



A study of security guard deployment and crime reduction in three locations in southern Sweden

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Abstract

This study evaluates the impact of the LOV3 policy—which allows the police to mandate security guards to patrol and maintain order in public environments—on local crime rates in three locations within Malmö and Helsingborg, the two largest cities in southern Sweden, using data from March 2020 to November 2022. We use interrupted time series analyses on daily crime data to assess the policy's effects on reported crime rates. Our findings, which reveal no significant impact of the LOV3 policy on reported crimes in the examined locations, underscore the need for further research and refinement of crime prevention strategies.

Keywords

crime prevention, policy evaluation, regression discontinuity design, differences-in-differences, urban security, Sweden

In recent years, policymakers and law enforcement agencies have adopted targeted interventions to mitigate urban crime. These strategies, from bolstering police presence to launching community-based initiatives, officially aim to curtail criminal behavior and enhance public safety. However, the actual impact of these interventions on crime rates remains a subject of ongoing debate among researchers, practitioners, and policymakers. Ideally, any intervention should rest on evidence-based practice, but in the real world, this is often not the case (cf. Magnusson, 2022). This is particularly true for Sweden and the Nordic countries, where the research evidence for important concepts such as hot spot policing is much weaker than it is in the United States and the United Kingdom (See Braga et al., 2019). This underscores the urgent need for more empirical studies to evaluate their effectiveness and guide future policy decisions, a need that our research aims to address to some extent.

This research centers on evaluating a policy intervention, LOV3 (§3 in “lag om ordningsvakter” – law on order guards), in three locations in Malmö and Helsingborg, the two largest cities in southern Sweden, using data from March 2020 to November 2022. The LOV3 initiative allows for deployment of security guards in designated areas following a detailed assessment by the Swedish Police Authority. This assessment process requires a demonstrated need for security guards to address specific safety concerns or threats. The evaluation criteria include the local crime rate, potential for disturbances, and the anticipated benefits to the community’s welfare. The official aim is to bolster public security and safety and deter crime through the visible presence of authorized personnel. Furthermore, the LOV3 initiative mandates that security guards in these areas possess the necessary training and certification to manage various situations effectively (Norén Bretzel, 2020). Their responsibilities range from crowd control to providing first aid and liaising with law enforcement to report incidents while adhering to legal and ethical standards. This study leverages daily crime data to scrutinize the policy’s impact on reported crime incidents in three locations, contributing valuable insights to the discourse on the efficacy of targeted crime reduction strategies. This is of interest in international research on non-police patrolling to deter crime and the public policies of Nordic countries where security guards are often used. In Sweden, LOV3 areas have become standard across larger cities, often employed by the municipalities to complement the police. In addition to the southern Swedish cities studied here, major Swedish cities like Stockholm, Gothenburg, and Uppsala have large LOV3 areas (Norén Bretzel, 2020).

Crime concentrates in certain places (Weisburd, 2015) and concentrating police resources on high-crime locations – so-called “hot spot policing” – may reduce crime. Notably, this crime reduction is not just about moving crime to a nearby location; crime tends to also go down in nearby locations where the police have not increased their presence (Braga et al., 2019). An exciting aspect of the crime reduction effect is that most of it is residual. Crime does not just go down when officers are on site. For instance, a large-scale randomized controlled trial from London showed that 97% of the crime reduction from hot spot patrolling occurred when the police were not at the location (Ariel et al., 2020). Hot spot policing, therefore, appears to be a cost-effective approach to reducing crime and has been widely implemented across the globe. Although most of the research on the topic comes from the United States, there are also examples of studies from the Nordic countries, with a recent meta-study identifying four Swedish studies and one Danish study (Braga et al., 2019). The Nordic studies, however, show mixed results, and the only Nordic randomized trial found no general effect on crime, although significant reductions for vandalism and motor-vehicle-related crime were noted (Atterman, 2017). A more recent study on a large, uniformed, seven-month trial targeting neighborhoods with gang-related crime similarly found no crime-reducing effect. However, the effort did lead to significantly increased seizures of weapons and narcotics (Gerell & Ellberg, 2021). While we still expect hot spot policing to have a crime reducing effect in the Nordics, the evidence is much weaker than it is internationally, and it is possible that the effect is smaller in the Nordics.

The demand for police, however, tends to outweigh the available resources in many countries (Bland et al., 2021). There have been some attempts to reduce crime without using sworn police officers, for instance through using security guards. (See Hansen Ljöfstrand, 2021, for an analysis of this process as a marketization of policing.) Across European countries, the rate of private security personnel has increased while the rate of public security personnel has remained stable (Wenzelburger & Staff, 2022). In England,

Police Community Support Officers (PCSO) are employed by the police but have few formal powers to make arrests or similar. PCSO hot spot policing appears to work, with a study from Peterborough identifying a 39% crime reduction (Ariel et al., 2016) and a Bedfordshire study of combined police and PCSO patrolling noting a 40% crime reduction (Bland et al., 2021). In a randomized controlled trial of train stations in the South West of England, Ariel et al. (2017) found that directed security guard patrols (more visits and more patrol time) reduced victim-generated crimes by 16% and increased police-generated detections by 49%. It has also been argued that private security guards can complement police authorities and that collaboration between private security guards and police may be desirable (Ariel, 2023). However, there are significantly fewer studies on the impact of private security guards on crime compared to the extensive research on the police. Most of the existing literature on private security guards focuses instead on issues of legitimacy and the dynamics of interactions between security guards and citizens (Brandén, 2023; Mulinari, 2024; Saarikkomäki & Alvesalo-Kuusi, 2020). This body of work often emphasizes the negative effects associated with private security guards and their use as a politicized tool.

There are only a few studies from Sweden on private security guards' effect on crime. In the Swedish city of Örebro, a study found that private security guards patrolling a nightlife district had no impact on crime (Frogner et al., 2013). Additionally, Norén Bretzel (2020) interviewed stakeholders in the Gamlegården neighborhood of Gothenburg about how they perceived the implementation of order guards through a LOV3 area. The respondents perceived that the situation in the region improved after security guards were employed and that the problems had not moved elsewhere. It was also noted that crime in the area had gone down, but this was not formally tested statistically (Norén Bretzel, 2020).

Considering the commonplace and increasing use of security guards to patrol public areas in Sweden, there is a need for more knowledge on whether this can reduce crime. The present study attempts to provide some such knowledge by focusing on three large and recent LOV3 areas in southern Sweden. Contrary to expectations from deterrence theory (Pratt et al., 2008) that enhanced security measures would deter criminal activities, our interrupted time series analyses reveal no significant effect of the policy on reported crimes. The results cast doubt on the official goals of current crime policies and underscore the complexities of designing and implementing effective crime reduction interventions. By examining the policy's effectiveness, this study contributes valuable empirical evidence to the literature on crime prevention and public policy, highlighting that crime reduction may not be readily achieved merely through increased surveillance.

Data and method

Data

We use daily data on reported crimes in locations in the two largest cities in southern Sweden (Malmö and Helsingborg) from March 2020 to November 2022. The locations are Mollevångstorget in Malmö (0.05 km²), Central Helsingborg (0.31 km²), and Southern Helsingborg (0.4 km²). Malmö is Sweden's third largest city, with more than 360,000 inhabitants, while Helsingborg's municipality is the eighth largest, with more than 151,000 inhabitants. The locations in question are central parts of their respective cities and are burdened by recurring crime and disorder problems. In their applications for appointing the locations as a "LOV3-location", the municipalities highlight a range of issues, includ-

ing, for instance, shootings and other forms of violence, thefts, narcotic use and dealing, and citizens' high levels of unsafety at the locations.

We analyze 19 types of crimes, of which 16 are non-police-initiated (e.g., outdoor assault, vandalism) and three are police-initiated (traffic, narcotics, and weapon offenses). The former refers to crimes primarily victim- or witness-generated, and the latter to crimes mainly detected by the police. Distinguishing between the two is important because the increased presence of security guards should affect the crime types differently. Non-police-initiated crimes would likely decrease if the policy is effective, but the effect on police-initiated crimes is unclear. While security guard presence can have a deterring effect on crimes such as drug offences, it can also lead to more offences being detected and thus reported (Ariel et al., 2017). More security guards at a location could in that sense drive up crime rates for crimes that mostly are police-initiated, which makes it important to study the two types of crime separately.

The implementation of security guards varied across the locations. In *Malmö*, the policy was approved on February 2, 2021, and 12 security guards were employed to enable daily patrols of Möllevångstorget starting June 14, 2021. In *Central Helsingborg*, the policy was approved on January 26, 2022, with two security guards patrolling the area Wednesdays to Saturdays starting February 9, 2022, until June 30, 2022, and subsequently from Mondays to Saturdays from July 1, 2022. Finally, four changes were made in *Southern Helsingborg* during the period. After the policy approval on June 26, 2020, two guards started patrolling the location on September 10, 2020, on Thursdays and Fridays until December 31, 2021. From January 1, 2022, patrolling by two security guards increased to include Wednesdays to Saturdays and then increased on August 1, 2022, to include Mondays to Saturdays. At all locations the security guards were meant to patrol the area, maintain order, and be of assistance to residents, visitors and local businesses when possible. The security guards in Malmö received additional training from the municipality with a focus on building relationships in the neighborhood.

Method

We used an interrupted time series (ITS) design. ITS is a quasi-experimental method of studying the effect of an intervention using observational data collected regularly at evenly spaced time intervals. This method allows for the testing and quantification of the impact of an intervention by examining an abrupt change in the intercept and/or trend of the regression line of the outcome under study at the point when the intervention was implemented. An important advantage of this method is its ability to control for a time trend, which can distort the interpretation of the results in other study designs. As a result, the ITS design is associated with a higher degree of internal validity than simple pre-post designs. The ITS design does not require a control group due to the nature of continuously collected time series data. Instead, estimated counterfactual outcomes under a scenario without the regulatory change are based on extrapolating the pre-intervention trend into the post-intervention period (e.g. Lopez Bernal et al., 2017). Specifically, we specified time series regression models of the following form:

$$Y_t = \alpha + \beta_1 t + \beta_2 D_1 + \beta_3 D_2 + \beta_4 D_3 + \beta_5 D_4 + \epsilon_t,$$

where Y_t is the outcome (reported crimes); t is time in days, and $D_1 - 4$ ($1-4$ depending on the number of interventions at each location) are intervention dummies coded as 0 before and 1 after the policy changes. In robustness regressions, the coefficients $\beta_2 - 5$ capture the

intercept changes on the days of the policy changes. In the results presented here, we did not allow for trend changes, only intercept changes; results (available in the replication package, see “Data availability”) are very similar, independent of specification. We use Stata (Version 18; StataCorp LLC, TX).

Results

Figure 1 illustrates the interrupted time series analysis of the LOV3 policy’s impact on reported crimes. The figure shows that the policy had no apparent effect on non-police-initiated crimes (top panel) or police-initiated crimes (bottom panel).

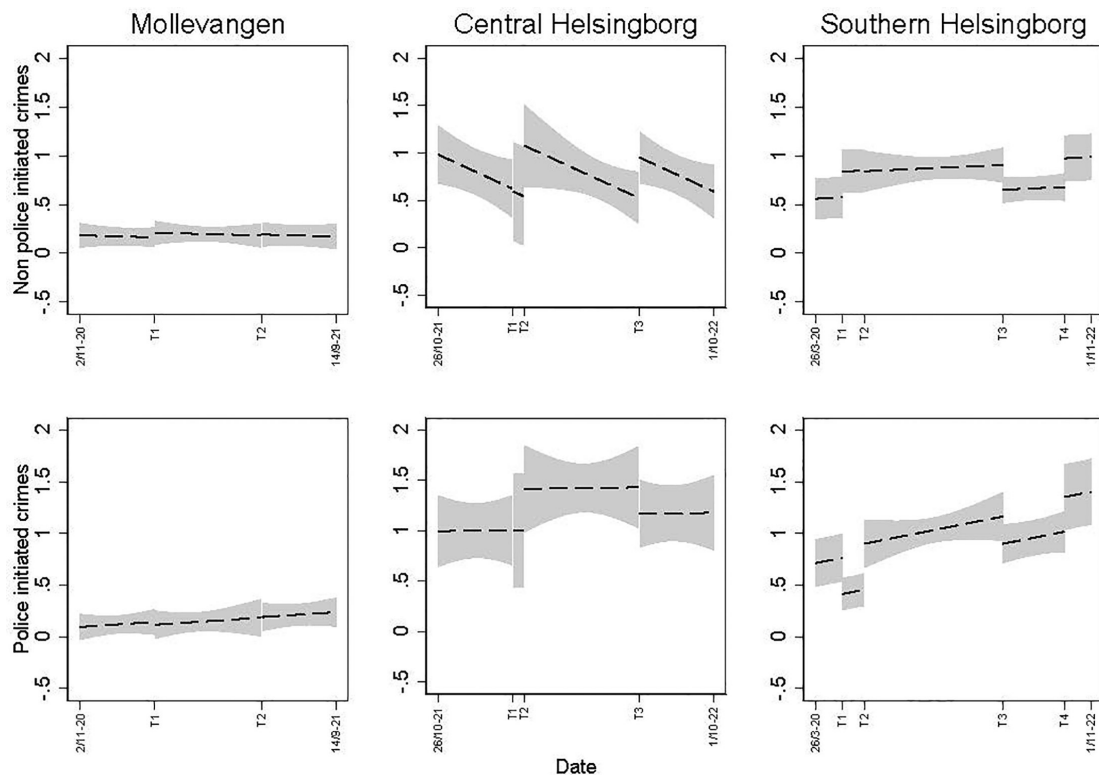


Figure 1 Time series graph illustrating the impact of the policy change on reported crimes in Mollevangstorget, Central Helsingborg, and Southern Helsingborg. *Note.* The top panels are non-police-initiated crimes and the bottom panels are police-initiated crimes. The dashed lines are predictions based on OLS regressions, and the grey areas are the corresponding 95% confidence intervals.

The panels on the left describe the development at Mollevangen. The policy, approved on February 2, 2021, saw security guards deployed starting June 14, 2021. The x-axis represents dates from November 2, 2020, to September 14, 2021, and the y-axis shows reported crimes scaled from -0.5 to 2. The dashed line represents predicted crime rates based on OLS regressions, with shaded grey areas indicating 95% confidence intervals. The figures include three periods: pre-policy approval (before February 2, 2021), post-approval but pre-deployment (February 2 to June 14, 2021), and post-deployment (after June 14, 2021). The relatively flat dashed line indicates no significant change in reported crimes, with consistent confidence intervals suggesting stable crime rates over time.

The panels in the middle represent the development of Central Helsingborg. The policy was approved on January 26, 2022, with security guards employed starting February 9,–

2022, and extended patrolling from July 1, 2022. The x-axis spans from October 26, 2021, to October 1, 2022. The figures show the periods pre-policy approval (before January 26, 2022), initial deployment (from February 9, 2022), and extended deployment (after July 1, 2022). The prediction line shows some variation up and down, but generally, there is no significant change in reported crimes.

The panels on the right represent the development of Southern Helsingborg. The policy was approved on June 26, 2020, with security guards patrolling from September 10, 2020, and increased patrolling starting January 1, 2022, and August 1, 2022. The x-axis covers March 26, 2020, to November 1, 2022, with the y-axis scaled from -0.5 to 2. The figure shows the pre-policy approval period (before June 26, 2020), initial deployment (from September 10, 2020), and increased patrolling periods (after January 1, 2022, and August 1, 2022). The prediction line shows slight variation around policy changes, with positive and negative effect estimates at different time points. For instance, the first policy changes decreased police-initiated crimes while the subsequent changes increased them. Overall, however, the policy has no apparent effect. Clear changes in reported crimes at these three locations generally do not follow decisions to locate security guards at the location or increase their time deployed there. Table 1 presents the estimations from which the results in Figure 1 are estimated.

Table 1 OLS regressions with reported crimes as the outcome

	Mollevangen		Central Helsingborg		Southern Helsingborg	
	(1)	(2)	(3)	(4)	(5)	(6)
Time	-0.000	0.001	-0.004*	0.000	0.000	0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.000)	(0.000)
Treatment 1	0.047	-0.026	-0.026	-0.006	0.265*	-0.351**
	(0.093)	(0.114)	(0.310)	(0.344)	(0.155)	(0.144)
Treatment 2	0.010	0.005	0.536	0.414	-0.007	0.444***
	(0.103)	(0.141)	(0.351)	(0.371)	(0.161)	(0.153)
Treatment 3			0.430*	-0.263	-0.254*	-0.264
			(0.238)	(0.325)	(0.131)	(0.177)
Treatment 4					0.293**	0.335*
					(0.143)	(0.194)
Constant	0.184***	0.095	0.986***	0.995***	0.559***	0.713***
	(0.063)	(0.064)	(0.157)	(0.182)	(0.106)	(0.116)
Obs	317	317	341	341	951	951
R ²	0.001	0.007	0.013	0.017	0.013	0.030

Note. Columns 1, 3, and 5 are non-police-initiated crimes, while columns 2, 4, and 6 are police-initiated crimes. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Discussion

Our investigation into the effects of the LOV3 policy on reported crime rates in designated areas suggests that the initiative did not significantly influence crime levels within the three zones we studied. Deterrence theory posits that a heightened security presence should deter criminal activities (Pratt et al., 2008), and while we generally have no

reason to think otherwise, the present study paints a more complicated picture, with a large uniformed presence not consistently being associated with reduced crime. However, limited to the implementations' effect but not its process, and to only a few treated locations, the current study's scope restricts a comprehensive exploration of the policy's effectiveness across diverse urban environments. An important limitation of our study is that it is a case study of three locations, and the data restricts us so that we cannot rule out the possibility that there are policy effects that we cannot detect. Another limitation is that other events or policy decisions could confound our results. Although our treatment areas have many activities that can impact daily routines and crime, we are not aware of any major events that could serve as confounders during the investigated periods. Furthermore, we cannot know if the tiny effects we find for these three areas hold for other areas or in different periods. A broader examination that includes varied settings such as shopping malls, outdoor squares, and district courts is necessary to better understand the policy's impact. Beyond crime reduction, a broader examination could also entail perspectives such as citizens' fear of crime and perceptions of safety. Such an approach could reveal differing outcomes of the policy's application, providing deeper insights into its differential effects. Future research should extend the investigation to additional regions within Sweden. Expanding the geographical reach of the study would enhance the robustness of the analysis and allow for the identification of more nuanced effects of the policy. This research would contribute valuable perspectives to the ongoing discussion on the efficacy of security measures in crime prevention, ultimately informing policy decisions in similar urban contexts.

Disclosure statement

The authors reported no potential conflict of interest.

Data availability

The data and code used in this study are available from: <https://doi.org/10.7910/DVN/K1PEUE>

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