THE EFFECTS OF CIVIL GANG INJUNCTIONS ON REPORTED VIOLENT CRIME: EVIDENCE FROM LOS ANGELES COUNTY*

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ABSTRACT

Several cities have recently adopted civil injunctions as a means to reduce gang violence. To evaluate the effectiveness of such injunctions, I develop an extensive database of neighborhood-level reported crime counts from four police jurisdictions within Los Angeles County. I construct two different comparison samples of neighborhoods not covered by injunctions to control for underlying trends that could cause one to overstate the injunctions' effects. The analysis indicates that, in the first year after the injunctions are imposed, they lead the level of violent crime to decrease by 5–10 percent.

I. INTRODUCTION

In the early 1990s, violent crime rates increased to levels that were higher than they had been since the Federal Bureau of Investigation (FBI) started keeping records. In many locales, much of the increase in violence was blamed on gangs. Although gangs trouble a large number of U.S. cities, one of the worst gang problems afflicts Los Angeles County. Therefore, it is not surprising that Los Angeles would be the birthplace of a novel antigang tactic: the civil gang injunction. These injunctions arise from lawsuits in civil court, typically filed by either the city attorney or district attorney (or both), that seek relief from the public nuisance caused by gang members. The injunctions prohibit specific persons from engaging in particular activities within clearly defined "target areas."

Although the use of civil injunctions to meet criminal justice objectives is novel, the injunction strategy incorporates elements that are common to

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other recent law enforcement interventions, including place-based or "hot-
spot" enforcement strategies,\(^1\) community policing,\(^2\) and interagency co-
operation.\(^3\) Unlike many other interventions, however, the effectiveness of
the injunctions has yet to be evaluated. This lack of knowledge is increasingly
important in light of the increasing prevalence of the injunctions. In Los
Angeles County, 22 injunctions have been imposed since 1993; 12 have been
imposed since 1997. San Jose, San Diego, San Antonio, Houston, and Phoe-
nix have recently implemented gang injunctions as well.

My objective in this study is to estimate the extent to which the injunctions
reduce reported violent crime. To do this, I employ data from Los Angeles
County that permit me to assess the effectiveness of the injunctions over
roughly the first year after they are imposed. My analysis addresses two
specific questions: (1) whether the injunctions reduce reported crime in the
target areas and (2) whether the injunctions cause spillovers. The second
question is of interest because opponents of the injunctions argue that they
are likely to merely displace gang activities, leading to increases in the level
of crime outside the target areas that could offset any decreases within them.\(^4\)

These questions pose two primary research challenges. The first is to
assemble data of sufficient quantity that are suitable for tracking neighbor-
hood-level crime trends. To do this, I have computerized reporting-district-
level (RD-level) crime data on FBI Index Offenses from four Los
Angeles–area law enforcement agencies: the Los Angeles Police Department
(LAPD), the Los Angeles Sheriff's Department (LASD), the Long Beach
Police Department (LBPD), and the Pasadena Police Department (PPD). As
I explain in detail below, most of these data were provided as hard-copy
reports, which necessitated an extensive scanning, checking, and editing pro-
cess to generate a database that could be used for the analysis.

The second major research challenge is constructing a counterfactual, that
is, an estimate of how target-area crime levels would have changed if the
injunctions had not been imposed. The need for such a counterfactual is illustrated by Figure 1, which shows a plot of RD-level crime trends in three
types of areas: target-area RDs; adjoining RDs, which may be affected by
spillovers; and neighboring RDs, which abut the adjoining RDs. In all three
areas, as in the United States as a whole, the level of violent crime trended
sharply downward during most of the 1990s.

Clearly, this widespread trend could not have been driven entirely by the

\(^1\) Lawrence W. Sherman, P. R. Gartin, & M. E. Buerger, Hot Spots of Predatory Crime:
Routine Activities and the Criminology of Place, 27 Criminology 27 (1989).


\(^3\) David M. Kennedy, Anne M. Piehl, & Anthony A. Braga, Youth Violence in Boston: Gun
Markets, Serious Youth Offenders, and a Use-Reduction Strategy, 59 Law & Contemp. Probs.
147 (1996).

\(^4\) American Civil Liberties Union (ACLU) of Southern California, False Premise, False
gang injunctions. Moreover, in the presence of such a strong trend, simple before-and-after comparisons of crime rates within the target areas could lead one to greatly exaggerate the effects of the injunctions. Put differently, without a counterfactual to account for the change in the level of crime not caused by the injunctions, the analysis would confound the effects of the injunctions with the effects of other county- (and nation-) wide factors that were causing the level of crime to decrease.

Accounting for such factors requires a comparison sample. The ideal comparison sample would consist of RDs that were identical to the target-area RDs in every respect but one: they were not themselves covered by an injunction. In principle, this ideal could be achieved by running a randomized trial. Short of that, however, one must devise some means of matching the target-area RDs to RDs that will provide an estimate of how crime trends would have changed in the target areas had the injunctions not been imposed.

I employ two types of comparison samples in this analysis. The first consists of the neighboring RDs whose crime trends are depicted in Figure 1. As I explain in more detail below, these neighboring RDs lie in geographic proximity to the target-area RDs. As a result, their demographic makeup, population density, and traffic patterns should be fairly similar. Figure 1 shows that their crime trends are similar.

In addition to this intuitive matching procedure, I adopt a statistical matching procedure as well. For each target area, I construct a comparison area from RDs whose crime levels are similar to those in the target area during
a period of several quarters preceding the injunction, as measured by their percentiles in the overall distribution of violent crimes during the pre-injunction period. This percentile-matching procedure helps to solve a potential mean-reversion problem that arises from using the neighboring RDs as controls.

In the next section, I provide some further background on the injunctions and injunction procedure. This helps in illustrating how the injunctions deter crime. It also helps to clarify when in the process one might expect the injunctions to start taking effect. Section III provides a description of the data. In Section IV, I describe my methods in greater detail and present the results. In Section V, I compare the injunctions to other recent law enforcement interventions to provide a sense of the injunctions' relative effectiveness. Section VI concludes.

II. SOME BACKGROUND ON THE INJUNCTIONS

The injunctions are civil actions that prohibit specifically named individuals from engaging in particular activities within a clearly defined target area. The prohibited activities vary somewhat, but they typically include a mix of activities already forbidden by law, such as selling drugs or committing vandalism, and otherwise legal activities, such as carrying a cell phone or associating in public view with other gang members named in the suit. Once an injunction is imposed, prosecutors can pursue violations of the injunction in either civil or criminal court. The maximum penalty for civil contempt is a $1,000 fine and 5 days in jail. The maximum penalty under criminal prosecution is a $1,000 fine and 6 months in jail. Although civil procedures result in less stringent penalties, they have the advantage (as viewed by prosecutors) that their penalties can be imposed without criminal due process.

To understand how the injunctions may deter gang violence, some background on the injunction procedure is useful. As an action in civil court, an injunction begins with a petition to the court for relief from the public nuisance caused by specific gang members. In order to be successful, the petition must establish that particular individuals are indeed responsible for creating a public nuisance. Typically, prosecutors use two sources of evidence to make their case. The first comes from sworn statements made by residents of the community. The inhabitants of gang-infested areas often witness criminal acts, such as drug dealing and assaults. Although such acts may go unreported at the time, later reports often provide useful evidence in civil court, especially when they are corroborated by a number of residents in-
dependently or when reports of separate incidents tend to implicate the same gang members repeatedly.

As useful as they are, however, declarations by residents are often difficult for prosecutors to obtain. Residents are often fearful of retaliation by the gang and may have little faith in the commitment or ability of the prosecutor’s office to do much about their neighborhood’s gang problem. For this reason, the process of developing the complaint can be quite time-consuming, often taking several months as prosecutors organize neighborhood residents and gain their cooperation.

The other source of evidence used to develop the complaint comes from police officers and informants. Like residents, police are often aware of crimes committed by particular gang members. Undercover informants, in particular, often have extensive intelligence on gang members’ activities. Even evidence that is inadmissible in a criminal proceeding may be useful in a civil lawsuit.

When prosecutors file the complaint with the court, the intelligence provided by police and community members becomes known to the gang members, because prosecutors must serve each defendant with a copy of the complaint in order to include them in the lawsuit. This means that, at the time the complaint is filed, the defendants learn from the prosecutors much about what the prosecutors know about them. Since much of this information concerns crimes for which the defendant has not been arrested, let alone criminally prosecuted, this revelation of intelligence may come as a surprise to the defendant and reveal that his activities are being closely monitored. It may well raise the defendant’s sense of exposure to being criminally prosecuted for acts that he had believed to have gone undetected. Thus, it seems likely that the injunctions deter crime primarily by raising the gang member’s perceived probability of apprehension.6

Once the prosecutor’s complaint is filed, the court sets a date for a hearing, which typically occurs 1–3 months after the complaint is filed. At most hearings, the court has issued the injunction largely as requested. In many cases, however, the judge has struck the names of one or more defendants from the complaint, either because they had not been served with papers or because of evidence that the defendants in question were not associated with the gang. In a few cases, the judge has also modified the terms of the injunction.

Once issued, the conditions of the injunction become effective as soon as prosecutors can serve the defendants with a copy of the injunction order. Although defendants are under no obligation to appear at the civil hearing,

6 Steven D. Levitt & Sudhir Alladi Venkatesh, An Economic Analysis of a Drug-Selling Gang’s Finances, 115 Q. J. Econ. 755 (2000) (noting that the sanction for violating the injunction may play a role as well, since $1,000 represents a sizeable share of the typical gang member’s monthly income).
many do. Thus, the injunctions begin to be legally binding as soon as the court makes its decision.

Trials to make the preliminary injunction permanent occur much later. Although in principle an injunction could be reversed at trial, in practice every lawsuit that has come to trial in Los Angeles County has resulted in a permanent injunction. For the purposes of the analysis below, however, the important point is that the injunction becomes legally binding on the defendants as soon as the preliminary injunction is issued.7

Unfortunately, what happens once the preliminary injunction is issued is hard to assess. There is no systematic evidence on enforcement efforts since police are not required to track arrests that are made for violations of the injunctions. Anecdotal evidence provided by prosecutors suggests that enforcement levels vary among the injunctions but that, in many cases, the level of police patrols changes little in response to the injunctions.

The implication for the analysis that follows is that it will not be possible to determine whether the effects of the injunction are due to increased enforcement or simply due to the imposition of the injunction and the revelation of police intelligence that it entails. It is also impossible to estimate whether the effectiveness of the injunctions increases with the level of police enforcement activity. Thus, the estimates provided below represent the average effect of the injunctions, given the underlying distribution of enforcement activity.

III. Data

To determine whether these injunctions affect reported crime, I assembled data pertaining to 14 of the 17 injunctions that were imposed in Los Angeles County between 1993 and the end of 1998.8 The gangs named in the injunctions, the dates on which the preliminary injunctions were issued, and the law enforcement jurisdictions in which the target areas are located are shown in Table 1.

Three of these jurisdictions—they produce regular reports of crime counts by reporting district. Reporting districts are small geographical units that can be understood by inspecting Figure 2, which provides a map of all RDs for the Central Division of the LAPD (that is, downtown Los Angeles). Reporting districts are small geographical units similar to census tracts; indeed, most RDs within the city of Los Angeles are coterminous with census tracts. The PPD provided me with electronic incident-level files of crime reports, where each report was associated with

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7 The injunctions generally remain in effect indefinitely, although three injunctions were suspended in September 1999 (3 months before the end of my sample period) because of a police corruption scandal. I discuss those injunctions in greater detail below.

8 Five more injunctions have been imposed since the beginning of 1999. These most recent injunctions provide too little follow-up data to contribute to the analysis.
TABLE 1

INJUNCTIONS INCLUDED IN THIS STUDY

<table>
<thead>
<tr>
<th>Gang Named in Injunction</th>
<th>Preliminary Injunction Granted</th>
<th>Jurisdiction</th>
</tr>
</thead>
<tbody>
<tr>
<td>Blythe Street</td>
<td>April 27, 1993</td>
<td>LAPD</td>
</tr>
<tr>
<td>Orange Street Locos</td>
<td>August 25, 1994</td>
<td>LASD</td>
</tr>
<tr>
<td>West Side Longos</td>
<td>October 23, 1995</td>
<td>LBPD</td>
</tr>
<tr>
<td>Denver Lanes</td>
<td>December 26, 1995</td>
<td>PPD</td>
</tr>
<tr>
<td>Villa Boys/Krazy Boys</td>
<td>July 12, 1996</td>
<td>PPD</td>
</tr>
<tr>
<td>Lennox 13</td>
<td>September 11, 1996</td>
<td>LASD</td>
</tr>
<tr>
<td>Chopper 12</td>
<td>November 14, 1996</td>
<td>LASD</td>
</tr>
<tr>
<td>West Coast Crips</td>
<td>June 10, 1997</td>
<td>LBPD</td>
</tr>
<tr>
<td>18th Street (Jefferson Park)</td>
<td>July 11, 1997</td>
<td>LAPD</td>
</tr>
<tr>
<td>18th Street (Pico-Union)</td>
<td>August 29, 1997</td>
<td>LAPD</td>
</tr>
<tr>
<td>Mara Salvatrucha</td>
<td>April 13, 1998</td>
<td>LAPD</td>
</tr>
<tr>
<td>Shatto Park Locos</td>
<td>June 9, 1998</td>
<td>LAPD</td>
</tr>
<tr>
<td>Columbia Little Cycos</td>
<td>June 9, 1998</td>
<td>LAPD</td>
</tr>
<tr>
<td>Harpys</td>
<td>August 4, 1998</td>
<td>LAPD</td>
</tr>
</tbody>
</table>

NOTE.—LAPD: Los Angeles Police Department; LASD: Los Angeles Sheriff’s Department; LBPD: Long Beach Police Department; PPD: Pasadena Police Department. There were three injunctions that were imposed between 1993 and 1998 for which suitable data were not available (Redondo Beach v. Nort Side Redondo 13, Inglewood v. Crenshaw Mafia Gang Bloods, Compton v. Varrio Tortilla Flats). Five injunctions have been imposed since 1998. These provide too little follow-up data to be included in the analysis.

the census tract in which the incident occurred. Those data were electronically aggregated into tract-level crime counts. These RDs and census tracts are the basic unit of analysis for the study. For ease of exposition, I will refer to them all as RDs.9

For all jurisdictions except Pasadena, I have assembled quarterly data for each RD for the period from 1992 through 1999. The PPD provided data covering the period from 1993 to mid-1998. Although most of the jurisdictions provide monthly data, the LAPD publishes its reports quarterly. Therefore, in order to combine data from all four jurisdictions, it was necessary to aggregate the monthly data to the quarterly level. Because of the sheer magnitude of the LASD’s service area, it was infeasible to assemble data for all areas under its jurisdiction. Instead, I assembled data from five station

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9 The LAPD periodically renumbers its reporting districts. For the purposes of this analysis, I have constructed cross-walk tables that permit me to construct consistent time series by linking the crime data to each RD’s geographic location, irrespective of its number at any point in time. This is true of the LASD to a lesser extent as well, for which I have also constructed cross-walk files. In a similar vein, the LBPD has occasionally changed RD boundaries. However, the LBPD indicated that such changes occurred only within RD “groups” identified by the leading digits of the RD number. To generate consistent time series, therefore, I have aggregated the data into RD groups instead of using the RD-level data themselves. To simplify exposition, albeit at the risk of generating some confusion, I refer to the RD groups as RDs for the remainder of the paper.
areas that include or adjoin several of the target areas listed in Table 1.¹⁰
Like nearly all law enforcement agencies nationwide, all of these agencies participate in the FBI's Uniform Crime Reporting program. This ensures that the categories of crimes reported to the FBI are defined similarly by all four

¹⁰ These five station areas are Carson, East Los Angeles, Lakewood, Lennox, and Norwalk.
agencies. The violent crimes reported to the FBI, and the crimes analyzed here, are murder, rape, robbery, and aggravated assault. It is important to note that neither drug offenses nor gang-related crimes are uniformly reported to the FBI. Therefore they could not be included in this study. 11

With the exception of the PPD data, the data used in this study were made available in hard-copy form. To generate electronic files that could be analyzed using a statistical software package, the hard-copy reports were electronically scanned into bitmap image files. The image files were then converted to ASCII (character) data by an optical character recognition program, the results of which were checked for accuracy both electronically and by hand. The data were checked again as the monthly files were aggregated by quarters, and then again after the quarterly files were merged into the master file that was used for the analysis.

Table 2 displays average quarterly crime counts for three different area types included in the study. The first row of the table shows that between 1992 and 1999, the mean number of violent crimes per quarter in the RDs targeted by the injunctions was 29.4. By way of comparison, the mean number of reported violent crimes per quarter in the average RD in the West Los Angeles neighborhood was about 11. 12 Part of this difference may be attributable to differences in population density; injunctions tend to be applied in high-density areas. Much of the difference, however, merely reflects the fact that injunctions are imposed in areas with very high levels of violent crime.

The second row reports average crime counts for RDs adjoining the target areas. These include all RDs that touch the boundaries of RDs in the target area. One can think of the adjoining RDs as the doughnut and the target-area

11 These data allow me to estimate the effect of the injunctions only on reported crime. I have no information on crimes that are not reported to the police. If the injunctions embolden residents to call the police more often when they see crimes occurring in their neighborhood, then the estimated effects of the injunctions on reported crime could understate the effects of the injunctions on the true level of crime.

12 This refers to the area bounded by the Santa Monica city limit on the west, the San Diego Freeway on the east, Santa Monica Boulevard on the north, and Pico Boulevard on the south. It is a low- to middle-income neighborhood that is fairly distant from the nearest injunction area.
RDs as the hole. These are the areas that I analyze to detect spillover effects. The table shows that the mean number of violent crimes per quarter in the adjoining areas is somewhat lower than that in the target areas.

The third row reports average crime counts for the neighboring RDs discussed above, which are defined as RDs whose boundaries touch the outer boundaries of the doughnut defined by the adjoining areas. These neighboring areas provide one of the comparison samples that I use to estimate the counterfactual, that is, how the level of crime would have changed in the target areas if the injunctions had not been imposed. The level of mean quarterly violent crimes per RD in the neighboring areas are almost identical to those in the adjoining areas and, therefore, lower than in the target areas.

Another feature of the data that is evident in Table 2 is their substantial variability. The standard deviation of the level of violent crimes is about 19, which amounts to nearly 80 percent of the full sample mean. This high degree of variability makes it essential to bring as much data as possible to bear on the question of the effectiveness of the injunctions. Sample size considerations play an important role in determining the length of the follow-up period to be used in the analysis.

It is important to note that the data that I analyze here are quarterly crime counts from each RD. I analyze crime counts, rather than crime rates, for a number of reasons. The first is practical. The population data one would need to produce crime rates are not available for police RDs. Census tract population data could conceivably be used for RDs that are coterminal with census tracts, but populations for other RDs would have to be imputed from census tract data using fairly arbitrary procedures. Moreover, census population data are available only for 1990. Population data for other years would have to be extrapolated, which likely would involve considerable error.

There is a more conceptual reason for analyzing crime counts as well. For large, self-contained geographic areas, such as countries, states, or metropolitan areas, the population of the area provides a measure of the number of people at risk of being victimized in that area. Thus, the crime rate, formed by dividing crime counts by the resident population, provides a rough measure of the average victimization risk faced by residents in that area.

For small areas such as police RDs, however, the resident population is a poor measure of the "risk set," that is, the number of persons at risk of being victimized in the RD. Many residents spend much of their time outside their home RD while working or shopping. At the same time, RDs with many

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13 This is why there are many more RDs in the adjoining areas than in the target areas.

14 Many target-area RDs are only partially covered by an injunction. If crime simply moves from the covered part of such RDs to the uncovered part, then my focus on adjoining RDs will cause me to underestimate the true spillover effect. Even if there were spillovers within the RD, the analysis would provide no evidence of them. At the same time, partial coverage would cause me to underestimate the target-area effect, since reductions in the covered part of the RD would be masked by increases in the uncovered part.
employers have daytime populations that well exceed their resident population, and those daytime populations are at risk of victimization at and around their place of employment. Thus, for small geographic areas such as police RDs, dividing crime counts by the resident population may provide a poor adjustment for differing risk sets among different RDs.

To adjust for the risk set in each RD, I take a statistical rather than data-based approach, as I discuss in the next section. The difference-in-differences methods that I use to estimate the effects of the injunctions account implicitly for all RD-specific characteristics that are constant over time. Over the relatively short time periods that are actually included in the analysis, factors such as daytime and resident populations may be nearly constant. Thus, the methodology I employ below controls implicitly for the risk set in each RD.

IV. DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE EFFECTS OF THE INJUNCTIONS ON REPORTED VIOLENT CRIME

With a suitable comparison sample, the effects of the injunctions can be estimated in a fairly simple manner using the difference-in-differences approach. This method contrasts the mean change in the level of crime within the target areas, before and after the injunction is imposed, to the contemporaneous change in the level of crime within the comparison area. Contrasting changes in this manner nets out the effects of factors other than the injunctions that drive common crime trends in both the target and comparison areas.¹⁵

In order to implement the difference-in-differences approach, however, it remains to define the “before” and “after” periods to be used in the analysis. All else equal, a longer preinjunction period is preferable to a shorter preinjunction period because a longer period provides a more reliable baseline against which to measure the change attributable to the injunction. At the same time, however, the longer the preinjunction period, the smaller the number of injunctions that can be included in the analysis, since several of the earlier injunctions were imposed shortly after the beginning of my sample period in 1992. In particular, extending the preinjunction period back more than 5 quarters before the imposition quarter would require me to drop the Blythe Street injunction, which was imposed in April 1993. Since the Blythe Street injunction was the subject of the only previous attempt to evaluate the effect of an injunction on reported crime, it seems desirable to retain it

### Table 3

**Mean Violent Crimes by Area Type and Difference-in-Differences Estimates of the Effects of the Injunctions on Violent Crime**

<table>
<thead>
<tr>
<th>Area</th>
<th>Preinjunction* (1)</th>
<th>Postinjunction* (2)</th>
<th>Difference (3)</th>
<th>Difference in Differences (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Target areas</td>
<td>27.68 (2.44)</td>
<td>22.69 (1.99)</td>
<td>-4.99 (.76)</td>
<td>-1.96 (.83)</td>
</tr>
<tr>
<td>Cell size</td>
<td>250</td>
<td>200</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjoining areas</td>
<td>22.57 (1.41)</td>
<td>19.78 (1.28)</td>
<td>-2.79 (.47)</td>
<td>.24 (.58)</td>
</tr>
<tr>
<td>Cell size</td>
<td>680</td>
<td>544</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Neighboring areas</td>
<td>21.78 (1.09)</td>
<td>18.75 (1.96)</td>
<td>-3.03 (.35)</td>
<td></td>
</tr>
<tr>
<td>Cell size</td>
<td>1,020</td>
<td>816</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Values in parentheses are standard errors that account for the presence of multiple observations per reporting district.

* Includes the 5 quarters preceding the quarter in which the injunction was imposed.

b Includes the quarter in which the injunction was imposed and the 3 following quarters.

In the main analysis sample. Therefore, I define the preinjunction period to include the 5 quarters preceding the imposition of the injunction.

In defining the postinjunction period, a longer period would allow one to assess both long-run and short-run effects of the injunctions. However, adopting a longer follow-up period would necessitate dropping a substantial number of injunctions from the sample. There are only 5 quarters of data between August 1998, when the Harpys injunction was granted, and the end of the sample period in 1999. For the Mara Salvatrucha, Shatto Park Locos, and Columbia Little Cycos injunctions, there are 6 quarters of follow-up data. In order to retain as many injunctions in the analysis sample as possible, I limit the follow-up period for the bulk of the analysis below to roughly 1 year, including the quarter in which the injunction was imposed and the 3 quarters that follow.

With these definitions at hand, Table 3 presents difference-in-differences estimates of the effects of the injunctions. The top row gives the level of mean quarterly violent crimes for target-area RDs before and after the imposition of the injunction. Whereas the level of violent crimes averaged 27.68 per quarter during the preinjunction period, they averaged only 22.69 in the postinjunction period. The difference, in column (3), is -4.99 crimes per

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16 ACLU of Southern California, supra note 4.

17 In preliminary analyses, I experimented with two other preinjunction periods, one of which included the 10 quarters preceding the injunctions and the other of which included the period from 5 to 10 quarters preceding the injunctions. The resulting estimates were quite similar to those presented below.

18 In preliminary analyses I also experimented with a 6-quarter follow-up period. The estimates were slightly smaller than those presented below, which suggests that the effects of the injunctions may dissipate over time. Because these estimates are based on a relatively small sample, however, they should not be regarded as providing conclusive evidence on the long-term effects of the injunctions.
quarter. Relative to the preinjunction mean, this represents an 18 percent decline in the level of violent crime.

Of course, it is implausible to attribute this entire decline to the effect of the injunctions. The bottom row of Table 3, like Figure 1, shows that the level of violent crime also decreased in the neighboring areas. The level of quarterly crimes in the postinjunction period averaged 18.75, as compared with 21.78 in the preinjunction period. Thus, the decline in the neighboring areas, which were not subject to the injunctions, was 3.03 crimes per quarter.

The difference-in-differences estimator, reported in column (4), uses the before-and-after change in the neighboring areas as a control for the before-and-after change in the target areas. The resulting estimate indicates that the injunctions led the level of violent crimes to decrease in the average target-area RD by 1.96(= −4.99 − (−3.03)) crimes per quarter. The estimate is statistically significant. It indicates that the injunctions led the level of violent crime to decrease in the target areas by about 7 percent of the preinjunction mean.

The difference-in-differences approach can also be used to estimate spillover effects by comparing changes within the adjoining areas to changes within the neighboring areas. The estimate, in the second row of column (4), is .24, which is consistent with small spillover effects. It is statistically insignificant, however, which is consistent with the null hypothesis that the true spillover effect is zero.

Thus, under the assumption that the neighboring areas provide an adequate comparison sample, the estimates indicate that the injunctions significantly reduced the level of violent crime in the target areas without causing spillovers. The question remains, however, whether the neighboring areas provide an adequate comparison sample. The potential problem with the neighboring areas is that the level of preinjunction violent crimes is lower there than in the target areas.19

This raises the possibility that “mean reversion,” rather than the injunctions, is responsible for the negative difference-in-differences estimate in Table 3. That is, it may be that the injunctions are imposed while the target areas are experiencing particularly high levels of violent crime. If so, then one might expect the level of crime to decrease after the injunctions are imposed simply because it is more likely to revert to its mean level, rather than to persist at an unusually high level. Thus, if the injunctions are typically imposed during periods when the level of target-area crime is higher than average, then mean reversion could lead one to erroneously attribute to the injunctions a decline in the crime rate that would have occurred even if the injunctions had not been imposed.

The key to dealing with this issue is to construct an alternative comparison sample from neighborhoods that experience similarly high levels of violent crime.19

19 See Table 3, column (1).
crime during the same period of time. To do this, I employ a procedure that I refer to as "percentile matching." I construct a separate comparison sample for each target-area RD that consists of all RDs whose crime levels were similar to that of the target-area RD during the calendar time period corresponding to the preinjunction period in the target area. To determine similarity, I match RDs according to their percentile in the violent crime distribution during the 5 quarters preceding the injunction. The percentiles are tabulated over all the RDs within the entire four-jurisdiction sample.  

To be specific, consider the process of drawing the matched comparison sample for the 18th Street (Jefferson Park) injunction. Since the injunction was imposed in the third quarter of 1997, I first compute the level of mean violent crimes between the second quarter of 1996 and the second quarter of 1997 (inclusive) for all RDs in the full four-jurisdiction sample. I then assign to each RD its percentile in the resulting distribution of violent crimes. For each of the two RDs in the target area, I construct a comparison sample consisting of all RDs with the same percentile score. That is, each target-area RD is matched to all other RDs with the same percentile score in the distribution of violent crimes over the period extending from the second quarter of 1996 through the second quarter of 1997. Since there are 1,261 RDs in the four-jurisdiction sample, this results in a comparison sample with roughly 12 times more RDs than the target areas. By drawing the comparison sample in this way, I ensure that the difference-in-differences procedure compares the target-area RDs to comparison-sample RDs that were experiencing similarly high levels of violent crime at the same time. Since crime levels are contemporaneously high in both the target and matched comparison areas, there is less of a chance of attributing to the injunctions any reduction in the level of crime that is merely the result of mean reversion.

Table 4 presents difference-in-differences estimates of the effects of the injunctions on violent crime based on the percentile-matched comparison samples. The first row presents data for the target areas. With the exception of the difference-in-differences estimate in the final column, these are the same values that appear in the first row of Table 3.

The second row presents data from the comparison areas that were percentile matched to the target-area RDs. The preinjunction means show that the matching procedure indeed results in a comparison sample whose preinjunction crime level is similar to that in the target area. The mean number of preinjunction crimes per quarter in the matched comparison areas was 26.77, which is only .91 crimes fewer than that in the target areas. In contrast, 

\[ \text{Since this procedure involves sampling with replacement, the same comparison RD may be matched to more than one target-area RD. This results in statistical dependence among the observations in the matched comparison sample. Throughout the analysis, I employ standard errors that account for such dependence.} \]

\[ \text{I exclude from these comparison areas any target-area or adjoining-area RDs from other injunctions.} \]
preinjunction crimes in the neighboring areas that were used as the comparison sample in Table 3 averaged only 21.78 per quarter, nearly 6 crimes fewer than in the target areas.

The level of violent crime in the matched comparison areas decreased by 3.49 crimes per quarter between the pre- and postinjunction periods, compared with the decline of 3.03 crimes per quarter in the neighboring areas, shown in Table 3. As a result, the difference-in-differences estimate based on the matched comparison sample is smaller than that based on the neighboring-area comparison sample. The matched comparison sample yields a difference-in-differences estimate of -1.51, with a t-statistic of -1.91. This estimate is smaller than the one based on the neighboring-RD comparison sample, which is consistent with the mean-reversion hypothesis, but it is still significant and amounts to 5 percent of the preinjunction mean in the target areas.

To estimate spillover effects, I constructed a separate percentile-matched comparison sample for the adjoining-area RDs in the same manner that I used to construct the matched comparison sample for the target-area RDs. Preinjunction violent crimes averaged 21.58 per quarter in the comparison sample that was matched to the adjoining areas, which is similar to the preinjunction mean of 21.78 in the neighboring areas, shown in Table 3. Since the preinjunction means are so similar in the two comparison areas, one might expect the corresponding difference-in-differences estimates of spillover effects to be similar as well. Indeed they are: the spillover effect based on the matched comparison sample is -.32, whereas the estimate based
TABLE 5
DIFFERENCE-IN-DIFFERENCES ESTIMATES OF THE EFFECTS OF THE INJUNCTIONS ON VIOLENT CRIME, EXCLUDING THE RAMPART INJUNCTIONS

<table>
<thead>
<tr>
<th></th>
<th>Target area</th>
<th>Adjoining area</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Percentage-Matched Comparison Sample</td>
<td>-2.50 (1.12)</td>
<td>-16 (0.64)</td>
</tr>
<tr>
<td>Percentage-Matched Comparison Sample</td>
<td>2.349</td>
<td>13,662</td>
</tr>
</tbody>
</table>

NOTE.—Values in parentheses are standard errors that account for the presence of multiple observations per reporting district.

on the neighboring-area comparison sample is .23. Both estimates are small, both absolutely and in relation to their standard errors.

An important issue not yet addressed concerns the effects of the Rampart scandal on the analysis. Three of the injunctions in the sample are in the Rampart district, where police have been accused of widespread corruption beginning around 1997.\textsuperscript{22} Since this corresponds to the time that the first Rampart-area injunction was imposed, one might be concerned that the effects of the injunctions would be confounded with the effects of police corruption. Precisely how such corruption should affect the results is not clear. On the one hand, one might be concerned that widespread police corruption would be indicative of aggressive law enforcement generally, which could exaggerate the apparent effects of the injunctions. On the other hand, if corrupt police file false reports to obtain injunctions that otherwise would not be warranted, or if the corruption scandal is indicative of lax management generally, then it could attenuate the estimated effects of the injunctions.

To examine this issue, I present results in Table 5 that are based on samples that exclude the Rampart injunctions.\textsuperscript{23} To save space, I report only the difference-in-differences estimates themselves, rather than the full table of group-specific before-and-after means. The estimates based on the neighboring-area comparison sample indicate that the injunctions reduce the level of target-area violent crimes by 2.91 per quarter in each target-area RD. The estimate based on the percentage-matched comparison samples indicates a reduction of 2.5 violent crimes. Both of these estimates are larger than the corresponding estimates from Tables 3 and 4. The estimated spillover effects, in contrast, are fairly similar.

\textsuperscript{22}Robert J. Lopez & Rich Connell, Targets of Gang Injunctions Were Named by Officers in Police Probe, L.A. Times, September 23, 1999, at A1 (noting that these are the 18th Street (Pico-Union), Shatto Park Locos, and Columbia Little Cocos injunctions, the latter two which are actually two separate target areas covered by the same injunction order).

\textsuperscript{23}In column (1), I simply dropped the Rampart-area injunctions from the sample. In column (2), I have dropped the Rampart-area injunctions and constructed fresh comparison samples from which all Rampart-area RDs were excluded from the beginning of the matching procedure.
Tables 6 and 7 provide some more detail on the effects of the injunctions by providing difference-in-differences estimates for each specific offense that is included in the category of violent crime. Table 6 presents results based on the neighboring-area comparison sample, whereas Table 7 presents results based on the percentile-matched comparison samples. In each table, panel A presents estimates from the full sample, whereas panel B presents estimates from the samples that exclude the Rampart injunctions. As in Table 5, I present only the difference-in-differences estimates in order to save space.

Consider first the results based on the neighboring-area comparison sample that are reported in Table 6. Column (1) reports estimates for murder. Neither sample shows any evidence that the injunctions reduce murders in the target areas. Both estimates are small and statistically insignificant. The same can be said for rape, the results for which are reported in column (2).

Columns (3) and (4) report results for robbery and assault. Although the target-area effect for robbery is insignificant in the full sample, it is significant at the 5 percent level in the sample that excludes the Rampart injunctions. Both estimates for assault are statistically significant. The same can be said for rape, the results for which are reported in column (2).

The results regarding spillover effects are quite consistent across the various offense categories, comparison groups, and subsamples.
results are indicative of spillover effects. Indeed, both here and in Table 5, almost as many of the estimated spillover effects are negative as are positive.

Overall, the results from this analysis indicate that the injunctions reduce the level of violent crime in the average target-area RD by roughly 1.5–3 crimes per quarter, most of which is accounted for by a reduction in assaults. In relative terms, this amounts to a decline of roughly 5–10 percent. In the next section, I compare these effects to those of other recent law enforcement interventions.

V. COMPARING THE INJUNCTIONS TO OTHER RECENT LAW ENFORCEMENT INTERVENTIONS

As was mentioned in the Introduction, the injunction strategy combines a tool that is new to law enforcement—civil injunctions—with elements that appear in other recent law enforcement interventions, such as community policing, interagency cooperation, and place-based enforcement efforts. Here I review briefly what is known about such strategies in order to place the injunctions in perspective.

The principal objective in community policing is to increase information flows and cooperation between police and community residents. It comes in many varieties, from Neighborhood Watch programs to programs where citizens sit on police boards with policy-making powers.24 There is little reliable

<table>
<thead>
<tr>
<th></th>
<th>Murder (1)</th>
<th>Rape (2)</th>
<th>Robbery (3)</th>
<th>Assault (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>A. FULL SAMPLE</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Target areas</td>
<td>-.11 (.06)</td>
<td>-.02 (.08)</td>
<td>-.33 (.40)</td>
<td>-1.05 (.55)</td>
</tr>
<tr>
<td>Adjoining areas</td>
<td>.02 (.03)</td>
<td>.04 (.04)</td>
<td>-.33 (.24)</td>
<td>-.04 (.32)</td>
</tr>
<tr>
<td>Mean of dependent variable</td>
<td>.24</td>
<td>.51</td>
<td>7.98</td>
<td>13.04</td>
</tr>
<tr>
<td>B. EXCLUDING RAMPART</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Target areas</td>
<td>-.04 (.08)</td>
<td>-.13 (.09)</td>
<td>-.83 (.60)</td>
<td>-1.50 (.71)</td>
</tr>
<tr>
<td>Adjoining areas</td>
<td>.04 (.04)</td>
<td>.01 (.06)</td>
<td>-.13 (.31)</td>
<td>-.07 (.49)</td>
</tr>
<tr>
<td>Mean of dependent variable</td>
<td>.26</td>
<td>.50</td>
<td>8.44</td>
<td>13.14</td>
</tr>
</tbody>
</table>

NOTE. — Values in parentheses are standard errors that account for the presence of multiple observations per reporting district.

24 Bayley, supra note 2.
evidence that community policing reduces the level of crime.25 Most such programs are implemented on a city-wide basis, which leads to difficulties in constructing plausible counterfactuals.

Among interventions relying on interagency cooperation, the Boston Gun Project is probably the best known. It first used intelligence from several law enforcement agencies to identify chronically offending gang members. It then communicated to those persons that police would respond swiftly to future reports of gun violence and that the penalties for such violence would effectively increase.26 The cooperation of many levels of law enforcement—from police to state and federal prosecutors to probation and parole agencies—was instrumental in enabling the project’s staff to make good on its threats. The project has been credited with a nearly 60 percent reduction in youth homicides.27 Like many citywide interventions, however, the evaluation lacked a control group, raising the question of whether the intervention was solely responsible for the observed decrease in the level of crime.

Place-based enforcement interventions typically involve directed patrols by police. Such programs vary in their emphasis, but like the gang injunctions, they focus on small geographic areas with particularly acute crime problems. Because these interventions focus on small areas, it is often possible to evaluate them using quasi-experimental or even experimental methods.

One program in Kansas City deployed special police patrols that focused on seizing illegal guns within an 8-by-10-block neighborhood. Compared with a similar neighborhood, the intervention reduced the level of gun crimes in the target area by nearly 50 percent over a 6-month period.28 Another program in Minneapolis increased patrols within a randomly selected group of 55 crime “hot spots.” As a result, the number of calls to police regarding “hard crimes” decreased by about 5 percent in comparison to the hot spots not selected for increased patrol.29 The patrols also reduced disorder witnessed by trained observers by 25 percent.30 A Jersey City experiment, which also

28 Hard crimes include holdups, burglaries, shootings, stabbings, auto thefts, thefts from autos, assaults, and rapes.
29 Lawrence W. Sherman & David Weisburd, General Deterrent Effects of Police Patrol in Crime “Hot Spots”: A Randomized, Controlled Trial, 12 Just. Q. 625 (1995) (disorderly events include fights, drug sales, solicitations for prostitution, playing of loud music or shouting, urination, rummaging through garbage cans, and other “signs of crime.”).
included small target areas that were randomly assigned to treatment, reduced reported crime by 32 percent and calls to police by 14 percent.\textsuperscript{31}

In comparison with these other place-based enforcement efforts, the effects of the gang injunctions are relatively small. This may be the result of differences in patrolling effort. In all of the place-based interventions cited above, the evaluators document a substantial increase in police presence within the targeted areas. Although there are no data on injunction-related patrols, the smaller effects of the injunctions are consistent with anecdotal evidence from prosecutors that, in many cases, police patrols changed little in response to the injunctions.

Of course, it would be most useful for policy purposes to rank the interventions by their cost-effectiveness rather than merely by their effectiveness in reducing the level of crime. Unfortunately, cost data are largely unavailable. Of the intervention studies cited above, only one provides any cost-related data, and it is incomplete.\textsuperscript{32} Similarly incomplete is information about the costs of the injunctions, largely because police have not been required to track injunction-related enforcement activities. Cheryl Maxson, Karen Hennigan, and David Sloane report that the preparations for the Inglewood injunction required 1 year's effort on the part of the deputy district attorney in charge, but they provide no other information about preparation costs or enforcement costs.\textsuperscript{33} Clearly, the lack of information needed to assess the cost-effectiveness of the various interventions constitutes an important gap in knowledge that merits greater research attention in the future.

Another area requiring greater research concerns the civil rights implications of the various interventions. Critics of the injunctions have argued that they amount to an unconstitutional breach of the defendants' free speech rights.\textsuperscript{34} Concerns have also been raised that the injunctions will be viewed by police as license to detain anyone who happens to resemble a gang member, regardless of whether he is actually named in the injunction.

Similar issues arise in any law enforcement intervention, however. Moreover, the injunctions involve at least some degree of judicial supervision, whereas directed-patrol interventions typically involve none. Therefore, it is conceivable that the injunctions could provide greater civil liberties protections than other place-based enforcement efforts, although more work on this topic is clearly needed.

\textsuperscript{31} Anthony Braga \textit{et al.}, Problem-Oriented Policing in Violent Crime Places: A Randomized Controlled Experiment, 37 Criminology 541 (1999).

\textsuperscript{32} Sherman & Rogan, supra note 28 (reporting that the Kansas City intervention involved 4,512 person-hours of police patrol time, but they provide no information on planning or prosecutorial expenses).

\textsuperscript{33} Maxson, Hennigan, & Sloane, supra note 5.

VI. Conclusions

The estimates presented above, which are based on 8 years’ worth of data drawn from four law enforcement jurisdictions, suggest that civil gang injunctions lead the rate of violent crimes to decrease by somewhere between 1.5 and 3.0 crimes per quarter in the average target-area RD during the first year after they are imposed. In relation to the average level of crime in these RDs in the period preceding the injunctions, this amounts to a decline of roughly 5–10 percent. Most of this decline stems from reductions in assault, which is the most prevalent form of violent crime.

The injunctions thus represent an addition to the list of place-based intervention strategies that appear to be effective in reducing the level of serious crime. The effectiveness of these interventions varies, with the injunctions near the low end, but their costs presumably vary as well, as does their potential for civil rights abuses. Unfortunately, the literature provides little data on either the costs or civil rights implications of any of the place-based interventions. These represent important topics for future research.

Bibliography


