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PUBLIC SAFETY THROUGH PRIVATE ACTION: AN ECONOMIC ASSESSMENT OF BIDS*

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Private actions to avoid and prevent criminal victimisation and assist public law enforcement are vital inputs into the crime-control process. One form of private action, the business improvement district (BID), appears particularly promising. A BID is a non-profit organisation created by property owners to provide local public goods, usually including public safety. Our analysis of 30 Los Angeles BIDs demonstrates that the social benefits of BID expenditures on security are a large multiple (about 20) of the private expenditures. Crime displacement appears minimal. Crime reduction in the BID areas has been accompanied by a reduction in arrests, suggesting further savings.

Given the vital role of private individuals and firms in determining the effectiveness of the criminal justice system, and the quality and availability of criminal opportunities, private actions arguably deserve a more central role in the analysis of crime and crime prevention policy.[1] But the leading scholarly commentaries on the crime drop during the 1990s have largely ignored the role of the private sector (Blumstein and Wallman, 2000; Levitt, 2004; Zimring, 2007). The potentially relevant trends include: growing reporting rates; the growing sophistication and use of alarms; monitoring equipment and locks; the considerable increase in the employment of private security guards; and the decline in the use of cash (Cook and MacDonald, 2010).

Private actions can be encouraged or discouraged through regulation of the insurance industry, reducing the costs of private co-operation with police and courts, gun control measures, and other means. The justification for such measures is the reasonable presumption that many sorts of private action to avoid, mitigate and respond to crime generate substantial externalities.

Business improvement districts (BIDs) are a particularly promising institution for harnessing private action to cost-effective crime control. A BID is a non-profit organisation created by neighbourhood property owners to provide local public goods, including public safety. The organisation has the power to tax all the owners in the district, including those who did not sign the original petition. Previous evaluations of BIDs in Los Angeles (LA) indicate that they are successful in reducing crime rates (Brooks, 2008; MacDonald *et al.*, 2009). We provide a further analysis of the costs and benefits, including the effect on arrests and spillovers, and we estimate a dose–response

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[1] The theory of how private action to prevent and avoid crime interacts with observed crime rates has been developed by Clotfelter (1978), Ehrlich (1981), Cook (1986), Shavell (1991), Philipson and Posner (1996) and others. Gary Becker's (1968) seminal article on crime and punishment includes a brief discussion of optimal private expenditures on actions to influence the probability of victimisation (pp. 200 ff.).

relationship for private expenditures. We conclude that an additional $10,000 spent by an LA BID on crime control generates over $200,000 in societal benefits. The rules for creating BIDs differ widely among jurisdictions, and LA appears to be a model for how to facilitate this sort of private collective action.

We begin with a brief conceptual discussion of private action, followed by a more detailed description of how BIDs are created and managed in LA. Subsequent sections discuss the data and methods of our analysis, the econometric results and a cost–benefit analysis based on our estimated reductions in crimes.

1. Incentives and Consequences of Private Prevention Activities

Private security, and private crime-control efforts more generally, constitute an unwritten chapter in the recent literature on 'what works' in crime-control policy. Observed crime rates and patterns reflect private choices regarding co-operation and self-protection (Clotfelter,1977; Cook, 1986; Ehrlich, 1996; Cornish and Clarke, 2003). A systematic approach to public crime control requires understanding of the potential interactions between private and public efforts.

A place to begin the discussion of this complex topic is with the private security industry, which is ubiquitous. On any given outing, we are more likely to encounter a private security guard than a uniformed police officer (Sklansky, 2008, p. 124–5). The industry encompasses proprietary (in-house) security, guard and patrol services, alarm services, private investigations, armoured car services and security consultants, as well as security equipment (Cunningham *et al.*, 1990). Private security supplements and in some cases substitutes for public action: for example, businesses in many cases investigate and resolve employee theft and fraud without ever calling the police. More generally, as noted by Brian Forst, 'the central functions of policing – preserving domestic peace and order, preventing and responding to crimes – have always been conducted first, foremost, and predominantly by private means... Most crimes still are not reported to the police (Forst, 1999, p. 19)'.

Private security guards (and police officers who moonlight as private security guards) typically serve a narrow purpose, namely to protect the property and people they are hired to protect. The term of art is 'situational crime prevention' (Clarke, 1983). The guard's job is accomplished if the robbers avoid his bank, or his corporate executive is not kidnapped, or rowdy teenagers are successfully kicked out of his shopping mall, or the would-be burglar does not enter his gated community. One partial exception is the security hired by BIDs, whose assignment is to protect an entire neighbourhood.

Unfortunately there is little systematic evidence on the crime prevention effects of private security guards (Eck, 2006; Welsh and Farrington, 2009). An obvious possibility is that the crime will be displaced to other, unguarded victims and places.[2] If private security simply redistributes crime, then its public value (as opposed to private) is nil.[3]

[2] Displacement may explain the weak findings observed by Benson and Mast (2001). They analysed a 16-year panel on US counties to assess the effect of private security on crime rates. They got mixed results on various estimates of the effects of private security on robbery and homicide rates; only for the crime of rape did they find a consistent negative effect.

[3] Further, there is a danger that affluent people will become less willing to support public policing if they are purchasing effective private protection (Bayley and Shearing, 2001, p. 30).

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While displacement is a legitimate concern, whether it occurs in practice, and to what degree, is an empirical matter. Guerette and Bowers (2009) reviewed 102 evaluations of situational crime prevention interventions, which included 574 observations. They report that displacement was about as likely as the opposite, *diffusion* of benefits, and that if displacement did occur, it tended to be less than the direct effect. Draca *et al.* (2010) report that a surge in police presence in London following terrorist attacks reduced crime in the targeted boroughs without any evidence of displacement, a null finding that is typical of evaluations of hot-spots policing and related interventions.

There is also a conceptual point to be made. Lucrative opportunities, if unguarded, are likely to generate crime that would not otherwise occur. In Isaac Ehrlich's (1974) classic formulation, the supply of offences is a function of the relative wage rates for licit and illicit activities. An increase in the net return (payoff per unit of effort) to crime will stimulate participation in criminal activity. He postulates that the payoffs to property crimes 'depend, primarily, on the level of transferable assets in the community, that is, on opportunities provided by potential victims of crime' (p. 87). But if the most lucrative 'transferable assets' are well protected, then the payoff to crime is reduced. Of course, it is the most lucrative targets that tend to be most closely guarded. Banks invest more in security against robbery than, say, travel agencies. Jewellery stores display costume jewellery on open racks but keep the real thing in glass cases wired with alarms. People with meagre assets do not need bodyguards to protect against being kidnapped for ransom. Credit card companies have instituted elaborate systems for preventing fraudulent use.

The social welfare implications of private action to avoid victimisation depend on how well private incentives coincide with social costs. To the extent that private protection *does* have the effect of displacing rather than (or as well as) preventing crime, then such measures will tend to be oversupplied, as the private benefit will exceed the social benefit. That tendency may be exacerbated if the private action is subsidised by the public, as in the case of residential alarms that mobilise the police at no cost to the owner. On the other hand, a wide array of private actions appear to have positive externalities in crime control, including voluntary co-operation with the police and courts by victims and witnesses.

2. The Creation of Business Improvement Districts in Los Angeles

Business improvement districts offer an example of private action that combines situational crime prevention with a close working relationship with the police, and which, while non-governmental, are the result of collective action.

These self-taxing entities raise money to pay for private security guards, combat disorder and generally repair 'broken windows' directly, while also advocating improved policing and other city services (MacDonald and Stokes, 2006). Services provided by BID organisations within a defined district supplement those provided by public agencies. BID services often include trash collection, private security officers and CCTV cameras, as well as marketing and place promotion and development planning. BIDs exist in urban areas 'to make places attractive – safer, cleaner and more marketable' (Mitchell, 2008, p. 3).

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Business improvement districts are private entities but they are typically chartered by state legislation and regulated by local governments (Briffault, 1999; Mitchell, 2001). The method for collecting assessments for BIDs differs across jurisdictions. In some states the assessments are collected by municipal agencies and then transferred to a private sector non-profit organisation that manages the operations of the BID. In other locales assessments are collected directly by the non-profit organisations managing the BIDs. Relying on non-profit agencies to collect assessments can create difficulties when property owners are delinquent in paying (Briffault, 1999; Stokes, 2006). In any event, the number of BIDs is growing rapidly in the United States, from about 400 in 1999 (Mitchell, 2001) to something like 1,000 in 2010. BIDs have also been created in recent years in several jurisdictions in England, Canada and Germany.

In Los Angeles (LA), California BIDs are managed and operated by private non-profit organisations but they are chartered and regulated by the city government. The LA city clerk's Administrative Services Division manages the city's BID programme. The city levies an assessment on the BID's behalf through property or business tax collection, charges each BID a fee for the transaction, and then transfers the funds to the nonprofit organisation managing the daily operations of each BID (MacDonald *et al.*, 2009).

The adoption of a BID in LA requires extensive planning and support from business and property owners. A formal planning phase for the BID must be outlined and presented to the LA city clerk's office. In the planning phase LA requires the use of outside consultants to develop a formal BID plan, including a membership database, the design and geography to incorporate the BID, an assessment formula for financing services and a plan to incorporate a non-profit organisation to manage daily operations of the BID. At least 15% of the business owners or more than 50% of the property owners must sign supporting petitions for a formal BID proposal to be accepted by the city. Subsequent to a formal proposal being approved by the LA city clerk's and city attorney's office a laborious process of legal and legislative oversight ensues, including: a formal vote of the majority of property owners and merchants weighted by level of property assessment; a five-year service and budget plan for operating the BID; and a review of documents by the LA city clerk's and city attorney's office. After all planning stages have been successfully met, a series of public meetings are held prior to an enabling vote by the LA city council that officially charters BIDs (MacDonald *et al.*, 2009). After five years, the BID has to be reauthorised by another formal plan and vote of property owners to continue its operations. LA offers some financial assistance for BID formation planning.

Los Angeles has also embedded several accountability measures to regulating BIDs. For example, the non-profit organisations managing BIDs are required to provide the city with financial reports, and the city can audit and shut down any BID organisation whose operations are deemed to be out of compliance with the proposed service plan, or for financial irregularities (MacDonald *et al.*, 2009).

Many of the BIDs in LA focus their services on sanitation and private security of common public-space areas. 'Clean' and 'safe' are common terms used by BIDs in LA. Eleven of the 30 BIDs operating in LA in 2005 spent more than $200,000 a year on private security operations, with nearly equal amounts being spent on sanitation services. The Figueroa Corridor BID and Hollywood Entertainment BID provide good

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examples of BIDs with a focus on sanitation and safety. The Figueroa Corridor BID was formed in 1998 by business property owners in direct response to economic decline and a concern with area crime. From the outset its efforts were focused on improving community safety by employing uniformed private security workers (Safety Ambassadors) who patrol the district on foot, bike and evening vehicle patrols and assist in keeping order. It spends close to $500,000 a year, or almost half of its operational budget, on these officers. This BID also employs cleaning crews that remove trash, debris and graffiti (Holter, 2002). On a monthly basis the BID collects and removes more than 3,000–4,000 bags of trash and 1,000–5,000 square feet of graffiti.[4]

The Hollywood Entertainment BID employs armed private security officers who are retired law-enforcement officers. These officers patrol the Hollywood district seven days a week during evening hours, initiate citizen arrests when they observe violations of the law and work closely with the Los Angeles Police Department (LAPD). It spends just over $1 million a year on private security, or approximately 47% of its operating budget. It has also installed eight CCTV cameras at intersections in the district for use by the LAPD (http://www.hollywoodbid.org/).

Brooks (2008) conducted an evaluation of the effects of BIDs on crime in Los Angeles neighbourhoods and found that their adoption was associated with a significant drop in the number of serious crimes reported to the police between 1990 and 2002. Her analysis of BID effects on crime controlled for persistent differences between neighbourhoods, and used as a control group neighbourhoods that proposed BIDs but did not end up adopting them. A more recent analysis by MacDonald *et al.* (2009) using data from 1994–2005 in LA found significant pre-post declines in robbery and violent crimes in areas that adopted BIDs.

These evaluations treated BIDs as a binary phenomenon, whereas in fact they differ widely with respect to the amount spent on crime prevention. In what follows we estimate the dose–response relationship. Another limitation of previous studies is that they did not fully consider the cost of BIDs to the public, and in particular the use of police services. The BID provides greater capacity to mobilise the police, and BIDs have been criticised for encouraging the increased use of police arrest powers in their districts and displacing disorder and crime to adjacent areas (Harcourt, 2005). If BIDs reduce crime by increasing arrests in their districts, the additional cost to the public of arrests, related prosecutions and incarcerations should be incorporated in the cost–benefit analysis.

3. Data and Statistical Methods

The data for examining the effects of BIDs on crime and arrests consist of the yearly counts of nine officially recorded felony crimes that correspond to the Federal Bureau of Investigation's index offences (homicide, rape, robbery, assault-aggravated, burglary, burglary-theft automobile, theft-personal, theft-other, auto theft) and arrests by the LAPD for years 1994–2005, a time span which encompasses the creation of all BIDs. Crime and arrest data were aggregated to neighbourhoods represented by LAPD

[4] http://www.figueroacorridor.org/uploads/Spring2008Newsletter.pdf–sum2007.pdf

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police-reporting districts (the lowest level of available geography) and attached to corresponding BID areas. Similar to census tracts, reporting districts were created by the LAPD to correspond closely to neighbourhoods. Reporting districts occupy more territory in areas where the residential population and housing density is lower. The number of crimes or arrests per reporting district is effectively a rate per unit of resident population in more residential areas. The total number of crimes and arrests are a summation of counts for the Federal Bureau of Investigation's index offences noted above. We focus our analysis primarily on the counts of robbery, assault, burglary and auto theft because these crimes are more likely than other crimes to occur in public settings and be affected by BID services. As noted, those services seek to limit access to victims by improving the level of social control of public space through environmental design modifications, private security and increased co-ordination with the LAPD. Table 1 presents the descriptive statistics on these outcomes for the LA time series.

Identifying the effect of BIDs on reported crimes and arrests is complicated by the fact that neighbourhoods self-select to form BIDs. To address the potential selection bias, we estimated the effect of BIDs on crime and arrests by using a longitudinal analysis of neighbourhood-level crime and arrest data. Neighbourhoods adopt BIDs at different times, and we rely on the timing of BID adoption as our identification strategy. We assume that shifts in the number of crimes or arrests (Y), in a given neighbourhood, is a function of the timing of BID implementation and other unmeasured factors according to the following form:

$$Y_{it} = \mu + \alpha_i + \beta \mathrm{bid}_{it} + \delta_{d(i)t} \mathrm{Year}_t \times \mathrm{div}_{d(i)} + s_{it}. \tag{1}$$

In (1), i (= 1, . . . , 1,072) denotes the neighbourhood (police reporting district) and t the year (= 1994, . . . , 2005) of observation, α_i represents the fixed-effect parameter for each neighbourhood i, and bid_{it} is a dummy variable that assumes value 1 from the year

Table 1

Summary Statistics for Annual Crime and Arrest Count Data in LA Neighbourhoods

	Mean	SD: Overall	SD: Across neighbourhood	Min	Max
Crime					
Total	186.22	165.70	68.69	0	3,241
Robbery	17.82	20.56	9.65	0	193
Assault	29.80	33.30	12.89	0	268
Burglary	26.60	22.42	12.78	0	185
Auto theft	33.38	30.42	15.97	0	310
Arrests					
Total	23.64	40.99	29.19	0	579
Robbery	2.94	5.33	3.89	0	82
Assault	7.76	13.07	10.01	0	234
Burglary	2.87	5.04	3.78	0	72
Auto theft	2.64	4.61	3.39	0	76

Notes. The data represent 1,072 LA neighbourhoods over the period 1994–2005. Total crime includes nine crime outcomes (murder, robbery, aggravated assault, assault-aggravated, burglary, burglary-theft automobile, theft-personal, theft-other, auto theft). The standard deviation titled 'Across neighbourhood' utilises the deviations of the neighbourhood means from the grand mean. The full sample represents 12,864 neighbourhood years of crime data (1,072 neighbourhood reporting districts × 12 years).

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in which a BID becomes operational for those neighbourhoods that are eventually incorporated in a BID. We also include interaction terms for years (1995–2005) with each of the 19 police divisions that represent larger territories surrounding each BID area: $\text{div}_{d(i)}$ is a set of dummies indicating police divisions ($d = 1, \ldots, 19$).[5] Therefore, β is the estimated BID effect; see Brooks (2008) – for a somewhat similar specification.

The specification of the estimated BID effect on crime and arrests is extended by including controls for the two years prior to and after BID adoption. In particular, we introduce indicators $E_{T,i,t}$ which indicate when the neighbourhood had or will have a BID in place for $T = -2, -1, 0, +1$ or $+2$ years. (For example, $E_{2,i2001} = 1$ for all i that were in two-year-old BIDs as of 2001.) If BIDs have only short-run effects, this specification should capture those effects directly. If the timing of BID formation is influenced by short-term movements in crime, then the pre-BID coefficients in this formulation should indicate that sort of endogeneity.

$$Y_{it} = \mu + \alpha_i + \beta \text{bid}_{it} + \delta_{d(i)t}\text{Year}_t \times \text{div}_{d(i)} + \Sigma\theta_T E_{T,it} + s_{it}. \tag{2}$$

Finally, we examine the effect of BIDs by year, using a difference-in-differences model with neighbourhood (α_i) and year (ζ_t) fixed effects.

$$Y_{it} = \mu + \alpha_i + \varsigma_t + \eta_t \text{Year}_t \times \text{bid}_{it} + s_{it}. \tag{3}$$

In (3), the set of coefficients of interest are represented by the interaction terms (η_t). These coefficients are the effect of having a BID after controlling for neighbourhood and city-wide trends. By interacting bid with Year, we capture any time-varying heterogeneity in this effect. Huber/White robust standard errors are clustered at the neighbourhood level to allow for non-independence within neighbourhoods.

Our primary specification in (1) provides only an estimate of the effect of BID presence, and does not identify the mechanism by which the BID intervention affects crime rates. BIDs differ in size, location, management and priorities – including the scale of private security expenditures. We include a measure ($\text{Security\$}_{it}$) of private security spending to capture the effect of varying the BID 'dose'. This measure is the ratio of annual spending on private security (in 2005 dollars) for each BID to the number of neighbourhood reporting districts exposed to that BID. Nine BIDs do not have any line-item private security expenditures in their budgets, so for them Security\$ = 0 even for years in which there is a BID. As a test of our dose–response formulation we include a separate indicator for those 'zero expenditure' BIDs. ZeroSecurity\$ = 1 for all i, t in which there is a BID that spends nothing on security. Our prediction is that these BIDs will not affect crime rates.

$$Y_{it} = \mu + \alpha_i + \beta \text{Security\$}_{it} + \gamma \text{ZeroSecurity\$}_{it} + \delta_{d(i)t}\text{Year}_t \times \text{div}_{d(i)t} + s_{it}. \tag{4}$$

It is plausible that higher crime neighbourhoods tend to have greater variability.[6] We re-weight the standard errors by the inverse of neighbourhood's ranked crime rate to

[5] By including interaction terms for police divisions and years we capture the spurious relationship that could arise if division-level trends in crime happened to be correlated with BID creation (Anselin, 2001).

[6] As expected, the unadjusted variance was higher in neighbourhoods with higher rates of crime.

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reduce the influence of this form of heteroscedasticity on our estimate of BID security dosage.[7] This approach discounts the influence that high crime rate areas have on the estimate of BID private security dosage by increasing the standard errors for neighbourhoods with higher rates of crime.[8]

4. Econometric Results

Figure 1 depicts the average incidence of all felony crimes per year (1994–2005) for the neighbourhoods that ultimately adopted BIDs and those that did not. This figure makes it clear that neighbourhoods with higher crime rates were more likely to become part of a BID, but that BID and non-BID neighbourhoods followed the same overall downward trend during the 11 years. The overall drop in arrests in LA in BID and non-BID areas also reflects a discontinuity that occurred between 1999 and 2000, when the LAPD changed the classification of some arrests for aggravated assault to simple assault. This change in classification by the LAPD accounts for roughly half the net reduction in total arrests. Therefore, it is important to underscore that we control for overall yearly effects when assessing BID effects on arrests.

Table 2 shows that the number of neighbourhoods receiving the BID intervention by different years in time. We use the timing of the BID intervention as our identification strategy, with the implicit assumption that the timing of BID implementation is

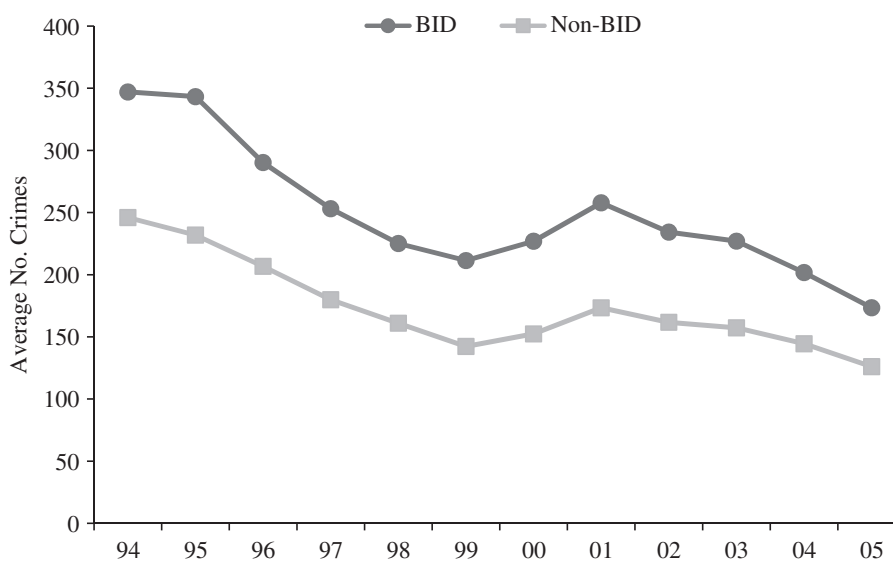

Fig. 1. *Change in Crime in LA Neighbourhoods*

[7] Neighbourhoods were ranked from 1–1,072 in order of lowest to highest average crime rate.

[8] Re-weighting the standard errors by the neighbourhood's crime rate rank has similar intuition to Huber/White standard errors corrections. However, in this case we actually specify what the standard error correction will be by assuming that the standard errors of the estimate of spending of BIDs on private security will be greater as the level of crime increases in neighbourhoods. By increasing standard errors for neighbourhoods with higher rates of crime we reduce the influence that this form of heterogeneity will have on our BID dosage parameter.

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Table 2

Implementation of LA BIDs

Year	No. RDs affected	No. BIDs started	BID area
1994	0	0	–
1995	0	0	–
1996	37	2	Wilshire Center, Fashion District
1997	47	2	Hollywood Entertainment, San Pedro
1998	96	6	Los Feliz Village, Larchmont Village Downtown Center, Figueroa Corridor Century Corridor, Greater Lincoln Heights
1999	146	11	Granada Hills, Canoga Park, Van Nuys Blvd., Tarzana, Studio City, Hollywood Media, Westwood Village, Historic Core (Downtown) Toy District, Downtown Industrial, Jefferson Park
2000	154	2	Chatsworth, Sherman Oaks
2001	163	4	Encino, Chinatown, Wilmington Lincoln Heights Industrial
2002	169	2	Northridge, Highland Park
2003	179	1	Reseda
2004	179	0	–
2005	179	0	–

independent of neighbourhood-specific trends (relative to district trends). We offer a partial test of that assumption in what follows.

Tables 3 and 4 report our estimates of the impact of BIDs on changes in incidence of total reported crimes and arrests, as well as separate estimates for the outcomes of robbery, assault, burglary and auto theft. Table 3 has the primary fixed-effects specifications (Model 1). Table 4 shows the results when we include indicators for the timing of the creation of the BID.

The results from these regressions indicate a substantial effect of BIDs on crimes and arrests. The introduction of BIDs is associated with roughly 28 fewer total serious crimes per neighbourhood. BID neighbourhoods averaged 249 crimes per year, implying an 11% relative decline in crime associated with BID implementation. The

Table 3

Effect of BIDs on Crimes and Arrests Regression Estimates, Model 1

	Total	Robbery	Assault	Burglary	Auto theft
Crime					
BID	−27.992***	−4.743***	−2.388***	−3.151***	−3.599**
	(4.99)	(5.71)	(2.52)	(3.30)	(2.09)
Arrests					
BID	−9.625***	−2.296***	−3.872***	−1.291***	−1.014***
	(3.46)	(5.10)	(3.08)	(4.21)	(2.99)

Notes. Each cell of the Table reports a coefficient estimate from a different regression. t-values reported in parentheses. All regressions include neighbourhood and division × year fixed effects. A Huber–White sandwich estimator was used to adjust standard errors for different variances within neighbourhoods. ***$p < 0.01$; **$p < 0.05$, $N = 12{,}864$ (1,072 reporting districts × 12 years).

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Table 4

Effect of BIDs on Crimes and Arrests Regression Estimates, Model 2

	Total	Robbery	Assault	Burglary	Auto theft
Crime					
BID	−40.644***	−7.577***	−4.218***	−4.484***	−5.198**
	(4.95)	(6.31)	(2.83)	(3.18)	(2.05)
Year −2	−14.077**	−3.311***	−2.404**	−2.241	−2.191
	(2.52)	(3.00)	(2.10)	(1.78)	(1.26)
Year −1	−9.607	−2.954**	1.619	0.381	−0.254
	(1.70)	(2.36)	(1.54)	(0.28)	(0.13)
Year 0	17.116***	3.472***	5.015***	2.445**	2.106
	(3.23)	(4.16)	(4.14)	(2.23)	(1.29)
Year +1	11.544**	2.446***	3.078***	1.023	2.384**
	(2.19)	(3.66)	(2.88)	(1.05)	(2.07)
Year +2	8.483	0.784	1.379	1.564	1.163
	(1.63)	(1.29)	(1.54)	(1.95)	(1.13)
Arrests					
BID	−16.158***	−3.940***	−6.690***	−1.918***	−1.547***
	(3.87)	(5.66)	(3.51)	(4.31)	(2.83)
Year −2	−3.222	−1.75***	−1.495**	0.086	−0.104
	(1.70)	(4.33)	(2.05)	(0.20)	(0.22)
Year −1	−1.621	−0.924	−1.188	−0.668	−0.260
	(0.66)	(1.66)	(1.26)	(1.64)	(0.54)
Year 0	14.199***	2.316***	5.900***	1.201***	1.241***
	(3.97)	(4.36)	(3.50)	(3.23)	(3.33)
Year +1	7.815***	1.684***	2.933***	0.738**	0.689**
	(2.79)	(3.42)	(2.83)	(2.49)	(2.47)
Year +2	8.591***	1.499***	3.397***	0.772***	0.579**
	(4.06)	(4.04)	(4.18)	(2.94)	(2.45)

Notes. Each column reports the results of two regressions: the first has crimes as the dependent variable, and the second has arrests as the dependent variable. t-values reported in parentheses. All regressions include neighbourhood and division × year fixed effects. A Huber–White sandwich estimator was used to adjust standard errors for different variances within neighbourhoods. ***$p < 0.01$; **$p < 0.05$, $N = 12{,}864$ (1,072 reporting districts × 12 years).

largest marginal shift in crime occurs for robberies (18%), followed by burglary and auto theft.

Additionally, BID introduction is not associated with increased arrests by the police. On the contrary, across all models BIDs are associated with significantly fewer police arrests over time. The introduction of BIDs is associated with an average BID neighbourhood reduction of 9.6 arrests, a 32% decline. Importantly, these models control for crime and arrest trends in adjacent reporting districts through the inclusion of division–year interaction terms.

The specification in Model 1 (reflected in Table 3) incorporates the implicit assumption that the effects of the BID on crime are the same during the start-up phase as during the later, mature phase. That assumption is tested in Table 4, which reports the results of inclusion of indicators for the five years around the time of the BID creation. The coefficients on the existence of a BID are larger in every case in this Table. The reason is clear: years 0–2 have positive coefficients, indicating that the initial effect of the BID on crime is less than later in its existence. Thus the effects of the 'mature' BID are considerably larger than reported in Table 3, which is in effect reporting the average effect of the initial and mature phases.

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The leading indicators of BID creation are also included in Model 2 as a test for spurious association between crime and BID creation. The potential concern that BIDs are created in response to a spike in crime rates is negated by the results reported here – if anything, crime rates are somewhat *lower* than typical in the two years prior to BID creation. In any event, the estimated effects of these leading indicators are small relative to the estimated effects of the BID.

Model 3 allows us to explore a different issue of timing, namely the possibility that the average BID effect differs from year to year. The results are displayed in Figure 2 (panel *a* and *b*). In general the results suggest that the effects of BID presence on

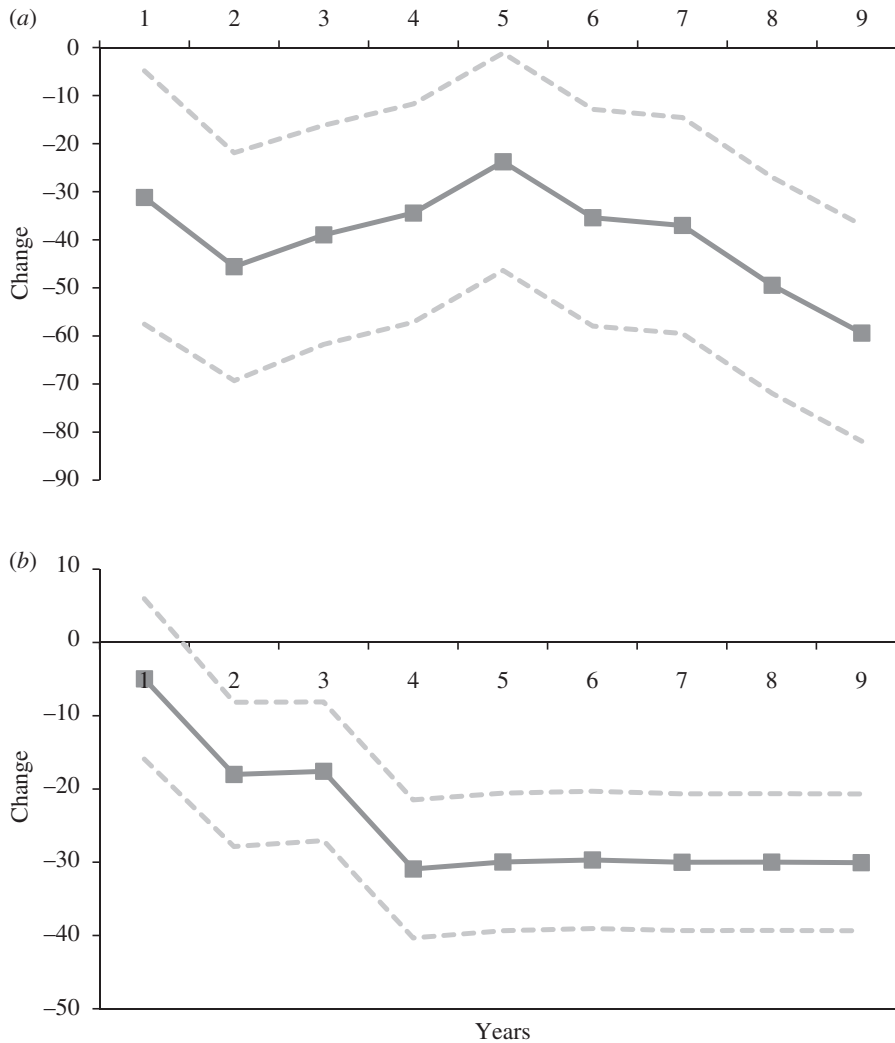

Fig. 2. *Difference-in-Differences Estimates of BID Effects on* (*a*) *Crime and* (*b*) *Arrests*
Source. Table 5.
Note. Years 1–9 correspond to 1997–2005.

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Table 5

Effect of BIDs on Crimes and Arrests by Year Regression Estimates, Model 3

Years with BID	Total	Robbery	Assault	Burglary	Auto theft
Crime					
1 (1997)	−31.212***	−6.084**	−2.479	−3.737	−3.497
	(3.49)	(2.65)	(1.05)	(1.55)	(1.23)
2 (1998)	−45.603***	−11.769***	−9.164***	−8.058***	−5.252
	(4.16)	(5.89)	(3.64)	(3.10)	(1.56)
3 (1999)	−38.989***	−12.124***	−8.912***	−6.431**	−4.600
	(3.34)	(5.76)	(3.43)	(2.61)	(1.35)
4 (2000)	−34.451***	−11.232***	−9.443***	−7.563***	−3.060
	(2.88)	(5.28)	(3.36)	(2.99)	(0.87)
5 (2001)	−23.779**	−9.098***	−8.493***	−5.152**	−2.801
	(2.02)	(4.38)	(3.01)	(1.99)	(0.81)
6 (2002)	−35.418***	−9.033***	−9.450***	−5.729**	−5.18
	(2.87)	(4.26)	(3.28)	(2.32)	(1.50)
7 (2003)	−37.032***	−9.224***	−9.092***	−7.193***	−4.158
	(3.01)	(4.46)	(3.07)	(2.88)	(1.18)
8 (2004)	−49.475***	−11.922***	−11.943***	−8.601***	−4.377
	(3.98)	(5.54)	(3.80)	(3.49)	(1.27)
9 (2005)	−59.42***	−12.975***	−11.714***	−8.785***	−5.490
	(4.58)	(6.01)	(3.42)	(3.55)	(1.55)
Arrests					
1 (1997)	−4.968	0.427	0.871	−0.511	−0.854
	(1.03)	(0.38)	(0.56)	(0.70)	(0.86)
2 (1998)	−18.010	−1.942	−4.942***	−1.823***	−2.382**
	(3.91)***	(1.90)	(3.43)	(2.89)	(2.50)
3 (1999)	−17.585***	−2.390**	−4.564***	−1.487	−2.217**
	(3.29)	(2.27)	(2.78)	(2.21)**	(2.26)
4 (2000)	−30.917***	−5.331***	−10.212***	−2.381***	−2.687**
	(5.01)	(4.31)	(5.29)	(3.41)	(2.52)
5 (2001)	−29.958***	−5.010***	−10.340***	−2.081***	−2.305**
	(4.93)	(4.20)	(5.45)	(3.09)	(2.20)
6 (2002)	−29.688***	−5.196***	−10.151***	−1.708**	−2.865***
	(4.82)	(4.25)	(5.35)	(2.48)	(2.69)
7 (2003)	−29.994***	−4.904***	−10.499***	−2.053***	−2.801***
	(4.97)	(4.09)	(5.60)	(3.07)	(2.61)
8 (2004)	−29.976***	−5.033***	−10.339***	−1.955***	−2.663**
	(4.90)	(4.26)	(5.51)	(2.91)	(2.47)
9 (2005)	−30.018***	−4.889***	−10.352***	−2.088***	−2.785**
	(4.95)	(4.07)	(5.54)	(3.01)	(2.65)

Notes. Each column reports results from two regressions, first with crime count as the dependent variable (top half) and then with arrest count as the dependent variable (bottom half). t-values reported in parentheses. All regressions include neighbourhood and year fixed effects. ***$p < 0.01$; **$p \leq 0.05$; $N = 12{,}864$ (1,072 reporting districts × 12 years).

crimes varied over the period, and that the effects on arrests were somewhat smaller in the 1990s than thereafter.

Table 5 presents the overall and crime specific estimates for these average BID effects. The disaggregated crime estimates show that the effect of a BID on crime is negative for all crime types, and that the magnitude of the effect tends to be larger in later years. Auto theft is in the same negative direction but the coefficients are not statistically significant at the $p < 0.05$ level. The substantial drop in the majority of these crime outcomes continues to be observed over the entire time series.

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Table 6

Effect of BID Private Security Spending on Crimes and Arrests Regression Estimates, Model 4

	Total	Robbery	Assault	Burglary	Auto theft
Crime					
Security$ ($10,000)	−3.371***	−0.590***	−0.431**	−0.533***	0.166
	(2.89)	(3.98)	(2.01)	(2.87)	(0.64)
ZeroSecurity$ (0–1 variable)	−5.66	−3.80***	−0.354	−1.320	0.223
	(0.50)	(2.78)	(0.16)	(0.63)	(0.07)
Arrests					
Security$ ($10,000)	−1.658***	−0.505***	−0.558***	−0.199***	−0.205***
	(3.28)	(5.06)	(3.02)	(3.48)	(2.65)
ZeroSecurity$ (0–1 variable)	−0.907	−1.515	−4.049	−0.746	−0.151
	(0.11)	(1.92)	(0.90)	(0.90)	(0.29)

Notes. Each column reports estimates from two regressions, first with a crime count as the dependent variable (top half) and then for an arrest count (bottom half). t-values reported in parentheses. Coefficients are multiplied by 10,000 for Security$. All regressions include neighbourhood and division × year fixed effects. Standard errors were adjusted for larger variances within higher crime neighbourhoods. ***$p < 0.01$; **$p \leq 0.05$; $N = 12{,}864$ (1,072 reporting districts × 12 years).

The results so far treat BIDs as binary, either present or not. In fact BIDs are highly heterogeneous with respect to resources devoted to crime prevention. Table 6 presents the overall and crime specific estimates for our measure of private security dosage – expenditure per reporting district. The results are consistent with those for BID presence but specifically indicate that BIDs with greater private security expenditures per neighbourhood have greater reductions in crime and arrests. The estimated coefficients on the dummy variable for the existence of a BID that does not spend on security, ZeroSecurity$, are negative but statistically insignificant (with one exception), suggesting that the mere creation of a BID does not reduce crime much unless the BID allocates some funds specifically to crime prevention. The dose–response relationship demonstrated by these regressions helps confirm the causal interpretation of our estimates.

This estimate of crime spending on total crime implies that an additional $10,000 per neighbourhood spent by BIDs on private security reduces the average number of crimes per neighbourhood by 3.37. Separate regressions by crime type indicate that an additional $10,000 per neighbourhood reduces robbery, assault and burglary counts by about 0.5 incidents each. The only exception to this pattern is autotheft, which has no discernible association with BID spending on private security. BID expenditures on private security also appear to be associated with a decreased use of arrest powers by the police. An additional $10,000 of spending per neighbourhood by BIDs is associated with 1.65 fewer arrests. Crime specific results indicate that most of the reduction in arrests is for robbery and assault.

An additional expenditure of $10,000 per neighbourhood (reporting district) would represent a 19.3% increase above the average amount spent ($51,906) by the 21 BIDs that provide private security to their neighbourhoods. The Hollywood Entertainment and Downtown Industrial BIDs have the highest dosage of private security expenditures per neighbourhood at $190,120 and $194,712 respectively. Excluding the Hollywood

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Entertainment and Downtown Industrial BID from the regression model has little effect on the private security expenditure parameter, suggesting that these findings are not driven by these outliers.

What effect do BIDs have on neighbouring, non-BID areas? It is possible that BID effects may be confined to their neighbourhoods and had no impact on other jurisdictions, in line with much of the literature reviewed by Guerette and Bowers (2009). Alternatively, BIDs may displace crime to their nearest neighbouring areas as robbers and burglars seek out targets that are less well protected. BIDs could also have the opposite effect, of reducing crime in neighbouring areas, if increased surveillance in the BID areas spills over.

To examine the potential displacement or spillover effects of BIDs on neighbouring areas we estimated the primary fixed-effects specifications from model 1 by substituting the crime and arrest counts of their 243 nearest neighbours as the outcomes. We removed the 179 BID neighbourhoods from the regressions so our estimates are only identifying the effects of BIDs on their nearest neighbours.

Table 7 presents the overall and crime specific estimates of BIDs on their nearest neighbours. The results suggest that rather than displacing crime to neighbouring areas, BIDs have no meaningful effect on crime in nearby areas. No coefficient estimate is significantly different from zero. We conclude that BIDs do not shift social costs to nearby locations, and actually reduce auto theft arrests by a modest amount.

The specifications of BIDs on nearby locations, however, leaves unaddressed whether the observed associations are significantly different from those observed in BID areas. To test whether the estimates for BID areas are significantly different from nearest neighbours we re-estimated model 1 but removed the 243 nearest neighbours from the regressions. We then compared coefficients across BID and

Table 7

Spillover Effect of BIDs on Crimes and Arrests in Neighbouring Areas Regression Estimates, Model 1

	Total	Robbery	Assault	Burglary	Auto theft
Crime					
BID neighbours	−4.38	0.25	0.42	0.99	−1.90
	(0.51)	(0.39)	(0.46)	(1.11)	(1.37)
BID areas	−25.11***	−4.27***	−2.22**	−2.46**	−3.69
	(3.93)	(4.81)	(2.02)	(2.36)	(1.89)
Comparison	−24.70***	−4.50***	−2.52***	−3.18***	−2.89***
Arrests					
BID neighbours	1.47	−0.11	0.40	−0.03	−0.55**
	(0.58)	(0.45)	(0.64)	(0.10)	(2.47)
BID areas	−8.15**	−2.07***	−3.66***	−1.22***	−1.24***
	(2.57)	(4.59)	(2.72)	(3.69)	(3.74)
Comparison	−8.51***	−1.87	−3.94***	−1.15	0.13

Notes. Each cell of this table reports results from a separate regression estimate. t-values reported in parentheses. All regressions include neighbourhood and division × year fixed effects. A Huber/White sandwich estimator was used to adjust standard errors for different variances within neighbourhoods. ***$p < 0.01$; **$p < 0.05$; BID areas $N = 9{,}948$ (1,072 reporting district × 12 years − 243 neighbouring reporting districts × 12 years); BID neighbours $N = 10{,}716$ (1,072 reporting district × 12 years − 179 neighbouring reporting districts × 12 years).

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neighbouring areas equations.[9] The results are that the effects observed in BID neighbourhoods are significantly different from those in neighbouring areas for all outcomes except auto theft arrests.

5. Effects of BIDs on Crime-Related Social Costs

Next, we consider direct crime and criminal justice cost savings resulting from BID investments in area-specific services. As we have seen, adoption of BIDs reduced the average number of crimes and arrests in affected neighbourhoods. How much are these reductions worth? The social costs of crime victimisation include direct costs related to medical and mental health services, productivity losses (wages, housework, etc.), and pain and suffering. Two methods have been used to monetise the consequences of crime. Most common has been to infer crime costs from jury awards for torts that have the elements of crimes like robbery or assault. This *ex post*, compensatory approach has most recently been pursued by Roman (2009). In principle the more valid approach is to estimate the willingness-to-pay for a reduced probability of victimisation, which provides an *ex ante* assessment that should include the costs of crime avoidance and concerns about family and friends as well as self. Ludwig and Cook (2001) utilised a contingent-valuation survey to estimate willingness to pay for a reduction in gun violence and Cohen *et al.* (2004) applied this method to estimating the value of reducing several other types of crime. Estimates utilising both approaches are reported in Table 8.[10] Note that the unit social cost of a robbery is quite similar in the two methods, but that the jury-award method produces a higher estimate for assault. Jury awards and WTP show that an additional spending of $10,000 per neighbourhood by BIDs produces a social-cost savings of $149,362 to $155,242 for robberies and $34,217 to $52,812 for assaults.[11] The conclusion is clear in either case – even if we just limit the assessment to robbery and assault, the social benefit of crime reduction is a large multiple (about 20) of private expenditure.

This conclusion is strengthened when the savings from reduced arrest rates are included. The average cost of an arrest and related prosecution in LA (in 2005 dollars) has been previously documented by investigators at the RAND Corporation (Turner *et al.*, 2007). An average arrest by the LAPD was estimated to cost $473, which includes the cost of officers at the crime scene and police station booking an offender (4 h total at $34.90 per hour), the cost of case review by a detective (1.5 h at $42.82 per hour), a citation package delivered to the LA district attorney (1 h at $34.90 per hour), and a booking fee of $25. The total cost of each court appearance related to an arrest was estimated to be approximately $2,474. Court costs included the costs associated with

[9] The BID and neighbours specifications are both nested in model 1. A simple difference test of the BID coefficients across equations is distributed asymptotically on a Z distribution according to the following form:

$$\beta\text{bid}_{it} - \beta\text{neighbour}_{it} / \sqrt{\text{SEbid}_{it}^2 + \text{SEneighbour}_{it}^2}.$$

[10] The average direct victim injury cost is much lower and estimated to be $30,690 per robbery and $23,212 per assault (Miller *et al.*, 1993). These cost estimates are conservative because they exclude a number of external social costs including how crime influences decisions about travel, housing, business locations, prices of insurance, the value that individuals place on avoiding victimisation, and other factors.

[11] A separate estimate using direct injury costs per crime from Miller *et al.* (1993) indicates that an additional $10,000 in BID spending on private security is associated with $18,199 social injury costs savings for robberies and $10,027 for assaults averted.

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Table 8

Cost Savings from BIDs for Crimes and Arrests per Neighbourhood

Incident costs	Per $10,000 private security	95% CI	Overall	95% CI
Robbery $263,122*	$155,242	$78,011–$232,473	$1,247,988	$815,235–$1,680,740
Assault $79,390*	$34,217	$511–$67,923	$189,583	$40,625–$338,542
Robbery $253,156†	$149,362	$75,056–$223,668	$1,200,719	$784,358–$1,617,080
Assault $122,249†	$52,689	$786–$104,592	$291,930	$62,557–$521,305
Arrest $2,947‡	$4,863	$1,963–$7,798	$28,366	$12,292–$44,440

Notes. Estimates of incident costs taken from the publications and converted into 2005 dollars. Per incident costs are multiplied by coefficient estimates from Table 7 (second and third columns) and Table 3 (fourth and fifth columns). *Estimate taken from Cohen *et al.* (2004). †Estimate taken from Roman (2009). ‡Estimate taken from Turner *et al.* (2007).

the district attorney's prosecution, the public defender representation and the costs of a case appearing in court. The average costs of an arrest and court prosecution is estimated at $2,947. These criminal justice cost estimates are conservative because they exclude the average cost of jail, future prison and potential lost wages due to incarceration.

Taking the estimated average reduction 1.65 arrests per additional $10,000 BID expenditures on private security per neighbourhood and multiplying that by the average costs of an arrest and court prosecution ($2,947) translates into an approximate savings of $4,863. This suggests that there is a substantial benefit to the public in reduced criminal justice expenditures for money spent by BIDs on private security, with no indication that these benefits are offset by arrests going up in neighbouring locations.

The bottom line is that the local security provided by BIDs in Los Angeles reduces crime and the number of people who are arrested and processed in the criminal justice system. These effects provide a social benefit that is a multiple of the private expenditure. While we do not have estimates of the effects of BID security on the profitability of the constituent businesses, the popularity of BIDs suggests that the participants are satisfied. In effect, BIDs may well increase the profitability of doing business in the central city.

6. Discussion

Analysis of criminal justice policy tends to focus on how best to use public resources to reduce crime by reducing the population of active criminals through deterrence, incapacitation and rehabilitation of criminals. That formulation of the policy problem ignores the role of private action. The volume and distribution of crime is not determined solely by the population of active criminals. Individuals choose whether to commit crime, and what crimes to commit, based in part on the characteristics of available opportunities. Those criminal opportunities are created primarily by private action. Expressed differently, private individuals and firms produce private security, providing the first line of defence in reducing the supply of tangible opportunities for crime, and to some extent making crime less attractive and profitable. If policy makers

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ignore the fundamental role of private action, they are in danger of misunderstanding observed trends and patterns in crime – and of failing to recognise effective tactics for reducing the costs of crime.

As reported here, BIDs that allocate funds to crime reduction are one form of effective private action. By hiring private security and working closely with the police, BIDs co-produce crime control – a local public good. We have demonstrated that BIDs reduce crime, and that the reduction in crime is coupled with reductions in arrest rates. BIDs in Los Angeles clearly pass a cost–benefit test. Indeed, the social cost savings from BID security expenditures is an order of magnitude greater than expenditures.

It appears very much in the interest of Los Angeles to continue supporting its BIDs. On the other hand, our analysis does not imply that other neighbourhoods in LA should organise – it is reasonable to suppose that those areas with the highest potential payoff have already organised. We have greater confidence that what has worked in Los Angeles will likely also work in certain high crime commercial districts of other cities around the world. What is required to solve the collective action problem in organising a BID is the legal and public administrative framework of the sort currently provided by California and LA in particular.

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