UNIVERSITY of PENNSYLVANIA

Department of Criminology

Working Paper No. 2018-3.0

Effect of Gang Injunctions on Crime: A Study of Los Angeles from 1988-2014

Greg Ridgeway
Jeffrey Grogger
Ruth Moyer
John MacDonald

Effect of Gang Injunctions on Crime: A Study of Los Angeles from 1988-2014

Greg Ridgeway
Department of Criminology
Department of Statistics
University of Pennsylvania

Jeffrey Grogger
Harris School of Public Policy
University of Chicago

Ruth Moyer
Department of Criminology
University of Pennsylvania

John MacDonald
Department of Criminology
Department of Sociology
University of Pennsylvania

Abstract

Objective: Assess the effect of civil gang injunctions on crime.

Methods: Data include crimes reported to the Los Angeles Police Department from 1988 to 2014 and the timing and geography of the safety zones that the injunctions create, from the first injunction in 1993 to the 46th injunction in 2013, the most recent during our study period. Because the courts activate the injunctions at different timepoints, we can compare the affected geography before and after the imposition of the injunction contrasted with comparison areas. We conduct separate analyses examining the average short-term impact and average long-term impact. The Rampart scandal and its investigation (1998-2000) caused the interruption of three injunctions creating a natural experiment. We use a series of difference-in-difference analyses to identify the effect of gang injunctions, including various methods for addressing spatial and temporal correlation.

Results: Injunctions appear to reduce total crime by an estimated 5% in the short-term and as much as 18% in the long-term, with larger effects for assaults, 19% in the short-term and 35% in the long-term. Analyses of interrupted injunctions yielded estimates of similar magnitude and provide further support of a crime reduction effect. We found no evidence that gang injunctions are associated with displacing crime to nearby areas.

Conclusions: Injunctions represent a powerful place-based intervention strategy for police and prosecutors. Courts have recently subjected gang injunctions to closer scrutiny. Los Angeles is not litigating new injunctions and is shrinking the list of enjoined individuals. Our analysis indicates that gang injunctions appear to have contributed to crime reductions in Los Angeles and may still have an important role.

Key words: gangs, gang injunctions, difference-in-difference, spatial-temporal model

1 Introduction

Several cities and counties have used civil actions and remedies to combat gang-related crime. When a city or county establishes by a preponderance of the evidence that gang-related activity constitutes a public nuisance, a court will issue an injunction, imposing a myriad of restrictions on gang member activities within defined geographic areas. Over the past three decades, the Los Angeles City Attorney's Office (LACA) has successfully filed over 40 gang injunctions.

There is a paucity of research on the effectiveness of gang injunctions in controlling crime. Two empirical assessments of the impact of gang injunctions on crime focused on Los Angeles County during the 1990s (Grogger, 2002; 2003-2004 Los Angeles County Civil Grand Jury, 2004). In the decade that followed these two evaluations, gang injunctions expanded in Los Angeles. Popular press has attributed the decline in violent crime in Los Angeles in the early 2000s to the extensive use of gang injunctions (Goff, 2013) and argued that these injunctions prevented a resurgence of gang violence (Los Angeles Times Editorial Board, 2013).

The present study investigates the accuracy of these claims. We examine 27 years of Los Angeles crime data, the location of gang injunctions, and their timing. During this timeframe, the number of gang injunctions in Los Angeles expanded from none to 46 injunctions that covered 22% of the city's land area. We use this expansion to estimate the impact of gang injunctions on crime over nearly three decades.

This study builds on previous research in several ways. First, the long-time series of data enables us to estimate the short and longer-term effect of gang injunctions on crime during periods when Los Angeles had relatively high and low crime rates. Second, we capitalize on the temporary suspension of three injunctions caused by 1999 Rampart Scandal. This temporary gap in injunction enforcement provides a natural experiment that enables us to estimate the effect these three injunctions had on crime.

In the following sections, we present a brief overview of gang injunctions in Los Angeles. We then discuss prior literature examining gang injunctions. Next, we discuss our analytic model and results. Finally, we conclude with a brief discussion of the implications of our findings for the claim that gang injunctions reduced crime in Los Angeles.

1.1 Gang Injunctions in Los Angeles

Los Angeles has historically suffered from street gang crime (Klein, 1995). During epidemic levels of violence in the 1990s, Los Angeles began experimenting with a variety of gang suppression programs, including civil gang injunctions.

LACA initiates an injunction against a gang by filing a civil complaint alleging that the gang and its members are a public nuisance. In drafting the complaint, LACA uses evidence that includes the gang members' criminal records and law enforcement intelligence about their activities. As a practical matter, the deterrent effect of an injunction could begin on the complaint service date, as this is when gang members first learn that law enforcement has amassed a substantial amount of evidence against them and that LACA has submitted this evidence to the court.

After the initial court hearing where LACA presents its evidence, the court issues a preliminary or permanent injunction. Some injunctions name specific members whereas others will include phrases such as "any other members" (Maxson, Hennigan, & Sloane, 2005, p. 580). Some injunctions target gang leaders, whereas others focus on the entire gang. In issuing the injunction, the court may modify the terms or remove some gang members' names.

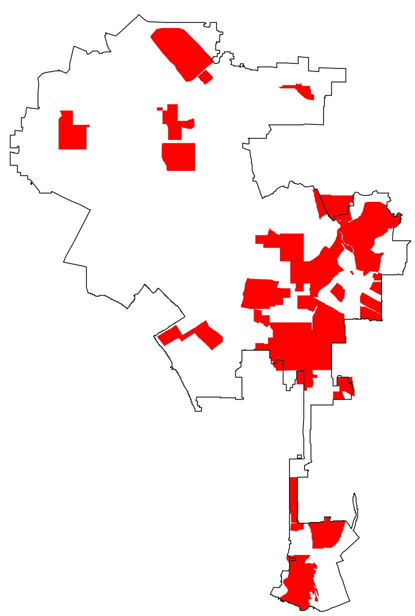
Typically, an injunction lists specific prohibited activities within a defined geographic area in which the gang is thought to operate. In Los Angeles these areas are termed “safety zones.” The geographic focus of injunctions in Los Angeles is important as most of the city’s gangs are territorial and attempt to control specific streets or parks (2003-2004 Los Angeles County Civil Grand Jury, 2004, p. 182). Injunctions should therefore make it more difficult for the gang to assemble, organize criminal activity, and function as a unit (Papachristos, 2013, p. 50).

In addition to prohibiting criminal offenses in specific geographic areas, gang injunctions typically impose a curfew and prohibit otherwise lawful activities like congregating in public, riding in a car with other gang members, gathering in common areas of housing complexes, and possessing spray paint. Violating an injunction can subject a gang member to maximum civil sanctions of a $1,000 fine and five days in county jail and a maximum criminal punishment of a $1,000 fine and six months in jail.

Gang injunctions allow law enforcement in the safety zones to subject gang members to greater scrutiny and surveillance, increasing the ability of law enforcement to engage in proactive police tactics. Perhaps unsurprisingly, civil rights advocates have been critical of gang injunctions. Despite successful challenges to some injunction provisions (Rubin & Reyes, 2016), injunctions remain constitutionally permissible (Wang, 2008).

In Los Angeles gang injunctions started in 1993 with the filing of an injunction against Blythe Street Gang. Between 1997 and 1999 LACA filed nine more injunctions. Figure 1 shows that by 2015 Los Angeles had 46 active gang injunctions covering 22% of the city.

Figure 1: Map of Los Angeles Gang Injunction Safety Zones, 2015



1.2 Prior Literature

Anti-gang policies and programs have included curtailing gang activities with the use of special police units, the use of vertical prosecution, and the enactment of gang-specific sentencing enhancements (Melde, 2013, p. 43). Civil gang injunctions are thought to be among the most innovative and effective gang suppression programs (Maxson, Matsuda, & Hennigan, 2011).

Grogger (2002) conducted the single published peer reviewed study of the effect of gang injunctions on crime. This study examined the effect of 14 of the 17 injunctions imposed in Los Angeles County, including several outside the City of Los Angeles, between 1993 and 1998. The study compared changes in violent crime before and after a neighborhood reporting district (RD) became part of an injunction with changes in violent crime (during the same pre-and post-periods) in geographically proximal RDs. The study also examined RDs that had similar pre-injunction levels of violent crime but never were part of an injunction. The resulting difference-in-differences analysis found that relative to the comparison RDs, serious violent crime decreased 5 to 10 percent within the first year of an injunction. There was also no evidence of crime displacement, as serious violent crime did not increase in adjoining RDs.

The Los Angeles County Civil Grand Jury conducted a similar study focused on a largely different set of injunctions and concluded that gang injunctions were "a successful weapon against criminal activity" (2003-2004 Los Angeles County Civil Grand Jury, 2004, p. 214). The few other studies that examine gang injunctions focus on self-reported neighborhood surveys on quality of life indicators or how perceptions among youth living in injunction zones differ compared to those not living in injunction zones (Maxson, Hennigan, & Sloane, 2005; Hennigan & Sloane, 2013).

2 Data

Our data on gang injunctions came from several sources. First, we gathered the LACA's listing of all gang injunctions currently in effect (Los Angeles City Attorney, 2016). The list includes both the case number and the permanent injunction order for each injunction. Second, we relied on O'Deane's (2012) listing of Los Angeles gang injunctions. We determined that three injunctions were suspended because of the 1999 LAPD Rampart corruption scandal and investigation.[1] Third, we entered the civil case docket numbers into the Superior Court of California website for Los Angeles County (Superior Court of California, County of Los Angeles, 2016). From this site we documented the date of the filing of the complaint. Table 5 in Appendix A provides a list of all injunctions that have existed in Los Angeles.

To map the location of gang injunctions, we received a shapefile from LACA with the boundaries of the safety zones. For the three defunct injunctions, we created our own shapefiles based on written geographical descriptions of the injunction boundaries described in the court orders.

To measure crime we relied on the quarterly crime reports produced by the Los Angeles Police Department (LAPD) from 1988 to 2014, a 27-year period covering the implementation of the first gang

[1] The Rampart investigation revealed criminal activity among some Los Angeles Police Department (LAPD) anti-gang unit officers (Lopez & Connell, 1999). Several implicated officers had been involved in the establishment of the three injunctions. All three injunctions were later reestablished when the LACA refiled complaints with new evidence (O'Deane, 2012).

injunction in 1993 to the most recent in 2013. These data came from LAPD archival data kept at the Los Angeles Public Library and incident-level data acquired from the LAPD directly. The archival data consist of roughly 2,300 pages of tables reporting the number of crime incidents by year, quarter, crime type, and reporting district (RD) (Ridgeway & MacDonald, 2016). RDs are LAPD's neighborhood area designation and, similar to census tracts, they occupy more geographical territory when the residential population is lower. As a result, the counts of crime per reporting district are effectively a rate per residential population (Cook & MacDonald, 2011). Our analysis includes only those RDs that would eventually be in or near a safety zone, 701 RDs out of a total of 939 RD.

For every quarter we labeled each RD as either being in a safety zone, adjacent to a safety zone, a second-order neighbor to a safety zone, or not having a safety zone nearby. On average there are 11 RDs for an injunction safety zone. Since injunction safety zones do not always follow RD boundaries, there is some ambiguity as to what it means for an RD to be directly targeted by an injunction. An RD that is partially covered by a safety zone presumably experiences both the direct and spillover effects of the injunction. To classify such RDs, we used majority rule. We classified RDs that were more than half covered by a safety zone (as a share of its surface area) as directly targeted by the injunction. RDs that were less than half covered were classified as adjoining RDs and contributed to the estimation of spillover effects.

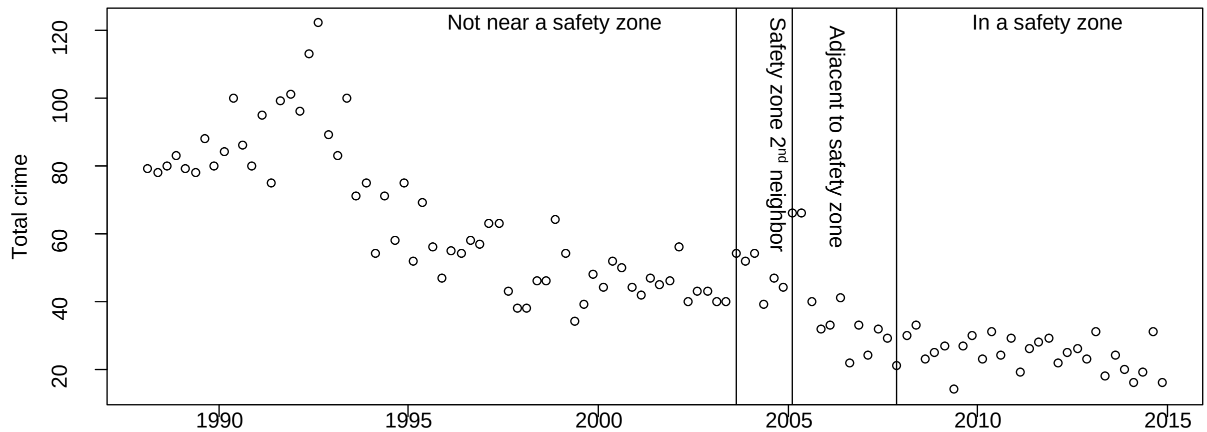
We focus our analysis on seven crime categories: Aggravated Assault, Burglary/Theft from a Vehicle, Burglary, Grand Theft Auto, Grand Theft Person[2], Homicide, and Robbery. These crimes were consistently documented throughout the 27-year study period.

For a given RD we can track trends in crime and assess how those trends change with abrupt introductions of a safety zone. Figure 2 shows an example crime trend for RD 1204 along with the evolution from not being part of an injunction, to being a neighbor of an injunction, to being part of its own injunction. From 1988 to 2003 RD 1204 is not near a safety zone. In 2003 RD 1204 becomes a second neighbor of a safety zone. In 2005 a safety zone appears on the border of RD 1204. In 2007 LACA included this RD in a safety zone. We capitalize on this variability in the crime trends, safety zone timing, and degrees of safety zone adjacency across 701 RDs to estimate the effect of gang injunctions on changes in crime over this 27-year time period.

---

[2] Grand theft person, listed under California PC 487(c), is the theft of property of value directly off another person without force or threat of force.

Figure 2: Crime trends in an example RD (RD1204) as safety zones are introduced



3 Statistical models

Our analysis used three statistical models to test the short-term, long-term, and interruption effect of gang injunctions on crime in Los Angeles.

3.1 Short term effect model

To estimate the short-term effect of gang injunctions on crime, our approach makes use of three sets of RDs: RDs that injunctions targeted directly, RDs adjacent to directly targeted RDs, and RDs that are adjacent to those neighbors. The group that is adjacent to neighboring RDs serves as the control condition. Limiting the analysis to the subset of RDs exposed to injunctions (injunction, neighbors, adjacent to neighbors) makes the estimation sample much more homogenous than a city-wide comparison with respect to observable and unobservable factors that likely determine which locations receive injunctions. This ensures that our estimates are not driven by comparisons between fundamentally different areas.

For the short-term analysis, we restrict attention to a sample period that includes the quarter in which the court imposed the injunction, which we refer as the "injunction quarter," plus 10 quarters before and 10 quarters after. This allows us to have a balanced panel to estimate the effects of the injunctions over a two-and-a-half-year period following their imposition date.

We align the data according to time since the injunction was imposed. That is, we measure time relative to the injunction quarter for all injunctions, regardless of the calendar time period in which it was imposed. We control for calendar time effects via a regression model.

Our design ensures that each injunction's treatment and comparison RDs are in close proximity to each other. As a result, the treatment and control groups resemble each other in terms of characteristics such as the level of pre-injunction crime. In addition to providing treatment and control groups that are similar in terms of observable crime, our design should also provide for similarity in terms of unobserved characteristics which may affect crime, such as demographics, population density, and traffic patterns. At the same time, our approach means that the data have some special features that require attention.

Since some RDs that neighbor an early injunction were targeted by a later injunction, an RD can appear more than once in the sample. This affects the notation that we use to discuss identification and estimation.

Identification

We seek to estimate two average treatment effects on treated RDs (ATT), the ATT of an injunction directly targeting an RD and the ATT of being adjacent to a directly targeted RD, which may be positive due to displacement or negative due to enforcement spillovers. Let D_{id} = 1 if injunction *i* directly targets RD *d* and let D_{id} = 0 otherwise. Let S_{id} = 1 if RD *d* is adjacent to an RD that injunction *i* directly targets and S_{id} = 0 otherwise. In this notation, the identity of RD *d* is always the same, but it may play different roles in different injunctions. This implies that if D_{id} = 1, then S_{id} = 0, and conversely, if S_{id} = 1, then D_{id} = 0.

To define potential outcomes, we extend the usual notation to allow for two treatment statuses plus control. Let potential outcomes at (relative) time *t* for RD *d* involved in injunction *i* be given by $Y_{idt}(D,S)$, where $Y_{idt}(1,0)$ is the potential outcome associated with being directly targeted by the injunction, $Y_{idt}(0,1)$ is the potential outcome associated with adjoining such an RD, and $Y_{idt}(0,0)$ is the potential untreated outcome. Let *t* = 0 denote the injunction quarter, so *t* ≥ 0 denotes the post-injunction period and *t* < 0 denotes the pre-injunction period. The ATT for being directly targeted by the injunction is given by:

$$ATT_D = E(Y_{idt}(1,0)|D_{id} = 1) - E(Y_{idt}(0,0)|D_{id} = 1) \text{ for } t \geq 0 \quad (1)$$

The spillover ATT is given by

$$ATT_S = E(Y_{idt}(0,1)|S_{id} = 1) - E(Y_{idt}(0,0)|S_{id} = 1) \text{ for } t \geq 0 \quad (2)$$

The problem for estimation is that the second terms on the right side of the above expressions are missing counterfactuals, namely, mean untreated outcomes in treatment RDs during the post-treatment period. We cannot observe them directly in the data. However, if we assume parallel trends in untreated outcomes, we can nevertheless estimate the ATTs via a difference-in-difference estimator. The population equivalent of the difference-in-difference estimators for the direct effect can be written according to the following form:

$$\begin{aligned}\Delta_D = {} & E(Y_{idt}(1,0)|D_{id} = 1, \mathrm{t} \geq 0) - E(Y_{idt}(0,0)|D_{id} = 1, \mathrm{t} < 0) - \\ & [E(Y_{idt}(0,0)|D_{id} = 0, S_{id} = 0, \mathrm{t} \geq 0) - E(Y_{idt}(0,0)|D_{id} = 0, S_{id} = 0, \mathrm{t} < 0)]\end{aligned} \quad (3)$$

The population equivalent for the spillover effect can be written according to the following form:

$$\begin{aligned}\Delta_S = {} & E(Y_{idt}(0,1)|S_{id} = 1, \mathrm{t} \geq 0) - E(Y_{idt-}(0,0)|S_{id} = 1, \mathrm{t} < 0) - \\ & [E(Y_{idt}(0,0)|D_{id} = 0, S_{id} = 0, \mathrm{t} \geq 0) - E(Y_{idt}(0,0)|D_{id} = 0, S_{id} = 0, \mathrm{t} \\ & < 0)]\end{aligned} \quad (4)$$

In each case, the difference-in-difference estimate subtracts the before-after difference in the control RDs from the before-after difference of the outcomes for the treated RD, using the control group to adjust for changes that would have taken place in the treatment group had the injunction not been imposed.

The terms Δ_D and Δ_S identify ATT_D and ATT_S under the assumption that the difference in mean untreated outcomes between treatment and control groups is constant across periods, or that they have parallel trends. Sufficient conditions for parallel trends can be established by the following form:[3]

$$\begin{aligned} &E(Y_{idt}(0,0)|D_{id}=1)-E(Y_{idt}(0,0)|D_{id}=0,S_{id}=0)=\lambda_D \text{ for all } t \\ &E(Y_{idt}(0,0)|S_{id}=1)-E(Y_{idt}(0,0)|D_{id}=0,S_{id}=0)=\lambda_S \text{ for all } t \end{aligned} \tag{5}$$

For $t \geq 0$, we cannot estimate the first term on the left side of the above expressions because it represents a missing counterfactual. However, for $t < 0$, we can estimate these expressions to test the validity of the assumption.

Estimation

We can define the observed outcome, the number of crimes at time *t* in RD *d* associated with injunction *i*, in terms of the potential outcomes and treatment indicators according to the following form:

$$Y_{idt}=D_{id}1(t\geq 0)Y_{idt}(1,0)+S_{id}1(t\geq 0)Y_{idt}(0,1)+(1-(D_{id}+S_{id})1(t\geq 0))Y_{idt}(0,0) \tag{6}$$

In equation (6) $1(t \geq 0)$ denotes a post-injunction indicator that equals 1 when $t \geq 0$ and 0 otherwise. Let $ATT_D=\beta_1$ and $ATT_s=\beta_2$. We use ordinary least squares (OLS) to estimate the ATTs with the regression:

$$\begin{aligned} &Y_{idt}=\beta_1 D_{id}1(t\geq 0)+\beta_2 S_{id}1(t\geq 0)+X_{it}\gamma+\mu_{id}+\varepsilon_{idt} \\ &\text{for } i=1,\dots,I, d=1,\dots,N_i, t=-10,\dots,-1,0,1,\dots,10 \end{aligned} \tag{7}$$

The ATTs are estimated as the coefficients on interactions between the treatment-group indicators and the post-treatment indicator. There are *I* injunctions in the sample and the sum of the number of directly affected, adjoining, and control RDs for the *i*th injunction is N_i.

The term X_{it} is a vector of indicator variables generated by mapping relative time *t* for each injunction *i* into calendar time. The elements of the associated vector γ are calendar time effects for each calendar quarter beginning with the 10th quarter prior to the first injunction and extending through the 10th quarter following the last injunction[4]. The period effects control for general trends and seasonal patterns that are common to RDs in the data, including the strong secular trend toward lower crime observed over much of the sample period.

The term μ_{id} is a fixed effect for each RD *d* associated with injunction *i*. This term captures all characteristics of RD *d* that are invariant over the period $t = -10,\dots,10$. This would include mean crime

---

[3] For identification to hold, we need to condition on the calendar time periods corresponding to the relative time periods for each injunction. We leave this conditioning implicit here in order to reduce notational clutter. We explicitly deal with calendar time in the estimation section.

[4] This is true for all but the last two injunctions listed in Table 5. Since our sample period ends with the fourth quarter of 2014, we have six quarters of follow-up data for the 6-Gang Glendale Corridor injunction and seven quarters for the Columbus Street injunction.

levels and demographics, among others. It also absorbs the main effects of the treatment indicators D_{id} and S_{id}. We allow RDs that are involved in different injunctions to have a different fixed-effect associated with each injunction. In practice estimates that imposed a single fixed effect for each RD were similar to those presented.

Finally, ε_{idt} is an idiosyncratic disturbance term that varies by injunction, RD, and time period. Crime counts within RDs exhibit some autocorrelation and overdispersion that needs to be accounted for in conducting inference about the estimated treatment effects. We discuss these issues in the next subsection.

Inference

The structure of our data raises the possibility of spatial and temporal dependence in the crime counts across RDs. Unobserved shocks, for example, may affect multiple RDs within the same general area. RDs that appear multiple times in association with different injunctions further contribute to this dependence problem.

As a general rule, dependence in the disturbance term should not affect the consistency of OLS estimates of (7). However, the covariance matrix of the OLS estimates would be inconsistent if no efforts were made to account for the potential dependency. As a result, inferences drawn on the basis of the conventional OLS covariance matrix would be incorrect.

There are two basic strategies for dealing with this problem. The first is to assume a parametric form for the spatial and temporal dependence and employ an estimator that imposes those assumptions. If the assumptions are correct, this approach may have the benefit of yielding unbiased and precise estimates.

The second approach is nonparametric. Rather than assuming a particular structure for the dependence, the idea is to obtain consistent estimates of the covariance matrix of the OLS coefficients. The approach involves clustering the covariance matrix by judiciously chosen groups, a method dating at least to Eicker (1967), Huber (1967) and White (1980).

In data involving geographical units of observation with multiple time periods per unit, clustering by the geographical unit is well known to yield estimated covariance matrices that are robust to arbitrary autocorrelation in the time-varying component of the disturbance term (Bertrand, Duflo, & Mullainathan, 2004). Thus, clustering by RD should yield a covariance matrix that is robust to autocorrelation in ε_{idt}. In the absence of spatial dependence, that covariance matrix then could be used to form conventional test statistics that would be approximately normally distributed as the number of RDs grew large. However, this approach does nothing to deal with spatial dependence.

If spatial dependence arose only among RDs associated with a particular injunction, clustering the covariance matrix at the level of the injunction could address it. Just as clustering by RD allows for arbitrary correlation among the observations nested within the RD, such as time periods, clustering by injunction allows for arbitrary correlation among the units of observation nested within injunctions, in this case, RDs and time periods. In the absence of dependence across injunctions, test statistics formed from the resulting covariance matrix would be approximately normally distributed as the number of injunctions increases substantially. Given that the sample includes nearly 50 injunctions, one might expect a finite-sample correction to improve the size of the test. This procedure is appealing since it accounts for the spatial dependence that arises since we are using RDs within close proximity of each other to estimate the effects of the injunctions. However, it fails to account for the cross-injunction dependence that arises when the same RD is involved in different injunctions.

Bester, Conley, and Hansen (2011) provide a more general approach that solves this problem. The key to their approach is to cluster a large number of RDs within a few large units of geography. We take these large units to be LAPD's bureaus, which are four high-level geographically specific organizational units that are overseen by Deputy Chiefs in which each RD is nested.[5] We then cluster the covariance matrix by these large units of geography.

By the logic above, clustering by bureaus should allow for arbitrary dependence among the RDs within the bureau, as well as arbitrary temporal dependence. However, one might be concerned about dependence among RDs on either side of a bureau boundary. A key result in Bester et al. (2011) is that, as the number of RDs within bureaus grows large, dependence among RDs on the boundaries becomes negligible, so accounting for spatial dependence within bureaus is asymptotically equivalent to accounting for arbitrary spatial dependence. The approach also accounts for arbitrary autocorrelation with RDs.

The limiting distributions of t- and F-statistics based on the clustered covariance matrix are non-standard (Bester, Conley, & Hansen, 2011). At the same time, such tests are simple to conduct. Bester et al. (2011) provide conditions under which, as the number of RDs per bureau grows large, the usual t- and F-statistics are approximately distributed according to t and F distributions with degrees of freedom equal to the number of bureaus minus one. Moreover, they provide simulation results indicating that tests based on a very small number of large geographic groups generally deliver better size than tests based on a larger number of smaller geographic groups. For the short-term model estimated we follow their guidance here in clustering by bureau.

3.2 Long term effect model

To estimate the long-term effect gang injunctions on crime, we follow the same identification strategy as the previous section. However, to model the long-term changes we use data from all 108 quarters and all 701 RDs that were at some point in the study period near a safety zone. In this design, RD *d* changes status over the study period like RD 1204 shown in Figure 2. We model the crime counts as:

$$Y_{dt} = \beta_0 + \beta_1 D_{dt} + \beta_2 S_{dt} + \beta_3 C_{dt} + \gamma_t + u_d + \epsilon_{dt} + \lambda \sum_{j=1}^{701} w_{dj} \epsilon_{jt} \quad (8)$$

$$\text{for } d = 1, \dots, 701, t = 1, \dots, 108$$

[5] http://assets.lapdonline.org/assets/pdf/Org%20Chart%204-27-17-DP-4B.pdf (Accessed August 10, 2017).

The outcome, Y_{dt} is the crime count in RD *d* in time period *t*. D_{dt}, S_{dt}, and C_{dt} are 0/1 indicators if RD *d* in time period *t* is, respectively, in a safety zone, a direct neighbor of an active safety zone, or a control RD that is adjacent to a direct neighbor. γ_t is a fixed effect for quarter, a coefficient for each of the 108 quarters except the first one. u_d is a fixed effect for RD *d*. ϵ_{dj} is an uncorrelated normal error term.

To accommodate for spatial dependence in our long-term estimates we use a spatial error model, the final term in (8). We first created a spatial weight matrix in which $w_{dj} = 1/n_d$, where n_d is the number of neighbors of RD *d*, if RD *d* and RD *j* were adjacent to each other and $w_{dj} = 0$ if RD *d* and RD *j* were not neighbors ($w_{dd} = 0$ for all *d*). λ measures the size of the spatial correlation component. This is a common approach for statistical modeling of spatial correlation and has been used in criminological research (Tita & Radil, 2010; Deane, Messner, Stucky, McGeever, & Kubrin, 2008). To adjust for temporal correlation, we used a robust standard error calculation that clustered the errors by quarter.

If the creation of a safety zone causes crime to decrease, then β_1 will be negative. If safety zones have beneficial spillover effects into neighboring RDs, then β_2 will be negative. If safety zones displace crime to neighboring RDs, then β_2 will be positive. RDs that are second neighbors are distant from the safety zones. If there are any effects there, then they presumably will be small.

3.3 Analysis of interrupted injunctions

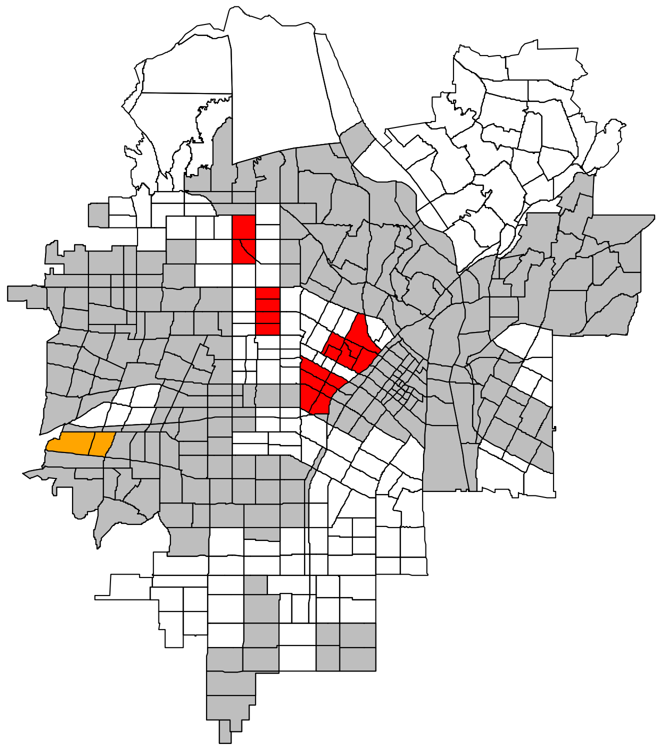
To estimate the temporary effect of gang injunctions on crime, we used the suspension of three injunctions in the Rampart Division[6] as our identification strategy during four time periods: (1) immediately before an injunction, (2) when an injunction is in effect, (3) during the temporary suspension, and (4) during resumption of the injunction.

For each of these three injunctions, we selected two distinct control groups consisting of RDs in the same or adjacent police divisions that either (1) had injunctions in effect throughout the entire period[7], or (2) never had an injunction in effect throughout the entire period but were nearby. Figure 3 shows the geography of these comparisons, highlighting the interrupted gang injunctions and the two control areas.

[6] The three injunctions that underwent a temporary disruption were Shatto Park (Case Number BC190334), Pico Union I (Case Number BC175684), and Mara Salvatrucha (MS13) I (Case Number BC187039).

[7] For each of the three interrupted injunctions, RDs associated with the Southwest (Case Number BC167915) injunction constituted the only continuously enjoined comparison RDs. The Southwest injunction was continuously in effect concomitant with the complaint-suspend-reinstate time periods for all three treatment injunctions. Furthermore, the Southwest RDs were not too far away from the Shatto Park, Pico Union I, and MS13 I RDs to be a useful comparison.

Figure 3: Map of the interrupted safety zones (red), the continuously enjoined Southwest safety zone (orange), and nearby RDs without injunctions (gray)



We estimated the temporary impact of injunctions on crime using crime data from 1996 through 2006, a period starting 6 quarters before the first of the interrupted injunctions began and 6 quarters after the restart of the last interrupted injunction. We estimated a model of temporary effect according to the following form:

$$Y_{dt} = RD_d + \gamma_t + \beta \text{active}_{dt} + \epsilon_{dt} \tag{9}$$

In (9) the model includes a fixed effect for the RD, a fixed effect for every year/quarter, and a term modeling the effect of an injunction being active in RD_d at time t. The continuously enjoined RDs have active_{dt}=1 for all t, never enjoined RDs have active_{dt}=0 for all t, and the interrupted RDs have active_{dt} that, depending on the timing of the specific injunction, switches from 0 to 1 back to 0 and back to 1 during this period. This model gives us the most power since we can use the combined effect of all interrupted injunctions in one analysis.

We also estimate a difference-in-difference model for each of the interrupted injunctions separately. For each of the three interrupted injunctions, we estimated a model of the following form:

$$\begin{aligned} Y_{dt} = \beta_0 + \beta_1 \text{interrupt}_d + \beta_2 \text{never}_d + \beta_3 \text{active}_t + \beta_4 \text{active}_t \text{interrupt}_d \\ + \beta_5 \text{active}_t \text{never}_d + \gamma_t + \epsilon_{dt} \end{aligned} \tag{10}$$

In (10) Y_{dt} is the crime count in RD d at time t and interrupt_d is a 0/1 indicator if RD d is in the interrupted safety zone. We fit the model in (10) for each of the three interrupted injunctions. That is, first we defined interrupt_d to be a 0/1 indicator for when the RD d was interrupted and never_d to be a 0/1 indicator for comparison RDs that had no injunction during this timeframe. RDs with continuous safety zones throughout the timeframe serve as the reference category.

Model (10) estimates two different difference-in-difference estimates, contrasting the interrupted injunction RDs with the continuously enjoined RDs and contrasting the interrupted injunction RDs with the never enjoined RDs. β_4 estimates the change associated with the interruption of the injunction compared to the continuously enjoined RDs when the interrupted injunction is active. $\beta_4 - \beta_5$ estimates the increase in crime in the interrupted RDs compared to the RDs that never had an injunction when the interrupted injunction is active.

$$
\begin{aligned}
\beta_4 = {} & \mathrm{crime}(\mathrm{treatment}, \mathrm{active}) - \mathrm{crime}(\mathrm{treatment}, \mathrm{inactive}) - \\
& (\mathrm{crime}(\mathrm{continuous}, \mathrm{active}) - \mathrm{crime}(\mathrm{continuous}, \mathrm{inactive})) \\
\beta_4 - \beta_5 = {} & \mathrm{crime}(\mathrm{treatment}, \mathrm{active}) - \mathrm{crime}(\mathrm{treatment}, \mathrm{inactive}) - \\
& \big(\mathrm{crime}(\mathrm{never}, \mathrm{active}) - \mathrm{crime}(\mathrm{never}, \mathrm{inactive})\big)
\end{aligned}
\tag{11}
$$

We also include fixed effects for every year and quarter to adjust for common trends in crime. The approach hypothesizes that the timing of the gang injunction is responsible for the crime reduction RDs. We computed robust standard errors to address non-constant variance of the ϵ_{dt}. We also computed permutation test p-values by randomly shuffling the "active" variable and refitting the model to obtain a reference distribution of the values of β_4 and $\beta_4 - \beta_5$ that are likely to occur when activation of the injunction is unrelated to crime counts.

4 Results

4.1 Short-term analysis

The results suggest that gang injunctions produced short-term meaningful reductions in total crime, and that these effects are mostly a result in the reduction in assaults.

Table 1 presents estimates of ATT_D and ATT_S, the direct and spillover effects respectively. Considering the importance of the temporal and spatial dependence problems, we present p-values computed from appropriate reference distributions associated with different levels of clustering for spatial correlation. The estimated treatment effects are the same regardless of the approach to estimating standard errors.[8]

The estimate of ATT_D is -2.05, indicating that on average, the injunctions significantly reduced total crimes in the directly targeted RDs by about two crimes per quarter. The estimate of ATT_S is -0.85 signals negative spillovers to adjacent RDs, but this effect is not statistically significant. In order to understand the magnitude of the effect, these estimates can be compared to the average number of crimes per quarter. Approximately 2 fewer crimes per quarter translates into a 5% reduction in total crime. The estimated 1.5 reduction in assaults translates into a 19% reduction in assaults per quarter.

---

[8] In addition to the variables shown, all regressions included calendar quarter dummies to control for general time trends and seasonal patterns.

In the “Conventional p-value” column, we computed statistics based on the conventional OLS covariance matrix and *p*-values based on the standard normal distribution. The estimates take no account of spatial or temporal dependence. These provide a useful benchmark, allowing us to assess the importance of spatial and temporal dependence for conducting inference about the effects of the injunctions. Based on these statistics, one would conclude that both the direct and spillover effects of the injunctions were highly significant for total crime, assaults, and burglaries.

The “RD” column reports *p*-values based on a covariance matrix that was clustered by RD. The *p*-values are larger than those in the “conventional” column, demonstrating the importance of serial correlation within RDs. Nevertheless, the estimated direct effect of the injunctions on total crime is highly significant, though the significance of the spillover effect is weaker than found using conventional standard errors.

The “Injunction” column reports *p*-values based on a covariance matrix clustered by injunction and a *t* distribution with 46 degrees of freedom. Accounting for dependence within RDs associated with an injunction reduces the significance of the direct effect, although it remains significant for total crime and assault. The spillover effect remains marginally significant for assaults.

The “Bureau” column reports *p*-values based on a covariance matrix that was clustered by bureau and a *t* distribution with three degrees of freedom. The estimated direct effect on total crime and assaults remains statistically significant. For no crime type is the spillover effect significant at conventional levels.

Table 1: Estimated short term effect of gang injunctions

			p-value with clustering by		
Total	Estimate	Conventional p-value	RD	Injunction	Bureau
$\widehat{ATT}_D$	-2.05	<0.001	<0.001	0.05	0.01
$\widehat{ATT}_S$	-0.85	<0.001	0.16	0.23	0.34
Theft from vehicle					
$\widehat{ATT}_D$	-0.10	0.40	0.64	0.75	0.68
$\widehat{ATT}_S$	0.01	0.96	0.98	0.98	0.99
Auto theft					
$\widehat{ATT}_D$	-0.11	0.26	0.53	0.69	0.67
$\widehat{ATT}_S$	*-0.16*	0.06	0.34	0.39	0.41
Homicide					
$\widehat{ATT}_D$	-0.01	0.47	0.55	0.66	0.42
$\widehat{ATT}_S$	*-0.01*	0.64	0.68	0.69	0.55
Assault					
$\widehat{ATT}_D$	-1.5	<0.001	<0.001	0.001	0.02
$\widehat{ATT}_S$	-0.44	<0.001	0.05	0.05	0.17
Burglary					
$\widehat{ATT}_D$	-0.29	<0.001	0.03	0.18	0.19
$\widehat{ATT}_S$	-0.18	0.02	0.17	0.27	0.44
Grand theft person					
$\widehat{ATT}_D$	0.03	0.14	0.24	0.34	0.65
$\widehat{ATT}_S$	0.02	0.26	0.29	0.29	0.51
Robbery					
$\widehat{ATT}_D$	-0.10	0.19	0.45	0.54	0.62
$\widehat{ATT}_S$	-0.10	0.17	0.43	0.43	0.38

Parallel trends

The estimated treatment effects of gang injunctions are valid under the parallel trends assumption. Although this assumption cannot be tested in full, we can test for parallel trends during the pre-intervention period. At the same time, we can estimate period-specific treatment effects for each quarter during the post-injunction period. These provide an indication of how rapidly the injunctions affect crime, and the extent to which their effects fade out.

To test for parallel trends and provide period-specific treatment effects, we estimate the following generalization of equation (7):

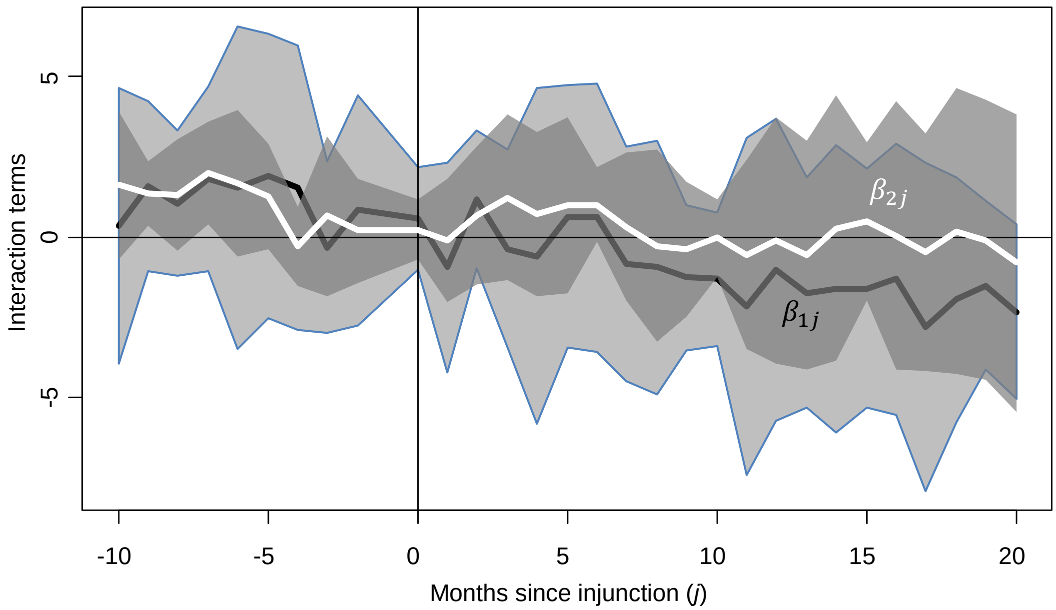
$$
\begin{aligned}
Y_{idt} = &\sum_{j=-10}^{-2} \theta_j 1(t=j) + \sum_{j=0}^{10} \theta_j 1(t=j) \\
&+ \sum_{j=-10}^{-2} \beta_{1j} D_{id} 1(t=j) + \sum_{j=-10}^{-2} \beta_{2j} S_{id} 1(t=j) \\
&+ \sum_{j=0}^{10} \beta_{1j} D_{id} 1(t=j) + \sum_{j=0}^{10} \beta_{2j} S_{id} 1(t=j) \\
&+ X_{it}\gamma + \mu_{id} + \varepsilon_{idt}
\end{aligned}
\tag{12}
$$

The first line in (12) denotes a set of main effects in relative time, indicators for each quarter over the 21-quarter period surrounding the imposition in the injunction. We exclude the period one quarter before the injunction was imposed (t = -1) to serve as the base period. The terms in the second line involve interactions between the treatment indicators and the relative time indicators for the pre-injunction period. If the parallel trends assumption holds, the coefficients associated with these interaction terms should equal zero. We use these coefficients to test for parallel trends. Finally, the terms in the third line involve interactions between the treatment indicators and the relative time indicators for the post-injunction period. The associated coefficients provide estimates of period-specific treatment effects.

Figure 4 shows estimates of the β_{1j}s and the β_{2j}s from equation (12). We computed the *p*-values using the covariance matrix clustered by bureau. None of the estimates associated with the interactions between D_{id} and the pre-treatment period indicators is significant, which is consistent with the parallel trends assumption. The parallel trends assumption also holds for the spillover treatment; the interaction between $S_{id}$and time period t = -10 and t=-2 are all at zero.

The post-injunction trend in β_{1j} shows that the period-specific treatment effects are negative. This suggests that the injunctions may take some time for their effects to be measurable. At the same time, there is little evidence of fade-out. The post-injunction trend in β_{2j} shows that there seem to be no spillover effects.

Figure 4: Estimates of parallel trends and period-specific treatment effects. Estimates of the β_{1j}s and the β_{2j}s along with their 95% confidence intervals



4.2 Long-term analysis

The long-term model uses data on every RD over the entire study period to estimate an average effect of the gang injunctions. Table 2 shows the results from model (8) and indicates that on average RDs with gang injunctions saw a reduction in total crime of 13 crimes per quarter. In RDs adjacent to the safety zone crime decreased by 5 crimes per quarter. The second column in Table 2 shows the average number of crimes per RD per quarter in order to understand the magnitude of the effect. Thirteen fewer crimes per quarter translates to an 18% reduction in injunction areas and 5 fewer crimes translates to a 7% crime reduction in adjacent areas. We have sufficient statistical precision on both of these estimates to conclude that the observed decline is not due to chance. These estimates are in the same direction as our short-term analysis, but indicate a substantially larger injunction effect and evidence of a spillover effect.

Table 2: Decline in crime attributable to being in, adjacent to, or a second neighbor to a safety zone

Crime type	Average crime count per RD quarter	In Safety Zone		Safety Zone Adjacent		2nd Neighbor to Safety Zone	
		Est.	95% interval	Est.	95% interval	Est.	95% interval
Total	73.3	-13.4	(-16.2, -10.6)	-5.1	(-7.2, -3.0)	-1.6	(-3.0, -0.1)
Assaults	11.3	-4.0	(-4.5, -3.4)	-0.6	(-0.9, -0.2)	0.7	(0.4, 1.0)
Theft from vehicle	20.3	-3.0	(-3.7, -2.3)	-2.0	(-2.6, -1.3)	-1.1	(-1.6, -0.6)
Burglary	14.7	-1.6	(-2.0, -1.2)	-0.6	(-0.9, -0.3)	0.0	(-0.2, 0.2)
Auto theft	17.3	-2.5	(-3.3, -1.6)	-1.0	(-1.7, -0.4)	-0.6	(-1.0, -0.1)
Grand theft person	1.5	-0.1	(-0.2, 0.0)	-0.1	(-0.1, 0.0)	0.0	(-0.1, 0.0)
Homicide	0.2	-0.10	(-0.13, -0.07)	-0.02	(-0.04, 0.0)	0.01	(-0.01, 0.03)
Robbery	8.1	-2.1	(-2.6, -1.6)	-0.7	(-1.0, -0.4)	-0.2	(-0.4, 0.0)

Note: All models also control for year-quarter and RD. Spatial lambda significant ($p<0.001$) in all models.

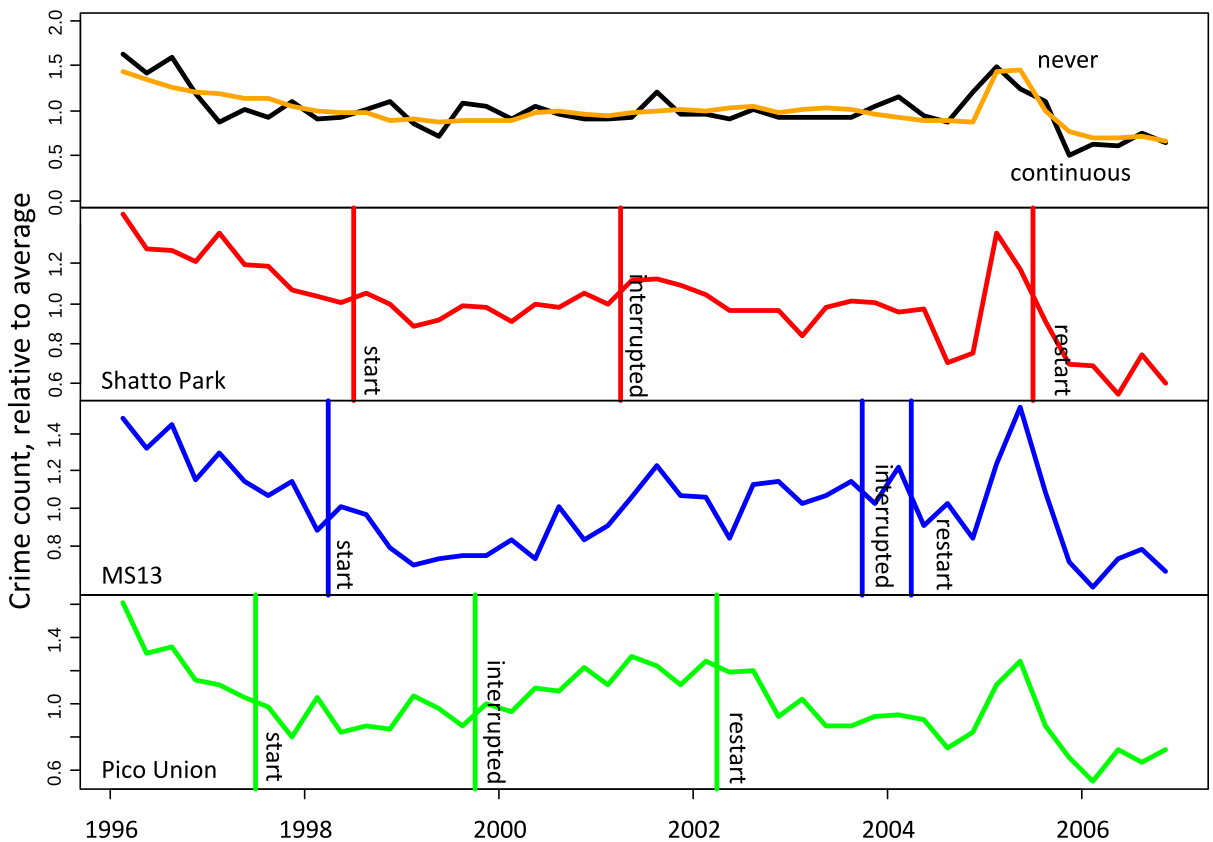
The magnitude of the coefficients suggests that the effect of the gang injunctions dissipates with distance, as we would expect. For total crime, as well as most crime categories, the safety zones experience a significant reduction in crime, the safety zone adjacent RDs experience about half of the effect of those RDs that injunctions target directly, and second-order neighbors experience no injunction effect.

4.3 Analysis of interrupted safety zones

Our third analysis used the interruption of three gang injunctions to estimate the effect of the gang injunctions.

The time series in Figure 5 shows the crime counts, relative to their average, for the three interrupted gang injunctions, continuously enjoined RDs, and RDs that never had injunctions. The vertical lines mark for each interrupted injunction the timing of the start, interruption, and restart. The trends show no visible effect of the injunctions.

Figure 5: Crime trends for the three interrupted gang injunctions (lower three panels) and the crime trends for the continuously enjoined comparison RDs and the never enjoined comparison RDs.



Note: Trends are relative to their average so that they are on the same scale.

Table 3 shows the estimate of the effect of the gang injunctions when they are active for the interrupted injunctions compared with the continuously enjoined and never enjoined RDs, estimates of β from (9). The analysis indicates a decrease in total crime, assaults, homicides, and robbery when the injunctions are active. The second column in Table 3 shows the average number of crimes per quarter per RD in the area shown in Figure 3 to put the crime reduction in context. When active, gang injunctions are associated with a 9% reduction in total crime, a 19% reduction in assaults, and a 14% reduction in robberies.

Table 3: Increase in number of crime when gang injunctions are active

Crime type	Average number of crimes per quarter per RD	Increase in crime	95% CI	p-value
Total	54.5	-4.9	(-8.5, -1.3)	0.01
Assaults	14.3	-2.7	(-3.7, -1.6)	<0.001
Theft from vehicle	11.9	-0.3	(-2.0, 1.4)	0.64
Burglary	7.3	0.2	(-0.6, 1.0)	0.69
Auto theft	10.1	-0.5	(-1.7, 0.7)	0.38
Grand theft person	0.7	-0.1	(-0.5, 0.3)	0.26
Homicide	0.3	-0.1	(-0.19, -0.04)	<0.001
Robbery	9.9	-1.4	(-2.0, -0.7)	<0.001

We also conducted a difference-in-difference analysis separately for each of the three interrupted injunctions using the statistical model in (10), comparing each of them to the continuously enjoined and the never enjoined RDs. The analysis had limited power to isolate the effect of the individual injunctions on specific crimes. However, as Table 4 shows for total crime, the general pattern is that the injunctions reduced crime. Total crime appeared to decrease slightly, about 2 to 5 fewer crimes per quarter per RD, but the results are not statistically significant regardless of whether we compare the injunction to the areas continuously under an injunction or never exposed to an injunction.

Table 4: Difference-in-difference estimate of the effect of the interrupted injunctions

	Interrupted v. Continuous			**Interrupted v. Never**		
Crime type	Change in total crime	95% CI	p-value	Change in total crime	95% CI	p-value
	(β_4)			$(\beta_4 - \beta_5)$		
Shatto Park	-4.1	(-11.0, 2.8)	0.23	-5.2	(-11.1, 0.7)	0.11
MS13	-2.4	(-14.9, 10.1)	0.71	-4.2	(-15.6, 7.2)	0.45
Pico Union	-3.3	(-10.7, 4.0)	0.28	-5.2	(-11.4, 1.0)	0.11

5 Discussion

The estimates presented in this study, based on 27 years of crime data drawn from the LAPD, suggest that civil gang injunctions produced clear short-run and longer-term benefits in reducing serious crime, especially assaults. While the estimates for temporary injunctions are not statistically significant, they are in the same negative direction. They are also temporary suspensions that were also correlated with the closing down of LAPD anti-gang unit during the Rampart Scandal, so these suspensions were also likely associated with a general decline in gang suppression in these areas. Overall, the size of the effects we observed are substantial, showing total crime was reduced by an estimated 5% in the short-term model and 18% in the long-term model. The effects are more substantial when it comes to assaults, suggesting that gang injunctions reduced assaults by an estimated 19% in the short-term model and 35% in the long-term model.

We believe that the primary reason for the difference between the long-term and short-term model results is that the effect of injunctions likely grows over time. In further exploration, we learned that each additional quarter post injunction appears to be associated with a decrease of about 0.4 crimes on average. Therefore, the average total crime decline per RD per quarter after 10 quarters would be around 2 ((0.4+0.4×10)/2), matching what we found in the short-term model. The average total crime decline per RD per year after 42 quarters (the average injunction followup period in the long-term model) would be 9 crimes ((0.4+0.4×42)/2), only slightly smaller than and of the same order of magnitude as what we found in our long-term model analysis.

Analyses of interrupted injunctions yielded estimates of similar magnitude and provide further support of a crime reduction effect. There is also no evidence that gang injunctions are associated with displacing crime to nearby areas. If anything, the imposition of injunctions is associated with spillover benefits to adjacent neighborhoods.

Gang injunctions were one of many factors that changed in Los Angeles during this time period that likely impacted crime. For example, in the 1990s immigrants moved into what had previous been high crime areas (MacDonald, Hipp, & Gill, 2013; Leovy, 2015) and in the early 2000s the LAPD shifted to a COMPSTAT policing model that strategically deployed officers to high crime areas (Fagan & MacDonald, 2012). While these factors likely also played a role in crime reduction, if anything they push against finding an impact of gang injunctions on crime reduction. While the results of our analysis suggest there is a deterrent impact of gang injunctions, it is also possible that the effects may diminish over time. Specifically, challenges to the legality of gang injunctions may cause less police interest in enforcing them, or it is possible that gang members named in injunctions were already aging out of crime or moving to new locations.

Injunctions represent a powerful place-based intervention strategy for the police and prosecutors. However, courts have begun to scrutinize gang injunctions more closely. Since our analysis indicates gang injunctions have contributed to the crime decline, crafting and implementing gang injunctions will need to preserve their features that have crime reduction benefits while following court ordered limitations.

This analysis is limited in several ways. The analysis used only official reported crime data. Declaring an area a safety zone may draw in additional police presence causing the police to detect more crime through proactive enforcement actions or through an increase in citizen reports due to greater visibility of police. Increased reporting of crime would clearly offset an observed deterrent impact. As other scholars have noted, where an individual is subject to an injunction, there is a greater risk of detection and apprehension for criminal conduct (Hennigan & Sloane, 2013). Therefore, individuals who live in safety zones and are subject to an injunction would likely be more subject to arrest than individuals who do not live in safety zones – and are not subject to an injunction. With the existing data we cannot explore the mechanisms by which gang injunctions helped reduce crime. Consistent with perceptual deterrence, gang members may desist, knowing that they are under surveillance and subject to arrest for minor infractions. Alternatively, the reductions may have resulted from the specific arrests of key gang members. Or, the mechanism could be some combination of the two.

Future research should collect data on named gang members and enforcement actions to examine these possible explanations. Obtaining such data would require objective and systematic recording; as of now, existing categorization in LAPD data of incidents and individuals as "gang-involved" is largely too subjective. More objective and systematic data collection may reveal the specific components of the injunctions that generate the observed crime reduction effect.

References

2003-2004 Los Angeles County Civil Grand Jury. (2004). *Final Report.* Los Angeles: Los Angeles County.

Bennett, T., Holloway, K., & Farrington, D. P. (2006). Does neighborhood watch reduce crime? A systematic review and meta-analysis. *Journal of Experimental Criminology, 2*(4), 437-458.

Bertrand, M., Duflo, E., & Mullainathan, S. (2004). How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics, 119*(1), 249-275.

Bester, C. A., Conley, T. G., & Hansen, C. B. (2011). Inference with dependent data using cluster covariance estimators. *Journal of Econometrics, 165*(2), 137-151.

Cook, P. J., & MacDonald, J. M. (2011). The Role of Private Action in Controlling Crime. In P. J. Cook, J. Ludwig, & J. McCrary (Eds.), *Controlling Crime: Strategies and Tradeoffs.* Chicago: University of Chicago Press.

Deane, G., Messner, S. F., Stucky, T. D., McGeever, K., & Kubrin, C. E. (2008). Not 'Islands, Entire of Themselves': Exploring the Spatial Context of City-level Robbery Rates. *Journal of Quantitative Criminology, 24*(4), 363–380.

Eicker, F. (1967). Limit Theorems for Regression with Unequal and Dependent Errors. *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability* (pp. 59–82). Berkeley, CA: University of California Press.

Fagan, J., & MacDonald, J. M. (2012). Policing, Crime and Legitimacy in New York and Los Angeles: The Social and Political Contexts of Two Historic Crime Declines. In D. Halle, & A. A. Beveridge (Eds.), *New York and Los Angeles: The Uncertain Future* (pp. 219-262). New York: Oxford University Press. doi:10.1093/acprof:oso/9780199778386.003.0008

Goff, K. (2013, September 14). Sylmar, Pacoima helped by injunction, authorities say. *Los Angeles Daily News*.

Grogger, J. (2002). The Effects of Civil Gang Injunctions on Reported Violent Crime. *Journal of Law and Economics, 45*(1), 69-90.

Hennigan, K. M., & Sloane, D. (2013). Improving Civil Gang Injunctions: How Implementation Can Affect Gang Dynamics, Crime, and Violence. *Criminology & Public Policy, 12*(1), 7-41. doi:10.1111/1745-9133.12000

Huber, P. J. (1967). The behavior of maximum likelihood estimates under nonstandard conditions. *Proceedings of the Fifth Berkeley Symposium on Mathematical Statistics and Probability* (pp. 221–233). Berkeley, CA: University of California Press.

Klein, M. W. (1995). *The American Street Gang.* New York: Oxford University Press.

Leovy, J. (2015). *Ghettoside: A true story of murder in America.* New York: Spiegel & Grau.

Lopez, R. J., & Connell, R. (1999, September 21). Police Probe Forces Suspension of Rampart Anti-Gang Injunctions. *Los Angeles Times*.

Los Angeles City Attorney. (2016, March 9). *List of Gang Injunctions*. Retrieved from http://www.lacityattorney.org/gang-injunction

Los Angeles Times Editorial Board. (2013, November 7). Be wary of gang injunctions. *Los Angeles Times*. Retrieved February 28, 2017, from http://www.latimes.com/opinion/editorials/la-ed-gang-injunction-city-of-orange-rackauckas-20131107-story.html

MacDonald, J. M., Hipp, J. R., & Gill, C. (2013). The effects of immigrant concentration on changes in neighborhood crime rates. *Journal of Quantitative Criminology, 29*(2), 191-215.

Maxson, C. L., Matsuda, K. N., & Hennigan, K. (2011). "Deterrability" Among Gang and Nongang Juvenile Offenders: Are Gang Members More (or Less) Deterrable Than Other Juvenile Offenders? *Crime & Delinquency, 57*(4), 516-543. doi:10.1177/0011128709343137

Maxson, C., Hennigan, K., & Sloane, D. (2005). "It's Getting Crazy Out There": Can a Civil Gang Injunction Change a Community? *Criminology and Public Policy, 4*(3), 577-606. doi:10.1111/j.1745-9133.2005.00305.x

Melde, C. (2013). The Practicalities of Targeted Gang Interventions. *Criminology & Public Policy, 12*(1), 43-48. doi:10.1111/1745-9133.12004

O'Deane, M. D. (2012). *Gang Injunctions and Abatement: Using Civil Remedies to Curb Gang-Related Crimes.* Boca Raton, FL: CRC Press.

Papachristos, A. V. (2013). The Importance of Cohesion for Gang Research, Policy, and Practice. *Criminology & Public Policy, 12*(1), 49-58. doi:10.1111/1745-9133.12006

Ridgeway, G., & MacDonald, J. M. (2016). Effect of Rail Transit on Crime: A Study of Los Angeles from 1988-2014. *Journal of Quantitative Criminology*. doi:10.1007/s10940-016-9296-7

Rubin, J., & Reyes, E. (2016, March 16). L.A. to pay up to $30 million in curfew lawsuit settlement to provide job training for gang members. *Los Angeles Times*.

Superior Court of California, County of Los Angeles. (2016, March 9). Retrieved from http://www.lacourt.org/casesummary/ui/index.aspx?casetype=civil

Tita, G. E., & Radil, S. M. (2010). Spatial regression models in criminology: modeling social processes in the spatial weights matrix. In A. Piquero, & D. Weisburd (Eds.), *Handbook of quantitative criminology* (pp. 101–121). New York: Springer.

Wang, C. (2008). Gang Injunctions Under Heat from Equal Protection: Selective Enforcement As a Way to Defeat Discrimination. *Hastings Constitutional Law Quarterly*, 287-308.

White, H. (1980). A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica, 48*, 817–830.

Appendix A

Table 5: List of Gang Injunctions in the City of Los Angeles

Injunction	Case number	Complaint date	End date	Resumed as
Blythe Street Gang	LC020525	2/22/1993		
18th Street Gang Southwest (Alsace Clique, Jefferson Park)	BC167915	3/21/1997		
18th Street (Pico-Union) I	BC175684	8/1/1997	10/22/1999	Pico Union II
Mara Salvatrucha I	BC187039	3/4/1998	9/18/2003	Mara Salvatrucha II
18th Street Gang (Shatto Park Locos, Columbia Little Cycos)	BC190334	5/1/1998	3/2/2001	10 Gang
Harpys	BC192678	6/16/1998		
Langdon Street Gang	LC048292	3/26/1999		
Culver City Boys	SC056980	4/23/1999		
Venice Shoreline Crips	SC057282	5/21/1999		
Harbor City & Harbor City Crips	NC026769	11/12/1999		
Venice 13 Gang	SC060375	2/4/2000		
Pacoima Project Boys	PC027254	3/20/2001		
Eastside Wilmas Gang & Westside Wilmas Gang	NC030080	5/23/2001		
Canoga Park Alabama	BC267153	1/29/2002		
18th Street - Pico Union II (Hoover St, Red Shield)	BC272030	4/16/2002		
KAM	BC282629	10/3/2002		
Avenues	BC287137	12/17/2002		
Rolling 60 Crips	BC298646	7/8/2003		
Bounty Hunters	BC301433	8/26/2003		
18th Street - Hollywood	BC305434	11/4/2003		
Mara Salvatrucha II	BC311766	3/9/2004		
Wilshire 18th Street	BC313309	4/6/2004		
38th Street	BC319166	7/28/2004		
Varrio Nuevo Estrada	BC319981	8/12/2004		
42nd Street, 43rd Street & 48th Street Gangster Crips	BC326016	12/16/2004		
Grape Street Crips	BC330087	3/10/2005		
Hoover & Trouble	BC330272	3/15/2005		
10 Gang (18th Street, Crazy Riders, Down in Action, Krazy Town, La Raza Loca, Orphans, Rockwood Street Locos, Varrio Vista RIFA, Wanderers, Witmer Street Locos)	BC332713	5/2/2005		
Big Hazard	BC335749	6/28/2005		
School Yard Crips & Geer Street Crips	BC349468	3/23/2006		
Playboys	BC351990	5/8/2006		
Black P Stones	BC352951	5/25/2006		
White Fence (Hollywood)	BC353596	6/8/2006		
Clover, Eastlake & Lincoln Heights	BC358881	9/20/2006		
Dogtown	BC359945	10/6/2006		
Highland Park	BC359944	10/6/2006		
Rolling 40, 46 Top Dollar Hustler & 46 Neighborhood Crips	BC380229	11/5/2007		
5th & Hill	BC380877	11/16/2007		
204th Street & Eastside Torrance	BC381942	12/7/2007		
San Fer	BC388726	4/10/2008		
6 Gang (All for Crime, Barrio Mojados, Blood Stone Villains, Florencia, Oriental Boyz, Pueblo Bishops)	BC397522	9/5/2008		
Eastside Pain	BC399741	10/10/2008		
Temple Street	BC401190	11/3/2008		
Toonerville	BC401928	11/24/2008		
Barrio Van Nuys	BC413147	5/6/2009		
Fremont (Swan Bloods, Florencia 13, Main Street Crips, 7 Trey, Hustlers/Gangster Crips)	BC415694	6/12/2009		
Grape Street Crips (Central)	BC435316	4/7/2010		
Rancho San Pedro	BC460412	4/27/2011		
Columbus Street	BC501348	2/20/2013		
6 Gang Glendale Corridor (Big Top Locos, Crazys, Diamond Street Locos, Echo Park Locos, Frog Town Rifas, Head Hunters)	BC511444	6/11/2013		