

The Privatization of Public Safety in Urban Neighborhoods: Do Business Improvement Districts Reduce Violent Crime Among Adolescents?

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The business improvement district (BID) is a popular economic development and urban revitalization model in which local property and business owners must pay an assessment tax that funds supplementary services, including private security. BIDs constitute a controversial form of urban revitalization to some because they privatize economic development and public safety efforts in public space. This study examines whether BIDs provide tangible benefits beyond their immediate boundaries to local residents in the form of reduced violence among adolescents. The empirical analysis advances an existing literature dominated by evaluation studies by introducing a theoretically driven dataset with rich information on individual and neighborhood level variables. The analysis compares violent victimization among youths living in BID neighborhoods with those in similarly situated non-BID neighborhoods. We find no effect of BIDs on violence. However, we do find that youth violence is strongly correlated with neighborhood collective efficacy and family-related attributes of social control. In conclusion, we argue that BIDs may be an agent of crime reduction, but this benefit is likely concentrated only in their immediate boundaries and does not extend to youths living in surrounding neighborhoods.

Community economic development models have garnered substantial public and scholarly attention in recent decades as a promising strategy for addressing urban problems. In many cases, community-based organizations take the place of governmentally funded programs in promoting housing, employment, or business opportunities aimed at enhancing the quality of life for residents (Simon 2001). The business improvement district (BID) is perhaps the most popular iteration of this form of urban revitalization approach (Briffault 1999). BIDs have grown in the United States

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from around 400 in 1999 (Mitchell 2001) to close to 1,000 today. BIDs typically assess a tax on local business and property owners to fund supplementary neighborhood services including security, sanitation, place marketing, and urban planning (Mitchell 2001). Some BIDs also offer a wider range of services, including homeless outreach, employment programs, and school-based youth activities (Stokes 2002).

The popularity of the BID movement derives, at least partially, from the widespread belief that BIDs are efficient (Pack 1992) and even “more effective than government” at solving major urban problems, including crime, economic stagnation, and physical disorder. Several media outlets, including the New York Times, have reported that BIDs are the “engine” of urban renaissance (Mitchell 2001). Scholars have identified a number of key features that render BIDs particularly desirable, including their independence from local government, and their ability to solve the free rider problem, which has plagued other private revitalization efforts.

However, the BID model is one of the more “controversial recent developments in urban governance” (Briffault 1999: 366) because it raises a number of serious problems for residents of the local community (Briffault 1999; Garodnick 2000; Hochleitner 2003; Hoyt 2004; Pack 1992). Thoughtful critics argue, for example, that BIDs threaten local democracy and political accountability (Garodnick 2000; Lewis 2010), and perpetuate inequality through the differential delivery of services traditionally provided by public agencies (*Kessler v. Grand Central Dist.* 1998; Stark 1998). Critics further allege that some BIDs have excluded and even exploited the poor and homeless. In one controversial case, the Grand Central Partnership in New York was accused of hiring “goon squads” to use physical force to push the homeless out of the geographic boundaries of the BID. A federal judge subsequently found that the same partnership had violated the Fair Labor Standards Act by paying homeless and jobless persons to perform clerical and outreach work far below minimum wage (Briffault 1999). Given that BIDs are often a collective business venture, it is not difficult to imagine that they operate without full consideration for the interests of residents who live in or around the neighborhood in which they operate.

BIDs, however, appear to be a particularly attractive method of reducing crime. Several evaluation studies have found that geographic areas containing BIDs experience reductions in crime compared to non-BID areas (Brooks 2008; Cook & MacDonald 2011; Hoyt 2004). The crime reduction benefits of BIDs also appear to be substantially larger than their direct costs spent on private security (Cook & MacDonald 2011). BIDs can run the risk

of merely shifting, or displacing crime into adjacent residential areas. Examining this question using data from Philadelphia, Hoyt (2005) found little evidence to suggest that crime is displaced from BID areas into adjacent residential zones. Cook and MacDonald (2011) also find no evidence that crime increases in neighborhoods adjacent to BIDs in Los Angeles (LA) after they begin operation. On the other hand, research suggests that neighborhood crime rates are “ratcheted up” or down by geographic proximity to places with higher or lower rates of crime (see Morenoff, Sampson, & Raudenbush 2001; Sampson 2012: 239–45), suggesting that spatial diffusion is a potential effect of any crime reduction strategy. Under a spatial diffusion explanation, we would expect that BIDs may produce lower rates of crime in adjacent neighborhoods, such that the benefits of the crime prevention efforts of BIDs would have a ripple effect on their surrounding residential areas.

Absent from empirical work on the crime control benefits of BIDs, however, are theoretically informed studies that take into account neighborhood and family mechanisms that are known correlates of crime.¹ For example, studies examining the effects of BIDs on crime have not controlled for key neighborhood-level characteristics identified in the social disorganization literature (Sampson & Groves 1989; Shaw & McKay 1942). Empirical evidence suggests that collective efficacy—the institutional capacity of a community to solve collective problems—has significant effects on neighborhood crime victimization (Sampson 2012; Sampson, Raudenbush, & Earls 1997). Omitting collective efficacy from a statistical model of BIDs, therefore, may bias estimates of their effect on crime. Aside from a RAND Corporation technical report published on the baseline data used in this article (MacDonald et al. 2009), studies have not examined the effect of BIDs on the lived experience of individuals and families who reside within or around BID neighborhoods. This study extends the previous cross-sectional analysis published in the RAND technical report by incorporating a second wave of data, and by explicitly exploring the theoretical implications of our new statistical models for the public debate on the privatization of public space and neighborhood improvement.

This study thus advances the empirical literature on the effect of BIDs on crime in three important ways. First, we utilize a sophisticated neighborhood matching strategy to compare residents living in neighborhoods exposed to BIDs to those living in similarly situated neighborhoods not exposed to BIDs. Second, we control for critical neighborhood-level social disorganization variables to

¹ The lone exception is the technical report published by the RAND Corporation (MacDonald et al. 2009), on which the current study expands upon.

address a clear source of endogeneity in estimating the effect of BIDs on crime. Third, we use household-level victimization data to examine the effect of BIDs on individual households and adolescents who live within or near BID areas.

Ultimately, we are interested in the effect of BID activities on self-reported violent victimization among adolescents in LA. Youths serve as a useful target population because they experience the highest age-adjusted rate of violent victimization (Farrington 1989; Thornberry & Krohn 2000). Across a range of model specifications, we find little evidence that victimization is reduced among adolescents who reside in neighborhoods surrounding BIDs. These results should temper expectations about what BIDs can do for crime control in surrounding communities.

BIDs as Agents of Community Economic Development

BIDs are best understood within the wider context of place-based urban revitalization strategies in the United States. We therefore begin with a brief overview of other revitalization models, and then focus on BIDs in particular.

Community revitalization efforts are typically driven by the leadership and funding of government agencies. Over the past three decades, policy makers have pursued a wide range of public programs to combat concentrated poverty and crime in urban areas. Many state governments, for example, designated Enterprise Zones in low-income urban neighborhoods throughout the 1980s, and the federal government followed suit by enacting its own Enterprise Zone program in the early 1990s.² Enterprise Zones are government programs that attract business investment through tax abatement and commercial and industrial deregulation (Riposa 1996). These efforts promote community economic development primarily through job creation. The most recent evaluations of Enterprise Zone programs indicate little evidence of significant improvement in employment for local residents (Elvery 2009; Greenbaum & Engberg 2004; Neumark & Kolko 2010). No studies, to our knowledge, examine the effect of Enterprise Zones on crime.

In 1992, the federal government initiated Empowerment Zones (EZs), which added social services and employment training to the existing tax incentive and regulatory strategies embodied by

² Another well-known place-based community-level strategy is Weed and Seed. Unlike other programs discussed here, which focus primarily on the promotion of private investment and business, Weed and Seed combines intensified law enforcement activities with community-based prevention programming. Weed and Seed programs tend to vary substantially across sites making systematic evaluations difficult. In general, the results from evaluations of Weed and Seed programs are mixed (see, e.g., Dunworth & Mills 1999).

Enterprise Zones. EZs were designed as a “comprehensive attack on urban distress by blending economic tax incentives and social services in a coordinated effort among federal agencies and local communities” (Riposa 1996: 545). There are only a few rigorous evaluations of the EZ program, and they find that the effects on local communities are inconsistent and modest in size (Busso & Kline 2008; Oakley & Tsao 2006; Rich & Stoker 2010), particularly given the cost of the program.

The results of local development programs targeting neighborhoods with higher levels of poverty have proven similarly unpromising. Los Angeles Revitalization Zones, for example, provided tax credits on sales, property, and hiring to promote private investment in a wide range of neighborhoods in LA damaged by the 1992 riots (Spencer & Ong 2004). The program did not exert an observable effect on private investment.

The failure of large-scale place-based programs to produce measurable results has led to a call for community economic development models. In contrast to public place-based revitalization efforts—which are typically driven by federal, state, or local government—community economic development models emphasize the participation of local residents and private organizations in planning and implementing development programs.

BIDs emerged in the 1980s as a popular community economic development model for central business districts and tourism zones. Under this approach, businesses and properties located within designated areas are assessed an additional tax that funds BID services and operations. The growing popularity of BIDs has led some cities to apply the strategy, beyond its historical use in business districts and tourism zones, in areas that include residential living.

BIDs are primarily a private venture, but their establishment requires approval by local government. Property owners and business merchants in the same neighborhood typically band together to submit a BID proposal, which specifies the geographic boundaries and the strategic plan for financing and services. Cities vary in the number of signatures necessary to initiate the BID application process. Briffault (1999) suggests that this number ranges from one-fifth to two-thirds of property owners in the proposed BID area. If the government approves the BID proposal, city hall typically passes a local law or ordinance that establishes the BID as a formal entity. In some states, approval from city hall on its own is not sufficient. Local property and business owners are given a period of time to voice opposition to the proposal, and the proposal can be blocked if the number of written protests satisfies a statutorily specified threshold (Briffault 1999). Once a BID is established, however, all business and property owners within the district are

required to pay the BID assessment tax regardless of their support or opposition to the initiative.

The popularity of this community economics development (CED) model derives, at least partially, from the widespread belief that BIDs “really work” (MacDonald 1996). BIDs have been credited with “transforming” many urban business districts in Philadelphia, Cleveland, San Diego, Washington, and New York City. Scholars have reinforced the view that BIDs are effective agents of local change, pointing to a number of their valuable features. First, BIDs represent a form of governance largely freed from bureaucratic and political constraints. Thus, scholars have suggested that BIDs are positioned to provide efficient services, and to pursue innovative solutions to entrenched community problems (Garodnick 2000). Second, BID governance is highly sensitive to the needs of at least some local residents.³ Unlike public taxes, which are disbursed throughout the city to fund a variety of functions, BID tax collections all return directly to the BID to finance local projects. Third, BIDs permit local neighborhoods to increase taxes to supplement underfunded public services in political climates where doing so would otherwise be infeasible (Briffault 1999). This power is especially valuable when economic constraints deplete public support for nonvital public services. Investment in local security is a particularly strong example of this kind of expenditure. A 2001 survey of over 250 BIDs across the United States found that 36 percent were “very involved” in a range of security projects including the provision of supplementary private security guards, purchasing electronic security systems, and working closely with the city police force (Mitchell 2001). Fourth, BIDs remove “free-rider” or collective action problems in which local businesses or residents have an incentive not to contribute to a collective program if they can enjoy the benefits without paying for them (Davies 1997). This negative incentive structure is absent in the BID model because all business and property owners within the BID area are required to pay.

Despite their potential benefits, the BID structure can create serious problems for local residents, and especially nonproperty-owning residents. BIDs threaten the legitimacy of local democracy. BID board members typically consist of commercial landowners, merchants, elected officials, and local resident groups. These officers are either appointed by city government or elected by district

³ As downtown and commercial areas continue to pursue a mixed-use redevelopment strategy, the increase in residential dwellings within and around commercial and retail centers had blurred the distinction between commercial and residential interests. This redevelopment model has also complicated the traditional governance arrangements of BIDs. Recent conflicts have emerged over efforts to create BIDs in mixed use areas in Philadelphia (Russ 2012) and New York (Buckley 2012).

constituents. Where boards are appointed, state statutes sometimes require that board members have certain business expertise (e.g., real estate or finance) or that they be selected from a list assembled by the local chamber of commerce. Where the board is elected, some states restrict voting only to property owners, or individuals who pay the assessment. Furthermore, in some states, individual votes are weighted by the value of their property, or the size of their BID assessment fee (Briffault 1999). In short, the BID voting structure can deprive local residents of equal representation in quasi-governmental decisionmaking.

BIDs also pose potential accountability problems because they operate with little public oversight. Once the BID is established, state statutes do not typically require annual operational or financial reports, nor do they require approval from city hall for new projects (Barr 1997; Kennedy 1996). In the absence of oversight, the confluence of private business interests and quasi-governance creates a political space in which unfettered conflicts of interest can influence BID policy (Harcourt 2005; Lewis 2010). These concerns are heightened among BIDs that employ private security officers, and thus possess the capacity for exerting physical coercion. In response to evidence of illegal BID activity, a small number of cities have instated monitoring systems. One independent audit, for example, found “a wide variety of serious problems, including vastly inflated salaries for BID executives, conflicts of interest, illegal loans, use of illegal immigrants at below minimum wage, poor financial management, and dissatisfaction among property owners in the districts” (Kennedy 1996: 323; Lambert 1995). In response to these accountability concerns, LA, New York, and other cities have established annual reporting requirements on BID activities, but the reports provide only generic financial disclosure of BID operations.

Scholars and courts have also forcefully argued that BIDs perpetuate or even exacerbate social inequality in two ways (Reynolds 2004; Stark 1998; *Kessler v. Grand Central Dist.* 1998, dissent). First, BIDs may use institutional resources and political connections to divert the flow of capital investment from other neighborhoods in greater need of financial support. Second, the BID special assessment may decrease BID residents’ tolerance for taxes, and thus encourage them to oppose municipal tax increases designed to fund services for the rest of the city. The effect on BID residents’ attitudes toward municipal tax and spending decisions is particularly strong in relation to services already provided by the BID. Residents who receive adequate BID sanitation services, for example, have little to gain from a citywide increase in spending on sanitation; they may, as a result, voice opposition to such an increase. Similarly, BID residents may suffer from little discomfort

from a citywide decrease in spending on sanitation, and may therefore support such decisions regardless of their negative impact on poorer neighborhoods. These considerations suggest that BID special assessments realign the interests of BID residents—who tend to be wealthy and politically powerful—against the interests of the rest of the city. A final criticism of BIDs is their potential to turn public spaces into exclusive zones in which certain people, especially the homeless and poor, are discouraged from entering.

That BIDs threaten equitable, democratic, and accountable governance raises important questions about their value as strategies for community revitalization. Advocates of BIDs suggest that the negative externalities that BIDs impose—especially those imposed upon the residents of BIDs and their surrounding areas—are offset by the potential benefits they bestow in terms of safety. But very few studies evaluate the effect of BIDs on their surrounding areas, and their specific effect on residents who live in those areas. If BIDs are providing public safety benefits to their surrounding areas, we would expect to see that their crime prevention efforts spillover into adjacent residential neighborhoods by creating a “ripple effect” (Sampson 2012) of heightened social control.

BIDs, Crime, and Neighborhood Effects

Sociologists have long theorized that social and economic deprivation is an important cause of crime and related-problem behaviors. Substantial empirical research has linked poverty with crime (Elliott, Huizinga, & Menard 1989; Kposowa, Breault, & Harrison 1995). High levels of family disruption (e.g., single parent households) are also associated with serious crime and violence, particularly for minority adolescents (Glaeser & Sacerdote 1999; Ousey 2000; Sampson 1987; Sampson, Raudenbush, & Earls 1997; Shihadeh & Steffensmeier 1994). BIDs may decrease crime by attracting business, commerce, employment, and economic revitalization to the neighborhood that benefit adolescents most at risk for committing and experiencing violence.

Scholars have argued that signs of physical and social disorder lead to crime and violence. Wilson and Kelling’s (1982) broken windows theory explains that physical signs of decay and disorder, like graffiti and loitering, send a signal that people do not care about a neighborhood. The perceived erosion of the collective sense of “mutual regard” and “obligation of civility” leads law-abiding residents to feel less committed to participating in informal surveillance, and potential offenders to feel less bound by social norms. Potential offenders will also perceive that the environment is “uncontrolled and uncontrollable” and as a result, that the prob-

ability of detection and apprehension is low. Wilson and Kelling hypothesize that in the extreme, physical disorder can cause a serious violent crime. Relatively few rigorous studies have tested broken windows theory. A series of six small-scale field experiments in the Netherlands found that purposeful spreading of graffiti or trash lead to an increased likelihood of littering and other forms of disorder (Keizer, Lindenberg, & Steg 2008). While this study is suggestive, it is unclear whether disorder generates serious crime, and there is some correlation evidence suggesting that an association between disorder and violent crime is largely conditional on the level of collective efficacy in a neighborhood (Sampson & Raudenbush 1999).

BID activities could in theory decrease violent crime by repairing signs of disorder. Investment in street sanitation and physical beautification removes trash and graffiti, and improves decaying physical structures. An increase in local economic activity spurred by a BID may fill vacant commercial and residential lots in the neighborhood, thereby reducing signs of neighborhood abandonment. Spending on private security may also enforce the maintenance of "public civility." BID employment and homeless outreach services may also help reduce loitering in their areas.

BIDs also may affect crime by changing the criminal opportunity structure of neighborhoods. Routine activities theory suggests that crime victimization results from the convergence in time and place of the presence of a motivated offender, a desirable target, and the absence of an effective guardian (Cohen & Felson 1979). The key insight from this theory is that victimization can increase or decrease without any change in the underlying level of criminal propensity among individuals in a community. Holding criminality fixed, predatory crimes can change due to transformations in the geographic and temporal distributions of motivated offenders, targets, and guardians. Cross-sectional analyses show consistent associations between violent crime and the participation in activities that expose individuals to social circumstances in which motivated offenders, desirable targets, and low guardianship converge in both adolescent and general population samples (Messner & Blau 1987; Miethe, Stafford, & Sloane 1990; Schreck & Fisher 2004).

According to routine activities theory, BIDs may affect violent crime by their investment in private security and electronic surveillance equipment that increase the level of formal guardianship in neighborhoods.⁴ BIDs may also decrease violent crime by redesigning the architecture of public spaces, such as increasing street

⁴ Unfortunately, little systematic evidence is available regarding the effects of private security guards on crime (Welsh & Farrington 2009).

lighting, to make their neighborhoods less attractive for motivated offenders (Hanish & Guerra 2000; Felson 1987). BIDs, on the other hand, could increase the number of desirable criminal targets by promoting commercial transportation and sale of valuable products, and thereby attracting a larger number of persons who are both potential offenders and victims of crime.

Social disorganization theory emphasizes the effect of three key neighborhood-level processes on crime (Sampson 2012; Shaw & McKay 1942). Low participation in local community organizations leaves neighborhoods without effective institutions. Residential mobility weakens informal social networks that are important for establishing informal social controls. Ethnic and cultural diversity in neighborhoods impedes normative consensus among residents. Together, these attributes produce lower neighborhood collective efficacy, which theoretically reduces the ability of a community to address neighborhood crime problems (Sampson, Raudenbush, & Earls 1997). Prior research examining the effect of BIDs on crime has relied heavily on econometric models that do not take into account the theoretically important variables that are known neighborhood and individual correlates of crime.

Prior research has identified a number of important individual-level correlates of predatory crime. There is a clear associations between crime and gender (see McCord, Widom, & Crowell 2001), age (Loeber & Farrington 1998), race, and ethnicity (Dobrin 2001; Hanish & Guerra 2000). Research also suggests that serious crime and victimization is associated with the strength of adolescents' attachments to family and school (Lipsey & Derzon 1998; McCord, Widom, & Crowell 2001), and the extent to which they associate with peers who engage in delinquency, crime, and substance use.

There are also a number of family attributes that are important correlates of serious violent crime. Adolescents living in single parent-headed households and below the poverty line are at greater risk of both participating in and experiencing serious crime and violence (Loeber & Farrington 1998; McCord, Widom, & Crowell 2001). Immigration status is also correlated with reduced crime participation and victimization (MacDonald & Sampson 2012; Sampson, Morenoff, & Raudenbush 2005). Family residential mobility (moving frequently) is also associated with higher rates of serious crime victimization (see McCord, Widom, & Crowell 2001). In contrast, households with a significant family kinship network in their neighborhood have lower risks of crime victimization (see McCord, Widom, & Crowell 2001). Finally, stronger commitment to religious beliefs and practices is inversely related to crime victimization (see Hawkins et al. 1998). This study seeks to control for related theoretically important variables to determine the net impact of BIDs on violent crime among adolescents.

Study Setting

The BID program in LA started in 1994 with the establishment of a single, merchant-based district. By the beginning of this study in 2005, there were a total of 30 established BIDs located in 14 of the city's 15 council districts.⁵ BIDs are spread across a socioeconomically diverse group of neighborhoods in LA, reflecting a presence of BIDs in areas of relative poverty and affluence (see MacDonald et al. 2010 for details).⁶ For example, BID neighborhoods range from residential populations that are approximately 6–70 percent Hispanic, and from 5 to 23 percent female-headed households. BID neighborhoods also range from a 4 to 40 percent household poverty rate, and from a median household income of roughly \$10,000–\$100,000.⁷

BIDs are managed and operated by private nonprofit organizations, but they are chartered and regulated by the LA city government. An assessment on the BID's behalf is levied by the city through property or business tax collection within a designated geographic boundary and the city then transfers the funds to the nonprofit organization managing the daily operations of each BID (MacDonald et al. 2009).

BIDs in LA have annual budgets that range from \$50,000 to over \$4 million. On average, the BIDs in LA expend 35 percent of their budgets on beautification (e.g., trash removal, removing graffiti, landscaping), 23 percent on public safety (e.g., hiring private security, subsidizing satellite station for Los Angeles Police Department [LAPD]), 18 percent on administration (e.g., BID staff salaries, office space), 15 percent on marketing (e.g., advertising, market research, web design), and 5 percent on other functions.⁸ These averages, however, mask important variation across sites. Some BIDs, for example, provide no security services, while seven expend over 30 percent of their budget on security. Similarly, six

⁵ Twenty-eight of the 30 BIDs that existed in LA at the start of the study in 2005 are included in the subsequent analysis that yielded survey respondents in associated neighborhoods: Canoga Park, Century Corridor, Chatsworth, Chinatown, Downtown enter, Downtown Industrial, Encino, Figueroa Corridor, Granada Hills, Greater Lincoln Heights, Highland Park, Hollywood Entertainment, Hollywood Media, Jefferson Park, Larchmont Village, Lincoln Industrial Park, Los Feliz Village, Northridge, Reseda, San Pedro, Sherman Oaks, Studio City, Tarzana, Toy District, Van Nuys Blvd. Auto Row, Westwood Village, Wilmington, Wilshire Center.

⁶ Council district 6 is the only area in Los Angeles that does not have an established BID (see http://cityclerk.lacity.org/bids/bidgeol_est.pdf).

⁷ These statistics are based on 2000 census data, which correspond roughly with the midpoint of formation of the BIDs examined in this study.

⁸ Data compiled from BID annual reports provided by the Office of the City Clerk. (Retrieved on June 10, 2012 from <http://cityclerk.lacity.org/bids/index.htm>). See also MacDonald et al. (2009) for further details of BIDs in Los Angeles. Cook and MacDonald (2011) provide detailed descriptions of LA BID efforts focused on enhancing public safety.

BIDs expend 10 percent or less of their budget on beautification, while 16 spend over 30 percent. Fourteen BIDs expend 10 percent or less on marketing, while five spend roughly 30 percent or more.⁹

Methods

Data

The sample for this study consists of households in the city of LA living near BIDs or in a comparison group of neighborhoods not exposed to a BID. Subjects were selected based on a two-stage sampling design. First, we selected a sampling frame for surveying at least 10 randomly selected eligible households from each of the 147 census tracts in LA that either contain a BID or are directly adjacent to a BID.¹⁰ Second, we used a targeted sample allocation algorithm that randomly sampled between 10 and 50 eligible households in 85 of the remaining 690 census tracts in LA that neither contain a BID nor are adjacent to a BID.¹¹ This targeted sample allocation algorithm ensured that sampled households in the non-BID sample would be equivalent to households in the BID sample in exposure to structural correlates of violence measured by 2,000 census-tract level data on concentrated poverty, age composition, and ethnic makeup.¹² The average

⁹ Additional data on the budget, functions, and neighborhoods of BIDs in Los Angeles are presented by MacDonald et al. (2009).

¹⁰ We use census tracts to define neighborhood boundaries because they are designed to enclose demographically homogenous populations that range in size from 2,000 to 10,000 residents (mean = 4,000). We recognize that census tracts may not reflect neighborhood boundaries, and therefore conduct a sensitivity analysis using other neighborhood aggregations (e.g., neighborhood clusters that are larger than census tracts). Had we chosen a smaller unit of analysis, such as the census-block group, we would not have enough variation within blocks to estimate area effects with sufficient statistical power or reliability.

¹¹ Our allocation algorithm worked as follows. In BID census tracts, we sampled households randomly with only one constraint: at least 10 households were to be sampled from any of the 147 BID census tracts in which any households were sampled. We used the 2000 census data to estimate the expected characteristics of the average household sampled in these neighborhoods (see MacDonald et al. 2009: 56, table 4). There are 690 non-BID census tracts in Los Angeles. To form a comparison group, we subsample within these tracts using a targeted sample allocation algorithm to form a control group consisting of neighborhoods and households that are similar to those found in the BID neighborhoods. The allocation algorithm was driven by four primary rules: first, randomly sample households within the non-BID census tract that is most similar to BID census tracts on census variables; second, sample of no more than 50 households in any non-BID census tract; third, in a given tract sample no more than half of the number of households with children; fourth, construct a sample of at least 10 households in all tracts in which any households are sampled. Further technical details can be found in MacDonald et al. (2009).

¹² The specific measures were based on 2000 census tract data on: (1) concentrated disadvantage (percentage of people living below the poverty line, percentage of female single parents with children under 18, percentage of families on welfare, and percentage unemployed); (2) age structure (percentage of the population who are male and under 25

residential population of BID and comparison non-BID neighborhoods was equivalent.¹³

The sampling frame was constructed using a directory of listed telephone numbers in LA and their associated street addresses. This approach limits the sampling frame to listed numbers, which creates the potential for bias because of unknown differences between listed and unlisted households. We accepted this approach in place of random digit dialing because of the difficult challenge of determining the census tract in which unlisted households are located.¹⁴ Given our interest in the causal effect of BIDs on violent crime, the failure to sample unlisted households poses no threat to the internal validity of the study as long as unlisted households do not differ across BID and non-BID areas.

Trained interviewers conducted baseline telephone survey interviews with one adult¹⁵ and one youth (ages 14–17) in each household in the sample between October 2006 and February 2007.¹⁶ A second wave of data was collected 18 months later.¹⁷ Adolescents were questioned about a range of issues including violent victimization, school involvement, family relationships, peer delinquency, and recreational habits. Parents were questioned about their perception of safety, collective efficacy, informal social control, and local businesses in their neighborhood. These efforts yielded a sample of 737 households living in BID and non-BID comparison neighborhoods.¹⁸ The overall effective response rate

years of age); (3) residential stability (percentage of units occupied for 5 years or more, percentage of housing units that are owner occupied); (4) racial composition (e.g., percentage of Latino, percentage of foreign born); and (5) population density (e.g., population per square mile). The sampling algorithm was developed to have randomly sampled eligible households that were equivalent between BID and non-BID neighborhoods on their exposure to census tract features correlated with crime.

¹³ The average residential population according to 2000 census data for BID census tracts was 4,592 compared to 4,362 in comparison to non-BID census tracts. This mean difference in an independent sample *t*-test is not statistically significant ($p = 0.264$).

¹⁴ Survey firms have found that a high proportion of unlisted households collected through RDD refuse to provide their street addresses (see MacDonald et al. 2009 for further details).

¹⁵ For households with two adults or two or more eligible youths, we interviewed the adult or youth with the most recent birthday.

¹⁶ Calls were conducted between 9:00 a.m and 9:00 p.m. pacific standard time (PST). Project staff also held regular quality-control meetings with telephone interviewers and supervisors. Human subject protection committees (HSPCs) from RAND, RTI, and the Centers for Disease Control and Prevention (CDC) approved the procedures for the survey.

¹⁷ See MacDonald et al. (2009) for full details on the eligibility criteria. Research Triangle International (RTI) collected the baseline surveys and the Social Science Research Center (SSRC) at California State University Fullerton conducted the follow-up surveys.

¹⁸ Conserving interviewer resources for the important work of obtaining cooperation and conducting interviews also led to the use of market-research firms that were capable of dialing all list-assisted cases within two business days and assigning codes of (1) disconnected, (2) business, (3) English-speaking household, (4) Spanish-speaking household, or

was 40.2 percent. Of these 737 interviews, 85 percent ($n = 626$) completed a parent/youth dyad.¹⁹ Follow-up interviews were conducted with 635 (86 percent retention) households, of which 83 percent of households ($n = 535$) completed a parent/youth dyad.

Dependent Variable

The purpose of this study is to examine the effects of BIDs on violent crime outcomes for adolescents living in and around BIDs. We use household-level victimization data—rather than neighborhood-level official crime data—for two reasons.²⁰ First, youth victimization is highly underreported to the police (Finkelhor & Ormrod 2001). Second, household-level data permit us to examine the effect of BIDs on the subgroup of persons who live in or around BIDs, rather than the larger group of persons who use BIDs.

Adolescent respondents were asked whether they had experienced the following five forms of violent crime in the prior year: another youth trying to steal something from him or her by force; another youth threatening him or her with a gun, knife, or club; another youth hitting him or her badly enough to require bandages or a doctor; a physical attack by a group of two or more youth; seeing someone in the neighborhood being assaulted by a group of two or more youth.²¹ At baseline, the prevalence of violent

(5) unknown, ring, or no answer. Of the 44,762 cases that used this approach, only 16,323 (36.5 percent) were confirmed as residential numbers. Lead letters were mailed to these confirmed residences, and interviewers began conducting telephone follow-up for these numbers in December 2006. Because of low yields from initial list-assisted cases, we also modified our sampling procedure using marketing and driver's license data that identified households with a higher likelihood of containing an eligible adolescent.

¹⁹ A total of 96 households provided a parent interview but not a complete youth interview, and 15 households provided a youth interview but not a complete parent interview. A comparison of the targeted sample and the actual sample reveals that approximately 41 percent of census tracts ($n = 89$) did not meet the quota goal and 24 percent ($n = 52$) exceeded the quota goal. Under- and over-sampling, however, was distributed evenly across census tracts without distorting the balance of household features in BID and non-BID tracts. Inverse-probability weighting (IPW) of the BID and non-BID samples to their original allocations yielded effective sample sizes ($n = 365$ BID; $n = 390$ non-BID) that were statistically comparable to those actually achieved ($n = 362$ BID; $n = 374$ non-BID).

²⁰ Though BIDs are typically established in commercial areas, residential properties are frequently mixed into these neighborhoods. Furthermore, we measured BID areas as the legal boundaries of the BID and any immediately surrounding census tracts. Thus, there are generally a substantial number of residents in these areas. BID neighborhoods in our sample have an average population of roughly 4,600 residents.

²¹ The scalable measures of violent crime and neighborhood-level processes were adapted from publicly available instruments used in previous research, including the Project on Human Development in Chicago Neighborhoods, the National Longitudinal Study of Adolescent Health (Add Health), the Los Angeles Family and Neighborhood Survey, and other sources (see Peterson et al. 2004; Inter-University Consortium for Political and Social Research, undated; and CPC, undated).

crime ranged from a low of 2.3 percent for being seriously injured to a high of 16 percent for seeing someone in the neighborhood assaulted. At follow-up, the prevalence of these items was slightly lower for all variables (e.g., 13 percent saw someone assaulted), reflecting a general downward trend in violent crime in later adolescence and the city of LA.²² The five violent crime items were combined into a single scale. Witnessing violence served as the least serious reference category with the highest prevalence.²³

Independent Variables

This study uses both individual- and neighborhood-level independent variables. The first set of measures includes demographic characteristics of youths, including gender and age, and those of the parents in the household, including race (Asian, black, white), ethnicity (non-Latino, Latino), marital status (married or living as married vs. not), and socioeconomic status (SES). Approximately 52 percent of parents were identified as Latino. Because of the small number of households comprised of other racial and ethnic groups, we focus only on differences between Latinos and other ethnicities.²⁴ SES was measured with an average summed index of parents' reported levels of education, the household mortgage or rent, and the total household income. Parent's education was measured on a 6-point scale from less than a high school diploma to a graduate or professional degree. Household mortgage or rent was measured on a 6-point scale from \$500 or less to over \$2,500 per month. Household income was measured on a 6-point scale from \$20,000 or less to \$100,000 or more.²⁵ Parents were also asked whether they are immigrants, and roughly 57 percent were identified as such. Approximately 87 percent of immigrants self-identified as Latino.

The second set of measures taps elements of household social control. Parents were asked to rate the importance of religion in their household using a 4-point scaled question. Higher scores reflect less commitment to religious practice. To measure kinship

²² With this sample ($n = 1,372$) we have sufficient statistical power to detect effects even with a relatively low base of violent crime. If there is just a 0.04 difference (0.06 vs. 0.10) in the prevalence of violence between BID and non-BID comparison groups, we have a 0.84 probability of detecting this difference in a one-sided test. Furthermore, because we adjust for covariates and random effects within census tracts, the variance shrinks and increases our statistical power.

²³ For a similar approach, see Sampson, Morenoff, and Raudenbush (2005).

²⁴ Six percent of the sample is African American, 5 percent is Asian, and 34 percent is white. Because of insufficient counts of African Americans and Asians across the sampled neighborhoods, we could not parse out the correlation between these ethnic groups, and the individual, family, and neighborhood attributes that are the focus of our analysis.

²⁵ The average interitem covariance for this summed index was 1.41 ($\alpha = 0.75$) with an average correlation coefficient of 0.50.

networks in neighborhoods, parents were asked whether (= 1) or not (= 0) they have relatives, family members, or close friends living in the neighborhood. Finally, to capture household mobility, parents were asked the number of years they have lived in the neighborhood. Adolescents were asked to indicate their levels of attachment to family and school, as measured by eight 5-point scaled items that gauged how much they agreed with the following statements about school and family: you are close to people at school; you are part of your school; you are happy to be at school; the teachers treat you fairly; your parents care about you; people in your family understand you; you and your family have fun together; your family pays attention to you. We combined these items into an average summed scale, with an alpha reliability of 0.76. Higher scores on this scale represent higher attachment to family and school.

The third set of measures taps neighborhood attributes using theoretically derived questions related to social disorder and collective efficacy.²⁶ To measure disorder, respondents were asked on a 3-point scale—with response options ranging from “big problem” to “not a problem”—their view of 10 signs of physical and social disorder in their neighborhoods, including: litter or trash in the streets; graffiti; vacant housing or vacant storefronts; poorly maintained property; abandoned cars; drinking in public; selling or using drugs; homeless people or street panhandlers causing disturbances; groups of teenagers hanging out on street corners without adult supervision; people fighting or arguing in public. The 10 items were combined into an average summed scale, with an alpha reliability of 0.93.²⁷ Higher scores indicate that respondents perceived more disorder in their neighborhood.

To measure collective efficacy, parents were asked about their level of agreement with nine statements about their neighborhood: people around here are willing to help their neighbors; this is a close-knit neighborhood; people in this neighborhood can be trusted; people in this neighborhood generally don't get along with each other; people in this neighborhood do not share the same values; parents in this neighborhood know their children's friends; adults in this neighborhood know who the local children are; parents in this neighborhood generally know each other; people in this neighborhood are willing to do favors for each other, such as

²⁶ The household perceptions of neighborhoods may introduce some endogeneity into our model, as collective efficacy may be a cause and consequence of crime experience, but we use these variables following Sampson, Raudenbush, and Earls (1997) to measure neighborhood attributes that would otherwise not be available through the census.

²⁷ “Don't know” responses were recoded into the middle range for these items. *Neighborhood* was defined to respondents to include the block or street on which they live and several blocks or streets in each direction.

Table 1. Descriptive Statistics for Sample

Variable	Overall (n = 1,372)	BID Households (n = 672)	Comparison Households (n = 700)	t value	Range
Individual (Adolescent)					
Gender (=male)	53.6	54.2	53	0.42	0–1
Age of teen (years)	16.37	16.37	16.37	0.05	13–19
Social attachment	4.12	4.12	4.12	0.07	2–5
Household (Parent)					
Latino (%)	52.2	56.3	48.4	2.92*	0–1
SES (z-units)	3.09	2.99	3.19	2.73*	1–6
Married (%)	68.8	68	68.7	0.1	0–1
Immigrant household (%)	56.7	60.6	53	2.79*	0–1
Religion \wedge	1.58	1.6	1.57	0.53	1–4
Years in neighborhood	14.61	14.54	14.69	0.59	0–76
Kinship network (%)	21.7	23.3	20.2	1.38	0–1
Neighborhood attributes					
Disorder	1.69	1.71	1.66	1.51	1–3
Collective efficacy \wedge	2.18	2.21	2.16	2.23*	0.8–3.87

* $p < 0.05$.

Notes: n = 1,372 = 737 baseline + 635 follow-up interviews.

\wedge Higher values indicate less religious participation and less collective efficacy.

BID, business improvement district; SES, socioeconomic status.

watching each other's children, helping with shopping, or watching each other's houses when someone is out of town. The level of perceived informal social control in the neighborhood was assessed from six items asking residents how likely—with responses ranging from “very likely” to “very unlikely”—that neighbors would do something if: children were skipping school and hanging out on a street corner; children were spray painting graffiti on a sidewalk or building; children were showing disrespect to an adult; a fight broke out in public; a youth gang was hanging out on the street corner selling drugs and intimidating people; and a local school near home was threatened with closure due to budget cuts. Consistent with previous work (Sampson, Morenoff, & Raudenbush 2005), these two scales were closely associated ($r = 0.60$; $p < 0.01$) and were combined into a single average summed scale, with higher scores representing lower levels of collective efficacy. The alpha reliability was 0.86.

In addition to neighborhood-level variables collected through the survey, we also obtained annual homicide and robbery data for each of the police reporting districts from the Los Angeles Police Department.²⁸

Table 1 provides the descriptive statistics for the individual, household, and neighborhood variables for BID and non-BID

²⁸ We used spatial interpolation to integrate the shape files for the police reporting districts into census tracts using GIS software, such that homicide and robbery counts in census tracts consist of the proportion of land area overlapping a reporting district. There are, on average, 1.2 reporting districts per census tract.

comparison samples. A comparison of these descriptive data indicates only small differences between those living in BID and non-BID areas. The gender (~53 percent male), age (~16), and social attachment of sampled youths in BID and non-BID areas are remarkably similar. A significantly higher proportion of respondents in BID households were immigrant, Latino, and of lower SES ($p < 0.05$). In addition, BID respondents reported slightly lower levels of collective efficacy ($p < 0.05$) than those living in comparison areas.²⁹ Fortunately, the distribution of household variables in BID and non-BID neighborhoods suggests that the sample-allocation algorithm selected equivalent households across areas.³⁰

The results from the subsequent multivariate analyses are based on a final sample of 635 households clustered in 189 census tract neighborhoods interviewed at two periods in time ($n = 1,270$), with mean/median imputation adjustments made for missing data on the variables considered.³¹

Multivariate Analysis

We estimate a multilevel model that takes into account the hierarchical structure of violent crimes and respondent households being nested within BID and non-BID neighborhoods.³² The model is a three-level model with one level for household respondent, a second level for interview wave, and a third level for census tract. The model assumes that the variance is fixed for the household, but can vary randomly around the mean for the interview wave and census tract.³³

²⁹ Our sampling approach matches households in BID neighborhood to non-BID neighborhoods exposed to similar environmental attributes, but several years after the establishment of several BIDs. Therefore, it is possible that we have matched a set of BID neighborhoods to areas that are more similar after BIDs have fostered change than they were before.

³⁰ A more conservative Kolmogorov-Smirnov test for equality of distribution functions between the two groups found only a significant difference ($p < 0.05$) for SES.

³¹ Nonresponse in this study occurred because respondents indicated that they were unwilling or did not know how to answer some questions. Analysis of nonresponse patterns, with and without mean/median imputation adjustments, yielded estimates comparable to those herein, but deflated. We do not reweight neighborhoods because the sample allocation algorithm is designed to be self-weighting (for a similar approach, see Sampson, Morenoff, & Raudenbush 2005).

³² These models are also referred to as hierarchical linear models in the field of education statistics (Raudenbush & Bryk 2002) or variance component models in biostatistics and economics (McCulloch & Searle 2001).

³³ Sampson, Raudenbush, and Earls (1997) have used a similar model to examine the effect of collective efficacy on violence. Technically, these models are not identifying a true "causal" effect of neighborhoods as the conditions of neighborhoods are not randomized in an experimental design where everything aside from the BID condition is held constant. As Sampson (2013: 24) notes: "Unlike medical treatments that approximate a closed system,

$$\eta_{ijkt} = \mu + \sigma_i + \beta * BID_{jkt} + \delta * X_j + \lambda_k + \pi_l + \varepsilon_{ijkt},$$

where η_{ijkt} represents the odds of violent crime type i for household adolescent j residing in census tract k during interview wave t . BID_{jkt} is a dummy variable denoting whether the respondent lives in a BID neighborhood. X represents a matrix of individual, household, and neighborhood variables (immigrant or ethnic status, SES, age of teen, gender of teen, social bonds, religious importance, years in neighborhood, disorder, collective efficacy). Random intercept parameters λ and π are included to shift the model up or down by neighborhood and interview wave. The term σ represents a fixed effect parameter for each violent crime item i (witnessing a youth being attacked is the reference category). Thus, μ represents the intercept, or the average probability of violent crime adjusting for item responses, family and neighborhood attributes, census tract location, and interview wave. The error structure for this model is composed of fixed and random variance that is assumed to be normally distributed with mean 0 and constant variance.³⁴ From this model, we can estimate the difference in the odds of violent crime for respondents living in BID areas to those in non-BID comparison areas after controlling for observed family and neighborhood characteristics over time.

Table 2 displays models examining the association between BIDs and violent crime, controlling for the individual, household, and neighborhood variables. Model 1 presents estimates for household and neighborhood attributes, contrasting immigrants with all others. The results indicate that several of these household-related covariates are significantly associated with the odds of violent crime among youths. Living in a BID neighborhood, however, is not significantly associated with violent victimization. Residing in an immigrant household reduces the odds of violence by 45 percent compared to those of nonimmigrants (OR = 0.55; 95 percent

human behavior in social settings is *interdependent*—nothing is ever truly ‘held constant’” (see Sampson 2012 for extended discussion). In a related issue of identifying a BID effect, an anonymous reviewer wanted to know whether the multilevel model is necessary if we observed no statistically significant relationship between BIDs and violent crime in a bivariate analysis. However, we acknowledge that a statistically insignificant bivariate relationship would not imply a statistically insignificant relationship in the following multivariate model. Immigrant households, for example, one of the strongest protective factors against violent crime in our analysis, tend to cluster in poor neighborhoods without BIDs. Failing to account for immigrant households would bias the estimate of the effect of BIDs on violent crime downwards.

³⁴ These models were estimated using Stata 10.0, where the distribution of the random effects is assumed to be Gaussian and the conditional distribution of the response function (violence) is assumed to be Bernoulli, with probability of endorsing violence determined by the logistic cumulative distribution function (CDF). The log likelihood for this model has no closed form, so it is approximated in Stata by an adaptive Gaussian quadrature (see Stata Corporation 2005).

Table 2. Model of the Effect of BIDs on Youth Victimization—Census Tracts

Variable	Model 1		Model 2	
	OR	<i>p</i> value	OR	<i>p</i> value
Immigrant household	0.55	0.001	—	—
Latino	—	—	4.05	<0.001
Immigrant X Latino	—	—	0.27	0.001
SES	0.79	0.001	0.88	0.091
Age of teen	1.01	0.732	1.01	0.747
Married	1.05	0.712	0.96	0.823
Social bonds	0.68	<0.001	0.64	<0.001
Religion (>less)	1.18	0.028	1.22	0.009
Kinship network	1.93	<0.001	1.86	<0.001
Years in neighborhood	0.98	0.111	0.98	0.103
BID	1.09	0.644	0.99	0.98
Disorder (>more)	1.55	<0.001	1.43	0.004
Collective efficacy (>less)	1.49	0.011	1.55	0.006
X ²	96.41	<0.001	100.29	<0.001

Notes: *n* = 5,770 item responses, 577 households, 178 census tracts, 352 waves.

X² = likelihood ratio test comparing multilevel variance component to single variance logistic regression.

BID, business improvement district; SES, socioeconomic status.

CI = 0.39–0.78). Youths with increased bonds to family and school are also associated with less victimization. Greater social bonds were associated with a 0.68 odds of victimization (95 percent CI = 0.56–0.83). A standard deviation increase in one's reported social bonds to family and school reduced the odds of violence by 0.79.³⁵ Households reporting that religion is not important to them experienced higher odds of violence (OR = 1.18; 95 percent CI = 1.01–1.36). The presence of kinship (relatives, family, or close friends) in one's neighborhood is associated with twice the odds of violence (95 percent CI = 1.43–2.60). In terms of neighborhood features, both perceived social and physical disorders and collective efficacy are associated with violent crime. The results indicate that violent crime among adolescents is 1.55 times higher in areas with increased disorder (95 percent CI = 1.21–1.97). Converting the effect size into standard deviation units, a one standard deviation increase in disorder is associated with a 1.30 increase in odds of violent crime. Lower levels of collective efficacy in a respondent's neighborhood were associated with a 49 percent increase in the odds of violent crime (OR = 1.49; 95 percent CI = 1.09–2.03).

Given that a high proportion of immigrant households in this sample are also Latino, and given that our model estimated opposite effects on victimization for these two variables, we suspected they might share a nonlinear relationship with victimization. Model 2 captures the potential nonlinear relationship by interacting

³⁵ Calculated by taking the exponent of the log odds of social bonds multiplied by its standard deviation from the mean = $\text{Exp}(\text{Bx}_{\text{SD}})$.

Latino and immigrant. The results indicate a significant interaction term and suggest that youths from immigrant households of Latin American origin experience significantly lower odds of violent crime (OR = 0.27; 95 percent CI = 0.17–0.45), whereas nonimmigrant Latino youths experience higher odds (OR = 4.05; 95 percent CI = 2.50–6.55) relative to non-Latinos.³⁶

Again, we see no evidence of a systematic effect of BIDs on violent crime. The results from model 2 also indicate that higher perceptions of neighborhood disorder are associated with higher violent victimization (OR = 1.43; 95 percent CI = 1.11–1.84) and that collective efficacy continues to be significantly associated with a less violent victimization. A 1-unit decrease in reported collective efficacy is associated with a 55 percent increase in the odds of youth violence (OR = 1.55; 95 percent CI = 1.12–2.12).

Our models estimate no direct association between BIDs and the violent victimization among youth. It is possible that we have failed to detect the effect of BIDs because only some BIDs have an effect. BIDs that invest in private security, for example, may enjoy discernible reductions in violent crime for our respondents. We respond to this potential limitation by executing several additional model specifications that account for differences in BIDs.

First, we estimate whether any individual BID has an effect on violent crime by adding an individual parameter for each BID to the model. The results from this model shown in Appendix A1 include 13 of the BIDs that had sufficient sample size to estimate unique BID effects in the multilevel model. We find that no single BID has a significant effect on violent victimization.³⁷ Second, to investigate if more established BIDs have greater effects on violent crime, we estimated a model with a variable indicating the number of years each BID has been in operation. This variable did not have a material effect on violent crime outcomes.³⁸ These null effects can be seen graphically in Appendix A2. Third, we estimated a model with a variable indicating annual expenditure on private security

³⁶ These findings are consistent with Sampson, Morenoff, and Raudenbush (2005), who found that immigration status is associated with lower youth violence, even when comparing immigrant and nonimmigrant youths residing in the same neighborhoods with similar social and economic circumstances.

³⁷ An earlier analysis by MacDonald et al. (2009) indicated that 2 of the 19 BIDs examined had a higher prevalence of violent crime across multiple models. However, that analysis used only our baseline data and did not adjust standard errors for clustering within neighborhood census tracts.

³⁸ We also ran individual models for BID and non-BID areas separately. The estimates for the independent variables were substantively similar across models suggesting no specific BID interactions.

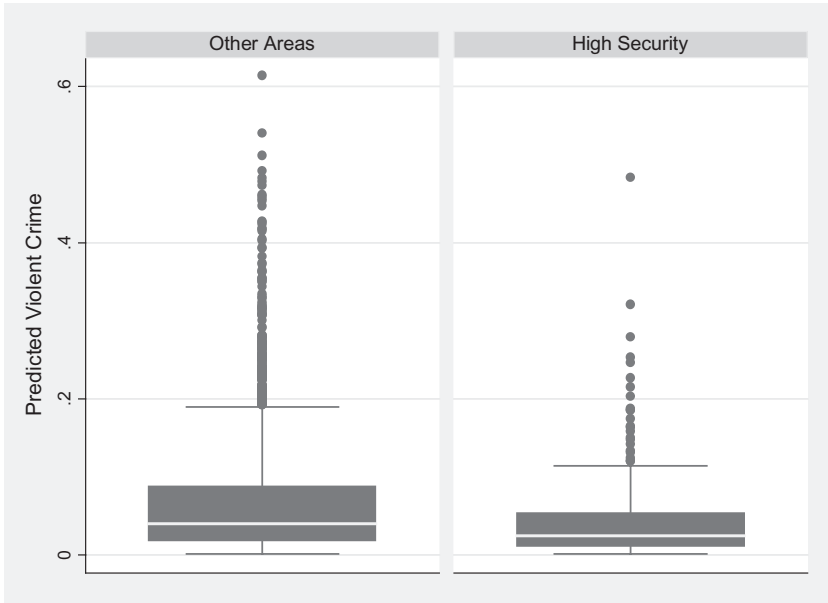


Figure 1. Box Plots of Probability of Violent Crime by BID Security Spending.

for each BID that has a budget line item for public safety.³⁹ The results (available from the authors) suggested no correlation between private security and violent victimization among our respondents. Figure 1 shows the box plots of the predicted probability of violent victimization for respondents living in the seven highest private security spending BID areas compared to all other neighborhoods. While the figure shows a slightly lower probability of violent crime in the high security spending BIDs, the distributions overlap substantially.

Fourth, we estimated an additional set of models that included neighborhood factors related to concentrated poverty,⁴⁰ residential stability (percentage of residents living in the neighborhood for

³⁹ We include the same measure reported in Cook and MacDonald (2011), which is the ratio of annual spending on private security (in 2005 dollars) to the number of neighborhoods exposed to the BID. Eighteen of the 28 BIDs examined in this study had line-item spending on private security and are included in the analysis. We included a dummy variable for BID presence and an interaction term between BID presence and private security spending to capture any potential effect.

⁴⁰ Concentrated poverty represents an index of the percentage of female-headed households, percentage unemployed, percentage living below the poverty line, and percentage receiving welfare. A principal component analysis indicated that 66 percent of the variation across all four measures could be explained by a single component (eigenvalue = 2.63).

5 years or longer), age (percentage of men under 25 years old) and Latino or immigrant concentration (average of percentage of Latino and foreign-born residents), population density (residents per square mile), as well as the violent crime rate per 100,000 residents (natural log of the average total 2004–2005 crime reports for murder, rape, robbery, and aggravated assault). Because these structural features of concentrated poverty, age, ethnicity, residential stability, and population density are highly correlated with each other (average $r > 0.60$), we estimate separate models for each covariate. The results of these models are displayed in Appendix A3 and show the same null effect of BIDs on violent crime.

These supplemental analyses present a consistent picture. BIDs have no direct association with violent victimization, but immigrant origin, parental social control, and neighborhood collective efficacy have strong correlations with violent victimization.

One limitation of the analysis is the small number of households in some census tracts which may lead to unstable results. To test the sensitivity of our estimates to this limitation, we conduct a second set of analyses in which adjacent census tracts are aggregated into larger geographic units, neighborhood clusters (NCs). We construct NCs using several selection criteria that result in 71 NCs representing 587 households over both time periods.⁴¹ We execute the same model on NCs as we do on the smaller census tract units, but the intercept is estimated at the NC rather than neighborhood census tract level.⁴² These NCs should be interpreted as geographically proximal census tracts with sufficient sample sizes to yield an area estimate.⁴³ The results from the NC model are displayed in Table 3 and are consistent with the census-tract level analysis in showing that BIDs have no significant direct association with youth violence in larger areas of aggregation. Disorder and collective efficacy are

⁴¹ First, no NC can consist of both BID and non-BID census tracts. Second, contiguous census tracts are clustered together. Third, NCs cannot contain more than four census tracts. Fourth, areas in which five tracts border each other were divided such that no single tract could account for more than two-thirds of the households in the NC. Fifth, BID neighborhoods must form a cluster with the closest BID location.

⁴² It is also possible to extend this model and add a random intercept for individual respondents, thereby allowing the effects of household and neighborhood attributes to vary within individual respondents and neighborhood locations. Such a model, however, would have to be based on the theory that there are different effects of SES and social bonds on violent victimization within respondents that are not confounded with the observed variables. We found such a theory highly improbable. We did, however, specify this three-level error structure, which contains random intercept terms at the individual and neighborhood levels. The results were not substantively different from the coefficients reported in the following section using our more conservative specifications.

⁴³ Unlike those in Chicago and other older cities, LA neighborhoods are not presently defined by the city planning agency by small area locations. Instead, LA neighborhoods are defined by larger geographic areas (e.g., neighborhood council areas) that make up diverse demographic and economic compositions. The definition of *neighborhood* in the household survey questions more closely applies to census tracts and not larger NCs.

Table 3. Model of the Effect of BIDs on Youth Victimization—Neighborhood Clusters

Variable	OR	95% CI	<i>p</i> value
BID	1.15	0.73–1.82	0.532
Disorder	1.26	1.00–1.59	0.043
Collective efficacy	1.25	0.93–1.67	0.123
X ²	54.02*		

**p* < 0.01.

Notes: Controlling for individual- and family-related variables shown in Table 3. *n* = 5,240 item responses, 542 individual households, 71 neighborhood clusters.

X² = likelihood ratio test comparing multilevel variance component to single variance logistic regression.

BID, business improvement district.

associated with the odds of youth violence, but the coefficients are substantially smaller than in the census tract-level model.⁴⁴

There are a number of notable limitations to this study. BIDs are not randomly allocated to neighborhoods in LA. Despite our best effort to allocate our sample in such a way as to balance the exposure of household respondents in BID and non-BID neighborhoods to similar structural features, it is reasonable to suspect that BID location is still not exogenous. A comparison of BID and matched area households indicated that the two groups differed only along a few observable features (e.g., SES, immigration status, percentage Latino). To remove the potentially confounding effects of these individual-level attributes, we estimated a series of multi-level models to control for these between-group differences. A key assumption of this modeling approach is that the features we observed create a set of equivalent comparison groups, so that the only remaining systematic difference is assignment to a BID or non-BID neighborhood. It is quite possible that there are systematic differences between households in BID areas and non-BID comparison areas that we did not measure with our household survey or census data.

As a result of some of these limitations, we explored whether we could predict living in a BID area based on variables measured in our survey. The results indicated that the observed individual- and family-related attributes, such as SES or ethnicity, were not predictive of living in a BID area and suggest that our design was effective in removing observed confounders.⁴⁵

⁴⁴ Individual/household attributes were also estimated but their coefficients were omitted from the table.

⁴⁵ Specifically, estimating the probability of living in a BID location by these observed features using logistic regression did not improve the predicted probability of living in a BID area over what one would get by using the intercept only, or the average case.

Conclusion and Discussion

BIDs pose a number of potential governance problems for residents in BID locales and their surrounding neighborhoods. Their governance structures and operational strategies have been described by critics as exclusionary and undemocratic. Their proponents point to their effectiveness in improving public environments, including crime reduction. Using a theoretically rich dataset on BID and non-BID comparison neighborhoods in LA, we set out to test whether BIDs do in fact provide safety benefits to residents. The primary analysis provided evidence that BIDs are not associated with a decrease in violent crime among youths living near BIDs compared to youths living in other neighborhoods matched on key structural conditions that are known to predict violent crime. These results suggest that BIDs do not produce systemic neighborhood change that improves violence outcomes for youths in and around BID areas.

The results from this study are important because they suggest that once relevant neighborhood-level characteristics are considered, the effects of BIDs on crime observed in the existing literature are likely more modest. While other research suggests that the adoption of BIDs leads to measurable reductions in officially reported crime within BID areas (see Brooks 2008; Cook & MacDonald 2011; MacDonald et al. 2010), these crime reductions may not affect the lives of youths and families living in or around them. At best, BIDs may provide enhanced local community services and social control that yield direct crime control benefits in their immediate boundaries, but these social benefits do not transcend to the outer borders of the neighborhoods they touch. Others have suggested that the crime control efforts of BIDs are designed mainly to impact customers and visitors of the commercial area (Hoyt 2005). We have found that in LA, even if BIDs make their service areas safer for users of the district, this outcome has little observable effect on the violent victimization of youths residing in the surrounding neighborhoods.

BID neighborhoods, compared to our matched control neighborhoods, are also not systematically correlated with differences in perceived neighborhood environments, such as collective efficacy or disorder. Including interactions between BIDs and these outcomes showed near zero correlations. Because we selected non-BID neighborhoods that are similar to BID neighborhoods, our methodology provides a very conservative test of perceived between-group differences in neighborhood environments.

For a policy discourse on citywide efforts to encourage the establishment of BIDs as catalysts of economic development and improved public safety, our study shows that understanding the broader neighborhood dynamics of the areas in which BIDs operate is more important for their roles in creating change than the simple adoption of

BIDs. While one of the main strengths of the BID model is its localized governance capability, in which local actors can tailor strategic responses to local economic development and community problems, our analysis suggests that BID organizations cannot be expected to serve as agents of change for larger systemic social problems related to neighborhood environments and violent crime.

Several supplemental results from our analysis confirm other seminal work in the sociological and criminological literatures. First, our analysis suggests that perceptions of neighborhood disorder and collective efficacy are associated with crime-related outcomes (Sampson, Raudenbush, & Earls 1997; Skogan 1990). In both BID and non-BID comparison neighborhoods, higher perceptions of trust and shared responsibilities for civic life were significantly associated with a lower likelihood of youth victimization. These findings are important because they suggest that neighborhood-level violent crime can be explained by more than differences in socioeconomic class and household characteristics, as Zimring also finds in his study of the decline in crime in New York City (Zimring 2011). Second, our results confirm a growing body of research indicating that immigrant households have lower rates of violent crime and victimization than households living in comparable neighborhood environments (MacDonald & Sampson 2012; Sampson, Morenoff, & Raudenbush 2005).

The analysis produced one puzzling result that we find difficult to explain. According to our model, adolescents whose parents indicate they have family members, relatives, or close friends living in the same neighborhood have higher exposure to violent crime. We wonder whether kinship network is correlated with other variables that increase the risk of victimization. Poorer households, for example, may develop stronger kinship networks over time if they lack the resources to move out of the neighborhood. Unfortunately, this is mere speculation, which we are unable to test with the data from the study.

Before concluding, one key limitation of our study is worth noting. Because we rely on observational data without a pre-post intervention design, we cannot know the extent to which the correlations observed are causally related. We attempted to remove the potential selection problems in two ways. First, we selected households in non-BID neighborhoods as a sample allocation algorithm to ensure that BID and non-BID comparison households were exposed to similar aggregate patterns of structural inequality. Second, in our statistical models on the effect of BIDs on youth victimization, we controlled for a number of theoretically salient individual, household, and neighborhood variables. However, it is possible that BID effects are observable only before and after their establishment. A more dynamic neighborhood-change process that affects youths may have already occurred that we cannot capture in this study.

Appendix A1: Individual BID Effects on Violent Crime

Variable	OR	95% CI	<i>p</i> value
BID1	0.71	0.38–1.34	0.303
BID2	0.26	0.04–1.45	0.126
BID3	1.97	0.72–5.35	0.182
BID4	0.39	0.07–1.94	0.253
BID5	1.39	0.65–2.96	0.393
BID6	0.78	0.18–3.24	0.735
BID7	2.34	0.89–6.14	0.082
BID8	1.85	0.58–5.84	0.291
BID9	1.70	0.64–4.52	0.284
BID10	1.04	0.26–4.13	0.948
BID11	0.32	0.05–1.79	0.197
BID12	1.43	0.47–4.38	0.523
BID13	1.56	0.46–5.23	0.465
X ²	81.76*		

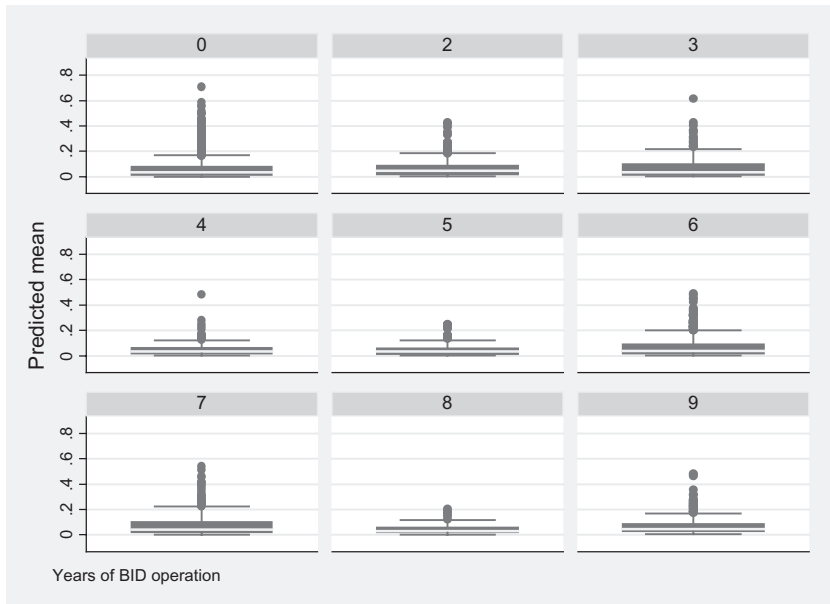
**p* < .01.

Notes: Controlling for individual- and family-related variables shown in Table 3. *n* = 5,700 item responses, 577 households, 178 census tracts, 352 waves. Individual BID names have been masked.

X² = likelihood ratio test comparing multilevel variance component to single variance logistic regression.

BID, business improvement district.

Appendix A2: Box Plots of Violent Crime by Years of BID Operation



Appendix A3. Structural Predictors of Violent Crime

Variable	OR	95% CI	<i>p</i> value
Model 1 (concentrated disadvantage)			
BID	0.98	0.66–1.46	0.948
Disorder	1.45	1.12–1.86	0.004
Collective efficacy	1.43	1.04–1.95	0.025
Concentrated disadvantage	1.08	0.93–1.25	0.312
Violent-crime rate (log)	1.00	0.67–1.450	0.983
X ²	84.12*		
Model 2 (percentage of men under 25)			
BID	1.03	0.69–1.55	0.849
Disorder	1.45	1.13–1.87	0.003
Collective efficacy	1.42	1.03–1.95	0.028
% of men under 25	1.01	0.96–1.07	0.475
Violent-crime rate (log)	1.04	0.70–1.54	0.827
X ²	84.69*		
Model 3 (immigrant concentration)			
BID	1.01	0.68–1.49	0.955
Disorder	1.46	1.13–1.87	0.003
Collective efficacy	1.43	1.04–1.97	0.024
Immigrant concentration	1.19	0.31–4.56	0.796
Violent-crime rate (log)	1.08	0.73–1.60	0.691
X ²	84.58*		
Model 4 (residential stability)			
BID	0.97	0.64–1.46	0.905
Disorder	1.45	1.13–1.87	0.003
Collective efficacy	1.43	1.04–1.96	0.024
Residential stability	0.64	0.18–2.24	0.496
Violent-crime rate (log)	1.06	0.73–1.54	0.720
X ²	84.46*		
Model 5 (population density)			
BID	0.97	0.65–1.46	0.911
Disorder	1.45	1.13–1.87	0.003
Collective efficacy	1.43	1.04–1.96	0.024
Population density	1.08	0.85–1.37	0.498
Violent-crime rate (log)	1.10	0.77–1.57	0.594
X ²	84.89*		

**p* < 0.01.

Notes: Controlling for individual- and family-related variables shown in Table 3. *n* = 5,485 item responses, 558 individual households, 178 census-tract neighborhoods.

X² = likelihood ratio test comparing multilevel variance component to single variance logistic regression.

BID, business improvement district.

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