Vigilante Neighborhoods Combat Crime: Business Improvement Districts and the Private Provision of Public Safety

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Abstract

Many urban commercial areas suffer from crime, vandalism, and lack of maintenance, and property owners have turned to Business Improvement Districts (BIDs) to provide public goods to solve these problems. A BID is formed when a majority of property owners in a given neighborhood votes in favor of additional taxation; state law makes these contributions mandatory for all owners. Can these BIDs reduce crime? Across a variety of methods – using fixed effects, comparing BIDs to neighborhoods that almost formed BIDs, using propensity score matching, comparing BIDs to their neighbors and near neighbors, and instrumenting for BID adoption with the era of neighborhood development - I find that BIDs are associated with crime declines of 5 to 9 percent. More strikingly, these declines are purchased cheaply: BIDs spend $3,000 to avert one crime, compared to the $5,000 the LAPD spends per crime committed, or the $20,000 social cost of a violent crime.

1 Introduction

Even after the boom years of the 1990s, crime, blight, and stagnant property values are familiar symptoms of many neighborhoods in major American cities. Individual property owners' desire to invest in troubled neighborhoods is thwarted by their reluctance to do so alone. Owners recognize that individual investment, particularly with regard to crime, is likely futile. To solve such problems of collective action, a neighborhood institution called a Business Improvement District has become popular, in which property owners volunteer for additional taxation in order to provide neighborhood-wide services.

Specifically, a Business Improvement District (BID) is formed when property owners in a given neighborhood vote to tax themselves in order to provide goods of mutual interest, usually cleaning, security and maintenance. What makes this extraordinary form of collective action possible is the proviso that once a majority votes in favor, all owners are legally bound to pay the tax.

This BID provision is an illustration of the private provision of public goods. Economists are concerned with the causes of private provision, the distribution of provision adopters, and the consequences of adoption (cite), and this study of BIDs will shed light on some of these issues. To municipal officials, BIDs may seem like a mechanism for near-free funding of municipal improvement, in exchange for a small cecession of sovereignty. Additionally, BIDs may present a welcome alternative to the dubiously reviewed efforts of Tax Increment Financing Districts, Enterprise Zones, Empowerment Zones, and local redevelopment zones (cite).

Of all the neighborhood ills which BIDs and policy makers tackle, both research and anecdotal evidence suggest that crime is among the most serious. In commercial neighbor-

hoods, crime keeps customers away, property values low, and expectations lower. Logically, then, one of the major stated and budgetary goals of BIDs is reducing crime. This paper evaluates how successful Los Angeles city BIDs are at this task.

Because BID adoption is a neighborhood's choice and not an assignment, BIDs are not located randomly across the city, and this presents difficulties in estimating the causal influence of BIDs on crime. This paper uses three major strategies to combat this selection problem: a fixed effects approach, matching, and an instrumental variables framework. If BID adoption is determined by time-invariant neighborhood factors, then the fixed effects approach can give an unbiased estimate of BIDs' influence on crime. In the likely event that BIDs are also caused by time-varying factors, I compare BIDs to neighborhoods that seriously considered forming BIDs, which controls non-parametrically for the causes of BID adoption. I buttress these estimates with two other types of matching: with a propensity score of pre-BID conditions, using observable characteristics to control for neighborhood similarity, and by proximity. Finally, I estimate BIDs' impact on crime by instrumenting for BID adoption with the neighborhood's era of development, which is correlated with the demand for BIDs, interacted with a major law change affecting BIDs.

In order to do this evaluation, I have assembled a novel dataset from the Los Angeles Police Department (LAPD) of crimes by neighborhood over a 13 year period. I have combined these data with information on properties from the Los Angeles County Assessor and a dataset I collected with information on BIDs from city council files and interviews. These data are all at a neighborhood level, where the median neighborhood size is 0.8 square kilometers.

Across all these estimations, BIDs are associated with large declines of 5 to 9 percent in total crime, where the bulk of this decline is attributable to decreases in violent crime.

However, if we do not know how BIDs reduce crime, these results are hard to interpret. Analysis of police enforcement patterns suggests that BIDs are only modestly, if at all, associated with changes in police enforcement, and that BIDs' success is not achieved at the cost of lowered police attentiveness to other areas. Left open is the impact of BID adoption on property owners' overall willingness to contribute to municipal taxation.

The drop in crime associated with BIDs is achieved very cheaply. Compared to the social cost of $20,000 per victim from a violent crime, BIDs spend approximately $3,000 to avert one crime, and more than half of crimes averted are violent ones [8].[1] Or, compare this to the LAPD, which averages roughly $5000 on patrols per reported crime. By opting out of municipal police provision, BIDs pay less to reduce crime in their neighborhood.

2 What is a BID?

Though BIDs in the state of California have existed since 1943, they did not arrive in the City of Los Angeles, the focus of this paper, until 1994. In 1994 the California State Legislature passed a law allowing for the taxation of property owners to fund neighborhood improvements; previous legislation had allowed only for the taxation of merchants.[2] As the residual claimants on the land, property owners have the most to gain from improvement, and were viewed as the most likely financiers, as well as those with the deepest pockets. After the passage of this 1994 law, and in response to neighborhood demand, the city set up an administrative apparatus to perform the city's end of BID administration.

In order to establish a BID, property owners in a neighborhood decide upon a boundary,

[1] This estimate includes tangible and quality-of-life costs of crime, but does not include the costs incurred by the criminal to the penal system, or more indirect social costs.

[2] This new law escapes the stringencies of Proposition 13 by calling the tax an assessment.

assessment schedule and budget for the district and attempt to convince other neighbors that they, too, should invest. Properties in BIDs may be assessed in any way comensurate with the benefits that property receives; usually the assessment is some combination of building square footage, lot square footage, and front footage. If a majority of assessment-weighted votes are cast in favor of the BID, it is established and taxes are mandatory for all owners within the district. The BID then functions as a not-for-profit corporation.

Los Angeles' 30 BIDs are shown in the first panel of Figure 1, and have a mean BID adoption year of 1999. From this map, it is clear that these BIDs are quite small. Exactly how small they are is presented in Table 1. BIDs are usually much smaller than a square kilometer, and they make up less than 2 percent of the area of the city of Los Angeles. The bottom half of the table details BID expenditures.[3] In 2002, BIDs spent almost 19 million dollars, with about a third of that going to security; the remaing funds went to a mix of marketing, cleaning, special projects and administration. The 19 BIDs that do spend money on security account for the vast majority of BID spending, and the median BID in this group spends a little over $200,000 per year to combat crime, with a few BIDs spending a great deal more.

Compared to the hundreds of millions in federal monies spent on the Section 8 housing program or Community Development Block Grants, these numbers may seem small. However, when compared to city spending, BID expenditures are large local investments. For example, the Chinatown BID, at 0.3 kilometers square, in addition to spending on security patrols, spends $280,000 annually on cleaning and maintenance. In comparison, the city of Los Angeles spends $55,000 per square kilometer.[4] Or consider the Downtown Center BID,

[3]These are budgeted expenditures, not actual expenditures. Actual expenditures are much more difficult, and in most cases probably impossible, to obtain.

[4]Source: LA City Budget: http://www.lacity.org/cao/budsumm02-03.pdf

the largest BID by expenditure, which spends approximately $1 million per square kilometer on security. It adds fully 25 percent to the $4.3 million per square kilometer that the LAPD spends in that area.[5] Outside the downtown areas, the figures can be even more striking: the Hollywood Entertainment District BID covers roughly three-quarters of a square kilometer and its $1.4 million per square kilometer of security spending roughly doubles LAPD expendiures of $1.3 million per square kilometer in the same area. Thus, though BID expenditures may be small in total, they are locally substantial, sometimes doubling the city's own expenditures. Security expenditures, particularly in the high spending BIDs, go to either hire private security guards, frequently retired police officers, or to employ entire crews of colorfully-shirted "neighborhood ambassadors" who patrol the streets, help tourists, deter panhandlers, and communicate via walkie-talkie with the LAPD.

3 Theoretical Framework

From this background on the BID institution, whether or not a neighborhood adopts a BID is clearly a choice based on local conditions. This presents obvious concerns in estimating the impact of BIDs on crime. However, if causes of this non-random selection into BIDs can be identified, the estimation can control for them and resolve the estimation dilemma. This section describes why and where BIDs form; I explore this issue in much greater detail in a separate paper [2].

When explaining the establishment of any institution, one must consider both the supply and demand sides. In the case of BIDs, I argue that a supply side constraint in the provision

[5]Source on LAPD is the Statistical Digest, http://www.lapdonline.org/pdf-underscore-files/digest/2003/2k3-underscore-digest.pdf, pg. 6; the city spends about 2 billion dollars on the police, and roughly half of that goes to patrols.

of public goods at first kept neighborhoods from private provision. Then, after the BID law obviated this constraint, neighborhood demand factors determined adoption patterns.

Turning first to the supply problem, consider the BID as an institution designed to provide public goods such as security, neighborhood cleanliness and neighborhood reputation. The provision of exactly such collective goods or goals is described in Mancur Olson's *Logic of Collective Action* [10]. Olson's collective goods are defined such that the cost of group provision is much lower than the high, and sometime infinite, cost of individual provision. Olson notes that these group goods are frequently not provided, even when it is individually rational to do so, because the free rider problem dominates. He suggests that groups are able to provide public goods under only two circumstances: when the benefits of the public good are excludable for non-members (which implies that the good cannot be a true public good), or if all group members can be coerced into contribution.[6]

The first Olsonian condition is impractical for neighborhoods – non-group members cannot be excluded from the benefits of declining crime, or from increases in neighborhood desirability. However, the structure of the BID law is an exact analogy to Olson's second condition, in which membership is coerced. In the BID format, this means that the minority which does not vote in favor of the BID is still required to pay the BID tax.

If the passage of the BID law did indeed relax a constraint on the supply of public goods, neighborhoods should provide public goods after the law when they did not before. This is exactly what happens. After the passage of these laws, neighborhoods adopt BIDs and provide cleaning, security and beautification.

If a neighborhood does, then, solve an Olsonian collective action dilemma via compulsory

[6]In the BID context, an alternative to solving the collective action problem via cooperation is to structurally eliminate the need for cooperation. In the 1960s, cities across the country used the power of eminent domain to seize properties. Cities re-grouped the small parcels into large ones and sold them to developers. Amid dual charges of racism and developer cronyism, this brand of urban renewal fell into disfavor.

membership, what are the consequences of this solution? Economic theory tells us that rational property owners should receive compensatory benefits for their investment in the BID – otherwise they would not make the investment. Though an owner could opt out of the tax by selling his property, any future buyer of that property would have to pay the tax, and it would be reflected in the sale price of the property. The compensatory benefits a property owner gains should be reflected in the value of his property and should also be reflected in important predictors of property value, such as crime (cite Thaler paper). Indeed, because BIDs specifically target crime, the thoery suggests that crime should drop in BIDs.[7]

However, despite this potential to combat crime and other ills, not all neighborhoods adopt BIDs after the supply constraint is lifted. This suggests that neighborhoods vary in their demand for goods that must be provided collectively. One element of this demand – the neighborhood's era of development – will be explored a greater length in the instrumental variables section.

In sum, BIDs result from the confluence of the relaxation of a supply constraint – the the adoption of the BID law by the California Legislature – and the revelation of demand for BID services. After BID adoption, theory suggests that crime should decline in BID-adopting areas.

[7]All this assumes that the city will maintain police services as before. It is possible that the city could choose to shift resources from neighborhoods served by BIDs. If this police departure raises crime, the predicted impact of BIDs on crime is unclear. What seems more likely, from the anecdotal evidence and interviews, is that BID neighborhoods are able to more effectively leverage the same amount of police services. For example BID security guards could do the work of apprehending criminals, and have the police perform the formal arrest. If the city does slacken enforcement in BID neighborhoods, BIDs may not be associated with declines in crime; if the police make no changes or increase enforcement, BIDs should be associated with crime declines.

4 Estimation Strategies

4.1 Fixed Effects

The fundamental difficulty in estimating BIDs' impact on crime stems from the fact that BIDs are not randomly assigned. Additionally, crime declined across the board in the 1990s, and any pattern of crime behavior in BIDs must be distinguished from this overall trend. This section presents a fixed effects estimation that deals with the most obvious causes of bias in the estimation.

The initial specification looks at crime before and after BID adoption, and the unit of analysis is the LAPD police reporting district, which is a census tract or smaller (more on this in the data section). The regression controls for time-invariant characteristics at the reporting district level (rd_i), annual city-wide shocks (year_t), and wider area-level trends ($\text{area trend}_{a,t}$).

The reporting district fixed effects control for many time-invariant, or very slow-changing, neighborhood characteristics that could likely effect the level of crime, such as distance to the freeway, zoning patterns, and proximity to wealthy neighborhoods and customers. These fixed effects should also capture time-invariant or slow-changing neighborhood characteristics that determine BID adoption. For example, given the slow turnover of commercial property ownership, the owners may be considered fixed; the level of neighborhood coordination, or personality conflicts might also be in this fixed effect. Also, BIDs are adopted at different times ($\text{BID}_i * \text{after}_{i,t}$), and this timing provides identification of the BIDs' effect. If this timing is determined by neighborhood-specific, unchanging characteristics such as the level of neighborhood organization, then it is controlled for by the fixed effect. In sum, if one believes that BID formation is caused only by time-invariant factors, then this method

effectively eliminates the selection problem.

The city-wide year fixed effects allow for estimation of the influence of BIDs net of non-linear city-wide trends in crime. These city-wide shocks to the path of crime could be due to changes in overall police budget or strategy, or city-wide shifts in demographic profile. To control for the possibility that all parts of the city are not evenly affected by crime declines, I include area-level trends for the 18 LAPD police areas, area $\text{trend}_{a,t}$, and the model drops a second year dummy.

With all these elements, the basic model is

$$\text{crime}_{i,a,t} = \beta_0 + \beta_1 \text{BID}_i * \text{after}_{i,t} + \beta_{2,t}\text{year}_t + \beta_{3,i}\text{rd}_i + \beta_{4,a}\text{area trend}_{a,t} + \epsilon_{i,a,t} \qquad (1)$$

If reporting districts with BIDs are associated with crime decline, then β_1 will be negative.

If this specification does not adequately account for all BID-forming attributes, this non-random assignment may well lead to a correlation between the BID variable on the right-hand side of the estimation and the error. But in what direction does this bias go? If BIDs are adopted in neighborhoods that are already improving along some dimension, then OLS will overstate crime declines. If BIDs are adopted as a desperation measure, as one interviewee suggested [11], then OLS will understate the difference in crime.

4.2 Matching

To address possible time-varying causes of BID formation, I present three separate matching strategies : matching BIDs with almost-BID-forming neighborhoods, matching BIDs via propensity score matching, and matching BIDs with their neighbors and their neighbors' neighbors. Finally, in the event that concern lingers, I use the neighborhood's era of devel-

opment interacted with the passage of the BID law to instrument for BID adoption in the neighbors sample.

Note that because the estimation framework already controls for time-invariant reporting district characteristics via a fixed effect, the only way to further address the selection problem is through the identification of a time-varying predictor of BID formation – as in the instrumental variable estimation – or by dropping non-comparable observations from the estimation via matching.

4.2.1 Almost BIDs

Ideally, a BID should be compared with a neighborhood with the exact same propensity to form a BID, but which did not for an exogenous reason. This comparison would put all observations on an equitable, though non-quantifiable, level of BID-forming propensity.

The first obvious source for Almost-BIDs would be neighborhoods that just voted against BID adoption. However, as of 2003 in the city of Los Angeles, no BID has ever lost an adoption vote. This BID voting success is not due to widespread BID adoption. Instead, it is due to the fact that neighborhoods which begin to consider a BID, and which realize that a BID vote will not win, drop the issue. Instead of using voting behavior, then, I identify 26 neighborhoods that seriously considered adopting a BID, 4 of which adopted a BID after the end of my sample in 2002 (more on the construction of this sample in the data section), and some of which remain in the process of BID formation. Some of the reasons for never forming a BID seem completely exogenous: personality conflicts ("they hated each other"), or the BID consultant died.

Assume for a moment that BIDs and Almost-BIDs are identical save for the adoption decision. If BIDs are adopted by improving neighborhoods, and a negative coefficient of

β_1 in the previous specification is due entirely to this effect, then the adoption of a BID should have no effect on crime relative to the Almost-BIDs. If the converse is true, and BIDs are adopted in neighborhoods which are declining, then the fixed effect specification may understate the decline in crime, and these results may be larger than the fixed effect ones. Finally, if neighborhoods choose not to adopt a BID because conditions improve on their own – in other words, because the institution is useless – then relative to the Almost-BIDs, reporting districts with BIDs should have no discernable pattern in crime behavior. Thus, the Almost BIDs sample provides a powerful test of the importance of BIDs. In order to implement this test, I re-estimate Equation 1 using only BIDs and Almost BIDs.

4.2.2 Propensity Score Matching

The previous section matches neighborhoods along the unobservable dimension of BIDness. Propensity score matching is a quantitative alternative to this, which matches treated reporting districts with reporting districts with similar pre-BID behavior.

Ideally, the propensity score would pair BIDs with reporting districts with similar potential for development success. However, factors that predict land price appreciation are as difficult to find as factors that allow investors to beat the stock market – in other words, potential is very difficult to quantify. In the absence of a quantifiable measure of neighborhood potential, I match neighborhoods propensity to form a BID with a logit model in terms of pre-BID – 1994 and earlier – crime behavior. I then refine this match by using not just average pre-BID behavior, but the trend and mix of pre-BID crime.

Matching via propensity score on pre-BID levels of crime addresses whether or not crime declines in BIDs relative to other, similar, neighborhoods. This method also addresses whether the crime decline found in BIDs is attributable to their high initial levels of crime;

one might argue that the marginal cost of eliminating crime is lower at higher levels of crime. Propensity score matching on annual levels of violent and non-violent crime compares BIDs to reporting districts with similar, non-linear trends and mix of pre-BID behavior.

In order to present matching results in a format similar to the other results in this paper, I use the combination propensity score-regression method as discussed in Imbens [5]. Let $e(X_i)$ be the propensity score from estimating a logit model of BID adoption as a function of pre-BID covariates X_i. The regression weights are then

$$\lambda_i = \sqrt{\frac{\text{BID}_i}{e(X_i)} + \frac{1 - \text{BID}_i}{1 - e(X_i)}} \; .$$

Using these weights, I re-estimate Equation 1. As before, theory argues that β_1 should be negative – that BIDs are associated with crime decline.

4.2.3 Geographic Matching

In addition to comparing BIDs to neighborhoods with similar crime behavior pre-BID, I also compare changes in crime in BIDs with their neighbors' changes in crime. Proximity, as embodied by these neighboring districts, controls for a variety of possible conditions that could affect both BIDs and their neighbors. For example, within these neighbor-groups, the preferences of the city council member, changes in the official in that office, the quality of the local police administration, the income of residents, the level of commercial rents, and the responsiveness of the neighbors to crime is, if not constant, then substantially more constant than in the sample at large. If these effects are indeed constant between the BID and its neighbors, comparing BIDs explicitly to their neighbors nets them out. In terms of the examples above, direct neighbors are likely better controls, but they are also more

subject to the contamination of spillovers than second neighbors, a subject to which I will return below.

Results from this geographic matching are also of interest because they are probably how property owners roughly judge the success of their BID investment. Certainly when BIDs ask consultants to evaluate their work, these consultants compare the BID with its directly surrounding area.

To construct the geographically matched sample, I identified reporting districts adjacent to any BID reporting districts, and called those the first neighbors of BIDs.[8] Similarly, I identified the adjacent neighbors of these first neighbors, and call them second neighbors.[9] These neighbors are pictured in Figure 5, where BIDs are darkest, first neighbors are lighter, second neighbors are lightest, and non-neighbor reporting districts are unfilled. The 124 BID reporting districts have 291 first neighbors and 472 second neighbors.[10]

In addition, with geographic matching there is a clear "after" for untreated observations, which allows for a true difference-in-difference estimation. Specifically, I add a dummy, $\text{after}_{i,t}$, that is 1 after the treatment for both the treated and the matched untreated. Using this sample, I then estimate

$$\text{crime}_{i,a,t} = \beta_0 + \beta_1 \text{BID}_i * \text{after}_{i,t} + \beta_2 \text{after}_{i,t} + \beta_{3,t} \text{year}_t + \beta_{4,i} \text{rd}_i + \beta_{5,a} \text{area trend}_{a,t} + \epsilon_{i,a,t} \ . \quad (2)$$

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[8] Some BID-adjacent reporting districts are themselves BIDs, and I excluded those BIDs from this sample.

[9] Within the same BID, I do not allow a reporting district to be both a first and second neighbor. The lower neighbor designation takes precedence. Across BIDs, a reporting district can be a first neighbor to one BID and a second neighbor to another.

[10] These include duplicate reporting districts, for reasons described in the previous footnote.

[11] Instead of the reporting district level fixed effects used in Equation 1, one might also use a fixed effect for the matched group, as in

$$\text{crime}_{i,m,a,t} = \beta_0 + \beta_1 \text{BID}_i * \text{after}_{m,t} + \beta_2 \text{after}_{m,5} + \beta_{3,t} \text{year}_t + \beta_{4,m} \text{mgroup}_m + \beta_{5,a} \text{area trend}_{a,t} + \epsilon_{i,m,a,t} \ . \quad (3)$$

Empirically it is unclear whether the estimate of β_1 contains spillovers. Suppose BIDs cause crime to decline. If criminals previously active in BIDs find other lines of work, or work outside of the city of Los Angeles, then this method estimates total crime declines in BIDs relative to their neighbors. If BIDs cause crime to decline by shifting criminals out of BIDs and into neighboring areas, this method could overstate BIDs' association with crime decline. However, for this type of spillover to be a serious estimation problem, crime must spill over quite far. The average diameter of a BID is 0.83 square kilometers, and the average diameter of a reporting district is 1.1 kilometers. Since the average BID is present in four reporting districts, this means that a BID would have to displace criminals to twice its own width in order to affect the estimates of BIDs relative to their second neighbors.

Also, a more likely possibility is within-reporting district spillovers. Returning to Figure 1, note that a BID does not usually cover the entire reporting district to which it is attributed. Potential spillovers of the type just described, but within a reporting district, would bias the estimation toward no result, even if a BID did cause crime to decline in its section of the reporting district. Thus, any effect of the BID here may be *underestimated*.

4.2.4 Neighbors and Instrumental Variables

In the event that the estimation using neighbors has not eliminated the selection bias into BIDs, I instrument for BID adoption using an interaction of a neighborhood's era of development and the passage of the 1994 BID law.

While describing the model, I argued that a supply constraint and demand factors jointly determine the pattern of BID adoption. Urban planners have long considered physical neighborhood structure – whether doors face the street or the parking lot, whether buildings are

Theoretically, the reporting district fixed effects are a special case of the matched group fixed effects, and are preferred.

built with enough ground floor retail to encourage pedestrian traffic, whether businesses open directly to the street or onto interior lobbies – to be important in the type and quality of public goods that neighborhoods provide [6, 3].

There is reason to believe that the types of services that BIDs provide are more highly demanded in neighborhoods with older buildings. For example, a mall's provision of security and parking for its customers is aided by the way the structure is designed. By building a wall along the exterior, or by placing the mall on the inside of a ring of parking lots, the mall provides itself with an effective shield from the problems of homelessness and the quality of pedestrian life. Inside the mall, mall security controls the pedestrian environment. In contrast, a neighborhood of older businesses with ground floor retail and street or shared parking has a communal interest in lowered crime, cleaner streets and better parking. Though these distinctions are clear when observing a neighborhood, their quantitative analog is less obvious.

Using the Assessor's property-level information as of 1999, which was the earliest year I was able to obtain, I calculated a distribution of structure year built for each reporting district in the city of Los Angeles.[12] What element of this age distribution approximates a neighborhood's era of development? By age of development, I mean the age at which the infrastructure of the neighborhood was largely determined, not the age of the first structure. For this reason, the minimum age may be misleading – it could describe the age of widespread development, but, depending on the neighborhood, it could also describe the age of first building, if that first building still exists. The mean year built for buildings in a reporting district is also misleading. Two reporting districts, one built entirely in the 1960s, and one

[12]The data are actually geocoded to the census block group, but block groups are uniquely nested within reporting districts (double check!).

built in the 1930s and greatly replaced in the 1990s, could have the same mean age, but vastly different physical layout. For this reason, I approximate the era of widespread development with the fifth percentile of structure year built in that reporting district (era_i). Results are not tied to this percentile of the distribution; they also hold, slightly less strongly, for the tenth percentile, and again slightly less strongly for the first quartile.[13]

Though this demand factor identified is time invariant, the supply constraint varies over time. This is ideal since, due to the fixed effects approach, any instrument that does not vary over time makes no improvement over the fixed effect. Therefore, I interact the neighborhood's era of development with the passage of the 1994 BID law. Though this was not California's first BID law, before the passage of the 1994 law, the city of Los Angeles had no administrative apparatus to process any BID applications, and a BID cannot proceed without city involvement. After the passage of the state law, in response to its new provisions, property owners asked the city to establish an administrative framework for BIDs, and the city complied. So this second half of the instrument is a dummy, law_t, which is 0 before the passage of the law and 1 afterwards.

In sum, the instrument is the interaction of the law change with the neighborhood's era of development: $\text{law}_t * \text{era}_i$. To be valid an instrument must be both correlated with the variable of interest, as I have argued it is with BID adoption, and also uncorrelated with the outcome variable. It is implausible to argue that a neighborhood's era of development is uncorrelated with a neighborhood's level of crime, and I do not do so here. What is mechanically true, is that, net of reporting district fixed effects and city-wide year effects, crime is uncorrelated with the era of development, as the era of development is itself fixed

[13]If anything, the data are predisposed not to find an effect of this type; newer buildings seem to have some complete information in the dataset than older ones.

for each reporting district. Thus, the first stage is

$$\text{BID}_{i,a,t} = \alpha_0 + \alpha_1 \text{law}_t * \text{era}_i + \alpha_{2,i}\text{rd}_i + \alpha_{3,t}\text{year}_t + \alpha_{4,a}\text{area trend}_{a,t} + \epsilon_{i,t} \quad (4)$$

and following the story above, α_1 should be negative.

Using these first stage fitted values, I then estimate the second stage:

$$\text{crime}_{i,t} = \beta_0 + \beta_1 \widehat{\text{BID}}_i * \text{after}_{i,t} + \beta_2 \text{after}_{i,t} + \beta_{3,t}\text{year}_t + \beta_{4,i}\text{rd}_i + \beta_{5,a}\text{area trend}_{a,t} + \epsilon_{i,t} \ . \quad (5)$$

If the instrument is valid, β_1 is then an unbiased estimate of the effect of BIDs on crime.

5 Measuring Crime and Neighborhoods

These estimation strategies require geographically small scale data which cover the pre- and post-BID eras. I use such data from the LAPD, and combine it with information on neighborhood characteristics from the County Assessor, and with records I have collected on the individual BIDs. For clarity, these data sources are presented in Table 2.

Data on the BIDs themselves is not collected in a database form by city officials. In order to develop a dataset of the adoption date, borders, and expenditures for all BIDs in the city of Los Angeles since their inception, I examined over a hundred public files from the Los Angeles City Council and spoke with city and BID administrators.

The Almost BIDs presented even more of a challenge; if they were never established, the city does not maintain records on them. However, because the legal requirements to establish a BID are formidable, neighborhoods always hire a consultant to guide them through the process. The city of Los Angeles has generally given money for these consultants if the

neighborhood can indicate seriousness in its BID consideration.[14] When a neighborhood appeals for municipal support to hire a consultant, it enters into the public record where a researcher can find it. Nearly all of the Almost-BIDs in my sample received funds to hire a consultant, and thus appeared in a city council file. However, the files did not always contain the borders of the proposed districts. When they did not, I called city council offices and BID proponents to ascertain the borders. Finally, I made an electronic map of these Almost BIDs to combine them with the other geographic data.[15]

To measure neighborhood characteristics, I use data from the Los Angeles County Assessor purchased from the vendor Dataquick. The Los Angeles County Assessor is the official collector of property taxes and adjudicator of property boundaries, and collects information on each of the 2.2 million properties in the county. This information includes a commercial or residential designation for each parcel, and the year any structure on that parcel was built.

The crime data come from the LAPD, which graciously provided totals for 21 types of crimes and 27 varieties of arrests from 1990 to 2002 by their smallest unit of geography, the reporting district. Each reporting district is either a census tract or a subdivision thereof. The size of the average reporting district, reported in Table 1, is 1.2 square kilometers, but the median is quite a bit smaller, at 0.8 square kilometers. In the second panel of Figure 1, dark BIDs are overlaid on a background of lighter polygons, and those lighter polygons are reporting districts. Unfortunately for this researcher, the city of Los Angeles changed the boundaries of these reporting districts over time. By examining maps of these reporting districts over 13 years, I assembled a geographically consistent time series of 1009 reporting

[14]Legally, seriousness is conveyed by petitions of support from 15 percent of the potential members, weighted by the value of the assessment.

[15]As I had more confidence in some of the border delineations than others, I estimated the regressions with and without those Almost BIDs about whose borders the information was less reliable. The results are not substantially different.

districts.

As did the rest of the country, and major urban areas in particular, Los Angeles experienced a large, across-the-board drop in crime in the mid-to-late 1990s, which has flattened out in the present decade. In 2002, the average reporting district had 105 violent crimes, and 180 non-violent crimes, following the FBI classification of crimes, which I will use throughout.

Using GIS software, I matched BID borders with LAPD reporting districts. If a BID is present in a reporting district, I call that reporting district a treated reporting district.[16] On average, BIDs intersect with approximately 4 reporting districts. Out of the 1009 total reporting districts, 124 have a BID presence. To attribute BID expenditures to each affected reporting district, I use that reporting district's share of the BID's area. These expenditures by reporting district are displayed in the bottom panels of Table 1. On average, BIDs spend about $150,000 total, $50,000 of which goes to security, in each reporting district in which they are present.

Before the passage of the 1994 BID law, BID reporting districts had higher crime on average than the rest of the city, as shown in Figure 2. The left panel of bars, displaying total, violent and non-violent crime in reporting districts with future BIDs versus those without, shows that BIDs have substantially higher crime than non-BIDs, regardless of the type of crime considered. Also, as shown in the percentage change graphs on the right, reporting districts with BIDs also seem to smaller increases and larger declines in crime than non-BID reporting districts.

However, this differential trend is accounted for by BIDs' uneven location across the city. To compare BIDs to their wider neighborhoods, I use the LAPD's division of the city into 18

[16]Except for a very few cases where the presence of the BID in the reporting district accounts for less that 3 percent of the BID's area

areas, pictured in Figure 3. These areas are boundaries used for administration, budgeting and patrol deployment by the LAPD. Figure 4 shows BID and non-BID crime trends for each LAPD area in the pre-BID years. Crime in BIDs is shown with a solid line, and non-BIDs with a dashed line; areas with only a dashed line have no BIDs. In the vast majority of these cases, BID trends in total crime track non-BID trends very closely, while having somewhat higher levels. Many of these series show a bump in 1992, likely attributable to the riots in that year. This is controlled for in the regression with the year fixed effect for 1992, and any fixed-neighborhood level propensity to have riots is controlled for with the reporting district fixed effect. Though not pictured, the same area-level pattern holds true for violent and non-violent crime separately.

6 Results

6.1 Average Effect of BIDs

With this picture of the data in hand, the results for the fixed effects specification from Equation 1 are presented in Table 3. The top panel shows mean crime by reporting district for BID and non-BID reporting districts. Both before and after the BID law, BIDs have higher levels of crime. This is true for the total categories discussed before, as well as for the three individual violent crimes – robbery, burglary, and auto burglary and theft – on the right side of the table. However, most BIDs were not adopted immediately after the passage of the 1994 law, so these means serve only as a guide to put the regression coefficients into context.

Results in the bottom panel indicate that adopting a BID is significantly associated with 51 fewer crimes per year, or with a drop of roughly 11 percent of the pre-BID level. Though

non-violent crimes are committed more frequently than violent ones, it is violent crimes that make up the bulk of the decline. This pattern of the decline in crime being dominated by the violent crime figures will repeat itself throughout most of the estimation results. The three individual violent crimes in this table collectively make up almost the entire decline in violent crime, with auto burglary and theft being the most dominant, posting a 27 percent decline.[17]

These are large declines in violent crime – about three-quarters of the roughly 40 percent decline in violent crime experienced by the nation as a whole over the course of the 1990s [7], and these are on top of the overall decline in crime in Los Angeles over the course of the decade. Is this a plausible size? BID proponents make claims for declines that are even larger (cite). Some of the remaining estimation strategies will diminish these coefficients, but only slightly.

For the first of these strategies, comparing BIDs with Almost BIDs, the top panels of Table 4 shows that the average Almost-BID is more like the average BID than the general non-BID population. The average Almost-BID covers 0.66 square km, about the same as the 0.69 square km covered by the average BID. Crime in the average Almost-BID is slightly lower than in the average BID, and Almost-BIDs tend to be in slightly older, and moderately less commercial, neighborhoods. A strategy that identifies the BID effect should compare BIDs to neighborhoods just like BIDs, save for the adoption decision; empirically, Almost-BIDs are the closest practicable solution to this proposition.

Even though the sample size plunges, reporting districts with BIDs still show significant crime declines relative to reporting districts with Almost-BIDs. With a drop of almost 24

[17]In this and all tables, standard errors are clustered at the reporting district level to account for serial correlation with a reporting district.

crimes, violent crime still accounts for the lion's share of the 42 crime, or roughly 7 percent, decline. Though this decline is slightly smaller than the original estimates, it is still a large decline – almost one-fifth of the size of the national crime decline of the 1990s. Auto burglary and theft again leads among the violent crimes, posting a decline of 18 crimes. Overall, the coefficients are slightly smaller than those in the initial specification, and the standard errors somewhat larger. Though this suggests that the selection effect have may modestly boosted coefficients in the initial specification, the instrumental variable estimates at the end will tell a different story.[18]

Interestingly, this is the only specification in which the decline in non-violent crime is an even partner in the decline in total crime. Where the previous estimates showed (and the following will also show) the decline in violent crime swamping the decline in non-violent crime, here the coefficients are of the same magnitude. This may suggest that there is an important component of non-violent crime in the decision to adopt a BID not featured in the other selection correction methods.

Results from propensity score matching, though the smallest overall, tell a broadly similar story. Matching using a propensity score of pre-BID average levels of crime, as in the top section of Table 5, shows that BIDs are associated with a 29 crime, or 6 percent decline in overall crime. Like the initial specification, this decline is predominantly violent crime.

[18]These Almost-BIDs also allow for a test of the importance of the level of standard error clustering to the results. There is a concern that clustering standard errors by reporting district, as I do throughout, may not be sufficiently restrictive, because a BID covers an average of four reporting districts. Ideally, the city would be divided into BID-sized chunks, and I could cluster the standard errors by these chunks. In general, BID-relevant areas for non-BIDs are not defined, and this is impossible.

However, with Almost BIDs, there is a BID-like group defined for BIDs and non-BIDs alike. Re-running the regression and clustering by BID or Almost-BID, does not invalidate the main results. The standard errors on the three total crime figures – violent, non-violent and overall – increase from 9.6 to 12.0, 6.6 to 7.2, and 14.3 to 16.5, respectively. While the coefficient in the violent crime estimation becomes insignificant, this new clustering leaves the coefficients on non-violent and overall crime significant. In other words, this test clustering suggests that there is indeed some within-BID (or BID-like grouping) homogeneity in the error term, but that it is not enough to invalidate the general thrust of the results.

Consistent with the previous results, auto burglary and theft shows the largest decline among the violent crimes.

The second panel of Table 5 weights by a propensity score that is a function of annual (1990-1994) pre-BID levels of violent and non-violent crime. This is a function of ten variables and is a considerably more stringent match than the previous one, controlling non-linearly for pre-BID crime trends, levels and type. However, the results remain very similar. Total crime declines of 7 percent are associated with BID adoption; the vast majority of this due to violent crime. Among the individual violent crimes, the pattern of importance is unchanged. Thus, even compared to neighborhoods with similarly high levels of crime, BIDs are associated with crime declines, and in the same pattern as in the initial specification, though the levels are somewhat reduced.[19] Of all the methods, this matching produces the lowest estimates of BIDs' association with crime decline. However, the estimates are still both statistically and economically significant.

Like the results from propensity score matching, results from a comparison of BIDs with their neighbors also shows that BIDs are associated with declines in crime. The top panel of Table 6 shows mean crime in BIDs relative to their first and second neighbors. BIDs have the highest levels of crime, and the first and second neighbors are more like each other than the BIDs, though the second neighbors have consistently somewhat lower crime than the first neighbors.

The second panel of Table 6 presents estimates of BIDs' association with crime relative to themselves, in order to put the differencing results into context. Relative to themselves

[19]Though I use a regression form in this paper for simplified exposition and ease of comparison with other results, I have also used the propensity score match to calculate differences in means over different pre- and post-treatment windows. Results are broadly similar.

One way to check the validity of a matching strategy is to see whether the matched treated and control difference-in-difference is significant before the adoption of the policy, as suggested by Heckman and Hotz [4]. Pre-treatment results are always smaller than post-treatment results, and the general results are similar.

before the BID, reporting districts with BIDs post an annual decline of 22 crimes after BID adoption. Though the magnitudes of the coefficients in this panel are in line with the estimates in the remainder of the table, and are roughly half the size of the fixed effects ones, they are not significant. This is may be due to the greatly diminished sample size.

The third panel of Table 6 compares BIDs to their first neighbors. This comparison controls for reporting district level fixed effects, city-wide time shocks, and area-level trends. In addition, these estimates include an $\text{after}_{i,t}$ term, which makes the coefficients true difference-in-differences, unlike the previous estimates. Comparing BIDs to their first neighbors somewhat reduces the size of the coefficient of interest relative to the initial specification – BIDs are now associated with crime declines of roughly 7 percent. However, the coefficients of interest remain negative and significant. The decline in violent crime is roughly twice that of non-violent crime, and auto burglary and theft accounts for almost a third of the total decline in violent crime. The addition of the $\text{after}_{i,t}$ term decreases the absolute value of the coefficients – using overall crimes as the outcome, the absolute values of the coefficient falls from xx to 43, while the t-statistic remains about the same. Coefficients in the other estimations are similarly affected.

The final panel of the table presents the same comparison as the third panel, now using second neighbors as the reference group. Compared to the results from the first neighbors, these coefficients are uniformly somewhat larger, though the relative size of the coefficients follows almost exactly the pattern in the first neighbors section of the table.

Note that the coefficients from these geographic regressions bound the coefficients from the initial fixed effects approach. First neighbors do better, relative to BIDs, than second neighbors. This may indicate that BIDs exert positive spillovers as far as thier first neighbors. For example, BIDs could attract nearby higher-end residential tenants more likely to pressure

city officials into combatting crime.

To push further in removing any selection effect from the results, I instrument for BID adoption using the interaction of the neighborhood's era of development with the lifting of the supply constraint on BID adoption. Unlike the other adjustments for selection bias, which have all caused some decline in the coefficient of interest, results from the instrumental variable specification are much larger than the initial specification.

The first stage regression of BIDs on the neighborhood's era of development interacted with the adoption of the BID law, along with the other fixed effect controls is highly significant. Table 7 shows the results from this first stage, along with other moments of the development year distribution as specification checks.

Consider first the top panel, which presents the results of the first stage in the full sample. The coefficient of primary interest is that on the the fifth percentile of year built (fourth column). This coefficient is interpreted as meaning that an increase of ten years in the era of development is associated with a xx percent smaller likelihood of BID adoption. The F-statistic, at 26, in the bottom row of the the top panel, clearly passes the weak instrument barrier. Comfortingly, the tenth percentile and twenty-fifth percentile of year built behave similarly, though slightly less strongly relative to BID adoption. The remaining columns are presented as specification checks. The mean and median year built by reporting district reflect a mix of the age of development and the quantity and timing of subsequent development, and theory does not argue that they should predict BID formation. Correspondingly, their coefficients are smaller, and their F statistics less sizeable. As the fifth percentile year built is the best predictor, and as the results are not sensitive to the exact point in the distribution, for the remainder of this section I will use the fifth percentile as the instrument.

In addition, I use the same instrumenting strategy in the first and second neighbors

sample. The first neighbors sample is not shown since the instrument is not not correlated with BID adoption in this sample. This is not surprising – in a sample of BIDs and their closest neighbors, there should be substantially less variation in the era of development and possibly none of the newer type of development from which BIDs are distinguished. However, the instrument can identify BIDs relative to their second neighbors. The correlation in the second neighbors sample is roughly of the same magnitude as in the entire sample, and is somewhat less precisely estimated. The F statistic, at 19, is still well over the weak instrument boundary.

This instrumenting strategy yields very large second stage estimates of BIDs' impact on crime, as shown in Table 8. The top panel presents the estimates of BID adoption on crime for the full sample. These estimates are implausibly large – more than twice the average level of crime in BIDs. Leaving magnitude aside for a moment, the pattern of first three the coefficients – with violent crime accounting for the bulk of the decline – is similar to the other estimates. Unlike the other results, the magnitude of the coefficent where auto burglary and theft is the outcome does not dwarf the coefficients from the other specifications.

What might account for these large coefficients? If BIDs are adopted in neighborhoods which are desparate, OLS would understate BIDs' impact on crime, and we would expect the IV results to be larger than those in the basic specification. Even so, these IV results are extremely large. There are two standard factors which could account for these results. First, the instrument could be weak. Though the instrument adds little to the R-squared, the F statistic for the fifth percentile age in Table 7 is very robust, and discounts this explanation. Second, the instrument could be correlated with the second stage outcome. Because the era of development is fixed, it is implicitly controlled for with the report district fixed effects.

After ruling out the two most obvious culprits, there are three more possible explanations.

First, the impact of BID treatment could be heterogeneous – that is, $\beta_{1,i}$ instead of β_1 – and this heterogeneity of treatment could be correlated with the quality of the IV prediction of BID adoption [1]. If this is the case, and if neighborhoods which spend more on crime prevention get more crime prevention, the size of the residual from the first stage should be correlated with the amount spent on crime prevention – reporting districts that spend more should be better predicted in the first stage. Second, the results could be due to an inappropriate extrapolation from BIDs to non-BIDs.

The second panel of Table 8 tests this second argument, that crime declines are strangely extrapolated, by re-doing the instrumental variables estimation on a more homogeneous sample – the geographically matched sample of the first and second neighbors. Results here are still very large, but much closer to the range of the plausible. The overall drop in crime after BID adoption is now 323 crimes, or a little over half of the average level in BIDs before 1995. Robbery falls an implausible 150 percent of pre-BID levels, while auto burglary and theft declines a possibly more realistic 60 percent.

Though the exact values from the instrumental variables estimates may be dismissed as implausibly large, they do suggest that the previous estimation strategies may have understated any associated of BIDs with crime decline.

6.2 Impact of BID Expenditure

All these estimates have used a dummy as the marker for BID adoption. If BIDs differ in strength or effectiveness, the resulting coefficient reports the average effect across all BIDs. Replacing this dummy with a measure more closely aligned with BID strength – total BID expenditure or BID security expenditure – allows for a per dollar estimate of the benefits of BID adoption, as shown in Table 9. Specifically, the $\text{BID}_i * \text{after}_{i,t}$ term

in Equation 1 (or its equivalent) is replaced by BID expenditure$_{i,t} *$ after$_{i,t}$. Each cell in this table comes from a regression of BID adoption on crime, using the control variables described above. The two righthand columns translate these regression coefficients into a dollar per crime averted figure. Theory suggests that the correct amount of expenditure to use in such a regression would be some amount between the higher total expenditure and the lower security expenditure. Clearly, security expenditures go directly toward combatting crime, but the total expenditure may also go toward solving free rider problems that would otherwise prove a hindrance in crime fighting, such as disputes among neighbors about the correct disposition of public space.

However, even the very largest of the dollar figures, taken from the regression of total BID expenditure on crime using the matching framework – $4,572 – is lower than the roughly $5,000 that the LAPD spends per crime. Among the dollar figures from the coefficient on the security budget, not one tops $2,000 per reported crime averted. In some sense these are underestimates of BID effectiveness, as BID security likely deters non-reported crimes, such as transiency. Using the average of the total and security expenditure figures, the most conservative method estimates that BIDs spend $3,000 per crime averted. The previous estimates have suggested that at least half of these averted crimes are violent. Compared to the conservative estimate of $20,000 of social cost per violent crime, BIDs are cheap. From the perspective of the property owner, it is certainly preferable to spend $3,000 extra in taxes in front of one's own front door in place of lobbying for higher city-wide taxes for police.

7 Specification Checks

So far, the results have been remarkably consistent in associating BID adoption with crime decline, regardless of the particular method chosen. This section investigates whether the results are consistent with other predictions from the theory: that certain types of BIDs are more effective than others, and that certain types of crime should be more affected than others.

Though I have been referring to BID members as property owners (and will continue to do so for convenience), the city of Los Angeles actually has both property- and merchant-based BIDs. Property based BIDs run for a finite term, usually 3 to 5 years, and require a new vote to re-establish at the end of this term. Merchant-based BIDs, after an initial vote, require a majority assessment-weighted protest to become inactive. Theory suggests that property owners should be willing to make larger investments in neighborhoods as they are the residual claimant on any successful investment. Though merchants may also have an interest in improving their neighborhood, if it is improved too much they are priced out. The theoretical prediction of property BID's greater willingness to invest is borne out by their disproportionate share, xx percent, of all BID investment.

If property BIDs are willing to make more significant neighborhood investments, they should be more successful than merchant BIDs in lowering crime. This prediction is borne out in the left panel of Table 10. The first column of Table 10 repeats the coefficients from the estimation of BIDs on crime across specifications. The second two columns report the results from replacing the single BID dummy, $BID_i * after_t$, with two dummies – one for merchant BIDs and one for property BIDs. The results are striking in the consistency with which property BIDs account for a much larger share of the total decline in crime. In the

fixed effects approach, property BIDs are associated with a roughly 12 percent decline in crime, while merchant BIDs are associated with an insignificant drop of 3 percent. This pattern holds across all estimation methods, and the minimum crime decline associated with property BID is 38 crimes, or 7 percent. These results are a good fit with the theoretical prediction that property owners should make larger investments and reap larger returns.

Theory also suggests that there are crimes on which BIDs should have no effect. BIDs target a specific type of collective action problem, and the resolution of this problem should be expected to affect auto theft or robbery, but no impact white collar crimes, such as forgery or fraud, or personal (?) crimes, such as domestic violence (family crime?). Unfortunately, only 1 (2?) of these white collar crimes has enough occurences to make a measurable comparison. The first column of the righthand side of Table 10 repeats the coefficients from the estimate of BIDs on auto burglary and theft, which are in the 12 to 22 percent range and significant across the board. In contrast, BIDs are frequently insignificantly associated with changes in forgery.

8 BIDs and Enforcement

The previous results of this paper have strongly suggested that BIDs to seem to be able to lower crime. Until now, though, this paper has remained mum on how exactly BIDs might impact the level of crime. But this "how" is a crucial issue, and has serious consequences for the redistributive impacts of a policy that eases the private provision of public goods. Whether lowered crime in BIDs is of benefit to the city as a whole remains an open question, and the issue of whether BIDs crowd out or suck up city services is essential for evaluating BIDs at a city-wide level, and as an overall policy choice.

One way in which BIDs could decrease crime is by doing tasks the LAPD might not be willing or financially able to do, such as keeping a closer eye on the streets, moving homeless people along, frequently scolding drunks, and aggresively pursuing unlicensed street vendors. Certainly BIDs do exactly this through their full-time staffs of "neighborhood ambassadors." If BIDs decrease crime in this fashion, it suggests that private services supplement, but do not crowd out, public ones. If BIDs decrease crime by crowding out public services, say by making their own anti-gang task forces, then the remainder of the city implicitly receives more policing.

Alternatively, BIDs could decrease crime by attracting more public services. After having resolved the collective action dilemma, a neighborhood with a BID could be better mobilized to attract more police enforcement, possibly because the neighborhood may now have full-time staff members to call when the police could be helpful. For example, the Hollywood Entertainment BID purchased wireless cameras to canvass Hollywood Boulevard, which will be monitored by the LAPD [9].

Unfortunately, measuring police enforcement in general is a tricky matter, and measuring police enforcement at the neighborhood level is even more difficult. I propose two suggestive measures to capture this enforcement. The first is the ratio of arrests to reported crimes, by reporting district. Under certain assumptions, an increase in this ratio could be interpreted as an increase in the level of police enforcement.[20] Results from regressing BIDs on arrests per crime are presented in the left panel of Table 11. BIDs are always associated with a decline in arrests per crime; the decline is never significant in the violent crime category, but is sometimes significant in non-violent and therefore total crime. These results fit sensibly with BIDs' crime prevention tactics, which focus more on non-violent, quality-of-life crimes.

[20] discuss assumptions

BIDs are unequipped to deal with serious violent crimes beyond reporting them to the police. Therefore, this panel suggests that at first glance BIDs may be associated with a slight decrease in police enforcement in non-violent crime.

A second way to measure police enforcement is to investigate a pattern in the change in arrests by type of arrests. In general, arrests of all kinds decline after BID adoption, along with the number of crimes. However, if BIDs do attract greater police enforcement, we would expect increases in arrests that are more discretionary – for example, arrests for drunkenness. The righthand panel of Table 11 presents the results of regression BIDs on three kinds of arrest outcomes – burglary, vehicle theft, and drunkenness. Across estimation methods, BIDs are consistently related with significant declines in arrests for burglary. This should not be taken as a slackening of enforcement per se, as the number of buglary crime also fell during this period in BIDs, as shown in previous tables. Compare these results for burglary arrests with the rightmost columns, estimating BIDs' association with arrests for vehicle theft and drunkenness. Due to the LAPD's data categorization, arrests for vehicle theft cannot be compared directly with auto burglary and theft crimes. However, it is interesting to note that during a period in which auto burglary and theft fell significantly in BIDs, arrests for vehicle theft have remained virtually unchanged. Arrests for drunkenness, possibly the most discretionary of the 27 arrest categories, are insignificantly changed by BID adoption. However, these coefficients should be understood against a background in which arrests of almost all other types fell. I would interpret these last two columns as suggestive evidence that BIDs may be modestly associated with an increase in a particular type of police enforcement.

9 Conclusion

By giving neighborhoods a tool to solve the collective action problem they face in the provision of public safety, the 1994 BID law has allowed neighborhoods to take charge of their own security. Across a range of estimation methods, BIDs are associated with estimated crime declines of 5 to 9 percent. Strikingly, BIDs are more frequently associated with declines in violent than non-violent crime. It is unclear whether this results contradicts the broken windows theory of crime prevention. Though BIDs do not seem associated with large declines in violent crime, it is clear that the actions of BID "neighborhood ambassadors" – keeping an eye on the street, reporting small crimes of vandalism – are generally in line with the kinds of previous activities the broken windows theorists would suggest.

Using BID expenditures on security as an independent variable, roughly $3,000 of BID spending is associated with a decline of one additional crime. This is much lower than the approximately $5000 per crime on patrol that the LAPD spends. This comparison is probably unfair to the LAPD, which has much less discretion than BIDs in the types of service offered, and has many more regulations with which to contend. In any event, it is clear from the perspective of a property owner that an additional $3,000 of taxes is more preferably spent and controlled locally than watered down across the city.

In the final analysis, however, even if BIDs do cause crime decline, how they do it is essential to understanding whether any BID-like policy is healthy for the city at large. The anecdotal evidence, and the quantitative evidence presented here suggests that BIDs are associated with a modest suck up of city services. On net, the city must balance this redistribution of services with any crime declines caused by BIDs. In the evidence presented here, the crime declines associated with BID adoption are far more sizable than the suggestive

evidence with respect to enforcement.

But BIDs perform many more services than just public safety. In future work, I plan to examine BIDs' impact on sales revenues, building permit issuances, and commercial and residential property values. More generally, the resolution, by BIDs, of collective action failure through membership coercion has implications for other place-based public goods provision, such as schools or homeowners' associations, but also for other types of professional or trade associations.

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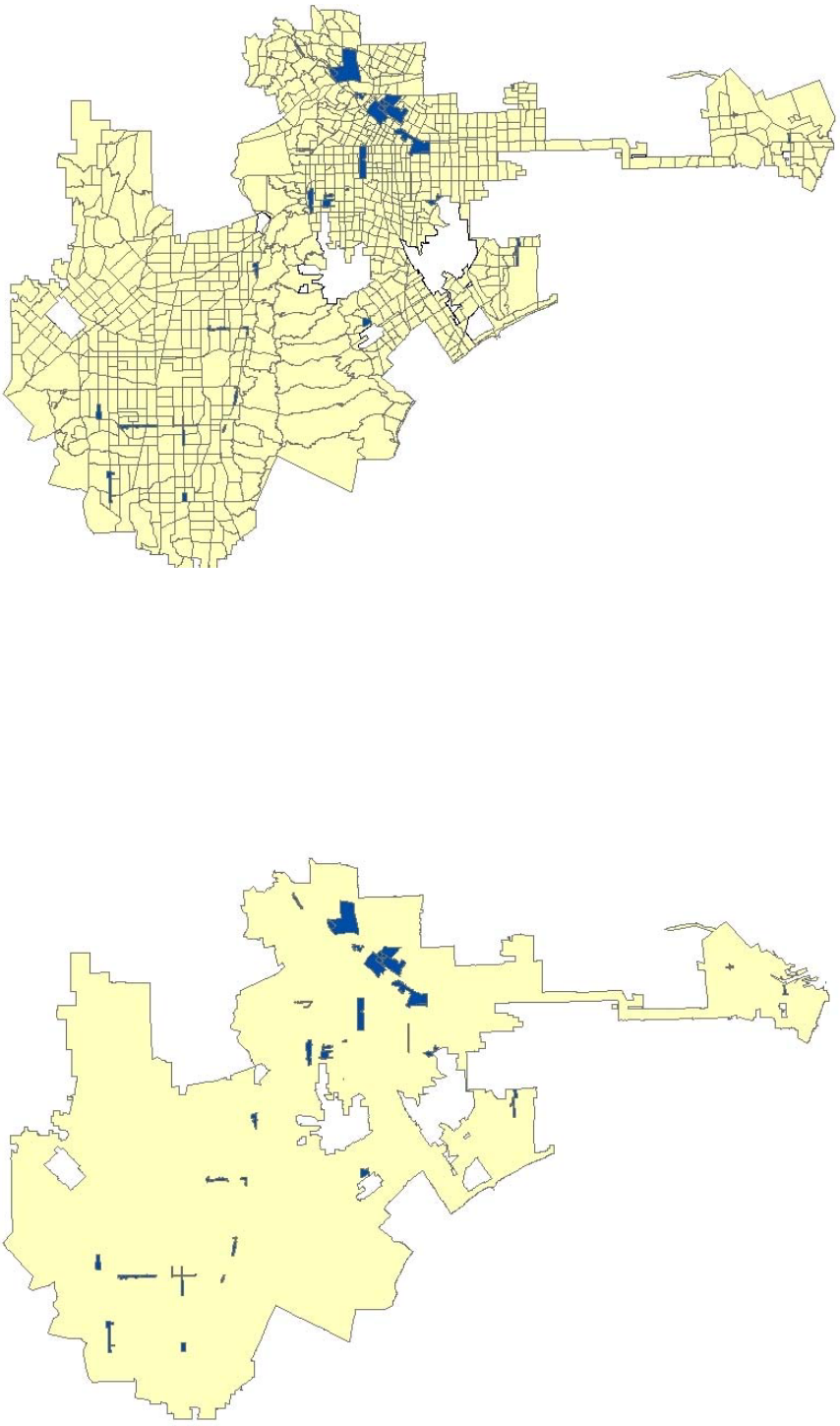

Figure 1: City of Los Angeles Business Improvement Districts and LAPD Reporting Districts

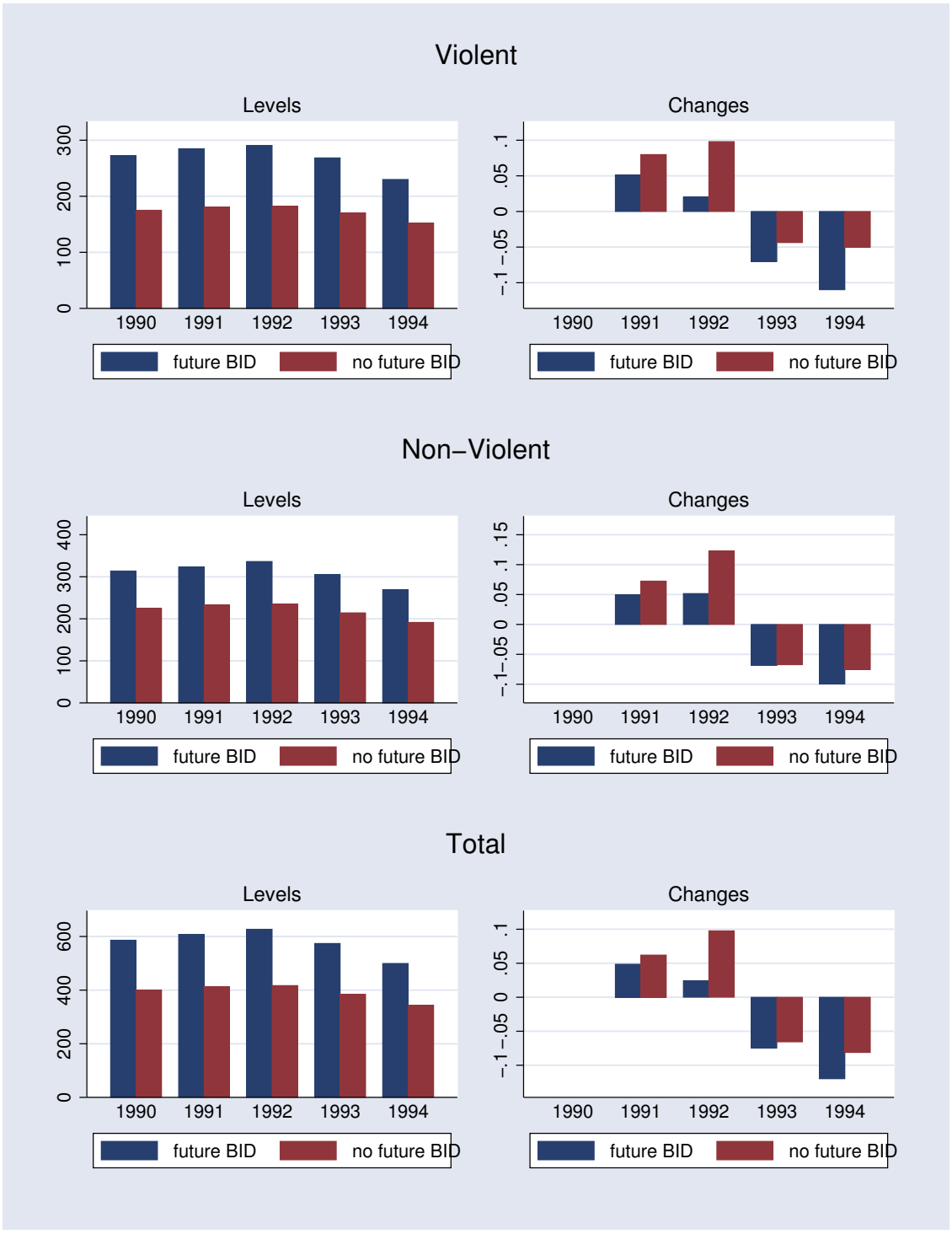

Figure 2: Crime in BIDs and non-BIDs Before the BID Policy

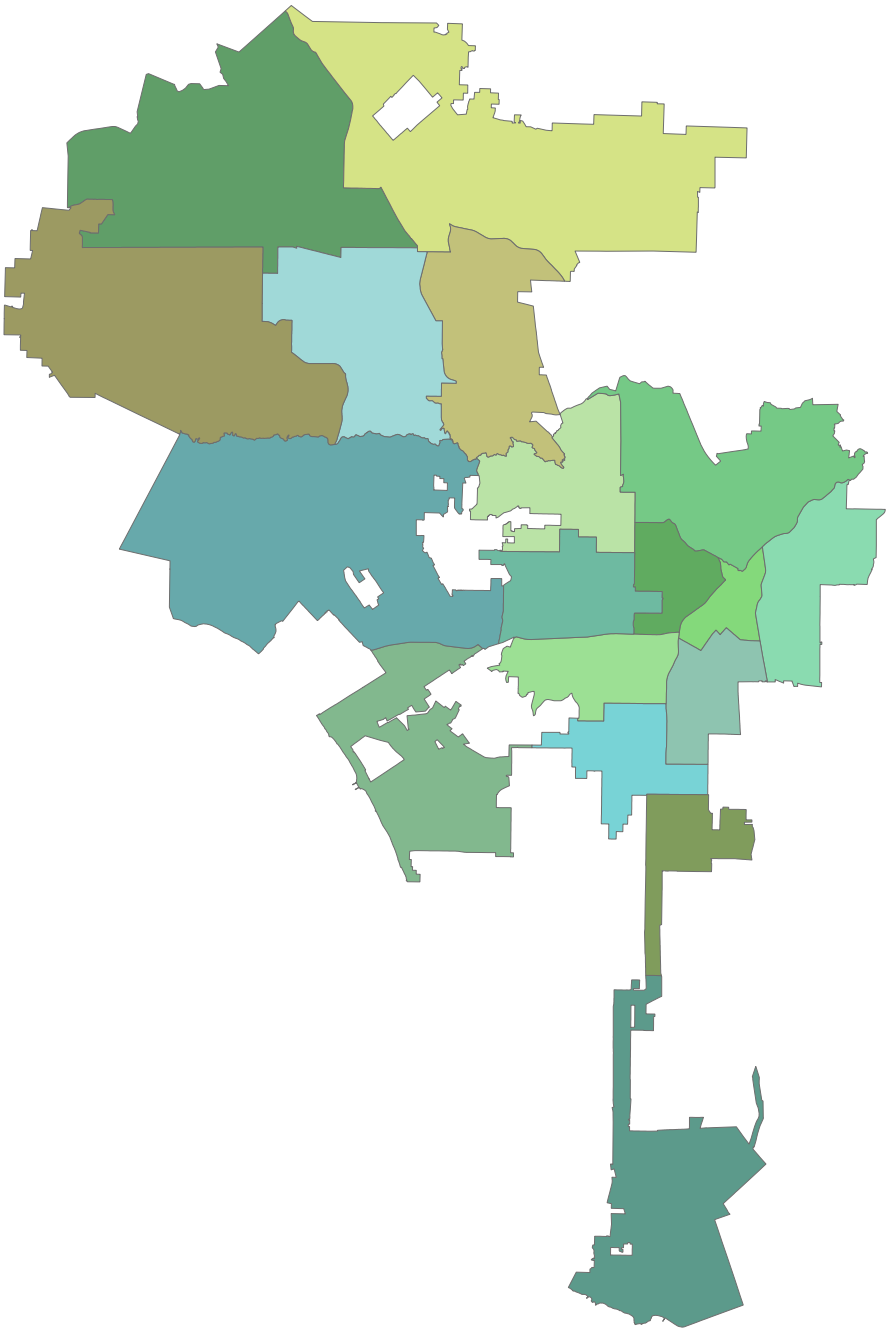

Figure 3: LAPD Administrative Areas

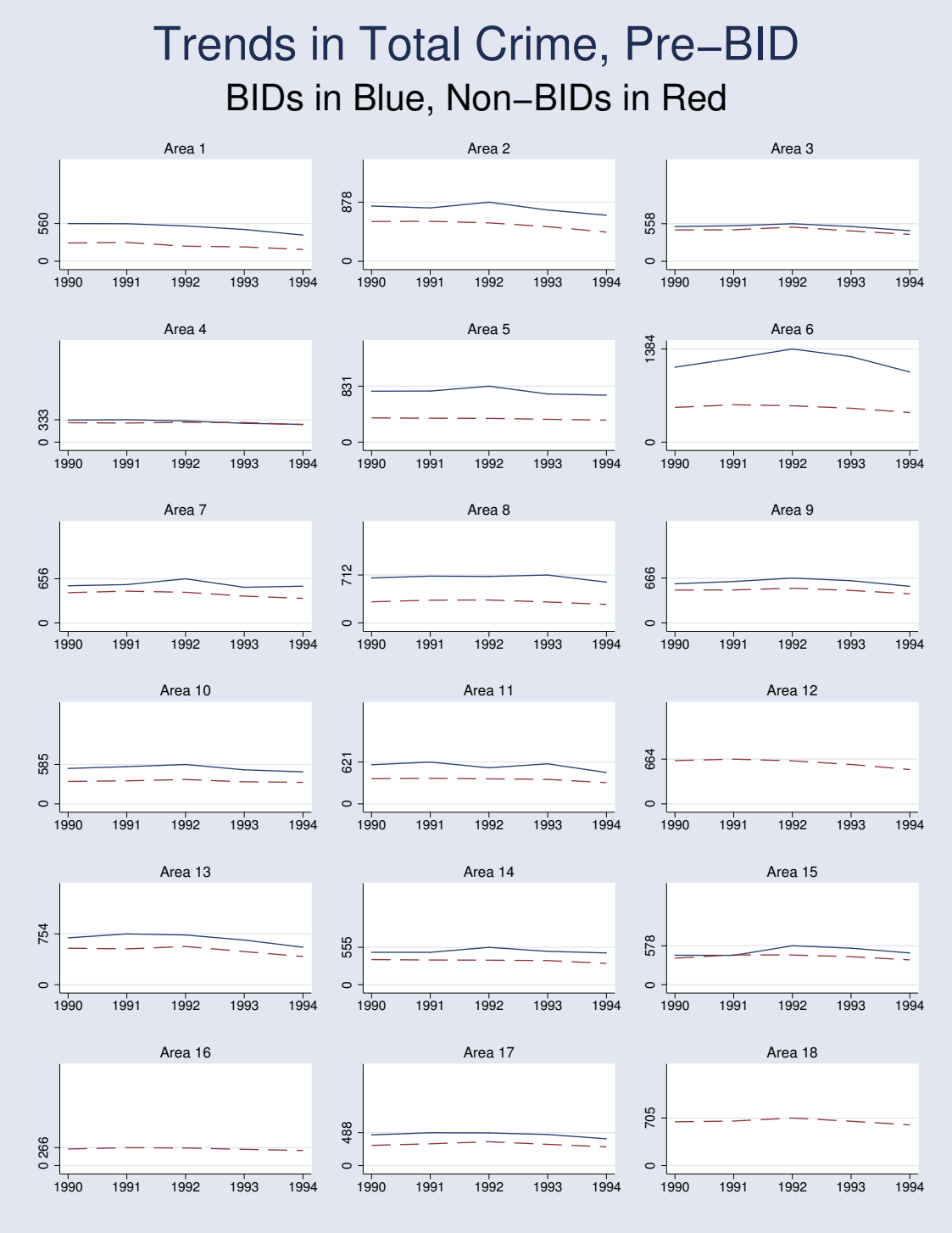

Figure 4: BID Crime Trends by Area

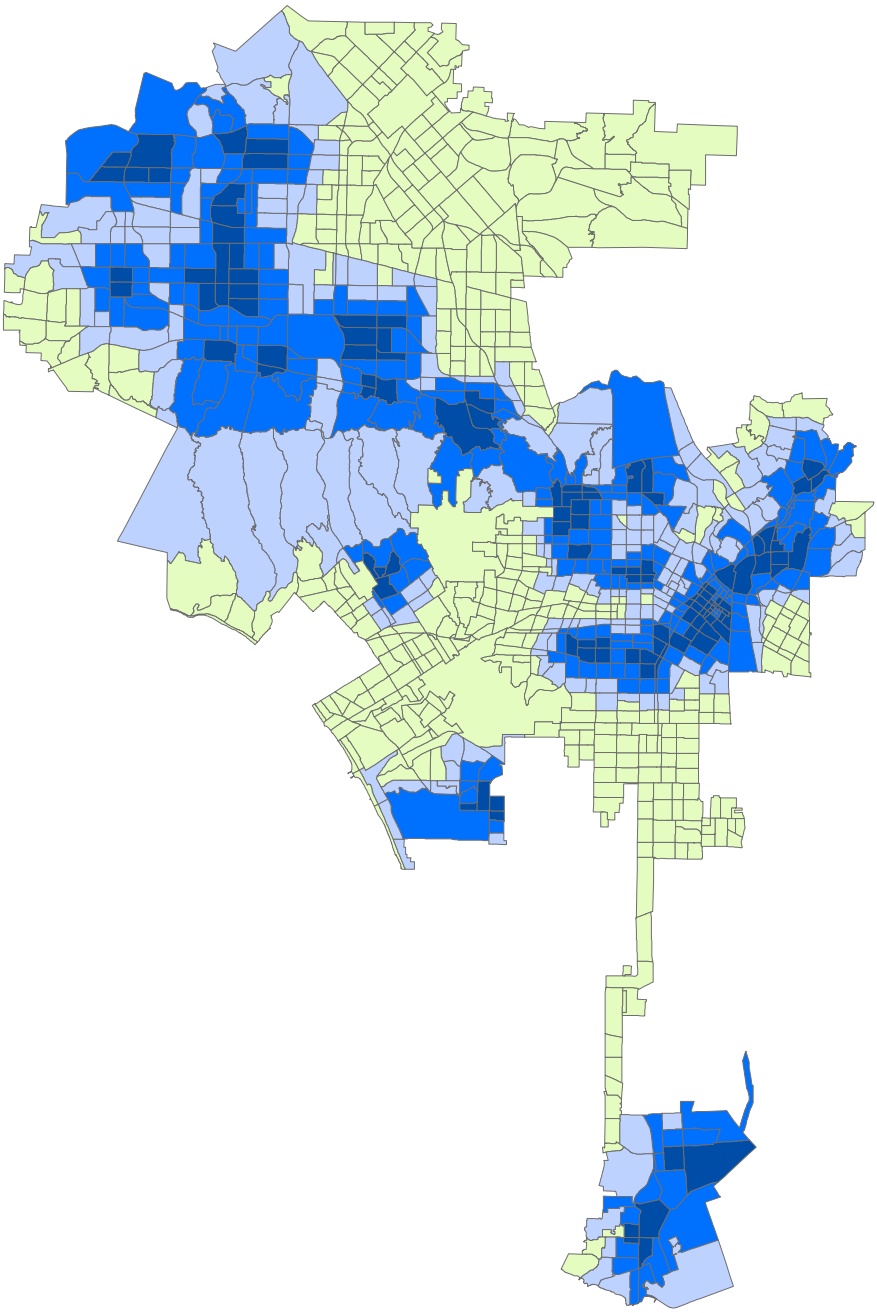

Figure 5: BIDs and Their Neighbors

Size, in square kilometers

	mean	median	total area
BIDs	0.69	0.37	21
reporting districts	1.23	0.81	1,240

Expenditures by BIDs

	all BIDs	all BIDs with positive security expenditures
number	30	19
total budget ($)		
average	628,843	924,262
median	282,180	501,827
total	18,865,293	17,560,985
security budget ($)		
average	216,594	341,990
median	23,144	210,842
total	6,497,813	6,497,813

Expenditures by BIDs by Reporting District

	all RDs[1] with BIDs	all RDs with positive security expenditures
number of reporting districts[2]	124	85
total budget		
mean	152,139	206,600
median	78,747	113,817
security budget		
mean	52,402	76,445
median	17,323	37,429

Notes:
[1] Reporting District
[2] These are unique RDs with BIDs. Three RDs are covered by two BIDs.
Source: Author's tabluations from Los Angeles City Council files, City of Los Angeles maps, and the Los Angeles Almanac (http://www.losangelesalmanac.com/LA/index.htm).

Table 1: BID Size and Expenditures

Dataset	Source	Unit of Observation	Variables
BID Information	• current & historical city council files • interviews: BID members, city officials, & those who considered adopting a BID • BID and community group websites	• BID • Almost BID	• BID adoption year • BID location • BID expenditures, each year of operation • Almost BID location
Crime	Research Department, LAPD	• 1009 reporting districts • 1990-2002, linked geographically by the author	• 21 types of crime • 27 types of arrests
Property Information	Los Angeles County Assessor, via Dataquick Inc.	• parcel, geocoded to the block group level	• year structure was built • designation of property as commercial or residential

Table 2: Data Sources Used

Table 3: Regressing BID Adoption on Crime, Using a Fixed Effects Approach

	(1)	(2)	(3)	(4)	(5)	(6)
Means		totals		violent crimes		
	no. of reporting districts-years	violent	non-violent	overall	robbery	burglary	auto burglary and theft
Entire Period						
BIDs	1,612	195.6	265.1	460.8	35.4	46.1	85.8
non-BIDs	11,505	131.5	185.1	316.6	24.1	35.4	47.7
Before 1995							
BIDs	620	269.3	309.6	578.9	50.2	65.4	118.3
non-BIDs	4,425	172.1	219.6	391.7	33.8	48.3	59.0
1995 and After							
BIDs	992	149.6	237.3	386.9	26.2	34.1	65.5
non-BIDs	7,080	106.2	163.6	269.7	18.0	27.3	40.7

Regression Results

Crime as a Function of BID Adoption

	totals			violent crimes		
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-39.31	-11.74	-51.05	-5.82	-8.39	-23.34
	7.7222**	5.4294*	11.0984**	1.3315**	1.8781**	5.1371**
year fixed effects	x	x	x	x	x	x
area level trends	x	x	x	x	x	x
reporting district level FE	x	x	x	x	x	x
Observations	13,117	13,117	13,117	13,117	13,117	13,117
R-squared	0.88	0.91	0.91	0.85	0.79	0.80

Standard errors below coefficient estimates, clustered at the reporting district level.

Source: Crime data 1990-2002 from Los Angeles Police Department; BID information is author's tabluations from city documents.

Table 4: BIDs Relative to Almost-BIDs

	(1)	(2)	(3)	(4)	(5)	(6)
Size, in square kilometers						
	number	mean	median	total area		
BIDs	30	0.69	0.37	21		
Almost BIDs	26	0.66	0.38	17		

Means

		totals			violent crimes		
	number of RDs	violent	non-violent	overall	robbery	burglary	auto burglary and theft
Entire Period							
BIDs	118	186.5	256.9	443.5	33.6	45.3	80.7
Almost BIDs	132	169.7	218.7	388.4	38.0	41.8	54.2
Before 1995							
BIDs	118	255.6	300.0	555.6	47.9	63.8	110.2
Almost BIDs	132	226.7	255.2	481.9	54.2	58.6	68.0
1995 and After							
BIDs	118	143.4	230.1	373.4	24.7	33.6	62.3
Almost BIDs	132	134.1	195.9	330.0	27.8	31.4	45.5

Regression Results

	totals			violent crimes		
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-23.70	-18.28	-41.98	-1.56	-3.35	-17.48
	9.6045*	6.6381**	14.3082**	2.04	2.44	6.0473**
year fixed effects	x	x	x	x	x	x
area level trends	x	x	x	x	x	x
reporting district level fixed effects	x	x	x	x	x	x
Observations	3,250	3,250	3,250	3,250	3,250	3,250
R-squared	0.86	0.93	0.92	0.83	0.78	0.79

Standard errors below coefficient estimates, clustered at the reporting district level.

Note: The number of reporting districts with BIDs is slightly smaller by 9 reporting districts in this table than in other tabulations; I dropped reporting districts that contained both a BID and an Almost BID.

Source: Crime data 1990-2002 from Los Angeles Police Department; BID information is author's tabluations from city documents and interviews.

Matching By Total Average Levels of Crime Pre-BID

	totals		violent crimes			
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-23.83	-5.08	-28.91	-3.16	-5.60	-14.80
	6.0172**	5.88	10.4080**	1.1688**	1.5628**	3.8964**
year fixed effects	x	x	x	x	x	x
area level trends	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x
Observations	13,117	13,117	13,117	13,117	13,117	13,117
R-squared	0.87	0.93	0.92	0.84	0.79	0.80

Matching By Annual Totals of Violent and Non-Violent Crime, Pre-BID

	totals		violent crimes			
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-22.08	-8.46	-30.54	-3.07	-4.64	-14.15
	5.1514**	4.99	8.6561**	1.0893**	1.3744**	3.2203**
year fixed effects	x	x	x	x	x	x
area level trends	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x
Observations	13,117	13,117	13,117	13,117	13,117	13,117
R-squared	0.88	0.91	0.91	0.84	0.79	0.80

Table 5: Using Propensity Score Matching to Estimate Crime Behavior

Overall Means

		total crimes		violent crimes			
	number of reporting districts	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BIDs	127	193.9	260.9	454.8	35.6	45.4	84.6
first neighbors[1]	291	129.5	184.3	313.8	22.6	33.0	53.7
second neighbors[1]	472	123.4	174.8	298.2	22.0	32.6	48.9

BIDs Only, Relative to Themselves

	total crimes		violent crimes			
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-15.09	-7.71	-22.80	-0.39	-3.79	-10.32
	8.83	6.35	13.94	1.97	2.30	5.39
year fixed effects	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x
area trends	x	x	x	x	x	x
Observations	1,794	1,794	1,794	1,794	1,794	1,794
R-squared	0.84	0.89	0.88	0.79	0.76	0.81

BIDs Relative to Their First Neighbors

	total crimes		violent crimes			
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-36.52	-6.84	-43.35	-6.36	-8.19	-20.51
	7.5003**	6.54	11.9986**	1.4578**	2.0125**	5.0988**
year fixed effects	x	x	x	x	x	x
area level trends	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x
after_i,t included separately	x	x	x	x	x	x
Observations	5,434	5,434	5,434	5,434	5,434	5,434
R-squared	0.86	0.92	0.91	0.81	0.79	0.81

BIDs Relative to Their Second Neighbors

	total crimes		violent crimes			
	violent	non-violent	overall	robbery	burglary	auto burglary and theft
BID_i*after_i,t	-43.81	-15.63	-59.43	-7.52	-8.97	-25.04
	7.9288**	5.7437**	11.5454**	1.4583**	2.1062**	5.1373**
year fixed effects	x	x	x	x	x	x
area level trends	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x
after_i,t included separately	x	x	x	x	x	x
Observations	7,787	7,787	7,787	7,787	7,787	7,787
R-squared	0.87	0.88	0.90	0.85	0.79	0.80

Note: This sample includes 3 more BID reporting districts than the previous sample. Three of the RDs cover two BIDs. The previous sample used the earliest BID adoption to determine the BID timing. This sample includes the duplicate reporting districts.

[1] These are not unique reporting districts. A RD can be a neighbor to more than one BID.

Standard errors below coefficient estimates, clustered at the reporting district level.

Source: Crime data 1990-2002 from Los Angeles Police Department; BID information is author's tabluations from city documents.

Table 6: Crime in BIDs Relative to First and Second Neighbors

Table 7: Instrumental Variables, First Stage Results

Full Sample

	function of year built						
	mean	median	minimum	**5th percentile**	10th percentile	first quartile	third quartile
IV variable	-0.00066	-0.00058	-0.00012	-0.00091	-0.00077	-0.00044	-0.00024
	0.00021**	0.00018**	0.00008	0.00018**	0.00018**	0.00018*	0.00017
year fixed effects, year>1994	x	x	x	x	x	x	x
area level trends	x	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x	x
Observations	12,935	12,935	12,935	12,935	12,935	12,935	12,935
R-squared	0.48	0.48	0.48	0.48	0.48	0.48	0.48
F Statistic	9.66	10.83	2.09	25.72	18.03	6.05	2.12

Second Neighbors

	function of year built						
	mean	median	minimum	**5th percentile**	10th percentile	first quartile	third quartile
IV variable	-0.00106	-0.00102	-0.00053	-0.0011	-0.00091	-0.00048	-0.00043
	0.00031**	0.00026**	0.00014**	0.00026**	0.00026**	0.00025	0.00025
year fixed effects, year>1994	x	x	x	x	x	x	x
area level trends	x	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x	x
Observations	7,618	7,618	7,618	7,618	7,618	7,618	7,618
R-squared	0.47	0.48	0.48	0.48	0.47	0.47	0.47
F Statistic	11.63	15.87	13.7	18.57	12.16	3.65	3.05

Standard errors below coefficient estimates, clustered at the reporting district level.

Source: Crime data 1990-2002 from Los Angeles Police Department; Property information from the Los Angeles County Assessor's Office via Dataquick software.

BID information is author's tabluations from city documents.

updated 9/20/04

Table 8: Instrumental Variables, Second Stage Results with Neighbors

Full Sample

	First Stage	*Second Stage* total crimes			violent crimes		
	outcome is BID_i*after_i,t	violent	non-violent	overall	robbery	burglary	auto burglary and theft
5th percentile age	-0.00091						
	0.00018**						
BID_i*after_i,t		-628.45	-236.71	-865.17	-199.13	-120.64	-142.47
		128.53**	81.77**	193.43**	41.19**	28.79**	38.63**
year fixed effects	x	x	x	x	x	x	x
area level trends	x	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x	x
Observations	12,935	12,935	12,935	12,935	12,935	12,935	12,935
R-squared	0.48	0.30	0.87	0.71		0.48	0.68
F Statistic	25.72						

BIDs Relative to Second Neighbors

	First Stage	*Second Stage* total crimes			violent crimes		
	outcome is BID_i*after_i,t	violent	non-violent	overall	robbery	burglary	auto burglary and theft
5th percentile age	-0.0011						
	0.00026**						
BID_i*after_i,t		-274.9	-48.75	-323.65	-133.2	12.55	-73.74
		73.36**	69.75	118.93**	32.23**	17.58	31.14*
year fixed effects	x	x	x	x	x	x	x
area level trends	x	x	x	x	x	x	x
reporting district fixed effects	x	x	x	x	x	x	x
Observations	7,618	7,618	7,618	7,618	7,618	7,618	7,618
R-squared	0.48	0.72	0.88	0.86	0.14	0.77	0.77
F Statistic	18.57						

Standard errors below coefficient estimates, clustered at the reporting district level.

Source: Crime data 1990-2002 from Los Angeles Police Department; Property information from the Los Angeles County Assessor's Office via Dataquick software.

BID information is author's tabluations from city documents.

updated 9/20/04

Table 9: Crime Behavior and BID Expenditures

	total crime change as a function of the BID's		dollars per crime	
	total budget	safety budget	total budget	safety budget
Full Sample, Fixed Effects	-0.31	-0.82	3,226	1,220
	0.06**	0.15**		
Almost BIDs	-0.27	-0.74	3,704	1,351
	0.06**	0.16**		
Matching on pre-BID Crime Levels	-0.2393	-0.6373	4,179	1,569
	0.0514**	0.1330**		
Matching on pre-BID Annual Totals and Types of Crime	-0.2187	-0.6463	4,572	1,547
	0.0567**	0.1213**		
First Neighbors	-0.2727	-0.7221	3,667	1,385
	0.0580**	0.1540**		
Second Neighbors	-0.3361	-0.9123	2,975	1,096
	0.0601**	0.1601**		
Second Neighbors, IV	-1.83	weak	546	.
	0.73*			

Note: Standard errors below coefficients.

All regressions contain year fixed effects, area-level time trends, and reporting district fixed effects.

Table 10: Specification Checks

Overall Means	outcome is total crime			outcome is auto burglary and theft	forgery	fraud	embezzlement	family?
	total crime	merchant BIDs	property BIDs					
BIDs	460.8	443.2	481.1	85.8	18.9	0.53	0.23	0.99
All non-BIDs	316.6			47.7	10.9	0.20	0.12	1.02
Almost BIDs	388.4			54.2	11.3	0.32	0.15	1.34
First Neighbors	313.8			53.7	11.9	0.21	0.12	0.79
Second Neighbors	298.2			48.9	12.6	0.20	0.13	0.77
	outcome is total crime			outcome is auto burglary and theft	forgery	fraud	embezzlement	family?
	all BIDs	merchant BIDs	property BIDs					
Fixed Effects	-51.05	-20.10	-67.89	-23.34	-5.24	-0.24	0.03	-0.10
	11.0984**	12.13	15.4628**	5.1371**	2.2249*	0.0806**	0.03	0.07
Almost BIDs	-41.98	-12.22	-55.11	-17.48	-4.43	-0.14	0.00	-0.09
	14.3082**	16.02	17.0070**	6.0473**	3.35	0.10	0.04	0.10
Matching on pre-BID Crime Levels	-28.91	-16.21	-37.83	-14.80	-2.65	-0.16	0.03	-0.07
	10.4080**	10.93	14.4760**	3.8964**	2.20	0.0744*	0.03	0.07
Matching on pre-BID Annual Totals and Types of Crime	-30.54	-16.75	-40.56	-14.15	-3.40	-0.18	0.03	-0.07
	8.6561**	10.62	12.0479**	3.2203**	2.12	0.0764*	0.03	0.07
First Neighbors	-43.35	-20.32	-54.20	-20.51	-4.88	-0.25	0.01	-0.07
	11.9986**	12.13	16.2553**	5.0988**	2.51	0.0922**	0.04	0.08
Second Neighbors	-59.43	-22.24	-76.99	-25.04	-4.78	-0.30	0.02	-0.14
	11.5454**	12.86	15.3559**	5.1373**	2.3487*	0.0871**	0.03	0.08
Second Neighbors, IV	-323.65	.	.	-73.74	-4.86	-0.17	-0.01	-0.03
	118.93**			31.14*	3.05	0.11	0.04	0.1

Table 11: Measuring Police Enforcement

Overall Means

		arrests/crime			arrests		
	number of reporting districts	violent	non-violent	total	burglary	vehicle theft	drunkenness
BIDs	124	0.30	0.98	0.64	8.1	6.3	14.7
All non-BIDs	885	0.32	0.86	0.62	5.2	5.4	6.9
Almost BIDs	132	0.34	0.96	0.67	6.5	8.3	11.5
First Neighbors	291	0.32	0.95	0.66	5.2	4.8	7.0
Second Neighbors	472	0.33	0.93	0.64	4.8	4.8	6.5

Regression Results

	arrests/crime			arrests		
	violent	non-violent	total	burglary	vehicle theft	drunkenness
Fixed Effects	-0.02	-0.19	-0.12	-2.62	-0.95	3.02
	0.03	0.0755*	0.0398**	0.6738**	0.3902*	3.20
Almost BIDs	-0.02	-0.11	-0.06	-1.94	-0.35	-1.03
	0.01	0.08	0.03	0.8175*	0.53	3.58
Matching on pre-BID Crime Levels	-0.02	-0.19	-0.12	-0.94	-0.43	0.40
	0.02	0.0752*	0.0352**	0.4024*	0.32	2.34
Matching on pre-BID Annual Totals and Types o	-0.02	-0.15	-0.10	-0.90	-0.37	-0.30
	0.02	0.0574**	0.0312**	0.3590*	0.30	2.14
First Neighbors	-0.02	-0.18	-0.10	-2.02	-0.68	2.18
	0.02	0.0860*	0.0435*	0.5571**	0.41	2.92
Second Neighbors	-0.06	-0.23	-0.14	-2.38	-0.79	2.53
	0.05	0.0959*	0.0559*	0.5660**	0.42	3.04
Full Sample, IV						
Second Neighbors, IV	-0.31	0.14	-0.03	-4.15	-4.33	9.1
	0.08**	0.1	0.04	0.99**	0.83**	3.43**