ORIGINAL ARTICLE

CrossMark

Tracking the Use of Exclusion Zone Orders in Denmark: Individual and Place-Based Crime Trends Before and After 161 Individual Orders

Julie Nielsen[1] · Molly Slothower[2,3] · Lawrence W. Sherman[2,3]

Abstract

Research Question To what extent does citizen-reported crime in 500-meter square areas in Denmark, and arrests of individuals legally excluded from those areas for intimidating behavior, decline in the 3-month time periods in which their Exclusion Zone Orders (EZOs) are in effect, compared to the most recent 3-month period prior to the EZO?

Data Individual-level data on 2441 arrests of the 161 offenders who were the subject of all 218 EZOs granted in Denmark from 2009 through 2016 were collected from the Danish National Police Database, as well as place-based reported crime data for the location of each of the busiest 7 (out of 41) of the Exclusion Zones (EZs) and matched control areas.

Methods The two units of analysis are individuals targeted by the EZOs and location of the EZs. A descriptive time-series analysis compared offending before and individuals received EZOs. Standardized mean differences were calculated between seven locations which received a high rate of EZOs and their matched control sites, all distinguishing police-reported crimes from those reported by victims or witnesses.

Findings A moderate decrease in the number of citizen-reported offenses was observed in six out of seven targeted zones when the EZOs were in effect. There was no indication of a displacement effect to offenses outside the EZ.

Conclusions The positive results of this tracking study suggest that more rigorous testing the use of EZOs across Denmark could demonstrate that EZOs can cause reductions in crime and harm.

Keywords Hot spots · Location bans · Exclusion · Displacement

✉ Lawrence W. Sherman
lawrence.sherman@crim.cam.ac.uk

Extended author information available on the last page of the article

⚛ Springer

Introduction

On June 12, 2009, the Danish government gave police a new legal power to combat gang violence. Without the involvement of a court, police were empowered to issue an "Exclusion Zone Order" (EZO) barring a named individual from a particular area for up to 3 months at a time. Police were granted a limited authority to issue an EZO when they could demonstrate that someone has acted in a particularly intimidating way, creating fear in the specified local area (Justitsministeriet 18th of June 2009). The order can also be renewed after 3 months, if there is still reason to believe that the person would otherwise repeat the unwanted behavior within the exclusion zone (EZ).

The government justified the introduction of the EZO by a series of violent episodes between gang-related groups involving the use of weapons. Some of the shootings led to the tragic injuring and killing of innocent passers-by. According to the Government, this created a situation that threatened everyday community life in the affected areas. As part of this legislation, the EZOs were introduced as means to "*provide the police with an effective tool*" (Justitsministeriet 2009), which also enabled them to conduct stop and frisks in the designated Zones.

Initially, an individual's breach of an EZO could be punished with a fine or imprisonment for up to 6 months. However, in 2013 the Danish government, supported by a majority of the Parliament, led a very visible campaign as a response to the gang conflict escalating once again. Another series of legislative interventions were proposed and adopted, among these an increase of the penalty for breaching the exclusion zone order to a maximum imprisonment of 1.5 years (Justitsministeriet 2013, 2014). By 2017, the maximum exclusion zone size was expanded from 500 up to 1000 meters (Justitsministeriet 2017).

Since 2009, five different Ministers of justice have recommended the use of the EZO as an important and effective tool in what has been named by the media as "the fight against gangs" as well as towards handling crime problems in vulnerable neighborhoods. Despite the consistent policy support, there has been no research of whether the EZO is effective in reducing the criminal behavior of the individuals subjected to them or of lowering the crime level in the affected areas.

By the end of 2016, a total of 218 EZOs had been issued for a period of 3 months, distributed across 161 unique individuals, 3 of whom are women. In Fig. 1, the distribution of EZOs across time shows the number of granted EZOs peaked in 2013 with a total of 64 declined through the end of the study period.

This study provides the first tracking of the use of EZOs in Denmark, with a focus on both individual and place-based crime trends. As an exercise in the "Targeting Testing and Tracking" framework of evidence-based policing (Sherman 2013), this study is only a first step towards rigorous *testing* of whether EZOs reduce crime and harm. The objective of the study is only descriptive *tracking* of what police activity has been associated with these areas and the people targeted, and whether citizen-reported crime has declined while EZOs are in effect. These issues are addressed for both the individual persons subjected to the orders, as well as for the seven areas subjected to the most EZOs. For individuals issued one or more EZOs, the study tracks whether there is an observable decline in their arrests within the EZs as well as elsewhere, during periods in which EZOs are in effect compared to 3 months prior to EZOs being issued. The study also tracks whether the EZOs create any observable displacement effect of victim-reported crime.
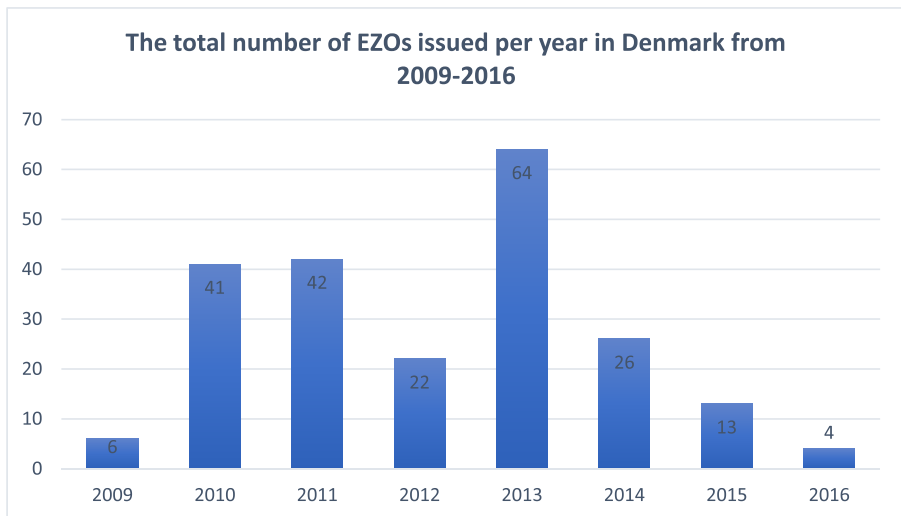
The total number of EZOs issued per year in Denmark from 2009-2016

Year	Value
2009	6
2010	41
2011	42
2012	22
2013	64
2014	26
2015	13
2016	4

Fig. 1 The number of EZOs issued in Denmark, between 2009 and 2016

Research Questions

To what extent does crime in places, or arrests of individuals, decline in the 3-month time periods in which EZOs are in effect compared to the most recent 3-month period prior to the EZO, and is there any evidence of displacement of crime by excluded individuals to other locations?

Data

Two units of analysis form the basis of this study, using two separate datasets. One dataset includes the individuals who have been subjected to one or more EZOs. The other focuses on the locations of the EZs and their control areas.

In order to identify every single EZO ever granted in Denmark, all EZOs were extracted from the national police records management system (POLSAS), and Control numbers double checked in the National Police database. All Control numbers in which more than one EZO was registered under the same number were split up, so that in the end every row matched one EZO and the individual subjected to it, during a period of at least 3 months. Seven Control numbers where the EZO had been granted for less than 3 months were excluded. In addition, 81 Control numbers were excluded because the registration in POLSAS was either incorrect or inadequate. While 218 EZOs have been granted for 3 months across 161 unique offenders, only the first EZO for each offender is included in the analysis.

Individual-Level Data

A search of all charges of the 161 individuals subjected to an EZO was conducted using the POLMAP-Lite system, allowing a geographically defined search for specific crime types by geo-mapping. The search was split into charges of crime committed inside the

EZ and all charges of crime committed anywhere outside the EZ, for each individual's first EZO. Each individual was tracked through their unique social security number (CPR), which enables a search for all police data related to this individual across all police districts of Denmark. The data set created a descriptive analysis of the offending histories over time of all 161 individuals subjected to an EZO. The search included charges of crime committed 12 months prior to each EZO until the end of the 3 months of the EZOs.

Charge information was gathered from 12 months before the first EZO was issued in 2009 until December 31, 2016 for all the individuals subjected to an EZO in all police districts across Denmark. This allowed the analyses to include EZOs expiring at the end of March 2017. The data was then sorted into charges per month relative to the issuance date of each offender's initial EZO.

The final individual-level dataset includes a total of 2441 criminal cases, in which individuals subjected to one or more EZOs have been charged with crimes committed inside or outside an EZ within a period of 12 months prior to the issuance date and the 3 months during the EZO.[1] In all, 41 EZs were identified across Denmark, in which one or more EZOs were granted between 2009 and 2017.

Place-Based Data

Among the 41 EZ locations identified with *any* EZOs, a subset of only 7 EZs was identified in which *multiple* EZOs (three or more orders) had been issued for the exact same period of time in the same location. A dataset of all reported crimes committed inside these seven locations was constructed with the aid of the POLMAP-Lite system. In all, a total of 27 individual EZOs were issued for the seven intervention study EZ areas included in this study's place-based tracking analysis.

For the place-based tracking data, the independent (*output*) variable is the aggregated police activity measures associated with several EZOs, measured as the time between issuance and expiry in the seven targeted locations. The dependent (*outcome*) variable is the number of citizen-reported crimes per month that occurred within the EZ for 3 months before and 3 months during the EZOs.

The final place-based dataset includes the same crime categories of citizen-reported and police-reported crimes as the individual-level dataset.

Recipients' Share of Crime in EZs In addition to the dataset just described, a similar dataset was constructed based on charges instead of reported crime. The purpose of this dataset was to calculate the percentage of charges in the EZs that the individuals subjected to the EZOs were responsible for. The reasoning behind this analysis is to see if these individuals account for a noticeable share of the total crime in the area.

Intervention and Control Area Matching The study identified comparable control areas with similar population characteristics and type to match each respective EZO location, but where no EZOs were granted in the period of measurement.

---

[1] The exclusion of Controls due to the incorrect registrations of Controls in the National Police Database means that the number of EZOs reported and included in this study may not correspond with the official numbers.

Five out of the seven selected EZO areas are located within areas on the government's controversial, but officially designated, so-called Ghetto list[2] of marginalized residential areas, as displayed in Table 1. These "listed" areas roughly correspond with the boundaries of the EZs, although the size of these five EZs does not correspond exactly with the "officially" marginalized areas displayed in the table. Some of the EZs make up only part of the marginalized residential areas (as is the case with Bøgeparken and Hothers plads) while others comprise a somewhat larger area (such as Gadehavegårdsvej). Table 2 provides descriptive characteristics of the EZs, such as the population density and the sociodemographic factors.

Prior to any analysis of crime in this study, EZs were matched by qualitative scanning with their closest corresponding Control Area (CA). This method of matching helps ensure a relatively high degree of similarity between the treatment and control areas in regard to the land use and characteristics of the people living or spending time in those locations.

The remaining two EZs are characterized as, respectively, a busy central train station (Banestrøget in Tåstrup) in the outskirts of the capital of Copenhagen and a busy night club area (Skolegade in Esbjerg) in the fifth largest city of Denmark. These areas are matched with similar types of areas, the main station in the second largest city Aarhus and the night clubbing area of the fourth largest city Aalborg. The crime data of the control areas were then extracted in the same way as in the EZs, with the aid of POLMAP-Lite, enabling a geographically specific search, including an area of precisely 500 meters in radius. All intervention and control areas have the exact same geographic size.

Tracking Police-Reported Crime as an Output Variable of the EZOs

Not all crimes are suited to measure crime trends because some of them are heavily dependent on police presence to be reported. These offense types are sometimes referred to as police-initiated, because they are usually reported by police officers themselves on random or targeted patrol as well as responding to specific calls for service (Sherman and Weisburd 1995). This applies to most reports on drug dealing, possession of arms, threatening an officer, public disorder offenses, and more. Because the level of police-reported crimes is affected by the level of police presence, changes in the level of police-reported crimes cannot be used as an indicator of whether the EZOs have any impact on the crime levels of individual offenders or locations. If measuring changes in the level of crimes such as drug and weapon offenses over time, the level of police presence could act as a spurious variable, affecting the level of reported crimes in one or the other direction (De Vaus 2001).

Instead, this study uses police-reported crimes as an output variable, indicative of police presence. However, a rise in police-reported crime can be related to both increased police presence as well as a number of other factors, such as a rise in the number of motivated offenders or in the number of suitable targets. With this in mind, the police-reported crimes are examined as a crude measure of police presence and enforcement outputs.

---

[2] This is, in fact, the current official name of the annual list published by the Government. The name and the criteria for being on the list are publicly debated every year on the grounds of stigmatizing language.

Table 1 the final list of intervention and control areas included in the study

Exclusion zone/intervention area	Name	Control area	Name
Banestrøget	EZ1	Banegårdspladsen	CA1
Gadehavegårdsvej	EZ2	Skovparken	CA2
Karlemosevej	EZ3	Æblehaven	CA3
Hothers Plads	EZ4	Finlandsvej 2	CA4
Mjølnerparken	EZ5	Finlandsvej 1	CA5
Bøgeparken	EZ6	Gudrunsvej	CA6
Skolegade	EZ7	Jomfru Ane Gade	CA7

Measuring Citizen-Reported Crime Trends as Outcomes Across Time and Locations

The indicator of the outcome crime level is crime reported by citizens, typically victims, their relatives, or witnesses. For the purpose of measuring crime trends committed by individuals and in locations subjected to the EZOs, these citizen-reported crimes were separated from the police-reported crimes. The citizen-reported category primarily consists of theft offenses, burglary, robbery, bribery, fraud, sexual offenses, vandalism, threats, violence, and homicides and will be measured and tested as the dependent variable.

Targeted Individual Arrests and Time at Risk A crucial element of the individual analysis is taking into account that some of those individuals subjected to the EZOs might be in custody for shorter or longer periods of time before and during the EZO. In his time-series analysis on gang injunctions, Carr et al. (2017)

Table 2 Characteristics of the treatment and control areas

Name of EZ or control area (CA)	Population size	Employed outside local labor market	Immigrants first- or second generation	Convicted	Primary school education only	Below mean income
	(1000+) 2016	> 40% 2014–2015	> 50% 2016	> 2.7% 2014–2015	> 50% 2016	< 55% 2014
EZ2: Gadehavegård	2153	42.0	57.3	2.5	59.6	56.1
CA2: Skovparken, Kolding	2540	41.8	62.6	2.4	57.0	61.2
EZ3: Karlemose Parken	1488	40.7	52.7	1.9	53.5	66.6
CA3: Æblehaven, Roskilde	1494	41.3	43.8	3.3	54.4	63.5
EZ4 and 5: Mjølnerparken (Hothers Plads)	1790	43.9	82.6	2.6	53.7	51.3
CA4 and 5: Finlandsparken, Vejle	1635	43.9	71.1	2.0	56.1	64.1
EZ6: Vollsmose (Bøgeparken)	9238	51.2	68.0	3.2	57.8	57.1
CA6: Gjellerupparken, Aarhus	6097	52.0	80.8	4.0	59.2	55.7

discovered that the cohort included in his analysis on average spent 6% of their time in custody before the gang injunction, whereas they spent an average of 14% of their time in custody after the injunctions. A failure to adjust for time spent in custody could then lead to an under-estimation of reoffending and lead to the wrong conclusions (Ferante et al. 2009; Carr et al. 2017). On this basis, the average days at risk per month has therefore been calculated and compared for the included period of time before issuance as well as beyond. However, in contrast with Carr et al. (2017), the data in this study shows no difference in time spent in custody before and after the intervention. When calculating the time at liberty for the individuals included in the study cases, they have 26.1 days per month at risk before and 26.4 at risk during the 3-month order. As a sensitivity analysis, the individual-level analysis was carried through, both with and without taking account of days at risk, displaying the same results either way.

Methods

The tracking analysis of police outputs includes several elements. The first is a longitudinal study of the whole population of individuals subjected to an EZO since police were empowered to issue such orders. Next, the presence of the police in locations associated with the EZOs is explored through a graph displaying the average number of police-reported crimes per month, committed by the EZO subjects for a period of up to 12 months prior to the issuance and during the EZOs. Breaches of the EZOs are examined as a separate category of police-reported crimes to estimate police presence during the period in which the EZOs are in effect. The police-reported crimes primarily include possessing and dealing drugs, most traffic offenses, possession of arms, threats against police officers, and public disorder.

Finally, to answer the main crime outcome question, several before and after analyses are reported. These compute the standardized mean difference in crime across the two time periods of before and after issuance of the EZO for any possible pattern of change in arrests for citizen-reported crime associated with the issuance of EZOs. For that purpose, the effect size of Cohen's d and confidence intervals are calculated to test the null-hypothesis of no difference between charges before and after the EZO in the individual-level data analysis (Hinton 2004). In the place-based analysis, the treatment and control areas are compared through a visual analysis of the difference between each of the paired EZs and Controls, comparing citizen-reported crimes 3 months prior to the issuance with the 3 months during the EZOs.

The design of this study does not allow any statements about causality between the EZOs and citizen-reported crime trends, since it will not be possible to rule out all rival explanations (Hagan 2014). Generally, a rigorous method such as a randomized controlled trial is needed to enable such conclusive statements (Ruane 2005). However, the descriptive time-series analysis and the difference-in-differences analysis give some indication of the relationship between the EZOs and changes in the crime level over time, particularly if these comparisons are repeated in a range of different locations and contexts and produce similar results.

Findings

Table 3 shows that some police districts have issued no EZOs (South Sealand and Midwest Jutland) while Copenhagen has issued almost half of the total number of EZOs. As the center of gang-related conflicts, Copenhagen has a large number of EZOs. More than half of the EZOs issued in Copenhagen (53) are located in the particular street of Jægersborggade, a street publicly known as a headquarters for gang-related drug dealing. For reasons unknown to the authors, the Police of Copenhagen suddenly stopped granting EZOs in 2014. Since then, the district of Copenhagen has only granted two in 2015 and not any at all in 2016. This decrease in EZOs issued by the police in Copenhagen is the main explanation for the total drop in the issuance of EZOs. Table 3 also illustrates that only a few police districts have used the EZO repeatedly over the years. Most of the districts have little experience in issuing EZOs.

Table 4 provides an overview of the reasons given by police Case Workers for issuing EZOs. In 61 cases, the case worker has not stated why the EZO should be issued; the text field only refers to the duration and location. In the remaining cases, one or more reasons are given. On the basis of these reasons, each issuance is categorized the descriptive analysis of reasons. In many cases, the case worker argued that the EZO should be issued because of both organized and gang-related drug dealing as well as intimidating behavior. In such cases all three reasons are registered, which is why the total sum exceeds the total number of EZOs in the previous table.

Some individuals have been subjected to several EZOs. As displayed in Table 5, the vast majority of individuals (129 out of 161) have only been subjected to one. However, 19 have been the subject of two and 13 have been subjected to more than two EZOs. The mean age of the recipients are 21.6 years, with a standard deviation of 5.6. The youngest is 15 and the oldest is 54 years of age.

Table 3 Distribution of EZOs across police districts between 2009 and 2016

Police district	2009	2010	2011	2012	2013	2014	2015	2016	Total
Bornholm						1	1		2
Fuhnen			3	8	17	4	2		34
Copenhagen	6	29	37	7	22		2		103
Midwest Sealand					16	1			17
West Sealand					3	18	5		26
North Sealand		12			6				18
South Sealand									0
South Jutland			2					3	5
Southeast Jutland				7					7
East Jutland							3	1	4
North Jutland						2			2
Midwest Jutland									0
Total	6	41	42	22	64	26	13	4	218

Table 4 Reasons given by police case workers for EZO issuance (not mutually exclusive)

Police district	Drug dealing	Intimidating behavior	Gang related	No reason
Bornholm	–	2	–	–
Fuhnen	–	34	–	–
Copenhagen	10	10	53	39
Midwest Sealand	–	14	17	–
West Sealand	7	22	4	
North Sealand	–	–	–	14
South Jutland	–	2	3	2
Southeast Jutland	–	7	–	–
East Jutland	–	–	–	4
North Jutland	–	–	–	2
Total	17	91	77	61

Recipients' Share of Charges in the Exclusion Zones

The following analysis indicates that in most EZs the recipients are present and criminally active during the 3 months prior to issuance. The share of charges in an EZ attributable to individuals who received an EZO varies as displayed in Table 6. On average, the recipients of the EZOs account for 15.7% of the total charges in the areas they are excluded from. In EZ1, the five recipients account for a total of 37% of all charges, and 43.3% of the police-reported crimes prior to their exclusion. However, in EZ5, the same number of individuals only account for a total of 2.6% of all charges in the EZ.

On average, the calculation of the recipient's total share of charges indicates that if successful in excluding these individuals from at least some of the EZs, the EZOs could have an observable impact on the crime level in those areas.

Place-Based Analysis of Police-Reported Crime as Outputs

Figure 2 illustrates the 12-month trend in the number of crimes reported by the police per month in the EZs compared to the control areas. The graph displays how there have been mixed trends over time in both treatment and control areas. Within the last 4 months before the issuance, the number of police-reported crimes in the EZs increases by 108%, exceeding the level of police-reported crime in the controls. The last 2 months prior to the EZO issuance, the number of offenses increases in both the treatment and control areas and then decreases immediately after EZO issuance. It then rises again in both groups of areas. These trends suggest that whatever effect the EZOs may have

Table 5 The distribution of EZOs by individuals

Number of EZOs	Subjected to 8	Subjected to 5	Subjected to 4	Subjected to 3	Subjected to 2	Subjected to 1	Total
No. individuals	1	2	3	7	19	129	161

Table 6 The EZO recipients share of total number of charges in the EZs, 3 months prior to the issuances

Location	Number of recipients in EZ	Recipients share of citizen-reported crime in %	Recipients share of police-reported crime in %	Recipients total share of charges in %
EZ1	5	30.0	43.3	37.0
EZ2	6	19.0	13.0	14.8
EZ3	4	2.6	28.6	10.8
EZ4	3	4.5	8.4	7.1
EZ5	5	2.5	2.7	2.6
EZ6	3	41.7	22.2	30.0
EZ7	3	9.2	5.2	7.4
Mean	4	15.6	17.6	15.7
Standard deviation	1.2	14.2	13.5	11.9

had, it is not from a spurious relationship with an increase in police presence and proactive enforcement.

Table 7 shows how the relative difference in four out of seven locations indicates a greater reduction of police-reported crimes in the EZs compared to the Controls. The average change in percent from 3 months before to 3 months after the issuance of the EZOs, when all of the seven treatment and control groups are pooled, indicates 31% fewer offenses in the EZs compared to the Controls.

Individual Breaches of EZ Orders A total of 60 recipients out of 161 have been caught breaching an EZO for a total of 297 times. This means that 37% of all the recipients
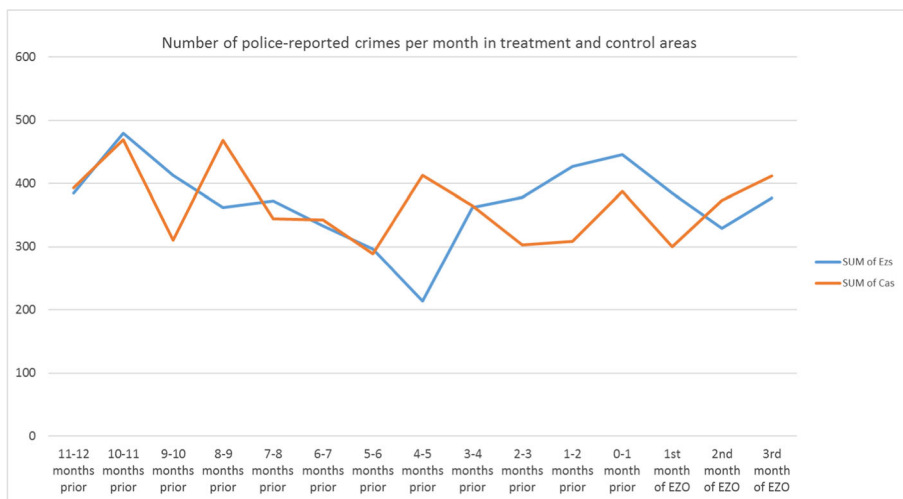

Fig. 2 The number of crimes reported by the police in all the EZs and Controls, 12 months prior to the issuance of EZOs and 3 months during EZOs

Table 7 Relative change in mean number of police-reported crimes, before and after issuance of EZO in intervention (EZ) and control areas (CA)

	Average offenses per month before (standard deviation in brackets)	Average offenses per month after (standard deviation in brackets)	Percentage difference between before and after	Absolute difference in relative percent change for EZs vs. Controls
EZ1	38 (18)	17 (6)	− 55%	− 42%
CA1	88 (16)	77 (9)	− 13%	
EZ2	40 (7)	28 (7)	− 30%	− 26%
CA2	25 (16)	24 (15)	− 4%	
EZ3	14 (4)	11 (6)	− 21%	− 203%
CA3	17 (5)	48 (20)	+ 182%	
EZ4	140 (20)	107 (6)	− 24%	− 12%
CA4	17 (4)	15 (7)	− 12%	
EZ5	102 (39)	81 (2)	− 21%	+ 32%
CA5	15 (1)	7 (2)	− 53%	
EZ6	27 (20)	43 (5)	+ 59%	+ 15%
CA6	34 (8)	49 (5)	+ 44%	
EZ7	62 (16)	77 (13)	+ 24%	+ 20%
CA7	136 (31)	141 (27)	+ 4%	

have breached one or more EZOs. Figure 3 below shows how the average number of breaches per day decreases during the 3-month time period of the EZO.

Figure 3 reflects the fact that 59.3% of the breaches happen within the first month, while 24.5% happen in the second month, and 16.3% of the breaches occur in the third month of the EZOs. Just about 1/3 of the recipients have breached an EZO once and 10 have breached twice. Four recipients have been caught breaching more than 20 times. One individual has been caught breaching 27 times - 13 times within the first month of the EZO, 8 times the second and 6 times in the third month. Thus the majority (63%) of the recipients are not caught breaching at all, while the number of breaches decline within the 3-month time period of the EZO.

For the remaining sections of the analysis, these breaches are excluded from the calculations and graphs.

Individual Time-Series Analysis of Police-Reported Crime

Figure 4 illustrates the changes over time of police-reported crimes committed by the EZ recipients. All the calculations in this section were also conducted taking account of days at risk, showing the same results. Figure 4 displays the average charges of recipients per month inside the EZs. It shows an increase in police-reported offenses starting 4 months prior to the EZ issuance. The last month *before* issuance, there is a total of 165 police-reported offenses, whereas the first month of the EZO only has a total of 92 offenses inside the EZ. There is an immediate decrease after the issuance inside the EZs. The next section illustrates the changes over time of police-reported crimes. All the calculations in this section were also conducted taking account of days
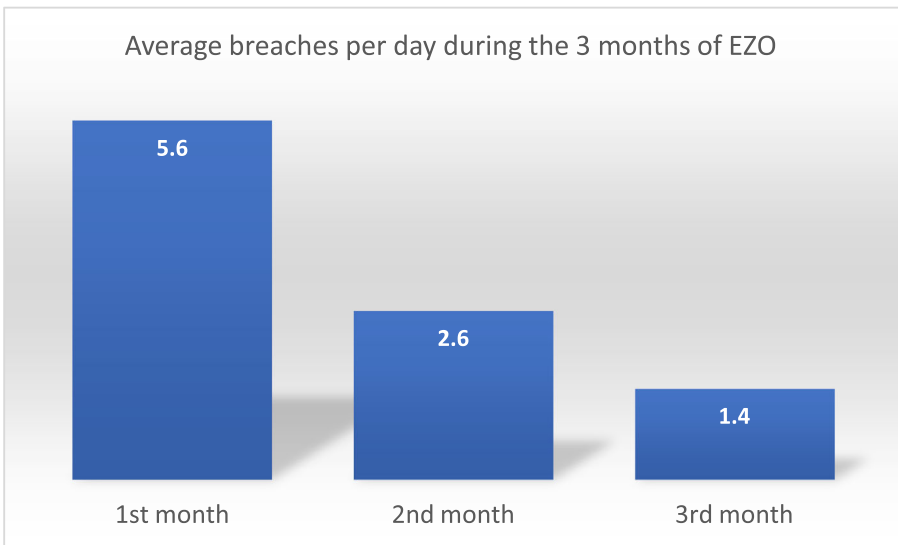
Fig. 3 The distribution of breaches during the 3 months of the EZO

at risk, showing the same results. Figure 5 displays the average charges of recipients per month inside the EZs. It shows an increase in police-reported offenses starting 4 months prior to the EZ issuance. The last month *before* issuance, there is a total of 165 police-reported offenses, whereas the first month of the EZO only has a total of 92 offenses inside the EZ. There is an immediate decrease after the issuance inside the EZs.
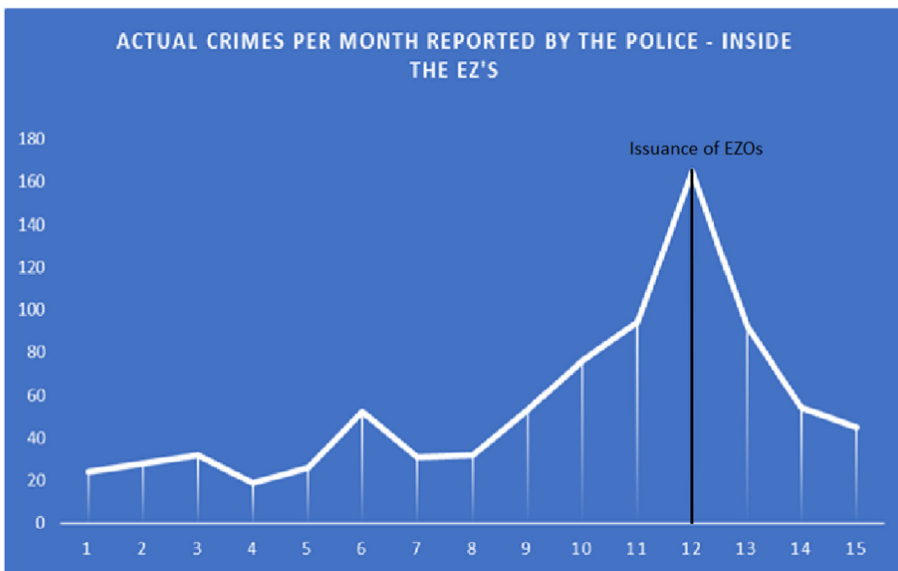
Fig. 4 Average charges of police-reported crimes per month against EZO subjects inside the EZs (Total crime reports $N = 823$)
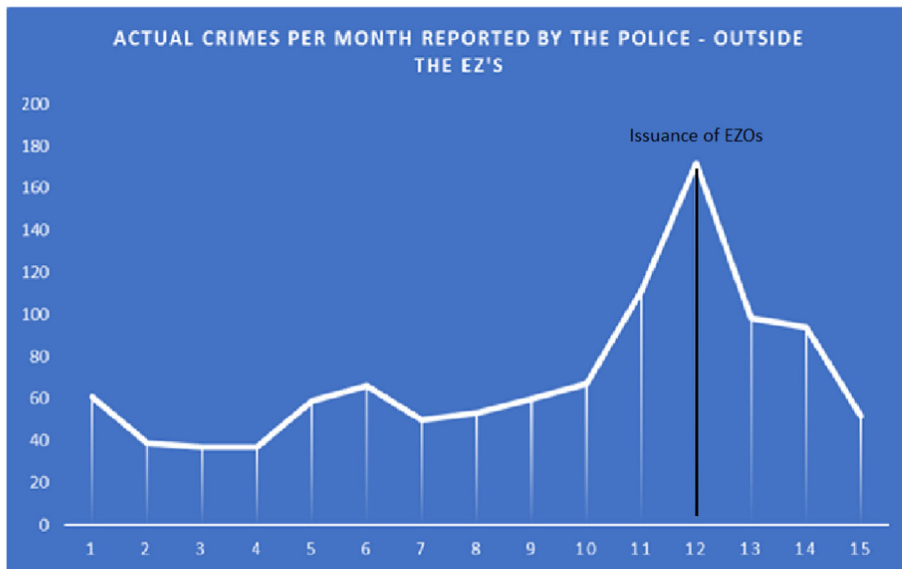
Fig. 5 Average charges of police-reported crimes per month by EZO subjects outside the EZs before and after EZO is issued (Total $N = 1056$)

Table 8 displays the calculation of the effect size of the difference between the number of charges 3 months before compared to 3 months after the issuance inside the EZs, indicates a small effect of d = − 0.236 (CI = − 0.392; − 0.079).

Figure 5 displays the average charges of EZO recipients per month anywhere outside the EZs. It shows an increase in police-reported offenses prior to the issuance similar to the trend inside the EZs, indicating that the police might be targeting these offenders regardless of location until they impose the EZO, or—in reverse—may decide to impose the EZO because of all the crimes they detect these individuals committing.

Table 9 displays the results of calculating the effect size of the difference between the number of charges filed against the EZO subjects for crime *outside* the EZO during the 3 months before, compared to 3 months after, issuance of the EZOs. These findings indicates a small effect of d = − 0.241 (CI = − 0.397; − 0.084). The findings provide some evidence against a displacement hypothesis.

Table 8 Difference in police-reported crimes, 3 months before and the 3 months during the EZO inside the EZs

	Mean	Standard deviation	N	Effect size	Confidence interval (95%)	
Before	0.71	1.74	316	d = − 0.236	− 0.392	− 0.079
After	0.40	0.66				

Table 9 Difference in the level of police-reported crimes, 3 months before and 3 months during the EZO outside the EZs

	Mean	Standard deviation	N	Effect size	Confidence interval (95%)	
Before	0.738	1.087	316	d = − 0.241	− 0.397	− 0.084
After	0.515	0.731	316			

Tracking Outcome Analysis of Citizen-Reported Crime Inside the EZOs

The last sections of the results focus on the citizen-reported crimes as a measure of the crime level in areas, because measuring these offenses is not dependent on police presence or arrest actions against individuals.

The question is whether a decline in reported crime can be observed, within the 3-month time periods in which EZOs are in effect compared to the most recent 3-month period in which they were not in effect for those places.

In order to establish whether the seven target EZs have a greater change in the crime level after the issuance, the relative change in percent in the EZs compared to the seven control areas is calculated. Table 10 displays a calculation of the relative changes in offenses when comparing the treatment and control areas. The calculation shows whether the reduction in citizen-reported offenses is bigger in the treatment areas compared to the control areas. In six out of seven Controls, the calculation of the relative change in percent, of crimes reported by citizens, indicates a greater reduction

Table 10 Relative change in mean number of citizen-reported crimes, before and after issuance of EZO in treatment (EZ) and control areas (CA)

	Average offenses per month before EZO (SD in brackets)	Average offenses per month after EZO (SD in brackets)	Relative percentage difference between 3 months before and 3 months after EZO	Absolute difference of relative percent change in offense counts in EZs compared to Controls
EZ1	49 (14)	49 (10)	0%	− 2%
CA1	234 (40)	239 (34)	+ 2%	
EZ2	51 (4)	38 (7)	− 25%	− 73%
CA2	42 (27)	62 (17)	+ 48%	
EZ3	55 (5)	112 (45)	+ 104%	114%
CA3	29 (10)	26 (5)	− 10%	
EZ4	222 (35)	181 (45)	− 18%	− 21%
CA4	31 (19)	32 (2)	+ 3%	
EZ5	200 (37)	212 (55)	− 100%	− 129%
CA5	24 (7)	31 (9)	+ 29%	
EZ6	41 (18)	47 (7)	+ 15%	− 1%
CA6	69 (15)	80 (14)	+ 16%	
EZ7	90 (9)	110 (13)	+ 22%	− 5%
CA7	183 (12)	232 (81)	+ 27%	

of crimes in the EZs compared to the Controls. The average decrease in the EZs compared to the Controls is 17%.

Conclusions

In the place-based analysis of the citizen-reported crimes, there is a relative percentage reduction in crime in six out of seven comparisons. The average number of citizen-reported crimes is reduced by 17% when comparing the level of crime 3 months prior to the 3-month time period during the EZOs.

Furthermore, the observation that 37% of the recipients are caught breaching in an EZ once or more during the 3 months they are excluded indicates that the police are present during the 3-month time period of the EZOs. This supports the claim that the reduction in police-reported crimes is not simply due to a sudden absence of police in the EZs after issuance of the EZOs.

This study finds no indications of displacement to any other areas in Denmark. In contrast, there seem to be indications of a positive diffusion, by which citizen-reported crime levels decrease inside the EZs, while police-reported crimes for the people subject to EZOs go down both outside and inside the EZOs. This finding is consistent with other studies concluding that diffusion of crime benefits is generally more likely than crime displacement to accompany a reduction in target area crime (Weisburd et al. 2016; Telep et al. 2014) even study displacement and diffusion in large geographic units, such as all 12 police districts of Denmark, and conclude that displacement of crime only occurs in a small proportion of the included studies, and that diffusion is just as likely. The Campbell systematic review on hot spots-policing by Braga et al. (2012) concludes that focusing police efforts on high-activity crime places does not inevitably lead to crime displacement, and crime control benefits may just as well diffuse into the areas immediately surrounding the targeted locations (Braga et al. 2012).

Summary The findings indicate that the EZOs are followed by reduced crime in the 500-meter square areas targeted as EZs, though it may look as if the police need to enforce their power through giving breaches before the full effect of the EZOs comes into place. The rapid decline in the relatively large proportion of recipients caught breaching within the first month may indicate that the perceived certainty of apprehension for breach could increase after an initial arrest.

Limitations and Policy Implications

The evidence in this study is generally supportive of the continued use of EZOs. Nonetheless, the study has two major limitations. One limitation is that the study lacks truly like-for-like control groups, in which EZO use is compared to not using it under similar circumstances and in similar areas. One example of such a design is a matched pair random assignment protocol.

A more important limitation is the lack of a crime harm index to translate the crime counts into indicators of the total seriousness of harm from the crimes that EZOs may

prevent. Since the powers were given to police for managing severe harm spikes, it is appropriate to examine their effect on high-harm crimes, rather than counting crimes as if they are all created equal in harm (Sherman et al. 2016). Even a replication of this study converting the counting of citizen-reported crimes in the seven high-EZO target areas into the Danish Crime Harm Index (Andersen & Muller-Johnson 2018) would be an important further contribution.

More rigorous testing with more sensitive, harm-weighted outcome measures is needed to make stronger conclusions regarding a causal effect of EZOs on different types of crime—and whether the visible presence of the police is essential to ensure the observed decrease in crime. The tracking analysis indicates that the EZOs reduce citizen-reported crimes, such as burglary, theft, violence, and sexual offenses. While testing with a stronger (randomized) assignment of control areas is needed to make stronger claims, this study provides moderately strong tracking evidence that continued use of EZOs can help to control sudden spikes in serious crime by known offenders in small areas. Other countries considering such legislation can combine this evidence with the even larger tracking effects of gang injunction orders in Merseyside (Carr et al. 2017) to consider legislating similar grants of limited police authority, as the renowned police scholar Herman Goldstein (1977) suggested decades ago.

Publisher's Note   Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.

References

Andersen, H. A., & Muller-Johnson, K. (2018). *Cambridge Journal of Evidence-Based Policing, 2*, 52–69.

Braga, A. A., Papachristos, A., & Hureau, D. (2012). Hot spots policing effects on crime. *Campbell Systematic Reviews, 8*, 1–96.

Card, D., & Krueger, A. B. (1994). Minimum wages and employment: a case study of the fast-food industry in New Jersey and Pennsylvania. *Am Econ Rev, 84*(4), 772–791.

Carr, R., Slothower, M., & Parkinson, J. (2017). Do gang injunctions reduce violent crime? Four tests in Merseyside, UK. *Cambridge Journal of Evidence-Based Policing, 1*(4), 195–210.

De Vaus, D. A. (2001). *Research design in social research*. SAGE Publications.

Ferante, A., et al. (2009). Assessing the impact of time spent in custody and mortality on the estimation of recidivism. *Curr Issues Crim Just, 21*(2), 273–287.

Goldstein, H. (1977). *Policing a free society*. Cambridge: Ballinger.

Hagan, F. E. (2014). The experimental model and its variations. In *Chapter three in Research methods in criminal justice and criminology*. Allyn and Bacon.

Hinton, P. R. (2004). *Statistics explained* (2nd ed.). London: Routledge.

Justitsministeriet (2009) Retrieved on the Internet: 21-04-2017: http://jm.schultzboghandel.dk/upload/microsites/jm/ebooks/andre_publ/thk40131.pdf.

Justitsministeriet (2013). *Ny rocker-bandepakke: Fast greb om banderne*. Retrieved from the Internet: 2nd of October 2017: http://www.justitsministeriet.dk/nyt-og-presse/pressemeddelelser/2013/ny-rocker-og-bandepakke-fast-greb-om-banderne.

Justitsministeriet (2014). Bekendtgørelse om politiets sikring af den offentlige orden og beskyttelse af enkeltpersoners og den offentlige sikkerhed mv., *samt politiets adgang til at iværksætte midlertidige foranstaltninger*, §6, stk. 4, Retrieved on the Internettet 21-04-2017: https://www.retsinformation.dk/Forms/R0710.aspx?id=163624.

Justitsministeriet (2017). Bekendtgørelse om politiets sikring af den offentlige orden og beskyttelse af enkeltpersoners og den offentlige sikkerhed mv., *samt politiets adgang til at iværksætte midlertidige foranstaltninger*, §6, stk. 4, Fundet på Internettet 21-04-2017: https://www.retsinformation.dk/Forms/R0710.aspx?id=188528.

Ruane, J. M. (2005). Some perfectly valid points: measurement, internal, and external validity. In *Chapter three in Essentials of research methods. A guide to social science research*. Blackwell Publishing.

Sherman, L. W. (2013). The rise of evidence-based policing: targeting, testing, and tracking. *Crime Justice, 42*, 377–451.

Sherman, L., & Weisburd, D. (1995). General deterrent effects of police patrol in crime hot spots: a randomized controlled trial. *Justice Q, 12*, 625–648.

Sherman, L., Neyroud, P. W., & Neyroud, E. (2016). The Cambridge Crime Harm Index: measuring total harm from crime based on sentencing guidelines. *Policing: A Journal of Policy and Practice, 10*(3), 171–183.

Telep, C. W., Weisburd, D., Gill, C. E., Teichman, D., & Vitter, Z. (2014). Displacement of crime and diffusion of crime control benefits in large-scale geographic areas: a systematic review. *J Exp Criminol, 10*(4), 515–548.

Weisburd, D., Eck, J., Braga, A., Telep, C., Cave, B., Bowers, K., . . . Yang, S. (2016). Frontmatter. In *Place matters: criminology for the twenty-first century* (pp. I–IV). Cambridge: Cambridge University Press.

Affiliations

Julie Nielsen[1] · Molly Slothower[2,3] · Lawrence W. Sherman[2,3]

[1]   Danish Police, Copenhagen, Denmark

[2]   University of Cambridge, Cambridge, UK

[3]   University of Maryland, College Park, MD, USA