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HOW DO VOLUNTARY BUSINESS IMPROVEMENT DISTRICTS CHANGE CRIME AND PERCIEVED SAFETY IN VULNERABLE AREAS? EVIDENS FROM SWEDEN

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How do voluntary Business Improvement Districts affect crime and perceived safety in vulnerable areas? Evidence from Sweden

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Abstract

Business Improvement Districts (BIDs) are an increasingly prominent form of place-based governance aimed at improving safety, attractiveness, and economic vitality in urban areas. While a growing body of research examines the effects of mandatory BIDs on crime, less is known about the effects of BIDs in countries that lack formal BID legislation and that are thus introduced on a voluntary basis. More specifically, little is known about how voluntary BID-type interventions influence crime and perceived safety in socioeconomically vulnerable neighborhoods, and whether crime reductions are necessary for improvements in perceived safety. We fill these research gaps by investigating the effects of voluntary BIDs on both crime rates and perceived safety in vulnerable areas in Sweden. Using synthetic difference-in-difference analysis, we estimate average and dynamic effects of BID implementation on recorded crime, while cohort-stacked difference-in-differences after coarsened exact matching is used as a robustness analysis. We find no consistent evidence that voluntary BIDs reduce recorded crime in vulnerable neighborhoods, either across the crime categories considered jointly or for specific categories. In line with these results, a survey across 5 BID neighborhoods and 11 control neighborhoods shows that businesses in affected vulnerable areas report changes in self-reported exposure to crime that are generally small and statistically insignificant. However, respondents in BID neighborhoods do report significantly more improvements in perceived safety and local business conditions. The findings suggest that BID-type interventions primarily influence how people perceive and experience local environments. The results highlight the importance of distinguishing between objective and subjective dimensions of safety when evaluating place-based interventions.

Keywords: Business Improvement Districts; crime; vulnerable neighbourhoods; Synthetic Difference-in-Differences; place-based policy.

JEL codes: R58, K42, C21, H76.

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1 Introduction

Crime often concentrates in public spaces such as city centers, retail streets, and neighborhood hubs. Yet these environments are also essential to everyday urban life, facilitating commerce, routine activities, and social interaction. In socioeconomically vulnerable neighborhoods, residents and firms are particularly exposed to crime in or around shops, cafés, restaurants, and other service points. Against this background, public and private actors have developed various forms of place-based collaboration as governance mechanisms aimed at enhancing safety, attractiveness, and the overall functioning of urban spaces. One widely adopted model is the Business Improvement District (BID). BIDs typically involve formalized cooperation among property owners, local businesses, and public authorities within a geographically defined area, through which actors jointly finance and implement measures to improve safety, cleanliness, maintenance, and place management (Hoyt & Gopal-Agge, 2007).

In many countries, actors establish BIDs through specific legislative frameworks, financing them through mandatory levies imposed on property owners and/or businesses within the BID district, following a vote. Such mandatory schemes may reduce free-rider problems and provide greater long-term stability and resources; however, they have also been criticized for lacking local anchoring, legitimacy, and stakeholder engagement (Ostrom et al., 1994; Ward, 2006; Daunfeldt et al., 2024).

Sweden introduced legislation on fees for BID-collaborations in August 2026 (The Swedish National Council for Crime Prevention, 2026). Thus, until very recently, there was no legislation regulating or mandating BID-like initiatives. Consequently, Swedish BID initiatives evolved primarily on a voluntary basis through locally negotiated partnerships among municipalities, property owners, businesses, and other stakeholders. While such voluntary arrangements may foster stronger local engagement and anchoring, they may also suffer from weak organizational capacity, limited strategic coordination, and insufficiently comprehensive measures to generate long-term change. Whether these comparatively weak and resource-constrained BID arrangements generate meaningful improvements in neighborhoods facing substantial social and safety-related challenges remains an open empirical question.

With few exceptions, previous research on the crime-reducing effects of these programs has been mainly focused on obligatory BIDs and has generally reported positive findings (e.g., Moir et al., 2024). However, much of the previous literature focuses on single case studies, often in large metropolitan areas such as New York, which may limit their external validity (e.g., MacDonald et al., 2010; Mello, 2019; Clutter et al., 2019), while other studies are primarily descriptive or rely heavily on anecdotal evidence regarding effects on crime levels (e.g., Mitchell, 2001; Hogg et al., 2003). Moreover, the evidence is not entirely consistent, as intensified commercial activity and pedestrian flows may increase potential targets and, e.g., increase the risk of robbery (Clutter et al., 2019). Swedish studies on voluntary BIDs are scant,

but indicates that such initiatives are associated with lower crime (Kronkvist & Ivert, 2020; Daunfeldt et al., 2024).

Furthermore, prior studies do not offer a complete picture of how those who live and work within the BID neighborhoods perceive interventions. Among the relatively few studies examining BID effects on perceptions of crime, Hinkle (2014) distinguishes between perceived risk, perceived safety, and emotional fear of crime, demonstrating that they constitute distinct constructs and that commonly used proxies may misrepresent the nature and extent of fear of crime. Recent Swedish research highlights that voluntary BIDs may primarily operate through improved coordination, environmental maintenance, and investment, thereby influencing perceived safety and local activity patterns more directly than underlying crime rates (Di Rocco et al., 2025).

An additional challenge concerns *how* BIDs should be evaluated. Crucially, objective crime outcomes and subjective perceptions of safety do not necessarily move together: Crime rates may decline without residents or businesses feeling safer, and perceived safety could also increase even in the absence of a measurable reduction in crime. Understanding whether BID initiatives influence either dimension and whether perceived safety changes correspond to changes in actual crime levels is therefore important for both theory and policy.

The existing literature leaves two unanswered questions which this study aims to answer: *i)* whether voluntary BID initiatives generate measurable reductions in recorded crime and *ii)* whether they influence perceived safety in vulnerable neighborhoods. In light of the new legislation on area-based collaboration, it is particularly important to understand what the voluntary BID programs have achieved to date and what lessons their experiences can offer for their continued development in Sweden. To address these research gaps, we first examine the actual effectiveness of BID programs in reducing crime in the urban areas defined by the Swedish Police as vulnerable, where crime problems and challenges related to social control are often particularly pronounced.

We employ a mixed-method approach in two steps. In the first step, we estimate the effect of voluntary BID initiatives on recorded crime using synthetic difference-in-differences, SDID. The design exploits variation in the timing and location of BID implementation across vulnerable neighborhoods, comparing treated areas with vulnerable areas that remain untreated throughout the study period (control neighborhoods). We examine both average and dynamic effects during the implementation year and the following three years. As a robustness analysis, we estimate cohort-stacked difference-in-differences models after coarsened exact matching, CEM. In the second step, we conduct a survey with business owners and employees operating in vulnerable areas in 5 BID neighborhoods and 11 control neighborhoods, examining how these groups perceive changes in safety, business conditions, and exposure to crime following the implementation of voluntary BIDs.

Overall, we find little evidence of a uniform effect on recorded crime. In the main SDID analysis, none of the average effects on total crime or the four individual crime categories is

statistically significant. The point estimates nevertheless differ across crime categories and locations, indicating substantial heterogeneity. The survey results show more positive developments in perceived safety and perceptions of the area as a place to do business, as well as a larger decline in firms' exposure to burglary in BID areas compared with control areas. Taken together, the results suggest that voluntary BIDs may affect perceived safety and local conditions even when recorded crime rates do not change.

The BID programs are generally well organized, but their activities focus primarily on measures such as cleaning and maintenance, safety walks, improved lighting, graffiti removal, safety stewards, place activation, and summer jobs for young people (The Swedish Property Federation, 2024). Such measures may strengthen informal guardianship, increase the probability of detection, and improve perceived safety, and could potentially affect public disorder and property crime. However, they are likely to have a more limited effect on criminal networks, drug markets, and serious violence. They may, nevertheless, increase the feeling of safety among residents, managers, and other groups in the neighborhood. At the same time, the programs' resources and mandates may be insufficient to address the extensive and complex crime problems found in vulnerable areas. Other kinds of interventions are also implemented in the control areas, meaning that our analysis captures the additional effect of BIDs over and above other ongoing initiatives. Taken together, the findings suggest that the effects of voluntary BIDs in vulnerable areas need to be understood in relation to the content and scale of the interventions, the local context, and the time required for different effects to emerge.

The remainder of the paper is organized as follows. Section 2 discusses the theoretical arguments concerning the expected impact of BID programs on crime levels and perceived safety, and the previous literature examining the effects of mandatory and voluntary BIDs. Section 3 presents the method and results for the crime analysis, while Section 4 presents the method and results for the perceived safety analysis. Finally, in Section 5, we summarize and discuss the findings.

2 Background

2.1 Theory

BIDs can be understood as part of the broader umbrella of place-collaboration programs, i.e., coordinated efforts by multiple actors to improve local neighbourhood conditions through investments in safety, environmental maintenance, business conditions, and community engagement (Bornhäll et al. 2026). A rationale underlying such arrangements is complementarity; by pooling resources and coordinating activities, actors – notably property owners, firms, and public officials – may achieve outcomes that would be difficult to realize individually.

To understand how BIDs affect crime and perceived safety, we take as a starting point the fact that BID initiatives may alter the incentives potential offenders face. According to Becker's (1968) *economics of crime* model, an individual will commit a crime if the expected benefits of crime exceed the expected costs, expressed as:

$$E(U_{crime}) = B - p \cdot S - O,$$

where B denotes the benefits, p the probability of detection or punishment, S the severity of sanctions, and O the opportunity cost of committing a crime. BID initiatives are unlikely to directly affect sanction severity *(S)*, meaning the potential local effects on crime and perceived safety operate primarily through changes in B, p and O. Three different mechanisms are of particular interest when analyzing these variables: the physical environment, economic activity and collective efficacy. BID's overall effect on crime and perceived safety depends on the relative strength of these competing mechanisms.

First, BID initiatives often improve the physical environment through, e.g., street cleaning, graffiti removal, improved lighting, façade renovation and maintenance. Visible signs of disorder signal weak social control and may encourage offending by making neighborhoods appear neglected and unmonitored (Wilson & Kelling, 1982). Thus, by improving the physical environment, BID initiatives could increase informal surveillance and raise the probability of detection *(p)*. BID programs may also introduce more direct security measures, including surveillance cameras, safety patrols and security guards. Such interventions could both increase the perceived risk of apprehension among potential offenders and the perceived safety among residents and visitors.

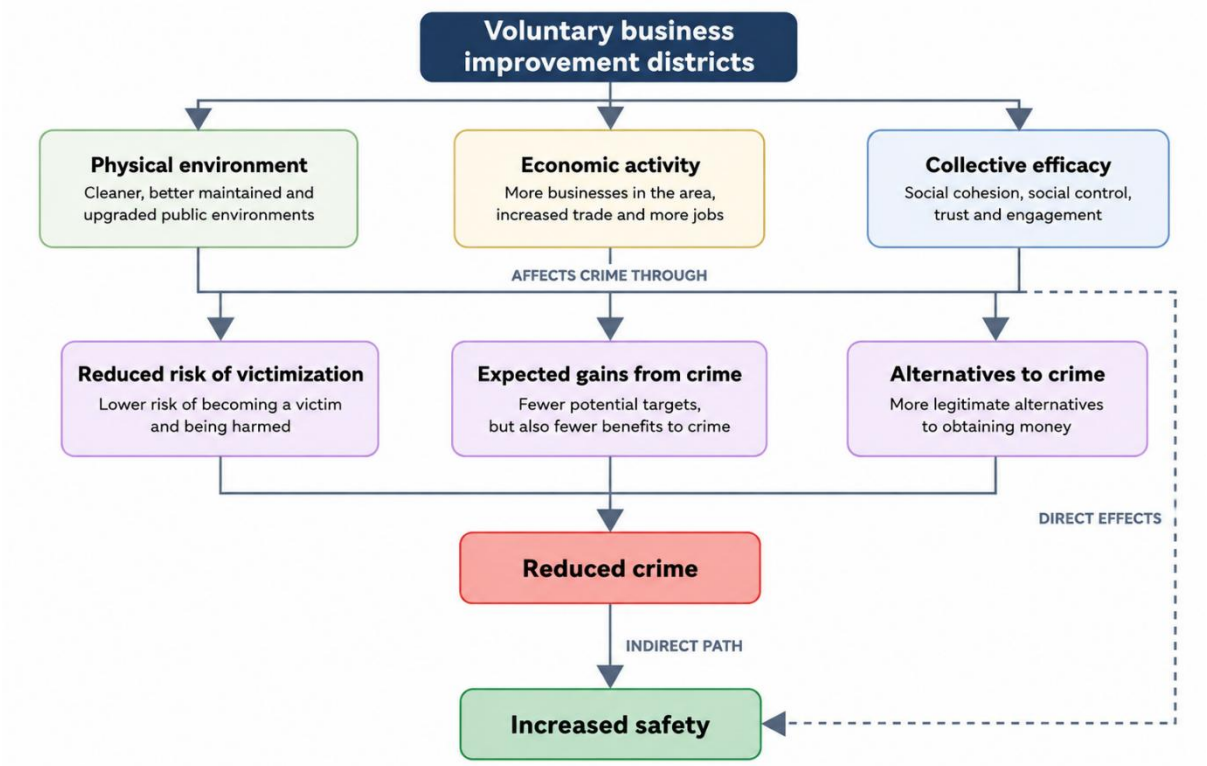
Second, BID initiatives often seek to improve business conditions and economic activity. By attracting businesses, customers and visitors, they may expand legitimate economic opportunities for residents, increasing the opportunity cost *(O)* of offending. Increased pedestrian activity may also generate more "eyes on the street" (Jacobs, 1961), strengthen guardianship and increase the probability of detection *(p)*. More active and populated public spaces could also increase perceived safety by making the area appear more supervised and socially integrated. Conversely, if the economic activity increases, crime may follow as the number of suitable targets encountered by motivated offenders increases (Cohen & Felson, 1979). Thus, the benefit of committing a crime *(B)* could also increase.

Third, BID initiatives may strengthen local collective efficacy among public, private, and civil actors. Residents who trust one another will be more willing to monitor their surroundings and intervene in situations that may lead to crime (Sampson et al., 1997). Stronger collective efficacy and informal social control may therefore increase the likelihood that offenses are noticed and sanctioned, increasing the probability of detection *(p)*. At the same time, stronger collective efficacy may influence perceived safety directly: when businesses, property owners, and public authorities demonstrate a shared commitment to maintaining order, individuals may feel safer and more confident that problems will be addressed when they arise.

Figure 1 summarizes the theoretical framework. BID initiatives may affect both crime and perceived safety through their effects on the physical environment, economic activity, and collective efficacy. The mechanisms affect the probability that a criminal will be detected and punished, the benefit of committing a crime, and the opportunity cost of legal activity. The

ensuing effect on crime may also directly impact perceived safety. Thus, there is a direct effect from physical environment, economic activity and collective efficacy on perceived safety, but also an indirect effect through crime levels. However, crime and perceived safety do not necessarily move together; BID initiatives may increase perceived safety even without measurable changes in crime levels (Khruakham & Lee, 2014).

Figure 1. Mechanisms through which voluntary BID initiatives may affect crime and perceived safety.



2.2 Empirical evidence and knowledge gaps

A growing research body investigates the impact of BIDs on crime, with a synthesis of the literature revealing that mandatory BIDs have a fairly consistent negative effect on crime (Moir et al., 2024). An exception to the pattern is Clutter et al. (2019), who, examining 37 BIDs in Cincinnati, find that the risk of robbery is 59% higher in BID areas and declines with distance from their boundaries. Most other studies find evidence going in the opposite direction, e.g., substantial reductions in robberies and violent crime (MacDonald et al., 2010; Mello, 2019; Piza et al., 2020).

Evidence concerning the crime effects of voluntary BIDs is not as substantial but points in a similar direction. Kronkvist and Ivert (2020) evaluate a voluntary, BID-inspired project in Malmö, Sweden, established by about twenty local property owners in 2014, comparing crime in the BID area with crime in nearby areas using a DiD analysis, finding a crime-preventive effect: compared with the control group, the BID area experienced a 23% reduction in total crime and a 29% reduction in vandalism, although no effect was found for violent crime. Furthermore, Daunfeldt et al. (2024) examine the implementation of BIDs in five smaller

Swedish municipalities in 2015. The results indicate that BID areas exhibit lower crime levels than both surrounding areas and the control municipalities included in the analysis.

More recently, attention has shifted toward perceived safety and disorder as key outcomes. Di Rocco et al. (2025) examine changes in perceived disorder and fear of crime following the implementation of a voluntary BID in a disadvantaged neighborhood using a quasi-experimental design with a comparison area. They find substantial reductions in both perceived disorder and fear of crime. However, similar – albeit smaller – improvements are observed in the comparison area, making it difficult to isolate the BID's causal impact. Valli and Hammami (2021) provide anecdotal evidence from a BID initiative in Gothenburg, suggesting the program contributed to increased perceived safety among residents and rising property values.

Taken together, the existing literature suggests that voluntary BIDs could influence both crime and perceived safety. However, important knowledge gaps remain. Much of the literature focuses on mandatory BID models implemented in large urban areas, particularly in North America and the United Kingdom (Ellen et al., 2007; MacDonald et al., 2010; Moir et al., 2024), limiting external validity. Previous research also suggests that the effects may depend on the characteristics of the areas in which BIDs are implemented and the resources available (Gross, 2005; Ellen et al., 2007). Smaller BIDs in economically disadvantaged often lack resources and focus primarily on maintenance and supporting existing businesses, whereas larger BIDs in economically stronger areas have greater resources and a broader mandate including investment and commercial development (Gross, 2005). Evidence on voluntary BID primarily rely on case studies, often early pilot initiatives in smaller urban environments, and is typically examined with limited data and relatively short follow-up periods. Moreover, previous studies typically focus on either crime or perceived safety, while few examine both outcomes within a common analytical framework. Our study addresses these gaps by examining the effects of voluntary BID initiatives on both crime and perceived safety in vulnerable areas in Sweden.

3 Crime: Data and analysis

3.1 Data and methodology

We combine annual data on reported offenses from the Swedish Police Authority, population data from Statistics Sweden (SCB), and information on the geographical coverage and implementation year of local BID initiatives. We identify voluntary BID initiatives through publicly available reports, articles, and other documentation, as well as contacts with experts on place-based collaboration in Sweden. We classify an initiative as BID-like when multiple local actors coordinate place-based activities aimed at improving the specific geographical area.[2]

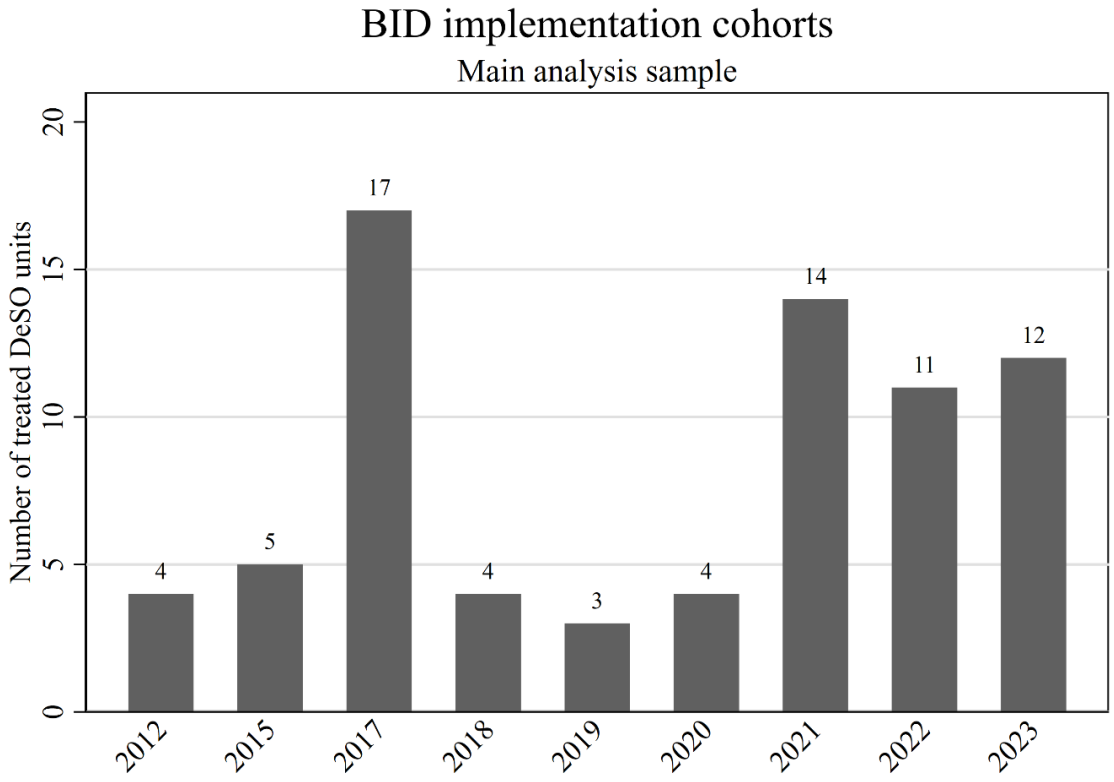
The study population consists of areas included in the Swedish Police Authority's 2023 classification of vulnerable areas. The analysis covers the period 2009–2024 and uses as the geographical unit of observation Demographic Statistical Areas (DeSO), a nationwide statistic developed by SCB. Sweden covers approximately 6,000 DeSO areas,[3] allowing us to examine crime at a substantially finer geographical level than the broader vulnerable areas in which the BID initiatives operate.

Because the analysis starts in 2009 and requires at least three complete pre-treatment years, we focus on BID initiatives introduced from 2012 onward. Four BID initiatives introduced in 2007 and 2008 are therefore not included in the crime analysis. The analysis covers 15 BID initiatives in vulnerable areas, introduced in nine treatment cohorts in 2012, 2015, 2017, 2018, 2019, 2020, 2021, 2022, and 2023.

The resulting estimation sample consists of 236 DeSO areas. Of these, 74 treated DeSO areas are located within the 15 BID areas, while 162 DeSO areas belong to 38 vulnerable areas that remained untreated throughout the study period and constitute the comparison pool. We exclude one DeSO area within a treated BID area because population data are unavailable for part of the pre-treatment period.

[2] Details on the classification procedure is available upon request.
[3] More information about the DeSO can be found at: https://www.scb.se/hitta-statistik/regional-statistik-och-kartor/regionala-indelningar/demografiska-statistikomraden-deso/.

Figure 2. Distribution of treated DeSO areas across BID implementation cohorts in the main analysis sample.



3.2 Crime outcomes

We construct five crime outcomes. Four correspond to broad categories of reported offenses: narcotics and other special-law offenses, vandalism, theft, and violence against persons. Our measure of total crime is the sum of these four categories. Other categories in the police data, including fraud, traffic offenses, and other penal code offenses, are not included in the analysis. For each outcome, we calculate the annual number of reported offenses per 1,000 residents. To reduce the influence of extreme local observations, the winsorized crime rates at the 1st and 99th percentiles. Thus, the outcome used in the estimations is the natural logarithm of one plus the winsorized crime rate, a transformation that accommodates observations with zero reported offenses and reduces the influence of the strongly right-skewed distribution of local crime rates.

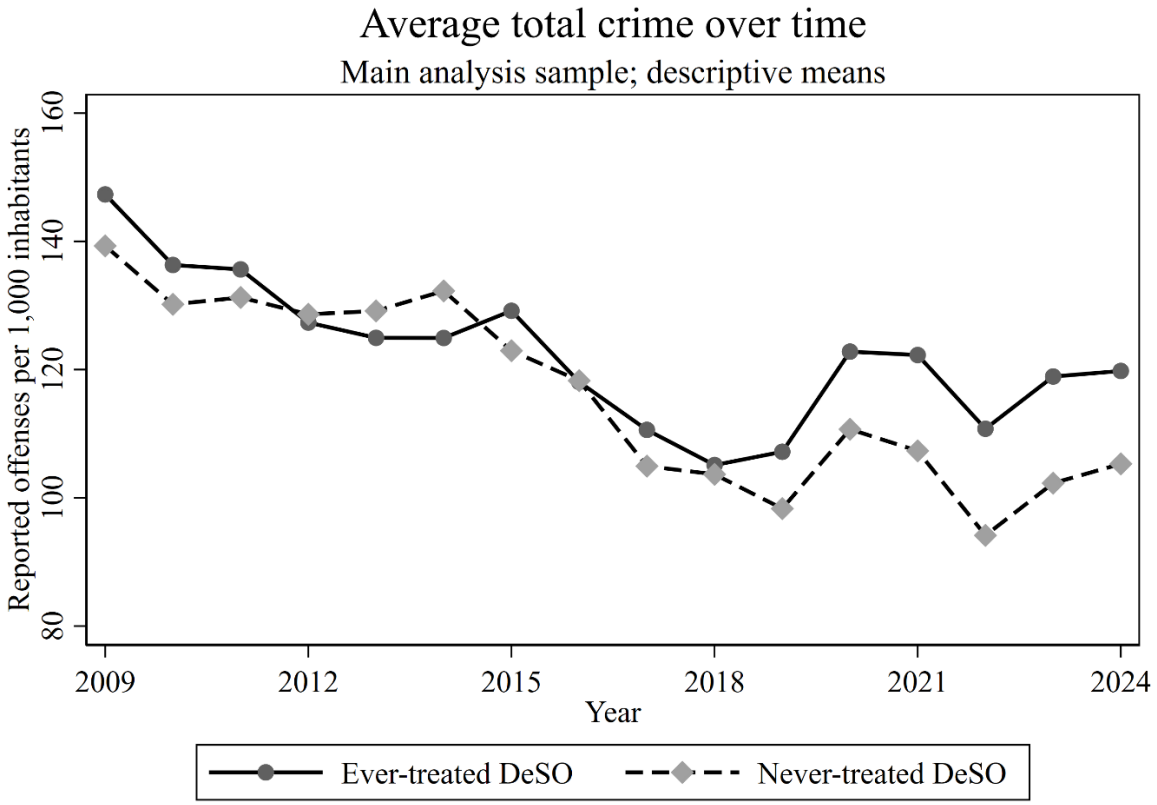
Statistics in Table 1 are based on annual DeSO-level observations in the main analysis sample. Crime rates are measured as reported offenses per 1,000 residents and are shown before winsorization and log transformation.

Table 1. Descriptive statistics for the five crime outcomes in the main analysis sample.

Outcome	N	Mean	Median	SD	Min	Max
Total crime	3,776	118.2	97.5	78.4	17.4	1,286.7
Narcotics/special offenses	3,776	19.5	15.5	17.7	0.0	481.3
Vandalism	3,776	23.2	14.4	41.0	0.0	1,207.5
Theft	3,776	36.8	28.2	32.8	1.1	422.0
Violence against persons	3,776	38.8	35.8	20.0	1.3	328.5

Figure 3 shows the development of total reported crime for the treatment and control group. The figure is descriptive and should not be interpreted causally because treatment is staggered and the series does not adjust for differences between treated and untreated locations.

Figure 3. Mean total reported crime per 1,000 residents, 2009-2024, by treatment group.



3.3 Empirical strategy

Our primary empirical approach is synthetic difference-in-differences, SDID (Arkhangelsky et al., 2021). SDID combines difference-in-differences with synthetic control methods to construct a weighted comparison group that follows the treated areas as closely as possible before treatment. The approach is well suited to our setting as BID initiatives were introduced at different points in time, while treated and untreated areas may differ in both crime levels and pre-treatment trends (Porreca, 2022).

As with other difference-in-differences designs, we cannot directly observe the causal effect because we do not observe how crime would have developed in a treated area had the BID initiative not been introduced. We therefore use untreated areas to construct a counterfactual development. The intuition is that comparison units that followed a similar trajectory before treatment provide a credible indication of how the treated areas would have developed in the absence of the BID initiative. SDID strengthens the comparison by constructing a synthetic comparison group that closely reproduces the pre-treatment trajectory of the treated areas. The causal interpretation rests on the assumption that, after SDID weighting, the synthetic

comparison provides a valid counterfactual for how the treated areas would have developed in the absence of treatment.

To formalize the SDID estimator used in the analysis, let Y_{it} denote the natural logarithm of one plus the winsorized crime rate per 1,000 residents in DeSO area i in year t, and let W_{it} equal one for treated DeSO areas in years following BID implementation, including the implementation year, zero otherwise. Because BID initiatives are adopted at different points in time, we estimate SDID separately for each treatment cohort using that cohort and the never-treated DeSO areas. For treatment cohort a, where a denotes the BID implementation year, the SDID estimator is obtained from the following weighted difference-in-differences regression:

$$\left(\hat{\tau}_a^{SDID}, \hat{\mu}_a, \hat{\alpha}_a, \hat{\beta}_a\right) = \arg\min_{\tau,\mu,\alpha,\beta} \sum_i \sum_t (Y_{it} - \mu - \alpha_i - \beta_t - \tau W_{it})^2 \, \hat{\omega}_{a,i} \hat{\lambda}_{a,t}, \quad (1)$$

where $\hat{\tau}_a^{SDID}$ is the treatment effect for cohort a, μ is the intercept, α_i and β_t are DeSO area and year fixed effects, and $\hat{\omega}_{a,i}$ and $\hat{\lambda}_{a,t}$ are the SDID unit and time weights, respectively. The DeSO area fixed effects absorb remaining level differences between treated and synthetic comparison units. We choose the unit weights so that the weighted control group closely tracks the treated units in the pre-treatment period. The time weights are chosen so that a weighted combination of pre-treatment periods predicts the average post-treatment outcome among the control units. Conditional on these estimated unit and time weights, Equation (1) yields the SDID treatment-effect estimate for each treatment cohort.

We estimate dynamic treatment for the year in which a BID initiative is introduced (i.e., event time 0) and the following three years (i.e., event times +1, +2, and +3). Our primary estimate averages the treatment effect over the implementation year and the first three subsequent years. Because the number of treated areas we can observe differs across post-treatment event times, we combine the event-time-specific estimates using the number of treated DeSO areas contributing at each event time as weights.

Let $\hat{\tau}_k^{SDID}$ denote the SDID treatment-effect estimate at event time k, and let N_k denote the number of treated DeSO areas contributing to that event-time estimate. The combined effect is calculated as:

$$\widehat{ATT}_{0:3} = \frac{\sum_{k=0}^{3} N_k \, \hat{\tau}_k^{SDID}}{\sum_{k=0}^{3} N_k}. \quad (2)$$

In our sample, 74 treated DeSO areas contribute at event times 0 and +1; 62 at event time +2; and 51 at event time +3. The combined estimate therefore summarizes treatment effects across 261 treated DeSO-year observations during the implementation year and the first three subsequent years.

In addition to this average effect, we use the event-time-specific estimates to examine how the estimated effects develop around BID implementation. Estimates at event times 0 through +3

represent post-treatment treatment-effect estimates. Estimates at event times -3 through -1 are placebo estimates used as pre-treatment diagnostics rather than coefficients relative to an omitted event time -1 reference period.

We base statistical inference on 500 placebo repetitions using never-treated comparison units, using the resulting placebo distribution to quantify uncertainty around the estimated treatment effects. The pre-treatment placebo estimates also provide a diagnostic of whether systematic discrepancies between treated and synthetic comparison units remain before BID implementation.

3.4 Robustness tests

To assess the robustness of the results and to choose the primary estimator without using post-treatment outcomes, we compared the pre-treatment performance of SDID with an alternative design based on cohort-stacked difference-in-differences after coarsened exact matching, CEM (Iacus et al., 2012). SDID provided a better overall fit to the pre-treatment data than the CEM-based specification. It also performed better in pre-treatment prediction tests, in which later pre-treatment observations were predicted using only earlier pre-treatment data, for four of the five crime outcomes. We therefore use SDID as the primary estimator and CEM as an alternative specification. We report detailed pre-treatment comparisons in Appendix Table B1.

We implement the CEM-based specification in two steps. First, we use CEM as a preprocessing procedure to improve comparability between treated and never-treated DeSO areas before treatment effects are estimated. Matching is conducted separately for each treatment cohort and crime outcome using three pre-treatment characteristics: the mean outcome over event times -3 to -1, the change in the outcome between event times -3 and -1, and the natural logarithm of population at event time -1. We coarsen each characteristic into three groups, yielding up to 27 joint strata. We retain strata containing both treated and comparison units. Treated units receive unit weight, while comparison units are reweighted within matched strata to reproduce the distribution of the treated units.

In the second step, we use the matched and reweighted sample to estimate the following weighted cohort-stacked difference-in-differences specification:

$$(\hat{\tau}_{0:3}^{CEM}, \hat{\alpha}, \hat{\gamma}) = \arg\min_{\tau,\alpha,\gamma} \sum_a \sum_i \sum_t w_{ia} \left[Y_{iat} - \alpha_{ia} - \gamma_{at} - \tau D_{ia} P_{at}^{0:3}\right]^2, \quad (3)$$

where Y_{iat} denotes the natural logarithm of one plus the winsorized crime rate per 1,000 inhabitants for DeSO i in cohort a in year t, w_{ia} denotes the CEM weight, α_{ia} and γ_{at} are DeSO-by-cohort and calendar-year-by-cohort fixed effects, respectively. D_{ia} equals one for DeSO areas belonging to treatment cohort a and zero for the never-treated comparison areas in the corresponding cohort-specific stack. $P_{at}^{0:3}$ equals one for event times 0 through +3 and zero otherwise. The coefficient $\hat{\tau}_{0:3}^{CEM}$ therefore captures the average treatment effect over the implementation year and the following three years. Standard errors are clustered at the DeSO level.

To examine whether the CEM-based results display similar dynamics around BID implementation, we estimate a corresponding event-study specification using the same matched sample and CEM weights. We replace the post-treatment indicator in Equation (3) with event-time indicators:

$$Y_{iat} = \alpha_{ia} + \gamma_{at} + \sum_{k\in\{-3,-2,0,1,2,3\}} \beta_k D_{ia} 1\{t - a = k\} + \varepsilon_{iat}, \quad (4)$$

where k denotes event time, $1\{t - a = k\}$ is an indicator for event time k, and $\varepsilon_{iat}$is the error term. Event time -1 is omitted as the reference period. The resulting β_k coefficients trace differences between treated and comparison units relative to event time -1. The coefficients at event times -3 and -2 serve as pre-treatment diagnostics, while coefficients at event times 0 through +3 represent the dynamic treatment effects.

Together, the average and dynamic CEM estimates provide an alternative matching and estimation strategy against which the main SDID results can be assessed.

3.5 Results

3.5.1 Average treatment effects

Table 2 presents the main SDID estimates averaged over the implementation year and the following three years.

Table 2. Main SDID estimates of the average treatment effect over event times 0 through plus 3.

Outcome	Estimate	SE	95% CI	p-value
Total crime	0.018	0.027	[-0.036, 0.071]	0.519
Narcotics/special offenses	0.023	0.053	[-0.082, 0.127]	0.670
Vandalism	0.064	0.051	[-0.035, 0.164]	0.205
Theft	-0.046	0.037	[-0.118, 0.025]	0.206
Violence against persons	0.000	0.030	[-0.059, 0.058]	0.989

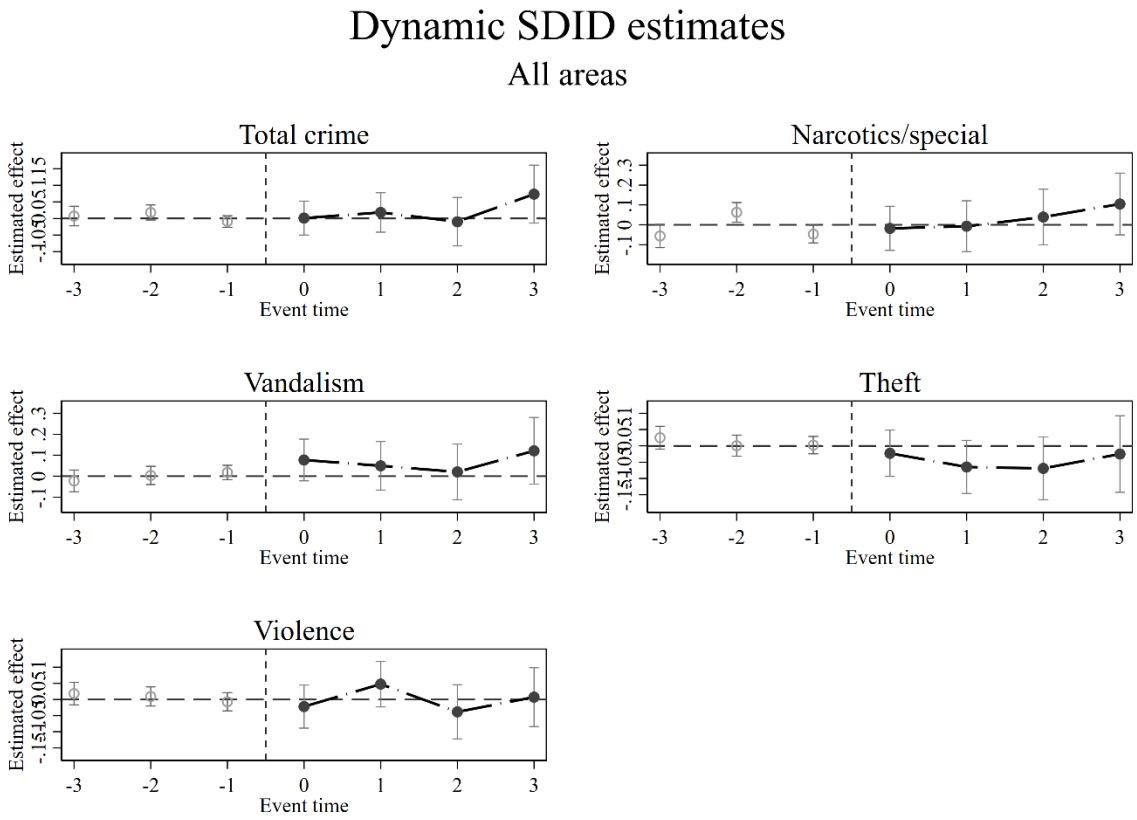
The estimate for total crime is close to zero. Across the underlying crime categories, the point estimates are positive for vandalism and narcotics and special-law offenses, negative for theft, and close to zero for violence. None of the five estimates is statistically significant, and all confidence intervals include zero. Thus, the estimates provide no evidence that BID implementation affected recorded crime on average during the implementation year and the following three years.

3.5.2 Dynamic effects

Figure 4 shows the corresponding dynamic SDID estimates. Event times −3 through −1 are pre-treatment placebo estimates, while event times 0 through +3 show the estimated treatment effects. The pre-treatment estimates provide a check on remaining pre-treatment discrepancies:

absent differential pre-treatment trends, the estimates should be close to zero and statistically insignificant.

Figure 4. Dynamic SDID estimates for total crime, narcotics and special-law offenses, vandalism, theft, and violence against persons.



Note: Open markers show pre-treatment placebo estimates and filled markers show post-treatment treatment-effect estimates.

Across four of the five crime outcomes (total crime, vandalism, theft, and violence against persons), the pre-treatment estimates show no statistically significant differences, providing support for the validity of the synthetic comparison. For total crime, the estimates remain close to zero during the implementation year and the first two subsequent years, with a more positive point estimate at event time +3. Overall, the confidence intervals include zero across the post-treatment estimates, and Figure 4 therefore provides no evidence of statistically significant dynamic treatment effects for any of the crime outcomes.

This said, the results for narcotics and special-law offenses warrant some caution. In contrast to the other crime outcomes, the pre-treatment placebo estimates show noticeable deviations from zero, raising concerns about the parallel trends assumption. Although the average post-treatment estimate is relatively small and not statistically significant, the pre-treatment pattern makes a causal interpretation of the post-treatment estimates less credible. We therefore place less weight on the results for narcotics and special-law offenses. We show the corresponding SDID-weighted trajectories for treated and synthetic comparison areas in Appendix Figure B1.

3.5.3 Alternative estimations

To assess the robustness of the results, Table 3 compares the primary SDID estimates with the cohort-stacked CEM estimates. Additional diagnostics, however, indicate substantial geographic heterogeneity within the treated sample. One area in particular, Hässelby/Vällingby, accounts for 12 of the 17 treated DeSO areas in the 2017 cohort and has a substantial influence on some estimates. To assess the extent to which this area influences the results, we report estimates for the full sample and estimates after excluding Hässelby/Vällingby for both methods.

Table 3. Robustness and geographic heterogeneity. Average treatment effects over event times 0 through plus 3 estimated using SDID and CEM, for the full sample and after excluding Hässelby/Vällingby (H/V).

Outcome	**SDID: all areas**	**SDID: excl. H/V**	**CEM: all areas**	**CEM: excl. H/V**
Total crime	0.018	-0.035	-0.005	-0.057**
	(0.027)	(0.030)	(0.026)	(0.026)
Narcotics/special offenses	0.023	0.082	-0.016	0.032
	(0.053)	(0.060)	(0.051)	(0.054)
Vandalism	0.064	-0.039	0.123**	0.019
	(0.051)	(0.054)	(0.052)	(0.052)
Theft	-0.046	-0.113***	-0.061*	-0.128***
	(0.037)	(0.038)	(0.037)	(0.036)
Violence against persons	0.000	-0.021	-0.029	-0.055*
	(0.030)	(0.030)	(0.028)	(0.030)

Note: * indicates $p < 0.10$, ** indicates $p < 0.05$ and *** indicates $p < 0.01$.

The CEM estimates are broadly consistent with the main SDID results: Both estimators produce estimates close to zero for total crime and violence, negative estimates of similar magnitude for theft, and positive estimates for vandalism. The main difference concerns vandalism, for which the CEM estimate is larger and statistically significant. The estimates for narcotics and special-law offenses are small under both specifications but differ in sign. Overall, the similarity in direction and magnitude across most outcomes indicates that the main conclusions are not driven by the particular estimation strategy used in the main SDID analysis. The corresponding dynamic estimates from the CEM specification are reported in Appendix Figure B2.

Excluding Hässelby/Vällingby changes the estimated effects for vandalism and theft considerably. The SDID estimate for vandalism changes from +0.064 in the full sample to -0.039, while the corresponding CEM estimate changes from +0.123 to +0.019. The most consistent results across the four specifications are found for theft. The SDID estimate becomes more negative and statistically significant at the 1% level, changing from -0.046 to -0.113, while the CEM estimate changes from -0.061 to -0.128 and becomes statistically significant at the 1% level, compared with the 10% level in the full sample. The estimate for total crime also becomes more negative, although it is statistically significant only under CEM. We report the

corresponding dynamic SDID estimates after excluding Hässelby/Vällingby in Appendix Figure B3.

The fact that the SDID and CEM estimates shift in similar directions suggests that this sensitivity reflects geographic heterogeneity rather than a feature specific to one estimator. The SDID-weighted trajectories in Appendix Figure B4 support this interpretation. After Hässelby/Vällingby is excluded, the treated and synthetic series remain closely aligned before treatment for most outcomes. Following implementation, however, the treated series falls relative to the synthetic comparison for both theft and vandalism, with the clearest divergence for theft.

We nevertheless retain Hässelby/Vällingby in the preferred specification. There is no evidence of miscoding, measurement error, or any pre-treatment reason for excluding the area. Excluding it based on its post-treatment development would make the sample definition dependent on the observed outcomes. We therefore interpret the exclusion analysis as evidence of geographic heterogeneity rather than as a basis for redefining the main sample. Overall, the results provide limited evidence of statistically significant effects, while the sensitivity analyses for vandalism and theft suggest that the estimates may vary across locations.

4 Perceived safety: Data and analysis

4.1 Data and methodology

To further examine whether BID initiatives influence not only crime but also perceived safety, we conduct a survey among firms operating in vulnerable neighborhoods. The survey complements the crime analysis by capturing business owners', managers' and employees' perceptions and experiences, which are not observable in the Police Authority's register-based crime statistics.

The survey was administrated by a professional survey company. We targeted businesses located in five vulnerable neighborhoods were BID initiatives had been implemented and eleven control areas without BID initiatives[4]. To assess the comparability between treated and control areas, we compared a set of socioeconomic variables commonly associated with neighborhood crime and economic condition, including income levels, employment, education, and demographic composition. As seen in Table 4, control areas share similar structural characteristics with the treated areas: Two-sided t-tests found no statistically significant difference between the treated and control areas for any of the observed variables, except the share of residents with a foreign background. Although the difference is statistically significant ($p = 0.04$), it is relatively modest in magnitude: the mean share is 73 percent in the treated areas compared with 79 percent in the control areas, corresponding to a difference of 6 percentage points.

[4] The treated BID areas are: Jordbro, Flemingsberg-Grantorp-Visätra, Vårby, Skogås, and Brandbergen. The control areas are: Hovsjö, Hallunda-Norsborg, Ronna-Geneta-Lina, Storvreten, Fisksätra, Hagalund, Rissne/Hallonbergen, Sångvägen, Alby, Saltskog, Hageby, and Navestad.

Table 4. Comparability between treated and control areas

Variable	Treated mean	Control mean	Difference (treated-control)	p-value	Source
Average income (kSEK)	267.89	261.03	6.86	0.38	Statistics Sweden
Employment rate, ages 20-64	0.70	0.68	0.02	0.11	Statistics Sweden
Share with tertiary education, ages 25-64	0.18	0.17	0.01	0.61	Statistics Sweden
Share with foreign background	0.73	0.79	-0.06	0.04	Statistics Sweden
Share of youths, ages 15-24	0.12	0.13	-0.01	0.14	Statistics Sweden
Crime rate (per 1,000 inhabitants)	103.07	104.77	-1.71	0.90	The Swedish Police Authority
Share of low-income individuals	0.44	0.46	-0.02	0.40	Statistics Sweden

Note: Values are averages over 2019–2023, except for the employment rate, which is averaged over 2020–2023 due to data availability. The employment rate is defined as the share of individuals aged 20–64 who are employed. The share with tertiary education is defined as the proportion of individuals aged 25–64 who have completed at least three years of post-secondary education. The low-income measure refers to the share of individuals in the lowest quartile of the national distribution of equivalized disposable household income, according to Statistics Sweden's definition.

The questionnaire was in the field between 16th of August 2025 and 14th of March 2026.[5] Most respondents operated in retail or in the hospitality sector, and more than three quarters represented firms in operation since at least 2020. The final analytical sample consists of 160 respondents from BID areas and 157 respondents in control areas. In addition, 73 store managers/employees declined participation (32 in BID areas and 41 in control areas), while 172 could not be reached despite repeated visits to their stores (64 and 108, respectively). Cooperation rates among contacted individuals were therefore high in both groups (83.3% in BID areas and 79.3% in control areas). Including both individuals who declined participation and those who could not be reached yields overall response rates of 62.5% for BID areas and 51.3% for control areas.[6]

The questionnaire combines closed- and open-ended questions covering four broad dimensions. First, respondents were asked to assess changes in local business conditions and perceived safety by comparing current conditions with those three or five years earlier, depending on the timing of the BID implementation (Q1-Q3). Second, respondents reported whether they had been exposed to different types of crime, both during an earlier reference period and during the previous twelve months, making it possible to compare changes over time between treated and control areas (Q4A-H, Q3A-H). Third, respondents evaluated the accessibility of support when exposed to crime, providing information on perceived institutional responsiveness (Q5, Q6).

[5] With a break between 10th of December and 25th of January.
[6] Details are provided in Appendix C.

Finally, open ended questions asked respondents to describe how crime had affected their businesses and what measures they had implemented to reduce safety-related risks. Businesses located in BID areas were also asked whether they were aware that BID initiatives had been implemented in their neighborhoods. The complete questionnaire can be found in Appendix C. We analyze the survey responses descriptively and, where comparable information is available for both earlier and current periods, we use a DiD framework comparing developments in treated and control areas before and after implementation.

4.2 Results

Table 5 reports mean responses for the structured survey questions, all measured on five-point Likert scales. Respondents in BID areas are considerably more likely to report that local business conditions have improved over time (Q1): the absolute difference is 0.68 on the 1-5 scale, significant at the 1%. BID respondents also report higher current levels of perceived safety (Q2) and greater improvements in safety compared to previous years (Q3). The differences are substantial and statistically significant, indicating more favorable perceptions of neighborhood development among businesses located in BID areas.

In contrast, differences are virtually non-existent for self-reported crime exposure: changes over time are similar in treated and control areas, and the estimated differences between BID and non-BID areas are not statistically significant. The only exception concerns burglary (Q4F-Q3F), where respondents in BID areas report a significantly larger decline in exposure than respondents in the control areas. Likewise, we observe no statistically significant difference regarding changes in perceived access to support following crime (Q6-Q5).

Table 5. Survey results.

Question	Treated (T)	Control (C)	Difference (T−C)	Significance
Q1. Is the area a better place for shops and businesses today compared to 3/5 years ago?	3.67	2.99	0.68	***
Q2. How safe do you consider the area today?	3.44	3.17	0.27	**
Q3. Do you feel safer in the area today compared to 3/5 years ago?	3.41	2.94	0.47	***
Difference (After–Before)				
Q4A–3A (Violence)	-0.006	0.000	-0.006	
Q4B–3B (Theft)	-0.045	-0.026	-0.019	
Q4C–3C (Vandalism)	-0.071	-0.013	-0.058	
Q4D–3D (Drugs)	-0.032	-0.026	-0.006	
Q4E–3E (Threats)	-0.013	0.000	-0.013	
Q4F–3F (Burglary)	-0.123	-0.006	-0.117	**
Q4G–3G (Robbery)	-0.006	0.006	-0.012	
Q4H–3H (No crime)	-0.310	-0.253	-0.057	
Q6–Q5 (Help access)	0.000	0.271	-0.271	

Note: * indicates $p < 0.10$, ** indicates $p < 0.05$ and *** indicates $p < 0.01$.

The open-ended responses provide a similar picture. Respondents in both treated and control areas describe some improvement over time, but approximately half of the comparable responses from the treated areas explicitly described a reduction in crime-related problems or their impact, compared with approximately one-third of the responses from the control areas. These proportions should be interpreted cautiously, as the classification necessarily involves some judgement given the open-ended nature and often limited detail of the responses.

Businesses in both groups report taking similar protective measures, most commonly surveillance cameras, alarm systems, and security personnel. Finally, awareness of the BID initiative among respondents in treated areas is not clearly associated with perceptions of neighborhood development and safety. This suggests that the observed effects are unlikely to be driven solely by signaling or informational channels. Instead, the results are more consistent with mechanisms operating through tangible changes in the local environment and patterns of everyday activity.

5 Conclusion and discussion

The aim of this study is to examine the effects of voluntary BID initiatives on crime and perceived safety in socioeconomically disadvantaged neighborhoods in Sweden. More specifically, we use synthetic difference-in-differences to estimate how reported crime develops following BID implementation, comparing treated DeSO areas with vulnerable areas that remain untreated throughout the study period. The analysis accounts for the staggered introduction of BID initiatives, examining both average and dynamic effects during the implementation year and the following three years. Moreover, we use a survey analysis to evaluate how business owners, managers, and employees in vulnerable neighborhoods perceive changes in criminal exposure *and* safety following the implementation of BID schemes, compared to similar areas where such initiatives have not yet been introduced. The two methods allow us to analyze whether BID initiatives affect crime levels and perceived safety in similar ways, or if effects differ.

The crime analysis provides little evidence of statistically significant average effects of BID implementation on recorded crime. In the main SDID specification, none of the average effects on total crime or the four individual crime categories is statistically significant. The alternative CEM specification produces broadly similar estimates, indicating that our main conclusions are robust to the choice of estimator.

At the same time, our geographical heterogeneity analysis identifies one specific treated area whose post-treatment development differs substantially from that of the other treated areas and exerts a disproportionate influence on some of the pooled estimates. When it is excluded, the results change particularly for theft and vandalism, while SDID and CEM continue to move in the same direction. For theft, the estimated reduction becomes approximately 11 percent under SDID (coefficient = -0.113) and 12 percent under CEM (coefficient = -0.128), with both estimates statistically significant at the 1% level. The estimate for total crime also becomes

more negative, although it is statistically significant only under CEM. This pattern therefore points to the influence of one unusually important area rather than to a general sensitivity of the results to which treated areas are included. Variation in effects across crime categories is also found in previous research. For example, MacDonald et al. (2010) find that BID programs in Los Angeles reduced robbery and violent crime, while no corresponding effect on homicide could be identified.

The survey results of BIDs' effects on perceived safety paint a somewhat similar picture: with the exception of a reduction in burglary exposure, changes in business managers' reported exposure to crime are generally small and largely indistinguishable between treated and control areas. However, businesses located in BID areas report significantly more positive developments in both perceived safety and the local business environment compared to control area businesses.

One possible explanation for the general lack of a clear effects on recorded crime is that Swedish voluntary BID initiatives may not be intensive enough to substantially alter the incentives facing potential offenders, as described by Becker (1968). While many BID activities are intended to improve neighborhood conditions, they may only have a limited impact on the expected benefits and costs of offending. A recent mapping of BIDs in the Stockholm region shows that common activities include cleaning, offering summer jobs for young people, undertaking safety walks, light improvements, graffiti removal and the creation of safe and creating attractive meeting places (The Swedish Property Federation, 2024). While such measures could increase the probability of detection by improving the physical environment and strengthening informal guardianship, they may be too modest to substantially alter offenders' behavior, particularly in neighborhoods where crime is driven by organized networks. Similarly, while initiatives such as summer jobs may increase the opportunity cost of offending, their scale might be insufficient to generate measurable reductions in aggregate crime rates.

The fact that changes in perceived safety are not fully reflected in changes in recorded crime is not necessarily contradictory, as the two dimensions may influence each other without developing in parallel (Khruakham & Lee, 2014). The discrepancy suggests that the primary effects of BID interventions may not operate through broad reductions in crime, but rather through changes in how local environments are experienced and interpreted. The interpretation is consistent with theoretical perspectives emphasizing the role of environmental cues, activity patterns, and informal social control in shaping perceived safety (Jacobs, 1961; Cohen & Felson, 1979). Improvements in lighting, maintenance, social coordination, and the presence of people and institutions may strengthen perceptions of order and reduce uncertainty, even in the absence of substantial changes in underlying crime levels. In this sense, BID interventions may function primarily as mechanisms for place management and strengthened social efficacy, influencing how urban spaces are used and perceived rather than directly altering the structural determinants of crime.

Previous research also suggests that the effects of BIDs are closely related to the functions of BID programs and the conditions in the areas where they are implemented. Positive effects found for BIDs in central and economically stronger areas may therefore not be directly transferable to economically disadvantaged areas with a weaker economic and resource base (Gross, 2005; Ellen et al., 2007).

The findings highlight the importance of distinguishing between objective crime measures and subjective dimensions of safety when evaluating place-based interventions. While reductions in crime remain an important policy goal, improvements in perceived safety may in themselves generate meaningful economic and social benefits by influencing behavior, e.g., investment decisions and the use of public space. Consequently, evaluations should not rely exclusively on police-recorded crime statistics. Improvements in perceived safety may generate important economic benefits through increased customer flows, business investment and the use of public spaces, even when crime reduction effects are limited or difficult to detect.

Structural and methodological factors may also help explain the results. Similar safety, crime-prevention, and development initiatives may occur in control areas, reducing differences between treated and control areas and making BID effects more difficult to detect. Also, place-BIDs may increase businesses' and individuals' propensity to report crime, for example by strengthening their contacts with the police and other local actors. Recorded crime may consequently increase or remain unchanged even if actual crime declines. Moreover, the survey is limited to business owners, managers, and employees and therefore does not capture how residents, customers, and other visitors perceive safety following BID implementation. Further research could address these limitations by perceived safety among a broader range of groups.

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Appendix A

Table A1. Voluntary programs included in the study, by start year and area.

Start year	Program	Area
2012	Rågsveds Fastighetsägare	Hagsätra/Rågsved
2015	Utveckling Bäckby	Bäckby
2015	BID Sofielund	Södra Sofielund/Seved
2017	Platssamverkan Gottsunda - Valsätra	Gottsunda
2017	Hässelby Vällingby Fastighetsägare	Hässelby/Vällingby
2017	Vi är Valsta	Valsta
2019	Bro Utveckling	Finnsta
2020	BID Flemingsberg (Samverkan i Huddinge)	Grantorp/Visättra
2021	Jordbro i samverkan	Jordbro
2021	BID Tureberg	Tureberg
2021	BID Vårby (Samverkan i Huddinge)	Vårby
2022	Bo bra i Biskopsgården 2.0	Biskopsgården
2023	BID Brandbergen	Brandbergen
2023	BID Skogås (Samverkan i Huddinge)	Skogås
2023	BID Frölunda-Tynnered	Tynnered/Grevegården/Opaltorget

Note: The start year refers to the year in which the program, according to available information, began its activities. In several cases, however, the programs were established gradually.

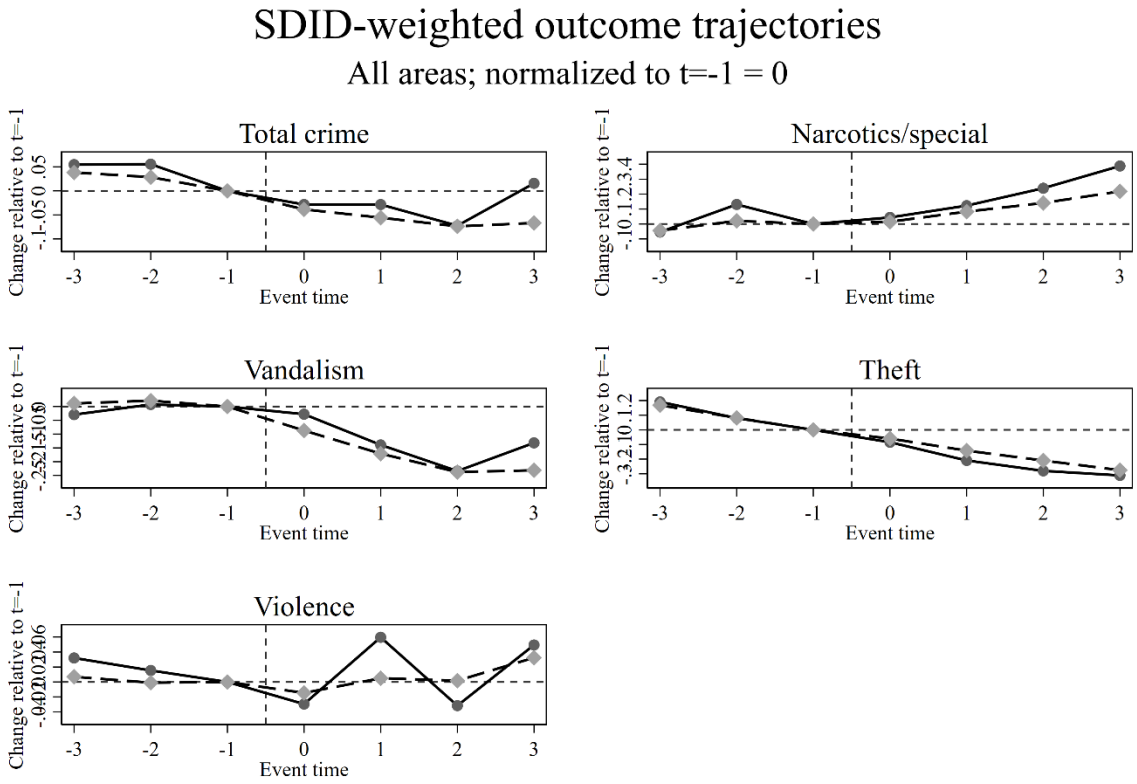
Appendix B

Table B1. Pre-treatment performance of SDID and CEM.

Outcome	RMSE: CEM	RMSE: SDID	First-difference RMSE: CEM	First-difference RMSE: SDID	Pre-treatment RMSE: CEM	Pre-treatment RMSE: SDID
Total crime	0.159	0.041	0.420	0.130	0.539	0.387
Narcotics/special offenses	0.342	0.084	0.485	0.159	0.522	0.457
Vandalism	0.285	0.102	0.511	0.225	0.636	0.581
Theft	0.186	0.052	0.464	0.154	0.519	0.552
Violence against persons	0.208	0.051	0.371	0.105	0.452	0.424

Note: The table compares the pre-treatment performance of SDID and the CEM-based specification. First-difference Root Mean Squared Error (RMSE) measures how closely the estimators reproduce year-to-year changes during the pre-treatment period. Pre-treatment prediction RMSE measures prediction error when later pre-treatment observations are predicted using only earlier pre-treatment data. Lower values indicate better performance.

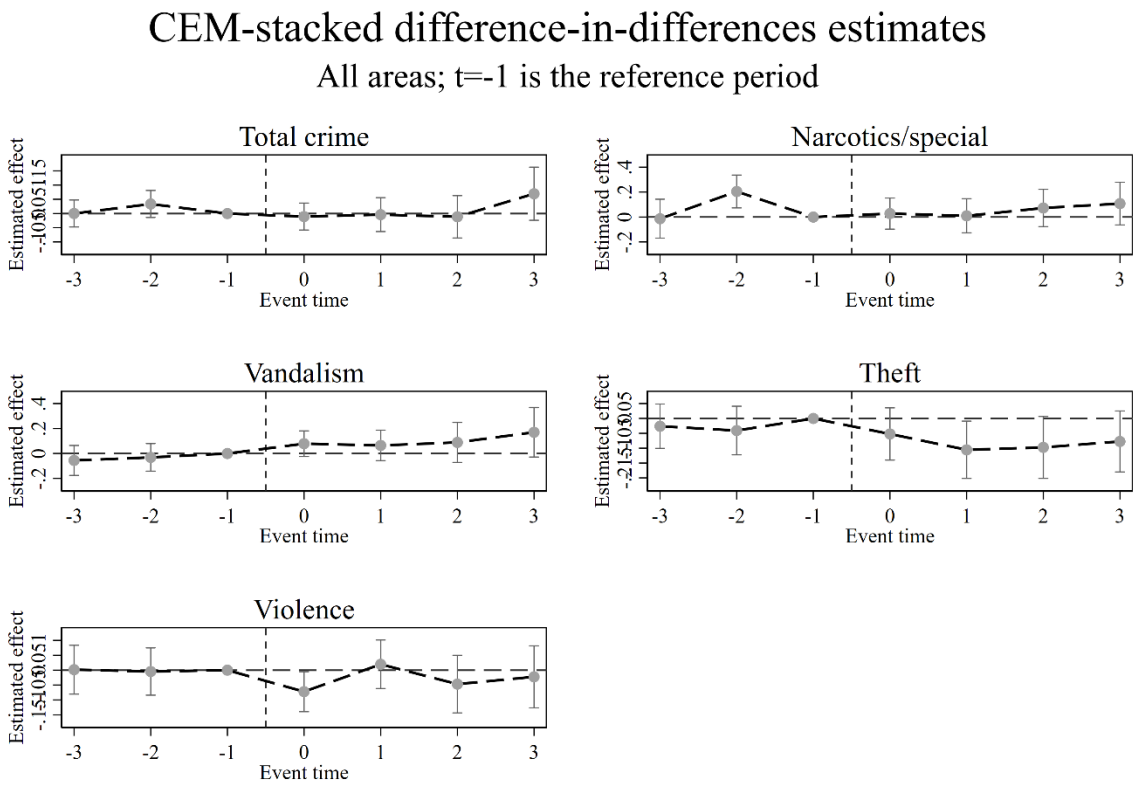
Figure B1. SDID-weighted outcome trajectories, full sample.



Solid circles = treated; dashed diamonds = synthetic comparison.

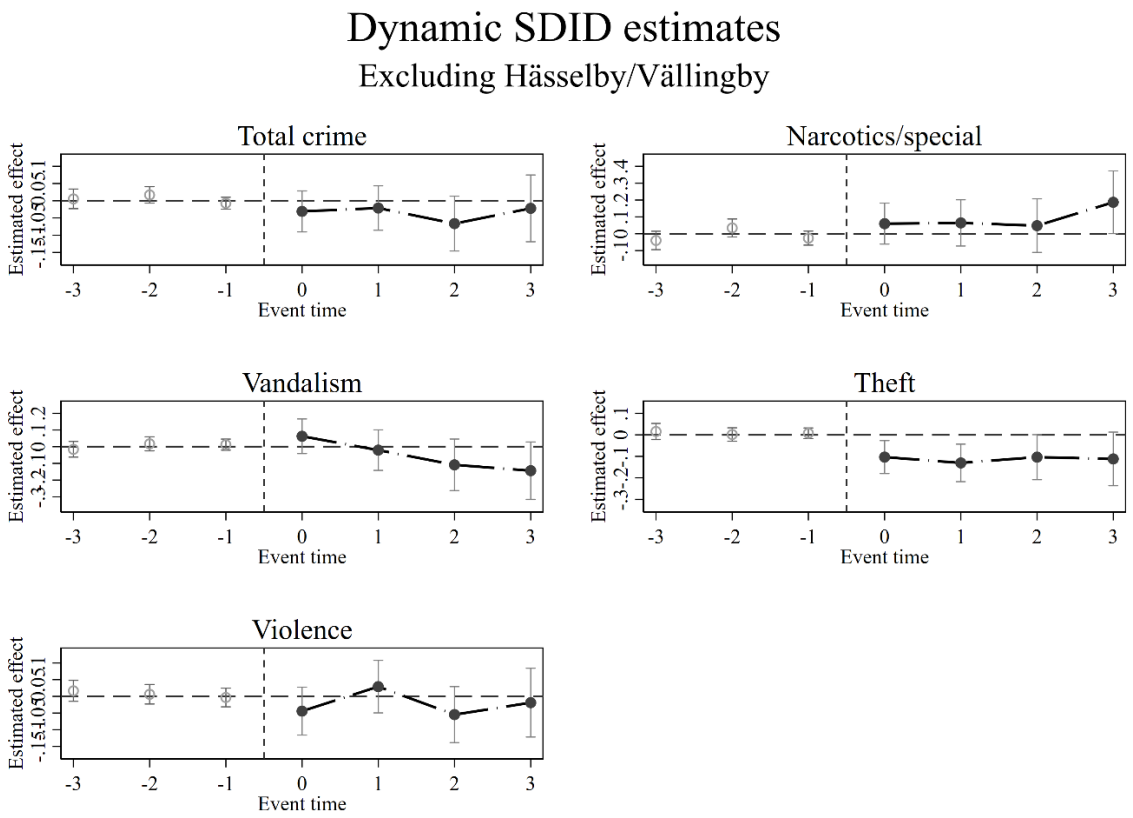
Note: Solid lines with circles show the treated trajectory and dashed lines with diamonds show the synthetic comparison. The series are normalized to zero at event time -1. The figure is intended as a visual diagnostic of pre-treatment fit and post-treatment divergence rather than as an alternative treatment-effect estimate.

Figure B2. Dynamic estimates from the CEM-stacked difference-in-differences specification.



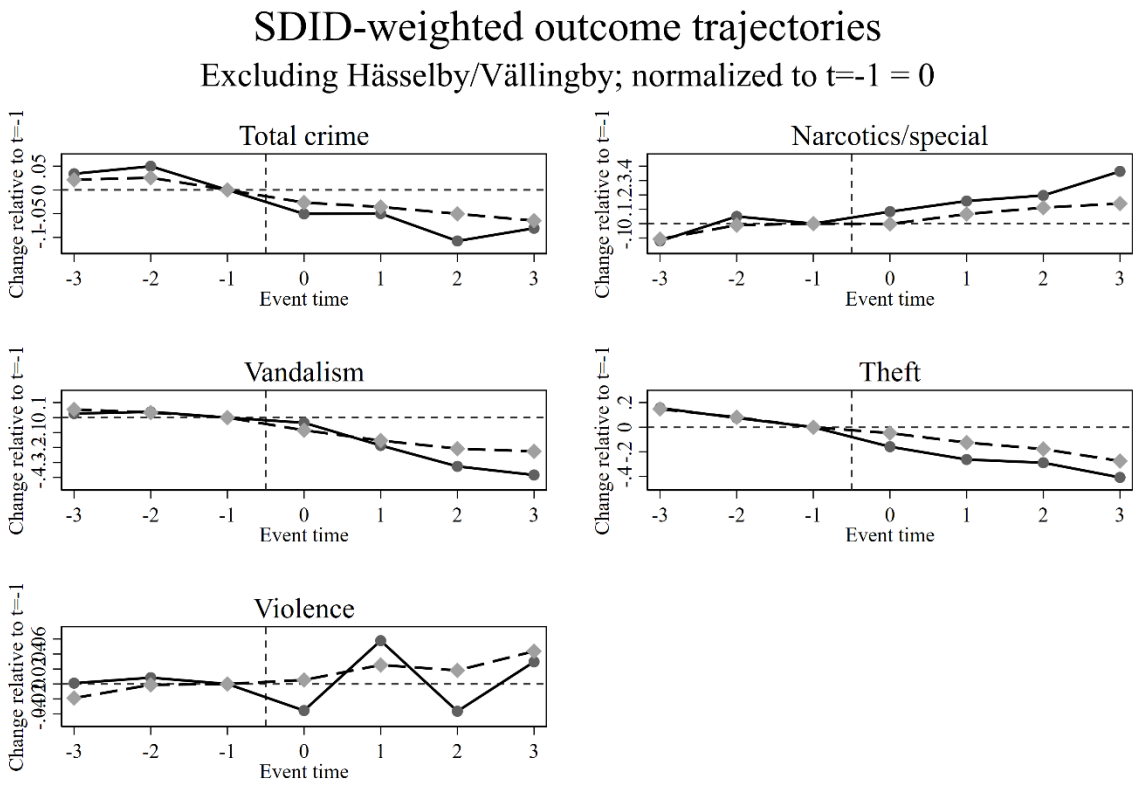
Note: The figure reports dynamic estimates from the cohort-stacked difference-in-differences specification after coarsened exact matching. Event time -1 is the reference period. Vertical bars show 95 percent confidence intervals, with standard errors clustered at the DeSO level.

Figure B3. Dynamic SDID estimates excluding Hässelby/Vällingby.



Note: The figure excludes the 12 DeSO areas in Hässelby/Vällingby belonging to the 2017 treatment cohort. Event times -3 through -1 show pre-treatment placebo estimates, while event times 0 through +3 show post-treatment estimates. Vertical bars show 95 percent confidence intervals based on 500 placebo repetitions.

Figure B4. SDID-weighted outcome trajectories excluding Hässelby/Vällingby.



Solid circles = treated; dashed diamonds = synthetic comparison.

Note: The figure shows the corresponding treated and synthetic trajectories after excluding Hässelby/Vällingby. The series are normalized to zero at event time -1 and should be interpreted as visual diagnostics rather than as treatment-effect estimates.

Appendix C

Table C1. Complete survey instrument.

Question	Description (English)	Measurement	Scale
Q1A	Is the area a better place for shops and businesses today compared to X years ago?	Likert	1–5 (worse → better)
Q1B	How safe do you consider the area today?	Likert	1–5 (very unsafe → very safe)
Q2A	Do you feel safer in the area today compared to five years ago?	Likert	1–5 (less safe → more safe)
Q2B	Do you feel safer in the area today compared to three years ago?	Likert	1–5 (less safe → more safe)
Q3	What types of crime was your store exposed to in the year 202X?		
	A. Exposure to violence (before)	Binary	1 = yes, 0 = no
	B. Exposure to theft (before)	Binary	1 / 0
	C. Exposure to vandalism (before)	Binary	1 / 0
	D. Exposure to drug-related crime (before)	Binary	1 / 0
	E. Exposure to threats (before)	Binary	1 / 0
	F. Exposure to burglary (before)	Binary	1 / 0
	G. Exposure to robbery (before)	Binary	1 / 0
	H. No exposure to crime (before)	Binary	1 / 0
Q4	What types of crime has your store been exposed to in the last 12 months?		
	A. Exposure to violence (last 12 months)	Binary	1 = yes, 0 = no
	B. Exposure to theft (last 12 months)	Binary	1 / 0
	C. Exposure to vandalism (last 12 months)	Binary	1 / 0
	D. Exposure to drug-related crime (last 12 months)	Binary	1 / 0
	E. Exposure to threats (last 12 months)	Binary	1 / 0
	F. Exposure to burglary (last 12 months)	Binary	1 / 0
	G. Exposure to robbery (last 12 months)	Binary	1 / 0
	H. No exposure to crime (last 12 months)	Binary	1 / 0
Q5	How easy was it to get help when exposed to crime X years ago?	Likert	1–5 (very difficult → very easy)
Q6	How easy is it to get help when exposed to crime today?	Likert	1–5
Q7	How did crime affect your business X years ago?	Open-ended	
Q8	How does crime affect your business today?	Open-ended	
Q9	What measures have you taken to handle crime or insecurity?	Open-ended	

Table C2. Survey non-response by area and response status.

Area	Declined	Not reached
BID areas		
Jordbro	7	15
Flemingsberg–Grantorp–Visättra	5	15
Vårby	8	9
Skogås	8	16
Brandbergen	4	9
Total BID areas	**32**	**64**
Control areas		
Hovsjö	4	12
Hallunda–Norsborg	6	13
Ronna–Geneta–Lina	2	8
Storvreten	3	5
Fisksätra	5	9
Hagalund	4	8
Rissne/Hallonbergen	5	10
Sångvägen	1	4
Alby	3	8
Saltskog	3	7
Hageby	3	11
Navestad	2	13
Total control areas	**41**	**108**
TOTAL	**73**	**172**

C3. Robustness test survey results

Table 4 presented the mean responses for the structured survey questions. Because the voluntary BID initiatives were introduced at different points in time, the reference period used in the survey questions was adjusted to the start year of each respective program. Respondents in the treated areas were therefore asked to compare the current situation with that three, four, or five years earlier, whereas respondents in the control areas consistently compared the current situation with the situation three years earlier. To examine whether this difference affected the results, we conduct a robustness test in which only treated areas with a three-year reference period are included. This ensures that the treatment and control groups answered the questions using the same time horizon.

The results (Table C3) remain largely unchanged. The differences between treated and control areas persist for Question 1A, concerning whether the area has become a better place for businesses over time, and Question 2, concerning improvements in perceived safety over time. By contrast, the difference in perceptions of current safety (Question 1B) is smaller and no longer statistically significant in the restricted sample.

Table C3. Robustness of survey results to using a common three-year reference period.

Question	Treatment group	Treated	Control	Difference (treated-control)	t-statistics	p-value
Q1A: Is the area a better place for shops and businesses today compared to X years ago?	All areas	3.67	2.99	+0.68	6.33	<0.001
	Only area 4–5	3.67	2.99	+0.68	5.24	<0.001
Q1B: How safe do you consider the area today?	All areas	3.44	3.17	+0.28	2.24	0.026
	Only area 4–5	3.40	3.17	+0.24	1.60	0.112
Q2: Do you feel safer in the area today compared to X years ago?	All areas	3.41	2.94	+0.47	5.00	<0.001
	Only area 4–5	3.27	2.94	+0.33	2.64	0.010