13.44 with a wider confidence interval while the per capita* semester indicates a lesser effect of -4.23 with a

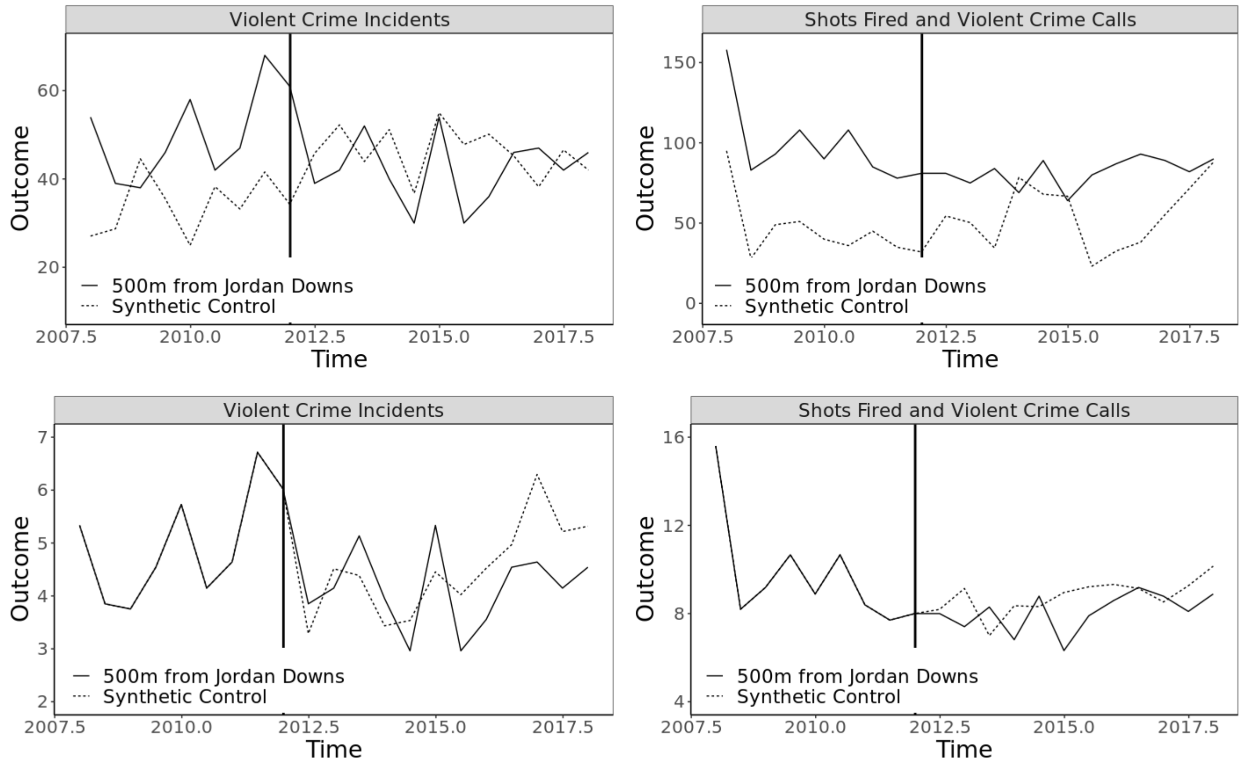

Figure 5: Crime outcomes trajectories for Jordan Downs (solid) versus the estimated synthetic control (dashed) do not indicate spillover effects between Jordan Downs and adjacent controls within the 500 m distance threshold. Top: count outcomes, bottom: per capita outcomes.

confidence interval containing zero. For *shots fired and violent crime* calls, both estimates are negative, of a similar scale to the original estimate of -8.60, and the confidence intervals do not contain zero.

4 Spatial Analyses

As introduced in (Abadie, Diamond, and Hainmueller, 2010) and (Abadie, Diamond, and Hainmueller, 2015), constructing a set of placebo effects across space can also be informative in contextualizing the results. For each control unit, we assign treatment to the control and shift the original treated unit to the donor pool. We then estimate the effect of CSP on the psuedo-treated control unit during the period of study. The control units were never exposed to CSP treatment during this time period, and as such the ASCM models should not detect an effect of CSP.

By estimating placebo effects for the control units, we construct a distribution of placebo effects with which to compare the observed treatment effect and construct a p-value. If the effect of CSP on the treated units is outside of the distribution of placebo effects, we have more confidence in the observed estimates of treatment effect.

Outcome Type	Time Spec.	VC	Shots.VC
Raw	Semester	-9.21 (-10.69, -7.73)	-8.60 (-10.68, -6.52)
Raw	Quarter*	-9.84 (-11.16, -8.52)	11.2 (-8.96, 31.36)
Per Capita*	Quarter*	-13.44 (-27.42, -0.54)	-10.62 (-15.72, -5.52)
Per Capita*	Semester	-4.23 (-16.45, 7.99)	-12.97 (-18.75, -7.19)

Table 3: Count and per capita ATT estimates for Jordan Downs and Nickerson Gardens with 95 % confidence intervals using jackknife standard error in parentheses. To compare the per capita results with the count results, we approximate the count estimates with row "Per Capita*" where we multiply the crime per capita (by 1000 residents) by 6.719. We adjust the Quarter results to the Semester level with "Quarter*" where Quarter estimates are multiplied by two.

In calculating the p-value, the root mean squared prediction error (RMSPE) is a commonly used test statistic, loosely defined as the ratio of the average psuedo-treated control fit over the post-treatment period to the average psuedo-treated control fit during the pre-treatment period:

$$RMSPE_j = \frac{\sum_{t=T_0+1}^{T}(Y_{jt} - \hat{Y_{jt}}(0))^2/(T-T_0)}{\sum_{t=1}^{T_0}(Y_{jt} - \hat{Y_{jt}}(0))^2/T_0} \tag{1}$$

As noted in (Abadie, Diamond, and Hainmueller, 2010) and (Firpo and Possebom, 2018), models with "poor" pre-treatment fit as compared to that of the treated unit should be removed from the analyses and p-value calculation. A poorly fitted model indicates an inability to construct an appropriate synthetic control with the available data, not insight into the relative significance or rarity of the estimated effect of the treated unit. Therefore, after calculating the RMSPE and before calculating the p-value, psuedo-treated control models with "poor" pre-treatment fit are removed.

For our data, We define "poor" fit as an RMSPE that is five times greater than the observed RMSPE for the treated. [1] The p-value is then the proportion of control models with higher RMSPEs than the treated model using this reduced set of control models. This approach is akin to testing a null hypothesis of no effect on the outcome where the p-value is calculated by giving equal weight to the included control models ((Firpo and Possebom, 2018)). This assumption may not be reasonable given the uniqueness of the PHD units as compared to the control units.

The results of the spatial robustness check are contained in Figure 6 and Table 4. From the figure, the effect appears significant. The ATT of Jordan Downs and Nickerson Gardens exceeds many of the control ATTs across time. The resulting p-values for the reduced sets are .33 $\frac{54}{164}$ and .68 $\frac{67}{98}$ for *violent crime* and *shots fired and violent crime* outcomes, respectively, under adjusted control group. For comparison, the p-values constructed from the full set of controls for the *violent crime* and *shots fired and violent crime* outcomes

[1] Regardless of the specification, the p-values derived from the ratios are not significant. We provide the unadjusted and adjusted visualizations and p-values to demonstrate this.

were .54 $\frac{126}{234}$ and .87 $\frac{203}{234}$, respectively. The discrepancy between the figure and table may be due to the convex hull violation. The scale of the outcome for the treated units are more extreme than the controls, and therefore the scale of the ATT effect is larger than the ratio of the post-pre effect. We fail to reject null hypotheses of no effect for both outcomes.

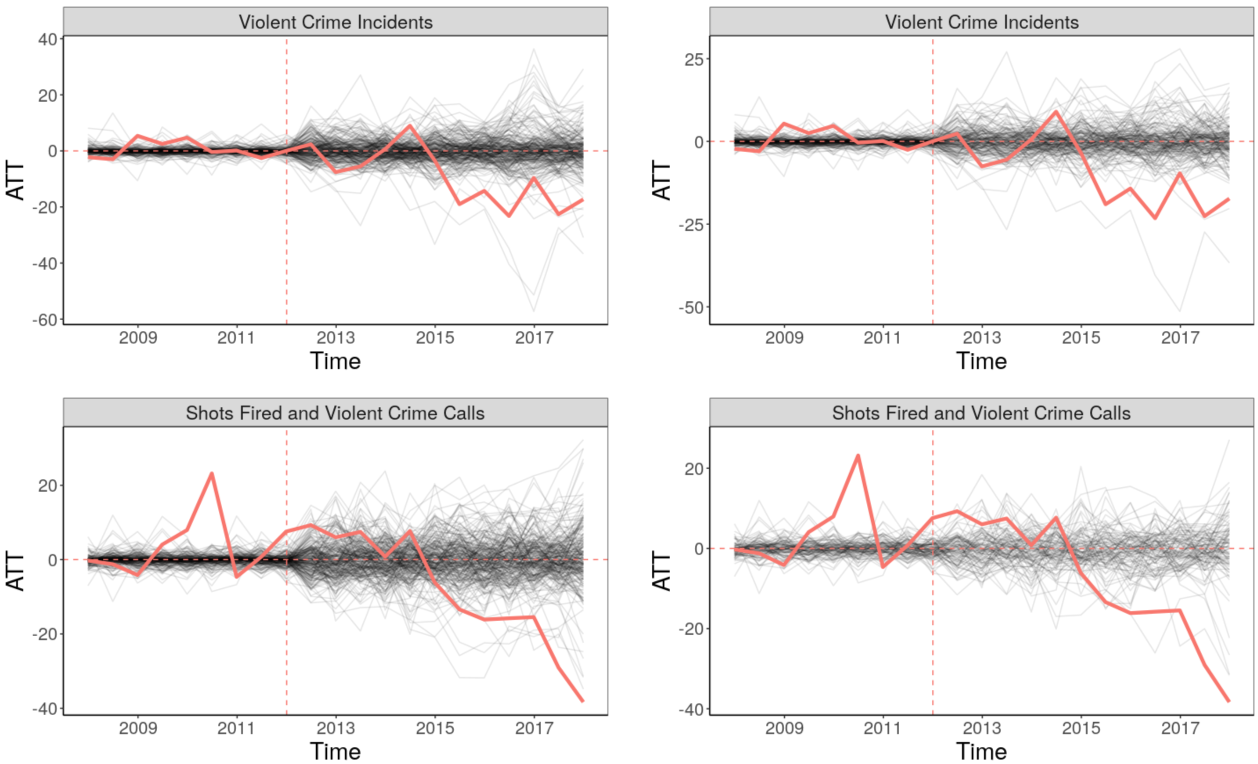

Figure 6: Spatial placebos for Jordan Downs and Nickerson Gardens for each count outcome. The comparative scale of the ATT effect appears significant, while the ratio measure of the post-pre effect is insignificant. Top to bottom: violent crime, shots fired and violent crime. Left to right: unreduced control group and reduced control group.

Control Group	VC	Shots.VC
Unreduced	.54 $(\frac{126}{234})$	.87 $(\frac{203}{234})$
Reduced	.33 $(\frac{54}{164})$	.68 $(\frac{67}{98})$

Table 4: The p-values, the proportion of control models with higher RMSPEs than the treated model using the reduced set of control models, are provided for the *violent crime* and *shots fired and violent crime* outcomes for Jordan Downs and Nickerson Gardens.

5 Jordan Downs, Nickerson Gardens, and Imperial Courts

In this section, results for the alternative PHD specification of Jordan Downs, Nickerson Gardens, and Imperial Courts are provided. These PHDs were all implemented in late 2011, treatment start is approximated with the start of 2012.

5.1 Model Evaluation

As shown in Table 5, the *violent crime* outcome placebo estimate is insignificant with an ATT of -3.42 and a 95 % confidence interval containing zero. The *shots fired and violent crime* estimate is of a similar scale with a narrower confidence interval that does not contain zero. FOr both outcomes, the estimated imbalance is roughly 10 % of the corresponding pre-treatment semester average and 40 % of the corresponding final estimate.

Outcome	Pre-T Average	Estimate (Conf.Int.)
Violent Crime Incidents	28.30	-3.42 (-11.02, 4.18)
Shots Fired and Violent Crime Calls	38.33	-3.72 (-6.02, -1.42)

Table 5: For Jordan Downs, Nickerson Gardens, and Imperial Courts, we provide the pre-treatment semester average for the treated PHDs, the "Pre-T Average," and the estimate with 95 % confidence interval using jackknife standard errors.

5.2 Temporal

The Jordan Downs, Nickerson Gardens, and Imperial Courts temporal analyses presented in Figure 7 do not indicate strong confounding events in the pre-treatment period. The psuedo-treatment ATT_t estimates closely follow the actual ATT_t estimate for both outcomes.

5.3 Distribution of Placebo Effects Across Space

Results for the spatial robustness check for Jordan Downs, Nickerson Gardens and Imperial Courts are contained in Figure 8 and Table 6. From the figure, the effects appear significant relative to the scale of the psuedo effects. Conversely, the p-values are insignificant for the reduced sets with values of .21 $\frac{41}{192}$ and .60 $\frac{75}{126}$ for *violent crime* and *shots fired and violent crime*, respectively. Therefore, we fail to reject null hypotheses of no effect for both outcomes.

5.4 Results

The final estimates for Jordan Downs, Nickerson Gardens, and Imperial Courts appear similar to the final estimates for Jordan Downs and Nickerson Gardens. To compare against the original estimates of -9.21 and -8.60 for *violent crime* and *shots fired and violent crime*,

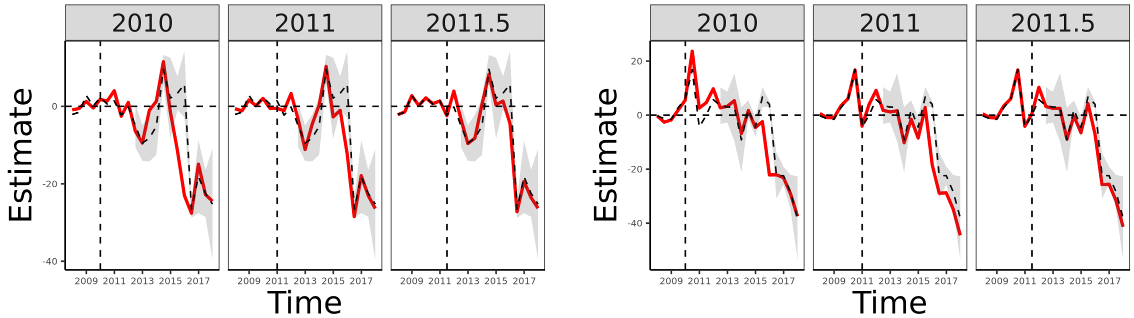

Figure 7: We evaluate robustness across time for Jordan Downs, Nickerson Gardens, and Imperial Courts by comparing psuedo-treatment ATTs (red) to the estimated ATT_t for the true implementation of 2012 (dashed line with shaded standard errors). Each panel contrasts a psuedo-model to the estimated model. For each panel, the psuedo-implementation date is both marked in the panel title and by a vertical dashed line within the panel. In the absence of confounding events during the pre-treatment period, we would expect to see the pseudo-implementation ATT_ts closely follow the ATT_t estimated from the true treatment period.Outcomes, from left to right: violent crime and shots fired and violent crime.

Control Group	VC	Shots.VC
Unreduced	0.35 $(\frac{83}{234})$	0.78 $(\frac{183}{234})$
Reduced	0.21 $(\frac{41}{192})$	0.60 $(\frac{75}{126})$

Table 6: The p-values, the proportion of control models with higher RMSPEs than the treated model using the reduced set of control models, are provided for the *violent crime* and *shots fired and violent crime* outcomes for Jordan Downs and Nickerson Gardens.

the updated estimates for the three PHDs are -8.29 and -8.54 for the two outcomes, respectively (Table 5.4). The ATT_t estimates (Figure 9) also follow a similar trend as compared to the Jordan Downs and Nickerson Gardens estimates.

Outcome Type	PHD	VC	Shots.VC
Raw	JD.NG.IC	-8.29 (-9.27, -7.31)	-8.54 (-10.42, -6.66)
Per Capita	JD.NG.IC	-2.25 (-4.19, -0.31)	-2.42 (-2.78, -2.06)

6 Avalon Gardens and Gonzaque Village

In this section, we present placebo analyses for Avalon Gardens and Gonzaque Village. CSP was implemented in these PHDs in mid-2016, therefore the post-treatment period consists of three periods using the available data. Final results for these analyses are preliminary in nature and not formally presented in this document.

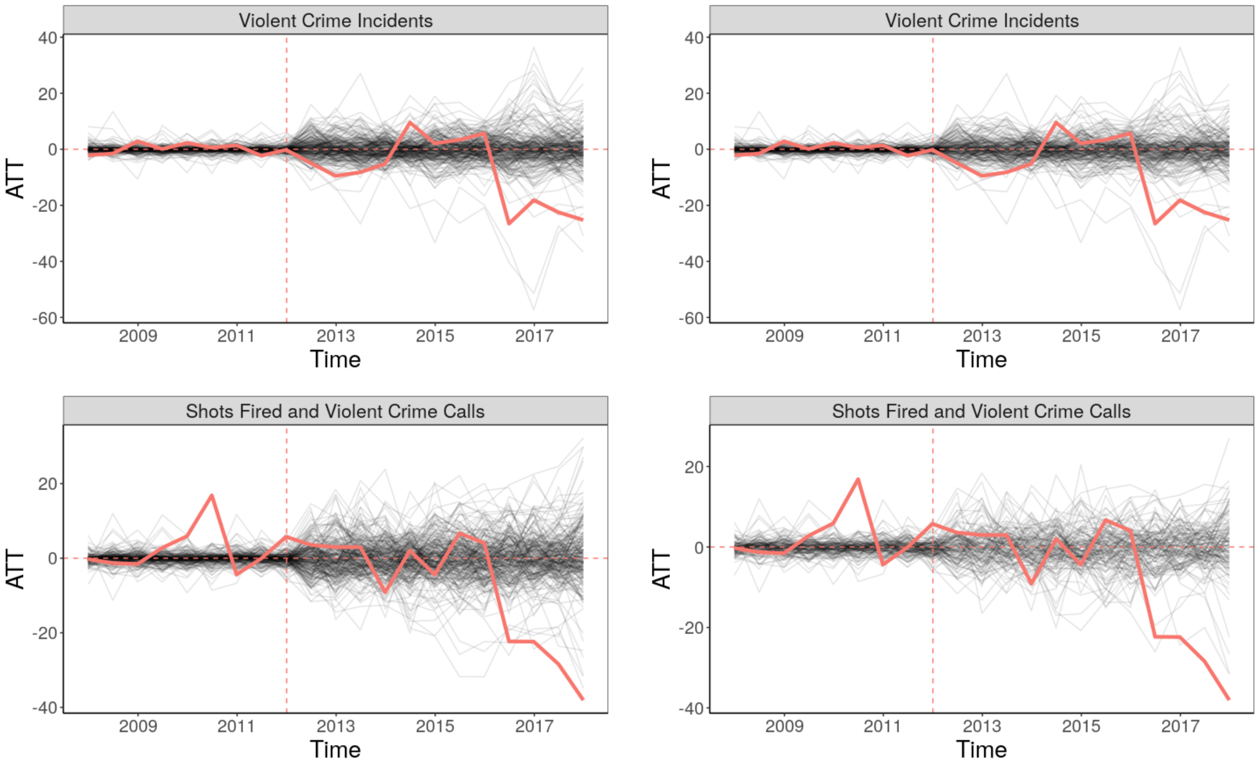

Figure 8: Spatial placebos for Jordan Downs, Nickerson Gardens, and Imperial Courts for each count outcome. The comparative scale of the ATT effect appears significant, while the ratio measure of the post-pre effect is insignificant. Top to bottom: violent crime, shots fired and violent crime. Left to right: unreduced control group and reduced control group.

6.1 Model Evaluation

Semester averages for *violent crime* and *shots fired and violent crime* in the pre-treatment period for Avalon Gardens and Gonzaque Village are substantially lower than those of the previous PHDs (Table 7). Therefore, the model evaluation placebo estimate of -3.27 for *violent crime* indicates poor balance for these PHDs. The *shots fired and violent crime* placebo model appears well-balanced with a 95 % confidence interval containing zero.

Outcome	Pre-T Average	Estimate (Conf. Int.)
Violent Crime Incidents	5.50	-3.27 (-13.27, -6.73)
Shots Fired and Violent Crime Calls	9.83	-0.44 (-1.68, 0.80)

Table 7: For Avalon Gardens and Gonzaque Village, we provide the pre-treatment semester average for the treated PHDs, the "Pre-T Average," and the estimate with 95 % confidence interval using jackknife standard errors.

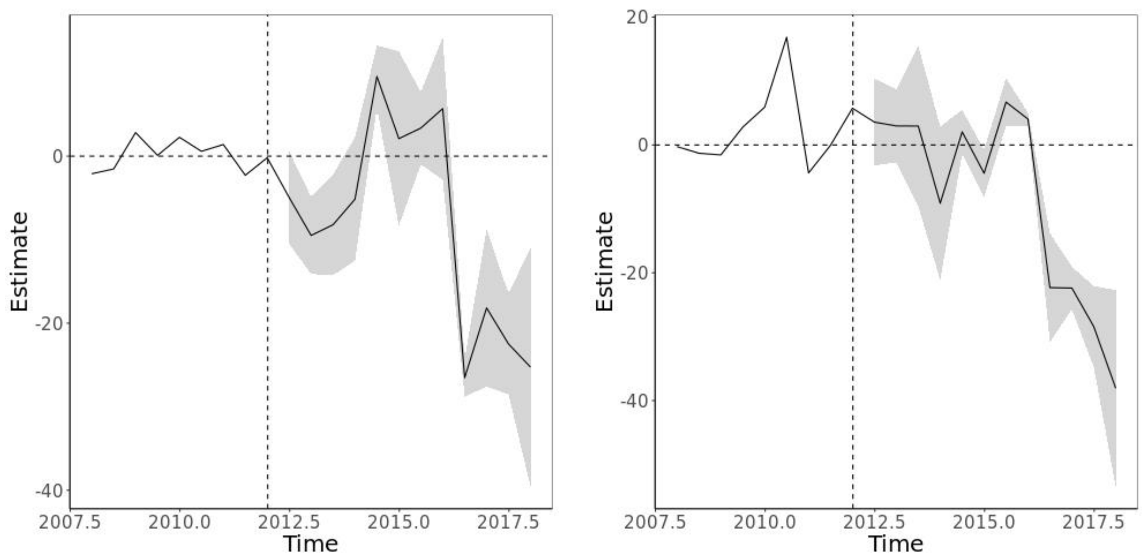

Figure 9: Jordan Downs, Nickerson Gardens, and Imperial Courts reported violent crime incidents ATT_t estimate across time with shaded bounds for two jackknife standard errors. 2012 treatment implementation is denoted with vertical line. Outcomes, from left to right: violent crime and shots fired and violent crime.

6.2 Temporal

The Avalon Gardens and Gonzaque Village temporal placebos closely follow the final ATT_t in the pre-treatment period (Figure 10). The estimated pre-treatment ATT_t's for *violent crime* and *shots fired and violent crime* (note the different scales in Figure 10) appear well-balanced across the pre-treatment period, closely following zero. This indicates an increasing ability to fit the ASCM models with a longer pre-treatment period.

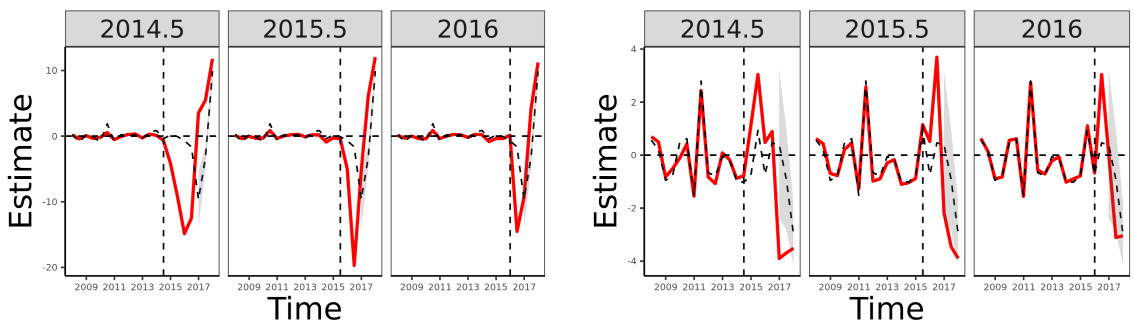

Figure 10: We evaluate robustness across time for Avalon Gardens and Gonzaque Village by comparing psuedo-treatment ATTs (red) to the estimated ATT_t for the true implementation of 2016.5 (dashed line with shaded standard errors). Each panel contrasts a psuedo-model to the estimated model. For each panel, the psuedo-implementation date is both marked in the panel title and by a vertical dashed line within the panel. In the absence of confounding events during the pre-treatment period, we would expect to see the pseudo-implementation ATT_ts closely follow the ATT_t estimated from the true treatment period. Outcomes, left to right: violent crime and shots fired and violent crime.

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