

Appendix: Additional Material Not Intended for Publication

1. Expenditures

So far the estimates have relied upon a dummy as the marker for BID adoption. If BIDs differ in strength or effectiveness, the resulting coefficient reports the average effect across all BIDs. Replacing this dummy with a measure more closely aligned with BID strength – total BID expenditure or BID security expenditure – allows for a per dollar estimate of the benefits of BID adoption, as shown in Unpublished Appendix Table 1. Specifically, the $BID_i^*after_{i,t}$ term in Equation (1) (or its equivalent) is replaced by BID expenditure $_{i,t}$. Theory suggests that the correct amount of expenditure to use in such a regression would be some amount between the higher total expenditure and the lower security expenditure. Security expenditures clearly go directly toward combating crime, but the total expenditure may also go toward solving free rider problems that would otherwise prove a hindrance to crime reduction, such as disputes among neighbors about the correct disposition of public space.

Each cell in the first two columns of Unpublished Appendix Table 1 comes from a regression of crime on BID total and security expenditure separately, using the control variables described above. The two right-hand columns translate these regression coefficients into a dollar per crime averted figure. Here a decline of 1 reported crime is associated with \$2,857 to \$3,226 of total BID expenditure and \$1,053 to \$1,235 of BID security expenditure. Averaging these two figures, a decline of 1 crime is associated with a relatively narrow range of \$2,000 to \$3,000 of BID expenditure.¹ Among the dollar figures from the coefficient on the security budget, not one

¹ Re-calculating these estimates by dividing total BID expenditure (or security expenditure) by the total crime averted by BIDs yields very similar results. This suggests that these results are not driven by the endogeneity of the choice of spending.

tops \$1,300 per reported crime averted.² In some sense these are underestimates of BID effectiveness, as BID security also addresses non-reported offenses, such as the presence of transients. My results suggest that 1 out of 4 averted crimes are violent ones. Compared to the conservative estimate of \$35,000 of social cost per violent crime, BIDs are cheap.

From the perspective of the property owner, it is certainly preferable to spend \$3,000 extra in taxes in front of one's own front door than to lobby for higher city-wide taxes for police. The low price of BID provision also indicates that this local provision is more efficient from a social welfare viewpoint. Local actors choose the level of provision that best suits them, and they provide it at a lower cost.

2. Enforcement

The results of this paper strongly suggest that BIDs are able to lower crime. But *how* do BIDs lower crime? Theory suggests that BIDs do not lower crime by crowding out municipal services. However, it is certainly possible that BIDs could lower crime by doing the reverse – by acting as a magnet for municipal services. In the unlikely event that BIDs crowd out police services, the city as a whole benefits; if BIDs capture city services, they harm overall municipal security and have detrimental redistributive repercussions. Here I assume that the city is constrained, legally and politically, to offer a similar quantity of police service across the entire city.³

² Ideally, I would compare this with the LAPD's cost per crime averted. Unfortunately, the closest reliable figures are national averages that I cite in the introduction (Levitt 2004). For a sense of the magnitude of local police expenditure, the LAPD spends approximately \$5,000 per committed crime.

³ This assumption is empirically grounded. Of all the measures of police distribution across the 18 LAPD areas – police expenditure per capita, per square mile, per street mile and per crime – expenditure per crime has the smallest coefficient of variation.

One way in which BIDs could decrease crime is by doing tasks the LAPD might not be willing or financially able to do, such as keeping a closer eye on the streets, moving homeless people along, frequently scolding drunks, and aggressively pursuing unlicensed street vendors. All of these activities increase the cost of committing a crime. Certainly some BIDs do exactly these things through their full-time staffs of “neighborhood ambassadors.” If BIDs decrease crime in this fashion, it suggests that private services supplement, but do not crowd out, public ones.

It is also very plausible that neighborhood with a BID could be better mobilized to attract more police enforcement, possibly because the neighborhood may now have full-time staff members to call when the police could be helpful. For example, the Hollywood Entertainment BID purchased wireless cameras to monitor Hollywood Boulevard, which will be operated by the LAPD (Ofc. Moore 2004).

To measure police enforcement at the neighborhood level, I investigate changing arrest patterns by the type of arrest. In general, arrests of all kinds decline after BID adoption, along with the number of crimes. However, if BIDs were to attract greater police enforcement, we would expect larger increases in arrests that are more discretionary relative to those that are less discretionary. The most discretionary arrest category in my sample is drunkenness, for which there is no corresponding crime. In comparison, I examine arrests for burglary and vehicle theft. Though these types of arrests may allow for some discretion, it is surely less than is allowed for in arrests for drunkenness. Therefore, if BIDs are able to draw significant police attention, they should have smaller decline in arrests for drunkenness relative to arrests for burglary and vehicle theft.

The right-hand panel of Unpublished Appendix Table 2 presents the results of regression BID adoption on three kinds of arrest outcomes – burglary, vehicle theft, and drunkenness. Over

this period, the reported crimes of burglary and vehicle theft drop dramatically; drunkenness is not reported as a crime in my data, so there is no direct comparison.

The results in Unpublished Appendix Table 2 do not support the hypothesis that BIDs draw significant police attention. The top panel of Unpublished Appendix Table 2 shows that Almost BIDs are most like BIDs in the distribution of arrest types, as they are the only comparison group to have more than 10 arrests for drunkenness annually. BID reporting districts have, on average, 15 such arrests.

The bottom panel of this table displays the coefficient on β_1 across the different estimation methods. Across methods, BIDs are consistently associated with significant declines in arrests for burglary. This should not be taken as a slackening of enforcement per se, as the number of burglary crimes also fell during this period in BIDs, as shown in earlier tables. Compare these results for burglary arrests with the rightmost columns, estimating BIDs' association with arrests for vehicle theft and drunkenness. Due to the LAPD's data categorization, arrests for vehicle theft cannot be compared directly with auto burglary and theft crimes. However, it is interesting to note that during a period in which auto burglary and theft falls significantly in BIDs, as shown previously, arrests for vehicle theft in BIDs remain virtually unchanged. Arrests for drunkenness, possibly the most discretionary of the 27 arrest categories, are little changed by BID adoption. Particularly when compared their closest counterparts in the Almost BIDs, BIDs do not show an increase in more discretionary arrests relative to less discretionary ones. I interpret these last two columns as suggestive evidence that any BID impact on enforcement is modest at best.

As a practical matter, BIDs should have a much easier time re-deploying existing resources than in getting the city to commit new ones. The detrimental effects of any small redistribution of

municipal services are outweighed by the benefits, to BIDs and to the city, of the crime decline BIDs cause.

3. Further Specification Checks

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

For a propensity score matching method to provide a valid counterfactual, there must be non-treated observations with propensity scores similar to the treated observations. Furthermore, within propensity score strata, the observable indicators of treatment should not be significantly different. Unpublished Appendix Table 3 presents means for each quintile of the propensity score distribution separately for BIDs and non-BIDs and shows that there is overlap of the propensity score distribution in BID and non-BID districts. Furthermore, within each quintile, mean observable predictors of BID adoption are usually insignificantly different between BIDs and non-BIDs. Because there are so many covariates in the propensity score, the table presents means for a representative sample. Out of the 100 possible differences (20 variables * 5 quintiles), 85 are insignificantly different.

The main paper estimates the impact of BID adoption as the average crime reduction across all years after BID adoption. Here I allow the impact of BID adoption to change over time with four dummies for each pair of years (1 and 2, 3 and 4, etc.) after BID adoption. I find that

regardless of estimation method, the BID effect increases with time. Results presented in Unpublished Appendix Table 4 show that the later years have larger estimates, though they are less precisely estimated, likely due to the small sample size at the tail.

In addition to this correction for serial correlation, I present two additional results consistent with the theory; the first of these deals with the heterogeneous effects of BIDs. Though I have been referring to BID members as property owners (and will continue to do so for convenience), the city of Los Angeles actually has both property- and merchant-based BIDs. Property based BIDs assess property owners, run for a finite term, usually 3 to 5 years, and require a new vote to re-establish at the end of this term. Merchant-based BIDs assess business owners, and, after an initial vote, require a majority assessment-weighted protest to become inactive. In practice, neither type of BID has dis-established during my sample period, so both types of BIDs are not viewed as short-term investments. Theory suggests that property owners should be willing to make larger investments in neighborhoods as they are the residual claimant to any successful investment. Though merchants also have an interest in improving their neighborhood, they are priced out if it is improved too much. The theoretical prediction of property BIDs' greater willingness to invest is borne out by their disproportionate share – 90 percent – of all BID investment, though property BIDs only account for 20 of the 30 BIDs in Los Angeles.

If property BIDs are willing to make more significant neighborhood investments, they should be more successful than merchant BIDs in lowering crime. This prediction is borne out in the left panel of Unpublished Appendix Table 5. The first column of Unpublished Appendix Table 5 repeats the coefficients from the estimation of BIDs on crime across specifications. The second two columns report the results from replacing the single BID dummy, $BID_i^*after_t$, with two dummies – one for merchant BIDs and one for property BIDs. The results are striking in the

consistency with which property BIDs account for a much larger share of the total decline in crime. In the fixed effects approach, property BIDs are associated with a 16 percent decline in crime, while merchant BIDs are associated with an insignificant drop of 4 percent. This pattern holds across all estimation methods, and the minimum crime decline associated with property BIDs is 54 crimes, or 11 percent. These results are a good fit with the theoretical prediction that property owners should make larger investments and reap larger returns. To economists, the presence of merchant BIDs is something of a mystery, as merchant capture a much smaller portion of improvements in the neighborhood than property owners. Empirically, the behavior of merchant BIDs appears to take this into account – they make smaller investments and, after the end of my sample, are much more likely to dissolve.

Theory leaves an open door for BIDs' impact on crimes they do not seek to directly address.⁴ To test whether BIDs are associated with changes in crimes that they do not target, the right-hand panel of Unpublished Appendix Table 5 presents results from two specifications – one where the outcome is all crimes a BID might be likely to affect, and one where the outcome is crimes that BIDs should be unlikely to directly affect. BIDs are likely to affect crimes such as robbery, theft, and assault.⁵ Crimes that BIDs are unlikely to affect are forgery (by far the largest contributor to the total), fraud, embezzlement, family crimes (domestic abuse) and non-prostitution sex crimes (i.e., child abuse). Across estimation strategies, BIDs are less likely to be associated with declines in this mix of unlikely crimes than they are with robbery. Of the four strategies, only 1 finds an association between BIDs and the unlikely crimes.

⁴ Broken windows advocates, led by Wilson and Kelling (1982), would argue that improvements in quality-of-life crimes would lead to improvements in all types of crimes.

⁵ The full list is robbery, assault, burglary, auto burglary and theft, personal theft, other theft, auto theft, other assaults, vandalism, pimping, disorderly conduct and vagrancy.

Unpublished Appendix Table 1 BID Expenditures and Crime Decline

	total crime change		dollars per crime	
	as a function of BID budget in the \$1000s		total budget	security budget
	total budget	security budget	total budget	security budget
Fixed Effects	-0.35 0.06**	-0.95 0.15**	2,857	1,053
Almost BIDs	-0.32 0.06**	-0.86 0.17**	3,125	1,163
Matching	-0.26 0.07**	-0.81 0.13**	3,846	1,235
Neighbors	-0.31 0.06**	-0.81 0.16**	3,226	1,235

** Significant at the 0.01% level. * Significant at the 0.05% level.

Notes: Using the fixed effects approach, \$1,000 of total BID spending is associated with a reduction of 0.35 crimes, which translates to a cost of \$2,857 per crime. All regressions contain area*year fixed effects and reporting district fixed effects. Sample sizes are as reported in the main paper, Table 3. Standard errors are below coefficient estimates, and are clustered at the reporting district level.

Source: Crime data from LAPD; BID information is author's tabulations from city documents.

Unpublished Appendix Table 2 Measuring Police Enforcement

Overall Means				
	no. of reporting districts	arrests		
		burglary	vehicle theft	drunkenness
BIDs	124	8.1	6.3	14.7
All non-BIDs	885	5.2	5.4	6.9
Almost BIDs	132	6.5	8.3	11.5
Neighbors	291	5.2	4.8	7
Regression Results				
		arrests		
		burglary	vehicle theft	drunkenness
Fixed Effects		-2.79	-0.90	3.54
		0.68**	0.41*	3.07
Almost BIDs		-1.91	0.31	-1.44
		1.00	0.69	4.14
Matching		-1.66	-0.28	0.69
		0.47**	0.33	2.24
Neighbors		-2.56	-0.91	3.13
		0.67**	0.45*	3.02

** Significant at the 0.01% level. * Significant at the 0.05% level.

Notes: Regressing BID adoption on the number of arrests by type, this table finds that, using the fixed effects approach, BIDs are associated with 2.8 fewer arrests for burglary and unchanged levels of arrests for vehicle theft and drunkenness. All regressions contain area*year fixed effects and reporting district fixed effects. Sample sizes are as reported in previous tables. Standard errors are below coefficient estimates, and are clustered at the reporting district level.

Source: Arrests data from LAPD; BID information is author's tabulations from city documents.

Unpublished Appendix Table 3 Propensity Score Evaluation

BIDs											
quintile	propensity score	serious crime 1990	less serious crime 1990	serious crime 1994	less serious crime 1994	95th percentile bldg age	share black	share hispanic	share hs education or less	median rent	obs.
1	0.02	163.25	238.50	140.00	166.88	1927	0.35	0.20	0.49	724.38	8
2	0.04	166.20	263.40	165.20	226.80	1927	0.10	0.28	0.45	781.80	5
3	0.08	153.13	213.13	157.67	182.33	1921	0.10	0.37	0.54	672.33	15
4	0.15	213.23	263.94	189.58	250.29	1919	0.04	0.47	0.58	590.63	24
5	0.43	358.27	341.41	282.53	298.94	1925	0.13	0.34	0.57	486.58	72

Non-BIDs											
quintile	propensity score	serious crime 1990	less serious crime 1990	serious crime 1994	less serious crime 1994	95th percentile bldg age	share black	share hispanic	share hs education or less	median rent	obs.
1	0.01	100.13	145.09	88.32	113.93	1938	0.18	0.29	0.48	802.50	189
2	0.04	143.35	188.01	129.99	170.77	1928	0.17	0.35	0.52	694.82	193
3	0.08	174.46	205.38	146.25	173.11	1922	0.13	0.38	0.53	621.86	182
4	0.14	223.12	281.12	190.01	234.70	1925	0.09	0.37	0.51	617.74	174
5	0.29	279.64	313.25	239.48	278.62	1921	0.12	0.37	0.58	533.32	146

t Test for Difference in BID vs. non-BID mean											
quintile	propensity score	serious crime 1990	less serious crime 1990	serious crime 1994	less serious crime 1994	95th percentile bldg age	share black	share hispanic	share hs education or less	median rent	obs.
1	2.92	2.84	2.14	3.18	2.36	-1.37	1.40	-2.32	0.14	-0.91	0.04
2	0.61	1.21	1.35	0.96	1.26	-0.09	-0.85	-0.52	-0.64	0.75	0.03
3	-0.45	-0.94	0.23	0.44	0.36	-0.16	-0.61	-0.08	0.22	0.91	0.08
4	1.38	-0.50	-0.61	-0.03	0.62	-1.43	-2.89	1.62	1.51	-1.03	0.14
5	5.25	3.07	0.92	1.84	0.73	1.23	0.30	-0.87	-0.16	-1.72	0.49

BID obs. share of total obs. 0.14

Notes: This table divides BID and non-BID observations into the same five quintiles based on the calculated propensity score. The final panel reports t-test values for the difference in means of the first two panels.

Source: Crime data from LAPD; demographic information from the 1990 census; BID information is author's tabulations from city documents.

Unpublished Appendix Table 4 BID Length

Years with BID	Fixed Effects	Almost BIDs	Matching	Neighbors
1-2	-50.64	-42.68	-31.30	-21.56
	8.60**	13.81**	7.15**	8.97*
3-4	-54.46	-60.59	-33.19	-36.67
	13.04**	21.36**	10.55**	15.91*
5-6	-81.49	-84.95	-54.95	-67.38
	18.66**	28.63**	17.29**	24.30**
7-8	-111.66	-106.38	-82.12	-101.35
	35.20**	42.89*	33.83*	40.69*

Notes: The dependent variable is total crime.

Source: Crime data from LAPD; demographic information from the 1990 census; BID information is author's tabulations from city documents.

Unpublished Appendix Table 5 Specification Checks

Overall Means					
	total crime	merchant BIDs	property BIDs	Should BIDs affect these crimes?	
				yes	no
BIDs	460.8	443.2	481.1	397	25.2
All non-BIDs	316.6			270.7	15.9
Almost BIDs	388.4			335.5	17.5
Neighbors	313.8			269.6	16.3
Regression Results					
	outcome is total crime			Should BIDs affect these crimes?	
	all BIDs	merchant BIDs	property BIDs	yes	no
Fixed Effects	-57.15	-17.39	-78.49	-28.44	-5.70
	11.18**	12.48	15.51**	6.56**	2.3044*
Almost BIDs	-54.76	-11.11	-67.16	-21.40	-7.56
	17.98**	20.75	19.22**	10.93	4.10
Matching	-35.75	-11.66	-54.31	-16.82	-4.08
	9.13**	10.28	13.42**	5.95**	2.48
Neighbors	-45.26	-12.95	-58.44	-19.96	-4.97
	12.49**	13.59	20.03**	8.47*	3.00

** Significant at the 0.01% level. * Significant at the 0.05% level.

Notes: The first panel of this table reports coefficients from estimations of BID adoption on total crime, separated into the effects of merchant-based and property-based BIDs. The second panel separates total crime into two categories and separately regresses BID adoption on each. All regressions contain area*year fixed effects and reporting district fixed effects. Sample sizes are as reported in Table 3 in the main paper. Standard errors are below coefficient estimates, and are clustered at the reporting district level.

Source: Crime data from LAPD; BID information is author's tabulations from city documents; property information is from the Los Angeles County Assessor via Dataquick software.