Neighborhood Labeling and Criminal Justice Decisions*

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Abstract

In 2015, the Swedish National Police unexpectedly began publishing their internal list used to identify and monitor "troubled neighborhoods." The release of this list, and its subsequent revisions, played a pivotal role in shaping a public discourse that portrayed these neighborhoods as "no-go zones," influencing global perceptions of crime in Swedish suburbs. The report was not accompanied by any immediate policy response. Using multiple data sources, we verify that the release of the list did not lead to significant changes in law-enforcement intensity. We also show that the report triggered a surge in negative coverage of the listed neighborhoods. We then adopt an RDD approach to estimate the causal effect of neighborhood listing on criminal-justice decisions, comparing defendants from listed neighborhoods who were already awaiting trial with those who had just completed theirs. The results show that sentence length increased by more than 60 percent for defendants who were sentenced after the list release. The effect is immediate and fades out as media coverage decreases. This pattern is consistent with models of decision-making under limited attention, in which courts may overemphasize widely publicized events even when they are unrelated to the case at hand. Courts in which relatively few judges have personal experience of living in these neighborhoods exhibited the greatest increase. We also assess potential long-term costs, showing that treated defendants experience a 16-percentage-point decline in employment over the three years following their sentence.

Keywords: Neighborhood effects; Stigma; Crime; Sentencing disparities; Discrimination
JEL: K42; R23; J15

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1. Introduction

Most major cities around the world contain neighborhoods characterized by high levels of crime, poverty, and segregation. Because of their structural disadvantages, these neighborhoods tend to acquire negative reputations and are often portrayed unfavorably in the media (Besbris et al., 2015). A longstanding concern is that such "place-based stigma" leads to disparate treatment by society, ultimately harming residents' life outcomes (e.g., Aaronson et al., 2021; Zenou and Boccard, 2000; Yamagishi and Sato, 2025; Wacquant, 2008; Sampson, 2012). Identifying behavioral responses to place-based stigma is, however, empirically difficult. The key challenge is isolating the causal effect of a neighborhood's reputation from the underlying characteristics that produced that reputation in the first place. As a result, there is little causal evidence on whether—and how—place-based stigma translates into disparate outcomes.

This paper examines whether neighborhood reputation affects sentencing decisions in criminal courts, which are fundamental institutions whose judgments help shape societal perceptions of fairness. Judicial decision-making occurs in settings characterized by substantial information complexity, time pressure, and incomplete case information. Under such conditions, decision-makers may place disproportionate weight on salient or readily observable cues, allowing reputational signals to influence outcomes even when formally irrelevant (Bordalo et al. 2012, 2015). These mechanisms are likely to be particularly consequential for defendants from stigmatized neighborhoods, where entrenched stereotypes and persistent negative media portrayals may magnify the role of such cues (Tversky and Kahneman, 1974).

The context of our study is Sweden, where the police in December 2015 unexpectedly started publishing a formerly internal situation report listing so called "troubled neighborhoods" (*utsatta områden* in Swedish).[1] The list had been used by the police to identify and track the situation in these neighborhoods suffering from long-run disadvantage. The list of neighborhoods and its publicly available supplement of maps has since been updated biannually on specific dates and without any prenotification of additions to or removals from the report, which originally included 55 neighborhoods. Its release and subsequent amendments have received considerable public attention,

[1]The goal in making the list public was to increase transparency in local policing and regain some of the trust that had been lost in 2013 when a media scandal over police ethnic profiling of entire Roma families (including newborns and toddlers) broke out in Sweden (`https://www.europarl.europa.eu/doceo/document/E-7-2013-011825$_$EN.html?`).

with the included neighborhoods being described as "no-go" zones, often highlighted on maps in media reporting. This series of events also strongly contributed to shaping international perceptions of crime in Swedish suburbs,[2] and narratives from these outlets have been heavily disseminated through international "alternative media" sources.[3] Yet, unlike most place-based interventions, the situation report was not initially followed up by any immediate policy changes, such as increased police resources, or even the creation of action plans (National Audit Office, 2020).

To identify the causal effect of neighborhood listing on sentencing disparities, we implement an RDD approach that exploits the precise timing of the police lists and focuses on defendants who had already been scheduled for trial within a narrow 10-week window around the date of the release or update of the report. This design allows us to isolate the behavioral response to the list while abstracting from underlying crime rates or other neighborhood characteristics, as well as any potential place-based interventions. Our analysis uses rich, population-based data containing geocoded information on neighborhoods in the police report and defendants' neighborhoods of residence.

To begin, we document that the release of the situation report produced a negative salience shock concerning the listed neighborhoods. To this end, we first classified the universe of media articles written in Swedish mentioning these neighborhoods since February 2015 based on their tone (positive vs. negative) and compare it to a sample of articles mentioning non-listed neighborhoods. We find a significant increase in negative media coverage—both in the number of articles and in the negativity of their tone—about listed neighborhoods relative to non-listed control neighborhoods. This observation aligns with numerous reports affirming that this event played a pivotal role in establishing a narrative of "no-go zones in the Swedish media landscape," potentially contributing to the stigmatization of these places (Backvall, 2019; National Audit Office, 2020). We further demonstrate that the list was compiled based on persistent traits rather than as a reaction to abrupt changes in social characteristics, that it was not followed up by any policy interventions or changes in neighborhood resources. The combination of a negative salience shock affecting the listed neighborhoods, the stability of their underlying characteristics, and the absence of public

[2]See e.g., articles in Newsweek or the Wall Street Journal: `https://www.newsweek.com/sweden-police-vulnerable-areas-no-go-zones-628029` (Engel Rasmussen, 2023).

[3]See e.g., `https://www.breitbart.com/europe/2017/06/29/sweden-police-commissioner-begs-help-no-go-zones-rises/?`.

interventions supports interpreting this event as an exogenous shift in stigma that may have confirmed and heightened the court's perception of the problems associated with these places.

We then show that, while there is no significant effect on the probability of conviction for individuals residing in listed neighborhoods, the average sentence length increases significantly by more than 60 percent (0.52 log points) for defendants sentenced just after the release of the list. The effect is immediate and fades out in tandem with the media coverage decreasing. Moreover, the results show that the increase in sentence length is significantly smaller in courts where a higher share of judges have had personal experience living in listed neighborhoods. Overall, these patterns are consistent with models of decision-making under limited attention, in which courts may overemphasize recent or widely publicized events even when they are unrelated to the case at hand (Bordalo et al., 2015). We further find that ethnic minorities are affected to the same extent as native Swedes residing in these neighborhoods, indicating that neighborhood reputation affects residents irrespective of ethnic background. In addition, we assess the potential long-term costs of these potentially biased decisions, showing that defendants from these neighborhoods experience a 16-percentage-point decline in employment over the three years following their sentence.

The results in this paper contribute to several strands of literature. First, we add to a large body of work documenting the causal link between neighborhoods and individuals' outcomes. Although this literature has made substantial progress in identifying causal place effects, neighborhood characteristics are deeply intertwined, making it difficult to isolate specific mechanisms even when research designs account for sorting on unobservables (Kling et al., 2007; Chetty et al., 2014; Chyn and Katz, 2021). Identifying the role of place-based stigma is particularly challenging because even quasi-randomly assigned place-based interventions typically convey both an intervention and an informational signal to the public that these locations require special attention. While a handful of studies have investigated place-based stigma in the context of redlining and credit access (Aaronson et al. 2021, 2023; Fishback et al. 2020), redrawn zoning maps for subsidized neighborhoods (Garrouste and Lafourcade, 2022), or the depreciation of housing values (Koster and van Ommeren, 2022; Besbris et al., 2015), the policy environment we study operated through salience rather than through changes in resources, incentives, or local services also connected to listed neighborhoods.[4]

[4]Using data from a large Italian bank, and controlling for the non-random selection of firing litigation of employees due to misconduct, Ichino et al. (2003) show that judges are less likely to decide in favor of firms when unemloyment

Together with our research design, this policy feature allows us to provide novel evidence that place-based stigma constitutes a potentially important, yet previously underappreciated, channel through which neighborhoods shape life outcomes.

Related to this, we also contribute to the literature documenting "excess sentencing" in the criminal justice system (Topaz et al., 2023). This literature shows that location-based sentencing differences persist even after controlling for crime severity and criminal history, ruling out crime levels alone as a sufficient explanation.[5] Possible mechanisms include judges relying on group-level risk beliefs when faced with imperfect information; these beliefs can vary by district, for instance depending on local experience or media narratives (Arnold et al., 2022). Some districts may also be harsher because they happen to have more punitive judges (Kling, 2006; Abrams et al., 2012a), judges respond to local administrative pressure through rules and incentives (Yang, 2017), as well as district differences in ideology and electoral accountability (Ash et al., 2020; Anwar et al., 2019; Huber and Gordon, 2004; Berdejó and Yuchtman, 2013). A growing literature also documents persistent district-level sentencing differences that remain stable over time and across personnel changes, consistent with locally stable institutional equilibria rather than contemporaneous crime conditions (Abrams et al., 2012a; Rehavi and Starr, 2014).[6] In addition to these channels, our findings suggest that neighborhood reputation constitutes a potentially important mechanism underlying excess sentencing. A central concern in the literature is that such place-based sentencing differences may reinforce local economic inequalities, since courts adjudicate cases arising from their own jurisdictions (Finlay et al., 2023; Chetty and Hendren, 2018). Importantly, our results confirm the worry that this bias also carries a long-term penalty in the labor market.

is high.

[5] Large geographic disparities in criminal justice exposure exist even within the same legal system. In Chicago, neighborhood incarceration rates range from under 500 to over 4,000 per 100,000 residents (Sampson and Loeffler, 2010). In Florida, the average felony prison sentence in 2020 was 21 months in Miami-Dade County and 45 months in Okaloosa County (Commission, 2020). Although some variation reflects spatial differences in crime, substantial disparities remain after accounting for crime and case characteristics. In Chicago, some neighborhoods commit residents to prison at more than three times the rate of other neighborhoods with comparable crime levels (Sampson and Loeffler, 2010). Even in Sweden, a comparatively egalitarian context, prison admission rates vary sharply across neighborhoods. Areas in the 99th percentile face a 10-percentage-point likelihood of incarceration for a given year, compared with only 0.08 percentage points in neighborhoods around the 50th percentile.

[6] A large literature documents the effects of incarceration on individuals' subsequent outcomes in the United States (Mueller-Smith, 2015; Agan et al., 2023; Garin et al., 2023; Ouss et al., 2023), Norway (Bhuller et al., 2020), and Sweden (Dobbie et al., 2018a; Grenet et al., 2024). Related work using random-judge designs includes Kling (2006) on sentence length, Aizer and Doyle (2015) on juvenile incarceration, and Dobbie et al. (2018b) on pre-trial detention.

Finally, we contribute to a small but growing literature demonstrating that salient events can influence decision-making within the criminal justice system. Several studies, such as Hanemaaijer et al. (2024), McConnell and Rasul (2021a, 2024), and (Grosjean et al., 2023), focus on how heightened media attention affects minority groups. Others examine how increased crime salience in France (Philippe and Ouss, 2018) or heightened terrorism in Israel (Shayo and Zussman, 2011) shapes judicial or law-enforcement behavior. In contrast to this literature, we highlight the importance of an event that increased the salience of certain neighborhoods, rather than that of an ethnic or religious minority group. Our results suggest that this event affected all residents within these neighborhoods uniformly, indicating that place-based stigma operates above and beyond ethnic stigma. This particular finding also informs the ongoing debate over whether "race" or "space" is the primary driver of persistent disadvantage among minority populations—a question that has proven difficult to answer because minorities tend to sort into particular residential areas that are themselves associated with disadvantaged outcomes (Zenou and Boccard, 2000).[7][8]

The remainder of the paper is structured as follows. Section 2 provides institutional background for the Swedish context. Section 3 describes the data and empirical strategy. Section 4 presents the estimation results, and Section 5 concludes.

2. Background

This section discusses relevant institutional details regarding the list of troubled neighborhoods. We also present design-based evidence supporting the view that inclusion on the list is associated with permanent disadvantage rather than changes in neighborhood characteristics, and we provide empirical evidence of the surge in media coverage following the publication of the report. Additionally, we present both qualitative and quantitative evidence showing that the report did not trigger any immediate policy response.

[7]Zenou and Boccard present a theoretical model demonstrating that both racial identity and spatial location matter for explaining high unemployment rates among Black workers.

[8]A large literature documents racial and ethnic discrimination within the criminal justice system (Ahrsjö et al., 2024; Arnold et al., 2022; Abrams et al., 2012b; Jordan et al., 2023; Hanemaaijer et al., 2024; Tebes and Fagan, 2022; Yang, 2015; Goncalves and Mello, 2021; Ang, 2021; Bindler et al., 2024; Agan, 2024).

2.1 The Introduction of the Police List

In December 2015, the Swedish police unexpectedly released an internal situation report listing "troubled neighborhoods," which was used to identify and monitor these named neighborhoods. The reason for making the list public seems to have been to enhance transparency regarding police operations, which had been debated since 2013 when a scandal of police ethnic profiling in one Swedish police area broke out.[9]

The list was compiled by combining objective statistics with careful assessment by the local police areas (NOA, 2015). Statistical criteria include poor socioeconomic conditions indicated by high unemployment and poverty (Gerell et al., 2022), but no specific criteria was publicly specified. As shown in Appendix Figure A1, there is as expected a large and stable gap over time in the socioeconomic characteristics of listed neighborhoods compared to non-listed urban neighborhoods. Importantly, these unadjusted trends display no discernible shifts in the pre-period. This suggests that listing was based on persistent gaps in neighborhood characteristics rather than sudden changes in these. In addition to these social factors, the police authority involved the local police districts in qualitative assessments on whether the neighborhoods had organized crime, parallel social structures, religious radicalization, and residential trust in institutions. The National Operative Unit (NOA) of the police authority then compiled the list into a situation report detailing all identified "troubled neighborhoods." The list has been updated and republished biannually since 2015 (NOA, 2017; NOA, 2019; NOA, 2021; NOA, 2023). A "troubled neighborhood" is then defined as a geographically delimited entity throughout these reports, and supplemental maps are included in the report's appendix (and accompanied by publicly available shapefiles).

The initial round of the list comprised 55 neighborhoods. Table 1 illustrates all the listed neighborhoods and the timing of their inclusion. It is evident that the majority of the neighborhoods initially listed remain on subsequent revisions, with only three neighborhoods removed from the list. Additionally, we observe the inclusion of 9 more neighborhoods in later rounds. Overall, there appears to be only limited changes in the composition of neighborhoods over time. This is again

[9]The media revealed that the local police in the county of Skåne had been maintaining an internal database on Roma people that included non-offenders. After the disclosure of this ethnic profiling the police authority of the county of Skåne lost a court case against the Civil Right Defenders (a Swedish extension of the Helsinki committee) in the Stockholm District Court and was heavily criticized by the European Union Comissioner of Human Rights and the Swedish Equality Ombudsman. See https://www.dn.se/nyheter/sverige/detta-har-hant-1/

consistent with the list being based on persistent traits of neighborhood disadvantage.

Appendix A provides further details on how these areas compare to other neighborhoods in Sweden, and to other disadvantaged areas in Europe and the US.

2.2 Media Portrayal

The police list, which has been widely debated both in national and international media, has played a crucial role in shaping a media discourse of Swedish so called, "no-go zones."[10] Since its introduction, major media outlets have consistently covered the listed neighborhoods, often making them the focal point of news coverage (Backvall, 2019).[11]

Figure 1 shows results of textual analysis of mentions of "troubled" neighborhoods in news articles in Swedish media. The analysis uses data from a media monitoring project called Global Database of Events, Language, and Tone (GDELT), which records instances of events based on articles from a comprehensive, global set of news feeds and is gaining increasing traction in economics (Campante and Yanagizawa-Drott, 2017). The content of each article is processed by the GDELT algorithm using text analysis and machine-learning methods to identify salient characteristics, such as event location, date of the event, and the tone-of-content of the article.[12] The data covers all media articles published online since February 2015. It is clear from the figure that the number of articles discussing troubled neighborhoods starts to increase just after the release of the first

[10]For instance, in an article entitled "Are There No-Go Zones in Sweden? Police Identify Dozens of 'Vulnerable Areas' Rife With Criminality", Newsweek discusses the Swedish police list of troubled neighborhoods and the reference to Sweden as a hotbed of criminality and radicalization made by President Donald Trump in his alarmist "Look what's happening in Sweden last night...." exclamation (`https://www.newsweek.com/sweden-police-vulnerable-areas-no-go-zones-628029`). The list has also been highlighted many times in major national media across Europe. See Appendix A.1 for examples of headlines.

[11]The police list and its pertinent dichotomy of neighborhoods into those that are troubled and those that are not has also worked as a convenient instrument for the political discourse of societal problems (Grönqvist et al., 2023). As Mack (2023) summarizes her results on the discourse of the Swedish listed neighborhoods, "Politicians have persistently positioned them as perilous places that never joined the present. [...] This history of the recent past focuses on how the "blame" for the problems of modernist urbanism – especially around perceived dangers – has shifted from buildings to people to a politically convenient combination of the two, or what I label "hereafters." I contend that discourses of "unfinished" and "dangerous" places with "criminal" residents have made modernist urbanism a perfect target for xenophobic political discourse, where buildings and landscapes have become scapegoats for less socially acceptable feelings and concerns."

[12]We use GDELT version 2.0 which debuted on February 19, 2015. It collects and translates in real time the content for a massive inventory of global media and decodifies the tone-of-content of each article based on sentiment analysis into a scale from negative to positive. See `www.gdeltproject.org` for a detailed description of the GDELT Project and its methodology.

situation report in December 2015. The release of the report and each subsequent revision (shown in dashed red vertical lines) coincides with clear spikes in media coverage. There is also a spike in June 2018 (shown in dotted red vertical line) when the police called to a press conference to present statistics on the general trends in socioeconomic disadvantage in listed neighborhoods but no list update took place. As a point of reference, the figure also plots the number of articles mentioning "crime." We can see that the spikes in the mentions of troubled areas do not track the trend in articles discussing crime. This suggests that the media interest is not tied to incidences of crime in these neighborhoods.

Appendix Figure A.2 depicts the relative number of Google searches for the terms "troubled neighborhoods." Clear spikes in searches for "troubled neighborhoods" are evident during the months when the list is updated. After these events, the relative number of searches for troubled neighborhoods even exceeds that of very common phrases like "best restaurants."[13] We interpret this as evidence of not only substantial media attention surrounding the report, but also significant public interest.

Capitalizing on the fact that GDELT provides for all articles published in online media a measure of tone, Figure 2 plots the average tone of the articles in Swedish media by calendar year and month for neighborhoods included in the Police List in 2015 and a random sample of equal size of non-listed neighborhoods. The algorithm first tallies the number of words with a negative tone and those with a positive tone, then creates an index by weighting each article according to its size and rescale it into an index ranging from –100 to 100. Negative values indicate a more negative tone in the article. To enhance interpretability, we rescale this index by reversing its sign and organizing it into percentiles. Higher values of this so called "negativity index" indicate a more negative tone in the article. Figure 2 reveals that listed neighborhoods are portrayed more negatively than non-listed neighborhoods. Furthermore, the gap in negativity between these groups substantially widens following the release of the police reports. In terms of magnitude, by 2019, the average negativity index for non-listed neighborhoods was near the 10th percentile, while for listed neighborhoods, it was close to the 60th percentile. This indicates that the gap in negative portrayal grew from a statistically insignificant 10 percentile difference pre-policy to a 50 percentile difference

[13]Since Google Trends changed the algorithm in 2015, we are not able to consistently measure searches before 2016.

by the end of 2019.

In summary, the significant public attention surrounding the listed neighborhoods, combined with their consistently negative portrayal in the media, likely contributed to their stigmatization (Backvall 2019; National Audit Office 2020). This salience may be particularly pronounced for highly educated judges, who are more likely to follow media reporting and to acquire knowledge more quickly than those with lower education (Tichenor et al., 1970).

2.3 (No) Responses to the Introduction of the Police List

Qualitative Evidence. Ample evidence from public documents indicates that the police did little to respond to the high-profile report with appropriate interventions or additional resources in the listed neighborhoods. An internal report by the police from June 2018 states that "even though the work [by the police] in troubled neighborhoods has high priority, no persistent improvements in manpower is perceived by workers in the field and that no surge [of police presence] has occurred in the troubled neighborhoods" (p. 10) (Hultqvist and Ygge, 2018). Further, the National Audit Office (NAO) reviewed the police work in troubled neighborhoods in 2020 and questioned whether any increases in local patrols had been directed to troubled neighborhoods (National Audit Office, 2020).[14]

Municipalities could address social issues and crime prevention in various ways, for instance, by contracting private security firms in certain areas. Our interviews with officers at the Swedish National Council for Crime Prevention (BRÅ) suggest that cooperation between local police and other community actors has improved gradually over the past two decades. However, the list of troubled neighborhoods did not align with any specific programs for increased inter-agency collaboration. According to a BRÅ report (BRÅ, 2018), the Police Authority began providing additional guidance on community policing to local police from 2015 onward. The report evaluates the implementation of community contracts through four case studies, in which local police, in

[14]An example of an intervention is the allocation of more foot patrol teams. According to Mission Investigate (in Swedish, Uppdrag granskning), the leading Swedish TV programme for investigative journalism, the police allocated a foot patrol team of two policemen on Tuesdays between 9 am and 3 pm to a troubled neighborhood in the city of Borlänge as a response to the release of the situation report in 2015. In 2017 the patrol shift was shortened to Tuesdays between 12 pm and 3 pm). Further, according to the same program, as of 2021, only three of the 28 municipalities had come up with any action plan for coming to terms with the social issues in troubled neighborhoods.

partnership with other local authorities, commit to a unique set of goals. BRÅ found little to no impact of these community contracts on police practices. The report cites low prioritization of community policing by local police districts and weak collaboration between police and other local actors as key factors behind this limited effect.

Quantitative Evidence. To empirically support the qualitative evidence indicating a lack of significant policy responses, at least during the first three rounds of the report, we utilize matched employer-employee data covering the entire Swedish population. Using this data, we link police staff to their geocoded workplaces. For the police station nearest to each neighborhood, we calculate the average headcount, contracted hours, and the proportion of those working overtime.[15] For each of these three variables, we estimate a staggered difference-in-differences model of the effect of neighborhood listing on police resources using similar non-listed neighborhoods in urban areas as controls. Details on the estimation strategy are presented in Appendix B.

The results in Figure 3 results show that the introduction of the list did not lead to any significant changes in manpower, average weekly contracted hours, or the share of police staff working overtime in the police stations closest to listed neighborhoods. Most of the point estimates are close to zero and relatively precisely estimated. In Appendix Figure B.1 we show that the results for police staff hold when only considering police officers, or alternatively considering private security guards.

Taken together, both the qualitative and quantitative evidence suggest that the introduction of the list does not seem to have led to any meaningful immediate changes in police resources.

One potential concern is that, abstracting from any changes in the size of police staff, police forces might have engaged in tougher law enforcement due to the release of the report, for example more frequent stop-and-frisking of young men. To confirm that no such surges occurred, Figure 4 compares trends in conviction rates in listed neighborhoods and non-listed urban neighborhoods. The first thing to note is that trends in crime are remarkably parallel before the first release of the situation report in 2015. This provides additional evidence that inclusion of neighborhoods on the list does not stem from sudden changes in neighborhood characteristics. Second, we find no discernible changes in the of conviction rate gap during the complete period of observation

[15]The results are similar also when using the two closest police stations and weighting by the inverse distance.

(2010–2021). Reassuringly, serious offenses defined as prison sentence, which are thought to capture organized gang crime, are remarkably stable over time in both types of neighborhoods.

2.4 The Swedish Criminal Justice System

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The criminal court system in Sweden consists of three levels: the district court, the court of appeals, and the supreme court. The vast majority of criminal cases are settled at the district court level, where each district court is generally responsible for all cases originating in its jurisdiction. In this paper, we focus on criminal cases tried in any of the 48 district courts in Sweden. The largest district courts are located in large cities, such as Stockholm and Göteborg, and have up to 40 judges, whereas the smallest courts are located in more rural areas.

District courts in Sweden are usually divided into divisions (*avdelningar*) and then sections (*rotlar*), although some small courts are only divided into sections. Each section consists of one judge, one clerk, and several administrative personnel. At the beginning of each year, courts typically assign judges to their sections. Although judges may be reassigned to different sections in subsequent years, many remain in charge of the same section for several years. District court judges are appointed for the duration of their career and can only lose their jobs if they are convicted of a serious crime. Before their appointment, district court judges must have a law degree, but they are not required to have prior experience working in the court system. Each district court also maintains a large pool of politically appointed lay jurors (*nämndemän*) that serve a similar function as juries in the American system. Each lay juror works approximately 10 to 15 days per year, with essentially random assignment of lay jurors to both cases and judges (Ahrsjö et al., 2024).[17]

In most district court trials, both the verdict and sentence are decided by both the judge and the three lay jurors. Information on the place of residence of the defendant is provided at several places in the documents available to the court. This includes the criminal investigation, as well

[16] This section draws heavily on Dobbie et al. (2018a)

[17] Lay jurors indicate their availability for different fixed dates in the upcoming year (e.g., the first Monday of each month). Lay jurors are then assigned specific dates in advance of any knowledge about the cases to be tried on those dates. For each date, a court administrator then forms juror triplets, with some attempt to balance gender, age, and political party. Finally, the court coordinator assigns each juror triplet to a courtroom scheduled to be in session in a quasi-random fashion. See Anwar et al. (2019) and Ahrsjö et al. (2024) for additional details on lay jurors in Sweden.

as potential statements from social services.[18] Following the hearing, the judge summarises the facts of the case and any relevant laws for the three lay jurors. The judge and the three lay jurors then discuss possible decisions, including the verdict and sentence. If the judge and the lay jurors disagree on the verdict, a vote is held to determine the outcome of the case. The votes of the judge and lay jurors have equal weight, but the judge holds the tiebreaker if there is no clear majority. If a defendant is found guilty, there is a second vote to determine the sentence, with the least severe option chosen if there is an even split between different sentencing options. If the severity ranking of the different options is unclear, then the judge holds the tiebreaker.

Appendix Figure C.1 provides additional details on how suspected crimes are processed in Sweden's criminal justice system using information from cases in 2004. If there is suspicion of a crime, the police or a prosecutor begins a preliminary investigation. The prosecutor then decides whether the individual should be charged with a crime and whether the case should proceed to a court trial.[19] Of these charged cases, 77% result in a court trial. The other charged cases are typically settled without a trial, usually because the suspect has confessed to a petty crime. A small number of charged crimes result in no prosecution, trial, or penalty, usually because the defendant is under 18 years of age. Once a case proceeds to trial, it is assigned to a judge in the relevant court. Of these court trials, 23% result in incarceration, 37% in a fine, 24% in probation, 10% in other types of punishment such as community service, and, in only 6% of the cases, the defendant is found not guilty. Our empirical strategy measures the impact of parental incarceration compared to a weighted average of these other forms of punishment, including acquittal.[20]

[18]While the postal adress is typically stated, the street names are often well-known to the courts. Also the zip-code area is typically stated with the explicit name of the residential area.

[19]Individuals arrested for a crime carrying a prison term of one or more years can be detained before trial if there is a risk that they will flee prosecution, obstruct the investigation, or commit a new crime. The initial decision on whether or not to detain an individual prior to trial is generally made by a randomly assigned judge at the time of the arrest. The assignment processes for the judge making the pretrial decision and the judge presiding over the trial are completely independent, meaning that the same judge will only be assigned to the pretrial and trial stages by chance.

[20]Sentence length, fine amounts, and probation length are all determined by the seriousness of the crime, with fine amounts also depending on taxable income. Individuals on probation (*skyddstillsyn*) are generally required to be in regular contact with a surveillance officer for the duration of their sentence. Individuals on probation may also be required to undergo regular drug tests, take courses in impulse control or psychiatric care, or even be institutionalised for additional treatment. Individuals with three-month or less prison sentences can also ask the prison and probation authorities to serve their sentence at home under electronic supervision. The decision of whether an individual can serve their sentence at home is made solely by the prison and probation authorities and does not depend on the assigned judge.

Incarceration Rates:

The criminal justice system in Sweden is broadly similar to many other developed countries, particularly other Nordic countries. Appendix Figure C.2 plots incarceration rates per 100,000 individuals in Sweden, the United States, and all remaining European and North American OECD countries. The incarceration rate in Sweden has been relatively stable over the last 35 years, increasing from 55 individuals per 100,000 in 1980 to 79 in 2006, then falling back to 61 in 2014. Incarceration rates in other OECD countries have followed a largely similar trend over time, albeit from a slightly higher base. In stark contrast, incarceration rates in the United States increased from 220 individuals per 100,000 in 1980 to 693 by 2014. Sentence lengths are also considerably shorter in Sweden compared to the United States with more than 80% of sentences being shorter than one year.

3. Data and empirical method

This section discusses the various data sources used in the empirical analysis. It also outlines the research design used to estimate the causal effect of the introduction of the list on criminal justice decisions.

3.1 Micro data

This study draws upon detailed administrative data covering all sentencing decisions in Swedish courts ("Tingsrätter") during 2007–2021. These data are merged with other administrative registers at Statistics Sweden using annonymized identifiers. Consequently, the dataset includes sociodemographic characteristics of the working-age population (ages 15–74) for all years, enabling us to characterize pre-determined demographic characteristics of all defendants and their geocoded residential locations and, therefore, to their neighborhood of residence in any given year.[21] Online Appendix contains relevant information on the cleaning and coding of the variables used in our analysis. This section summarises the most relevant information from the Appendix.

Information on criminal behaviour and court cases comes from data provided by the Swedish

[21]These registers are: Population register, Multigenerational register, School register, LISA register, Conviction register, and Suspicion register.

National Council for Crime Prevention (see `www.bra.se`). The crime data include information on all court cases between 1985 and 2021, including cases that did not end in a conviction. We observe the date of the crime, the date of conviction, the type of crime committed, the sentence imposed by the court, whether there are any co-offenders, and unique identifiers for district courts, and defendants.

We merge these data with information on family linkages contained in the multi-generation register created and maintained by Statistics Sweden (Statistics Sweden 2017).[22] We then merge these datasets with the LISA register provided by Statistics Sweden. LISA contains rich longitudinal data that include outcomes for every Swedish resident at least 16 years old. For each year, the data contain information on the families' entire formal sector earnings and transfers from the tax registers.

We restrict the estimation sample to defendants sentenced in a 10 week window around the week of the release and update of the report. Our main outcome variables are defined as the probability of being convicted, the probability of being sentenced to prison, and (log) sentence length in days.

3.2 Classifying Listed Neighborhoods

To identify listed neighborhoods, we use geocoded maps from the Swedish police authority.[23]

Figure 5a shows the geographical distribution of the police drawn maps in Stockholm municipality and Figure 5b zooms in on one of these neighborhoods in Stockholm. It is clear from Figure 5a that listed neighborhoods are located in the suburban areas of Stockholm city, and this applies in general. As all listed neighborhoods are located in urban municipalities, for our empirical analysis we restrict our sample to defendants in urban municipalities.[24]

Ideally, we would be able to exactly identify if a defendant lives inside the areas identified as

[22]The multi-generation register contains the personal identification numbers for all individuals born in Sweden starting in 1932 who were still alive in 1947 when the system was introduced, along with the personal identification numbers of each individual's parents and children.

[23]The police authority publishes online geocoded maps of the listed neighborhoods as shapefiles for citizens and the media to freely download.

[24]We follow (BRÅ, 2018) and define urban municipalities as ones with at least one town larger than 10,000 individuals and whose share of dense neighborhoods (with at least 200 dwellings within a maximum distance of 200 meters) is above the national average.

a "troubled neighborhood." However, the administrative data only provide their neighborhood of residence, or Regional statistical neighborhood units (hereafter, RegSO for the Swedish term Regionala statistikområden). There are approximately 3,360 RegSOs in Sweden, and each has between 650 and 23,000 inhabitants. Moreover, the police-drawn maps typically exclude commercial properties, highways, and green areas, and do not perfectly align with RegSO boundaries. We can see in Figure 5b that the police drawn map (shown in pink) overlaps relatively well with the RegSOs (boundaries shown in solid black lines), with the exception of some green area.

To overcome these limitations, we identify a neighborhood (RegSO) as being on the list if its area has an overlap of 30% or more with a "troubled neighborhood" area.[25] Consequently, we define an individual as living in a "troubled neighborhood" if its neighborhood of residence (RegSO) was identified as being on the list. There are 92 neighborhoods (RegSO) with 16,582 defendants within the boundaries of the listed troubled neighborhoods as drawn by the police.

3.3 Identification strategy

Our research design leverages the distinct timing of the police lists labeling neighborhoods as "troubled" and combine this with individuals already waiting to face trial or those who have just done so to get at the causal effect of a negative neighborhood stigma shock on sentencing disparities. To do so, we employ a regression discontinuity design (RDD), where sentencing week is the running variable (RV) plotted against sentencing outcomes around the time of the list releases.

In the main analysis, we estimate the following conventional RDD specification around the defendants facing trial right before or just after the inclusion of its neighborhood in the police list of troubled neighborhoods:

$$\text{Outcome}_i = \gamma + RV + RV \times Listed_i + \beta Listed_i + \lambda X_i + \epsilon_i, \tag{1}$$

[25] In the robustness analysis, we show that the results are insensitive to using alternative cutoffs. We also gauge the feasibility of this operationalization of exposure in Appendix Table D.1 by comparing descriptive statistics for age structure and employment of listed neighborhoods based on the 50 percent overlap rule with the definition used by Statistics Sweden based on exact individual-level coordinates of residence and geocoded delimitations of listed neighborhoods provided by the police. The latter descriptives are only available for year 2018 (Statistics Sweden , SCB). Reassuringly, our definition of listed neighborhoods shows on average almost exactly the same age structure and employment shares (by gender) as does the precise treatment definition of Statistics Sweden.

where Outcome$_i$ is the outcome of interest. The model assumes that the unobserved characteristics of the defendants are similar for listed versus non-listed defendants. We expect the effect of neighborhood labeling to manifest directly after the publication of the situation report, in which one's own neighborhood was listed, i.e., in a neighborhood that was listed in 2015–2021.

Initially, we fixate the bandwidth to ± 10 weeks to keep the comparison tight within the individual list releases, but we also show that the results are unchanged for longer choices of bandwidth (e.g., 20 weeks). Since there is some mismeasurement in treatment/control status precisely around and just after the cutoff, due to potential two-week lag for some cases between the court date and sentencing decision date (which is the RV we use), we run donut specifications at baseline where we exclude the cutoff week and the next week (t_0 and t_1). The RDD estimation employs triangular kernel weights, and the pre/post-trends are captured using linear fitted lines. Moreover, we ensure that no same individual is included in the sample as being or will be sentenced at multiple instances in the same list year.

We previously showed that the introduction of the list did not coincide with changes in underlying crime levels or the socioeconomic and demographic characteristics of listed and non-listed neighborhoods. We also presented both qualitative and quantitative evidence indicating that there was no immediate policy response associated with the introduction of the list. While reassuring, the research design we adopt also accounts for these possibilities. However, due to some concerns regarding the seasonality of the list releases (they are generally released close to the summer and winter break), there may be risk of cases with certain characteristics showing up before and after the cutoff even though the list releases did not impact the judicial system before sentencing at the time. In order to alleviate such seasonality concerns, we also present results employing propensity score 1-1 nearest neighbor matching (with replacement) based on a range of background characteristics and case crime type for each case. These results will ensure that the RDD comparison around the cutoff is not affected by any imbalance driven by case seasonality in case characteristics.

i) Descriptive statistics

Table 2 presents descriptive statistics for individuals sentenced just before and just after the week of the release or update of the report, pooling all years 2015, 2017, 2019 and 2021. This table

shows that criminals in listed and non-listed neighborhoods are similar in terms of the sex ratio, employment rate, and their propensity to be convicted or sentenced to prison, but that individuals in listed neighborhoods tend to be younger, more urban, less educated, and are much more likely to have a foreign background. The composition of individuals around the running variable cutoff is also shown to be stable for both listed and non-listed neighborhoods. However, average sentence length is shown to increase starkly just after the cutoff for those in listed neighborhoods, while it is only increasing modestly for individuals residing in non-listed neighborhoods.

ii) Validating the design

The research design relies on the assumption that cases processed just before and just after the cutoff are similar in terms of background characteristics. Table 3 presents results supporting this assumption. The estimates are obtained from regressions similar to Model 1, but using pre-determined characteristics as the dependent variable. None of the estimates are statistically significant.

The design further assumes that courts should not be able to sort cases just above or below the cutoff. Because the release and updating of the police list can be viewed as exogenous to the court, this minimizes the risk of such bunching. Still, while the release of the list was unexpected, the dates of later updates were sometimes announced in advance. In the presence of sorting, we would expect to observe a jump in the density of the running variable at the cutoff. Figure 6 shows that the relative frequency of cases is similar on both sides of the cutoff, and the McCrary test in Figure 7 for bunching on the prison sample is also statistically insignificant.

4. Results

This section outlines results from our empirical analysis of the effect of neighborhood listing on criminal justice decisions. All specifications include indicators capturing the list year and our preferred specification also control for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. Weeks 0–1 are excluded from the analysis (dougnnut) and the standard errors are clustered at the court level.

4.1 Main results

Table 4 presents the results for the effect of neighborhood listing on the probability that a defendant is convicted and on the probability of receiving a prison sentence. In our sample, 92.5% of defendants are convicted, reflecting the high evidentiary standard required for a case to proceed to trial. Among the cases that are tried in court, 14.7% result in a prison sentence. All estimated effects are negative, statistically insignificant, and small in magnitude. For example, the 95% confidence interval for the probability of conviction rules out effect sizes larger (in absolute value) than –4.6 to 1.7 percent of the sample mean. Overall, the point estimates indicate that the list releases had no impact on these outcomes.

For sentence length, on the other hand, we find large and significant increases around the list releases throughout the different empirical specifications in Table 5. In the main empirical specification, we estimate that log sentence length increases by roughly 0.519 log points (s.e. 0.176), which is sizable increase statistically significant at the 1% level. Sentence length in days, similarly, increases by about 260 days (260.513, s.e. 122.725). This result is similar in relative terms (+69%) when you compare it to the mean of the dependent variable sentence length for individuals to the left of the cutoff. Figures 8 and 9 provide the graphical equivalence to those presented in the tables above.

The fact that we only observe an effect on prison sentence length is also telling, as this likely involves the greatest degree of human discretion, and could therefore be more susceptible to influence from salient events. Interestingly, our results are broadly consistent with those in Hanemaaijer et al. (2024), who also find no significant effect on the probability of conviction or on receiving a prison sentence in the Netherlands for individuals of Moroccan descent following the salience shock created by the assassination of a high-profile lawyer overseeing a case against the so-called "Mocro Maffia." Their study further shows that the sentence length for convicted individuals of Moroccan descent increased by 79% after the shock.

4.2 Robustness

We have subjected the main results to a battery of robustness checks. Although the release and update of the report occurred on different dates, it is still possible that our estimates are capturing

seasonal effects.[26] Appendix Figure A2 presents results from placebo tests that use fictitious release and update dates set exactly one year before the actual dates. We find no significant effects in this analysis, suggesting that our results are not driven by seasonal patterns.

Appendix Table A1 presents results in which we replicate our analysis of sentence length using different bandwidths, including week t+1, and by varying the degree of overlap required between a neighborhood and the police map for the neighborhood to be classified as listed. The results show that our estimates remain similar to the baseline and that they become statistically significant when we include non-prison sentence length in the dependent variable.

4.3 Mechanisms

As shown earlier, the neighborhoods included on the police's list had persistently higher levels of disadvantage; however, there appeared to be no meaningful changes in disadvantage prior to the release or updates of the list. The literature suggests that the list could nonetheless constitute a salience shock for judges, particularly in the short run. Several studies show that intensified media reporting of events unrelated to the cases at hand can lead to short-run changes in sentencing severity for minority groups that are disproportionately represented in the criminal justice system. For instance, Hanemaaijer et al. (2024) show that the sentence length of Moroccan defendants increased during a four-week period following the salience shock created by the assassination of a high-profile lawyer overseeing a case against the so-called "Mocro Maffia." Grosjean et al. (2023) show that Trump rallies raised the probability of Black drivers being stopped by police in the two months afterward. Moreover, Ouss et al. (2023) find that media coverage of crimes in France leads to harsher sentencing the following day, and McConnell and Rasul (2021b) and McConnell and Rasul (2021a) show that the 9/11 attacks increased sentencing severity in subsequent months for both Muslim and non-Muslim minorities in US federal courts due to spillover effects.

To assess the feasibility of this mechanism in our context, we conducted a series of additional analyses. First, recall that media coverage spiked sharply around the release and update of the list and then declined within a few months, with the largest increases occurring in 2017 and 2019. Figure 10 presents results from an analysis in which we extended the doughnut from –20 to +10

[26] For instance, the release in 2015 and the update in 2021 occurred in early December, just before many courts process fewer cases due to Christmas.

weeks by shifting the doughnut window accordingly. Consistent with the short-run increase in media coverage, we observe that the estimated effect fades out approximately 6 to 8 weeks after the shock.

A related question concerns the extent to which judges are consistently shocked by each release or update of the list. To investigate this, we estimate regressions separately by list year. Table 6 shows that the effects are strongest in 2017–2019, coinciding with the largest spikes in media coverage. Thereafter, the effects appear more muted. One possible interpretation is stigma saturation. Supporting this explanation, media coverage increased the least in 2021.

In Table 7, we show evidence supporting the idea that judges with limited own personal experience of listed neighborhoods respond stronger to the introduction of the list. Using our administrative data we calculate that about 9% of the judges in our sample have at some point in their lives since 1982 resided in a listed neighborhood. High share judges exposed is defined as an indicator for being above the 75th percentile of judge exposure. The results show that the increase in sentence length is significantly smaller in courts where a higher share of judges have been exposed to listed neighborhoods.

Not all neighborhoods were highlighted equally in media reporting. Appendix Figure C.3 shows that coverage tended to focus more heavily on the neighborhoods classified by the police as highly troubled. Table 8 presents results from separate regressions by neighborhood classification—highly troubled, moderately troubled, or at risk. Consistent with the concentration of the media coverage to highly troubled neighborhoods, we observe a tendency toward stronger effects for these neighborhoods.

Overall, both the spatial and temporal patterns in the strength of court responses align with the increase in negative media coverage. Moreover, the stronger responses by judges with the least prior exposure to these neighborhoods suggest that courts are updating—or at least confirming—their beliefs about the problems associated with these neighborhoods.

Spatial differences in criminal justice interactions are intimately linked to the concentration of minority populations and socioeconomic disadvantage. This raises the question of whether judges may use neighborhoods as a proxy for underlying structural disadvantages. Because minorities are disproportionately concentrated in certain neighborhoods, racial and ethnic disparities may generate place-based sentencing differences that cannot be explained by crime levels or case characteristics

alone. The "race versus space" debate is central to understanding why minorities face persistent economic disadvantages. Zenou and Boccard present a theoretical model demonstrating that both racial identity and spatial location play important roles in explaining high unemployment rates among Black workers.[27]

To distinguish between "race" and "space," we present results from analyses estimating heterogeneous effects by predicted risk of reoffending, immigrant status, and co-offending. The idea is that if judges are using neighborhoods as a proxy for underlying disadvantages, then we would expect stronger effects in subgroups more closely linked to these disadvantages. We focus on immigrants and cases involving co-offenders because these neighborhoods are characterized by a high concentration of immigrants and are often associated with gang-related crime. We therefore use co-offending as a proxy for criminal networks. The results in Table 9 show that there is no significant difference in sentencing outcomes between immigrants and natives, nor between co-offending and non–co-offending defendants. These findings suggest that place-based stigma operates above and beyond ethnic stigma.

4.4 Long-term effects of neighborhood listing

The fact that our data contain information from several different registers allows us to investigate potential long-term effects of differential treatment on labor market outcomes and health. While we find no significant effects on various measures of prescriptions for mental health–related drugs (see Table 10), we do find a significant decrease in the probability of being employed (see Table 11). This effect persists three years after the trial, suggesting that it is not merely a mechanical consequence of time spent in prison. The point estimates indicate that defendants from listed neighborhoods experience a 16–percentage point decline in employment over the three years following their sentence.

[27] A large literature studies racial and ethnic discrimination in the criminal justice system (Arnold et al., 2022; Abrams et al., 2012b; Jordan et al., 2023; Hanemaaijer et al., 2024; Tebes and Fagan, 2022; Yang, 2015; Goncalves and Mello, 2021; Ang, 2021; Bindler et al., 2024; Agan, 2024).

5. Conclusion

The 2015 release of a police situation report publicly listing "troubled neighborhoods" triggered a significant negative information shock, unaccompanied by immediate policy interventions. Using data from various sources, we show that the introduction of the list led to a marked increase in negative media coverage, likely reinforcing the stigmatization of the listed neighborhoods.

Our main findings reveal that sentence lengths for individuals residing in listed neighborhoods increased by more than 60 percent immediately after the event, with the effect dissipating as media coverage declined. This temporal pattern aligns with models of decision-making under limited attention: when exposed to salient shocks, courts may overweight recent or widely publicized events—even when these events bear no direct relevance to the case at hand. We further document substantial downstream consequences. Defendants from listed neighborhoods experience a 16-percentage-point reduction in employment during the three years following their sentence, indicating sizable long-term economic costs.

These findings demonstrate that neighborhood reputation can systematically influence sentencing decisions in criminal courts. While we show that these sentencing disparities have downstream consequences for defendants' long-run labor-market outcomes, courts are institutions whose rulings shape not only individual trajectories but also broader perceptions of procedural fairness. This suggests an important avenue for future research: examining how place-based discrimination in sentencing may affect perceptions of fairness and trust in institutions both within affected communities and in the wider society. Moreover, because courts adjudicate cases drawn from their own jurisdictions, heightened responsiveness to neighborhood reputation risks reinforcing existing geographic inequalities and further disadvantaging residents of already stigmatized areas.

Taken together, the evidence points to place-based stigma as a meaningful, and previously underappreciated, mechanism behind neighborhood effects. From a policy standpoint, efforts to counteract negative perceptions of disadvantaged neighborhoods may provide a cost-effective way to improve residents' outcomes without requiring substantial financial investments or major institutional reforms. This could also potentially be achieved by standardized sentencing guidelines that reduce judicial discretion or judicial training for judges on the effects of cognitive biases.

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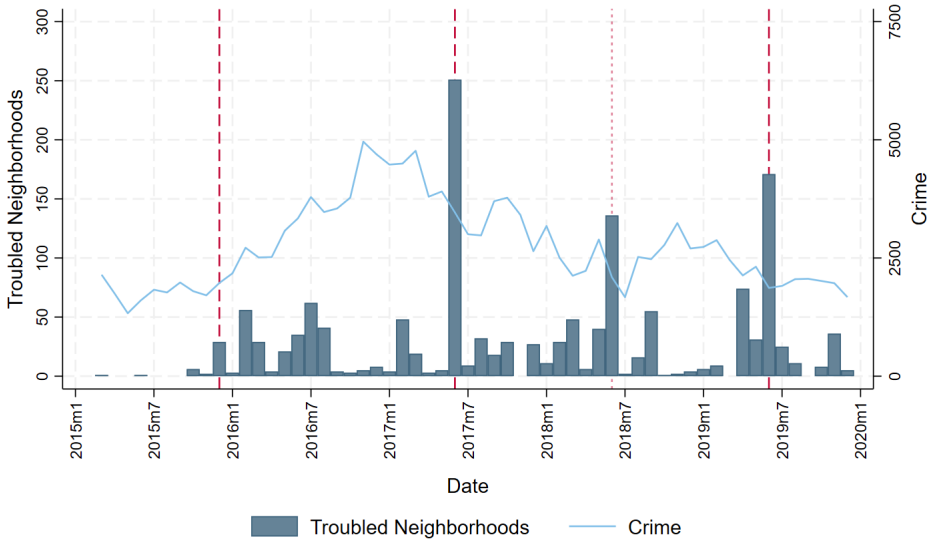
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Figures and tables

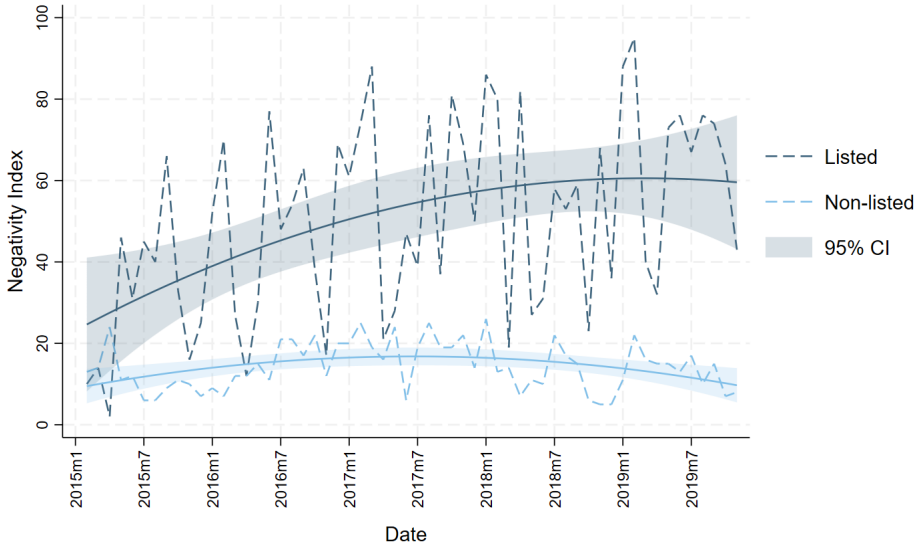
Result figures

Figure 1: Media Mentions - "Troubled Neighborhoods"



Notes: This figure plots the number of articles in Swedish media including the term "Troubled neighborhoods" (in Swedish, Utsatta områden). Red dash vertical lines indicate months in which the list was released or updated. The lighter red dotted line in 2018 indicates an update that was expected and did not take place. The data was obtained from the GDELT project, `https://www.gdeltproject.org/`.

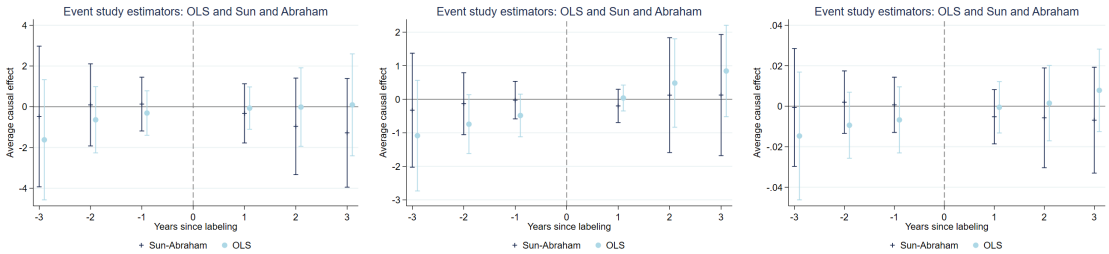
Figure 2: Negativity Index of Media Tone - Listed and Non-Listed Neighbourhoods



Notes: This figure plots the average negative index of the tone of the articles in Swedish media for neighborhoods included in the Police List, and a random sample of non-listed neighborhoods. Higher values of this negativity index indicate a more negative tone in the article. Dashed lines denote the observed tone and solid lines the seasonality-adjusted second order local polynomial. The data was obtained from the GDELT project, `https://www.gdeltproject.org/`

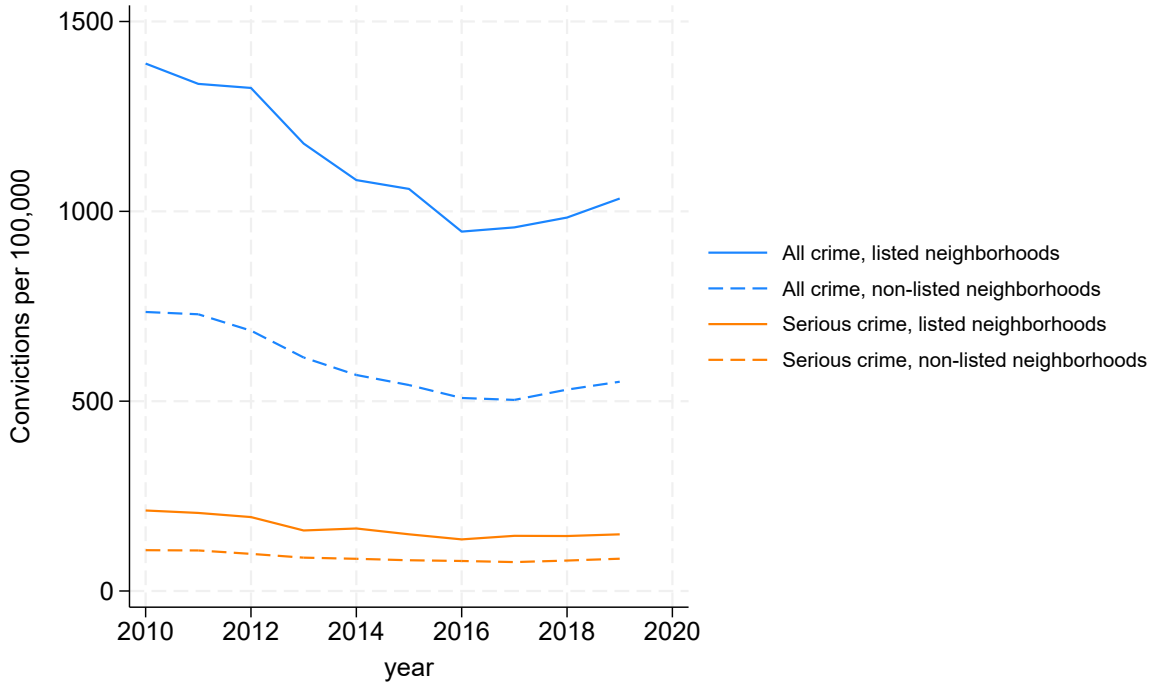
Figure 3: Police Resources - Treatment Effect in Listed Neighborhoods

(a) Employees per station (b) Average weekly hours (c) Share working overtime



Notes: This figure plots the average treatment effect of the police list on neighbourhood police resources. These are estimated using a dynamic difference-in-differences strategy, where the treatment group includes all listed neighborhoods, and the control group includes non-listed neighborhoods weighted by their propensity of being treated on pre-policy characteristics (see Appendix Section 2.1 for the details of the empirical model). TWFE and Sun and Abraham (2021) estimators are presented. Sub-figure (a) shows the effect on number of employees per station, Sub-figure (b) for average weekly contracted hours per police officer, and Sub-figure (c) for share of staff working overtime. 95% confidence intervals are based on standard errors clustered at the neighborhood level.

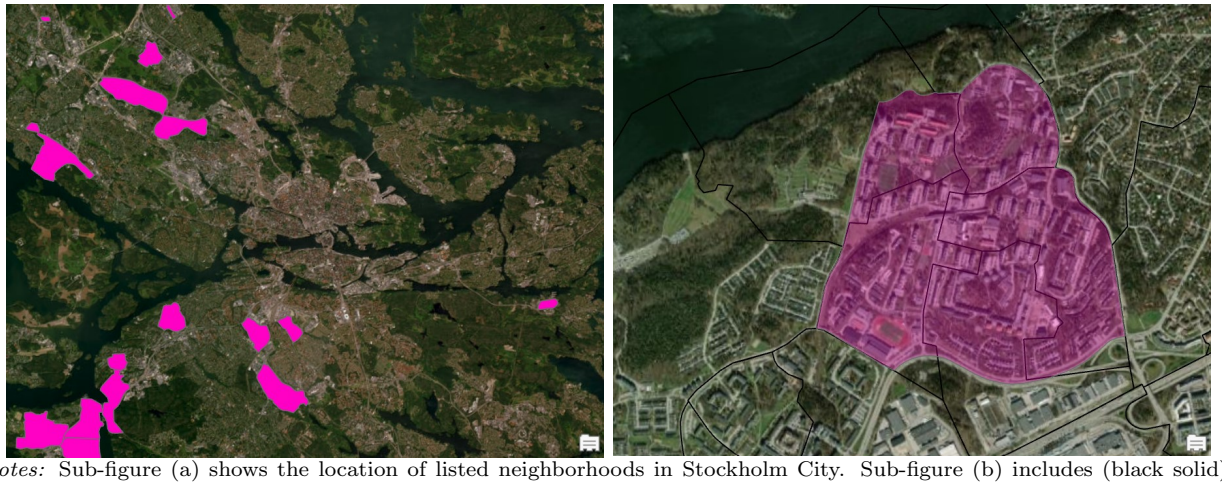
Figure 4: Conviction rates for Listed and Non-Listed Neighbourhoods



Notes: Statistics are based on the universe of convictions in the Swedish judicial system in years 2010-2019. Serious crime (orange time series) include all prison sentences. The statistics are aggregated from individual-level data that come from Statistics Sweden. The control group of non-listed neighborhoods refer to all other urban neighborhoods units except for the listed neighborhoods.

Figure 5: Map of Listed Neighborhoods in Stockholm City

(a) Listed neighborhoods in the city of Stockholm (b) One listed neighborhood (Bredäng)



Notes: Sub-figure (a) shows the location of listed neighborhoods in Stockholm City. Sub-figure (b) includes (black solid) boundaries of the neighborhood (DeSO) units belonging to a listed neighborhood in Stockholm called Bredäng. Zooming in on Bredäng, share of immigrants in 2023: 63.5 percent vs. Stockholm city avg. 34.9 percent; employment in 2021: 70.4 percent vs. Stockholm city avg. 79.5 percent (City of Stockholm, 2024). Geocoded data come from the National Operative Unit of the Swedish Police Authority. In the spirit of our paper, for the positive qualities of the surroundings of Bredäng, see https://www.flickr.com/photos/miljoforvaltningenstockholm/albums/72157646835975372/.

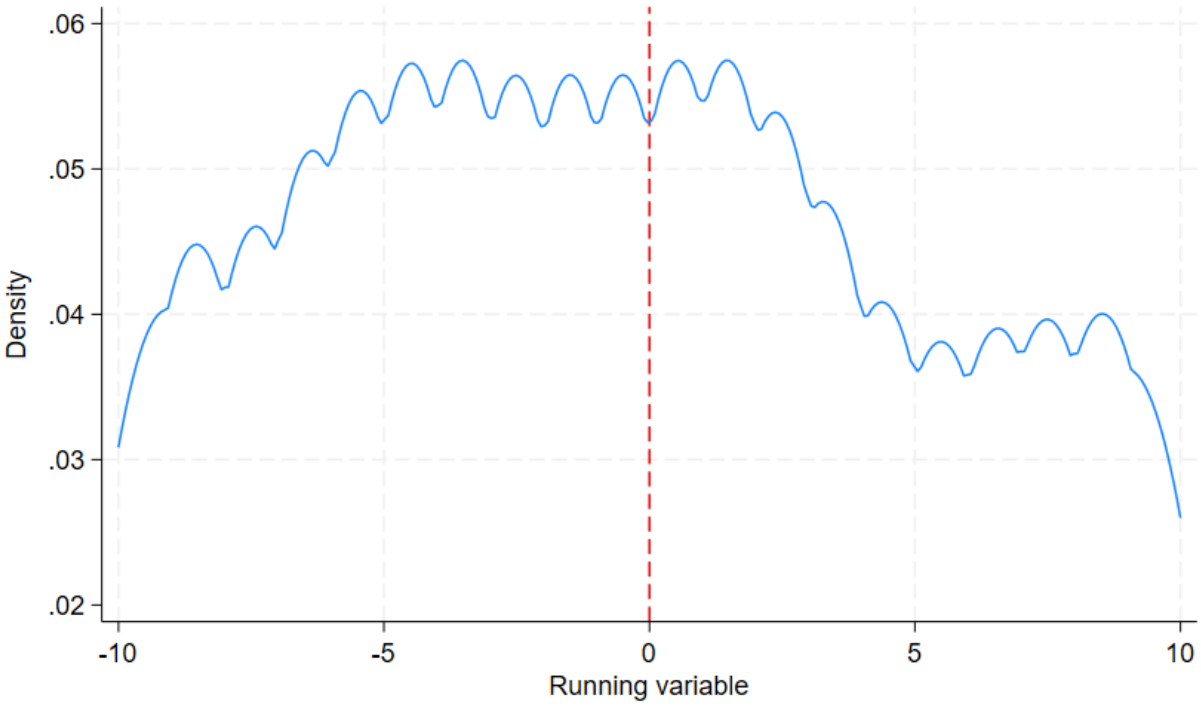
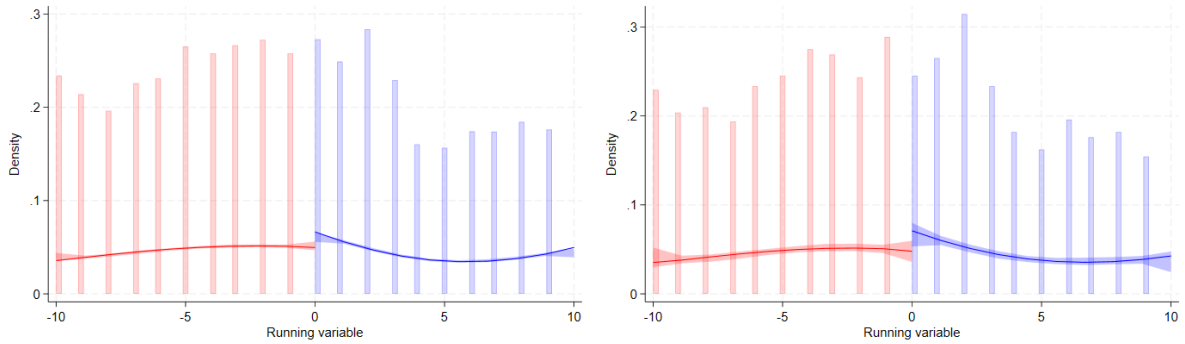


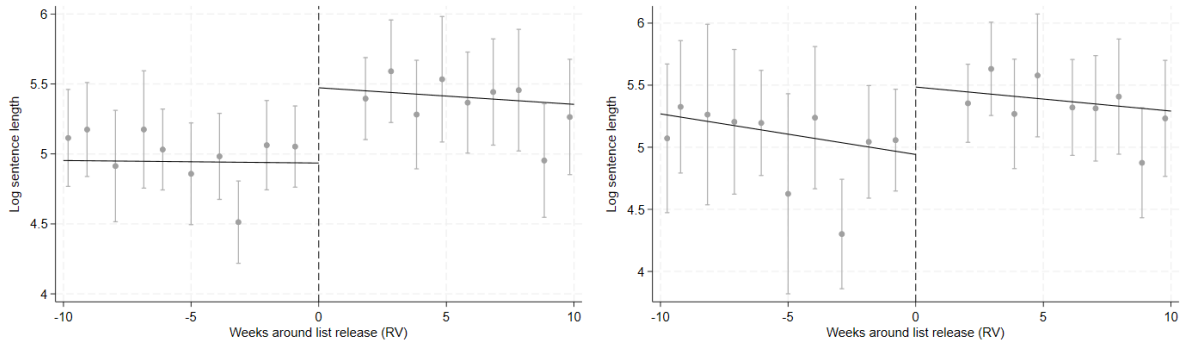
Figure 6: Density of cases around list release.



(a) McCrary test full sample (p-val. 0.18).

(b) McCrary test prison sample (p-val. 0.85).

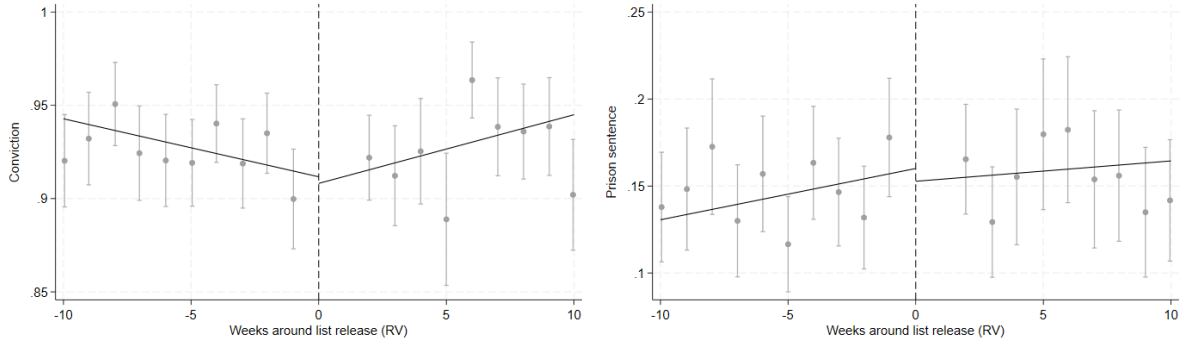
Figure 7: McCrary test figure around list release.



(a) Log sentence length (days).

(b) Log sentence length (days) - Matched sample.

Figure 8: Stigma effect: RDD figure with ±10 weeks bandwidth.
The panels show RDD figures for troubled areas with list years, excluding week 0–1 to remove potential mismatches between the decision date matching to when the sentences are actually decided and the list release date. CI95 are indicated in black and gray. Propensity score nearest-neighbor matching with replacement is based on gender, list year, foreign born, arabic background, high school education, earnings, residence in large city, age, and including 37 indicators for crime type.



(a) Conviction.

(b) Prison sentence.

Figure 9: Stigma effect: RDD figure with ±10 weeks bandwidth.
The panels show RDD figures for troubled areas with list years, excluding week 0–1 to remove potential mismatches between the decision date matching to when the sentences are actually decided and the list release date. CI95 are indicated in black and gray.

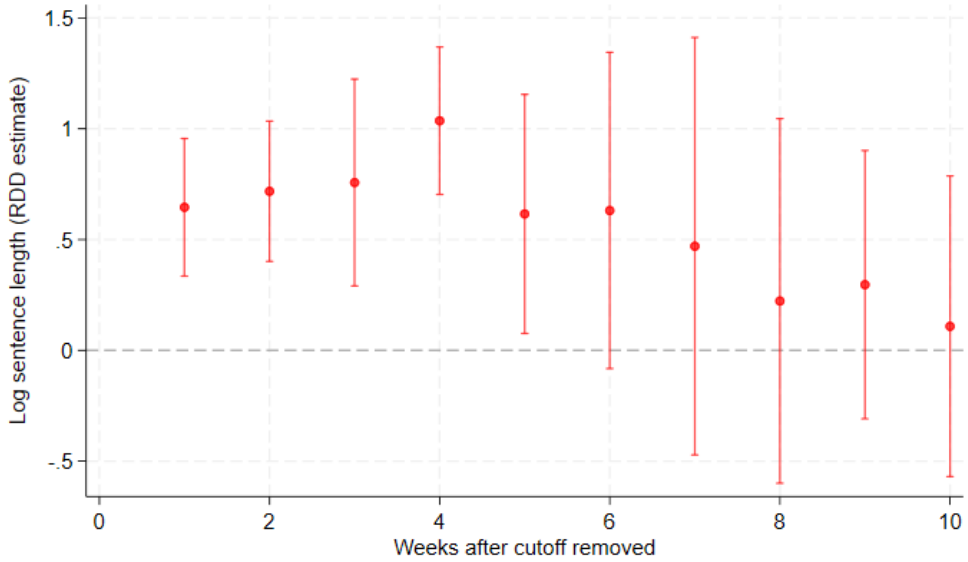


Figure 10: Extended doughnut figure indicating when effects fade. BW -20 up to 10 weeks (moving with doughnut).

The figure removes additional weeks around the cutoff week and shows that the treatment effect fades out circa 6–8 weeks after list release. CI95 are indicated in vertical red T-bars.

Table 1: Police List of Troubled Neighborhoods

Neighborhood	2015	2017	2019	Neighborhood	2015	2017	2019
Alby	×	×	×	Koppargården/Karlslund	×	×	×
Andersberg	×	×	×	Kronogården/Lextorp	×	×	×
Araby	×	×	×	Lagersberg	×	×	×
Bergsjön	×	×	×	Lövgärdet	×	×	×
Biskopsgården	×	×	×	Navestad		×	×
Brandbergen	×	×	×	Norrby		×	×
Bredäng	×	×	×	Nydala/Hermodsdal/Lindängen		×	×
Bäckby	×	×	×	Oxhagen/Varberga	×	×	×
Charlottesborg	×	×	×	Rannebergen	×	×	×
Dalhem/Drottninghög/Fredriksd.	×	×	×	Rinkeby/Tensta	×	×	×
Edsberg	×	×	×	Rissne/Hallonbergen	×	×	×
Falkagård	×			Ronna/Geneta/Lina	×	×	×
Finnsta	×	×	×	Rosengård, south of Amiralsg.	×	×	×
Fittja	×	×	×	Råslätt	×	×	×
Fornhöjden	×	×	×	Skiftinge	×	×	×
Fröslunda	×	×	×	Skogås	×	×	×
Gammelgården	×	×	×	Skäggetorp	×	×	×
Gottsunda	×	×	×	Smedby	×	×	
Gårdssten	×	×	×	Storvreten			×
Hageby	×	×		Sångvägen	×	×	×
Hagsätra/Rågsved	×	×	×	Söder		×	×
Hallunda/Norsborg	×	×	×	Södra Sofielund	×	×	×
Hammarkullen	×	×	×	Termovägen	×	×	×
Hisings Backa	×	×	×	Tjärna Ängar	×	×	×
Hjällbo	×	×	×	Tureberg	×	×	×
Holma/Kroksbäck/Bellevueg.	×	×	×	Tynnered/Grevegården/Opalt.	×	×	×
Hovsjö	×	×	×	Vivalla	×	×	×
Husby	×	×	×	Vårberg		×	×
Hässelby/Vällingby	×	×	×	Vårby		×	×
Hässleholmen/Hulta	×	×	×	Älvsjö/Solberga	×	×	×
Jordbro		×	×	Östberga	×	×	×
Klockaretorpet		×	×				

Notes: This table shows the troubled neighborhoods listed by the police in each of the three situation reports that were released within our study period (2010-2020), i.e., 2015, 2017 and 2019 releases. The data are sourced from the National Operative Unit (NOA) of the Swedish Police Authority.

Table 2: Descriptives table.

Sample:	Listed neighborhood			Non-listed neighborhood		
Time to sentencing:	$t_{-3} - t_{-1}$	t_0	t_1	$t_{-3} - t_{-1}$	t_0	t_1
Foreign bgr.	0.54	0.55	0.56	0.30	0.28	0.30
Male	0.83	0.84	0.83	0.82	0.82	0.81
High school	0.48	0.45	0.45	0.54	0.55	0.55
Birthyear	1984.41	1985.81	1985.39	1982.27	1981.69	1982.44
Nonemployed	0.44	0.48	0.45	0.44	0.44	0.43
Major city	0.51	0.51	0.50	0.25	0.24	0.25
Violent crime	0.12	0.11	0.14	0.13	0.13	0.13
Property crime	0.25	0.22	0.20	0.19	0.19	0.19
Drug crime	0.24	0.25	0.28	0.23	0.22	0.24
Sentence length (nonmissing)	313.20	342.37	440.97	291.68	308.69	333.89
Prison sentence	0.14	0.13	0.15	0.14	0.14	0.13
Convicted	0.92	0.92	0.90	0.92	0.92	0.93
Obs	2808	962	878	12750	4351	4302

Table 3: RDD balancing table.

	Pred. sent. length	Foreign backgr.	Male	High school ed.	Age	Earnings	Non-empl.	Live in m. city	Drug sentence	Property sentence	Violent sentence
RDD estimate	0.064	0.049	−0.009	−0.084	−0.750	−12.841	0.021	0.096	−0.046	0.031	−0.082
	(0.079)	(0.067)	(0.051)	(0.085)	(2.981)	(194.108)	(0.076)	(0.095)	(0.058)	(0.082)	(0.072)
Mean dep. var.	0.317	0.126	0.902	0.465	37.659	760.544	0.554	0.302	0.178	0.211	0.148
Obs.	707	707	707	707	707	707	707	707	707	707	707
R^2	0.005	0.005	0.005	0.013	0.030	0.024	0.035	0.029	0.015	0.009	0.013
Matched sample	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
List overlap $\geq 30\%$	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bandwidth	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.
Kernel	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.

Standard errors in parentheses are clustered at the court level. Predicted sentence length includes gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Matched sample is based on the covariates listed above and including 37 indicators for crime type, with nearest-neighbor-matching.

Table 4: RDD conviction and prison outcome table.

Sample:	Full sample			
Outcome:	Conviction probability		Prison probability	
RDD estimate	−0.011	−0.012	−0.002	−0.005
	(0.014)	(0.014)	(0.024)	(0.024)
Mean dep. var.	0.925	0.925	0.147	0.147
Obs.	6438	6355	6438	6355
R^2	0.002	0.009	0.002	0.025
Controls		✓		✓
List overlap $\geq 30\%$	✓	✓	✓	✓
Bandwidth	±10 weeks	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triang.	Triang.	Triang.

Standard errors in parentheses are clustered at the court level. Including controls for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: RDD outcome table.

Sample:	Prison sample			Prison sample		
Outcome:	Log sentence length			Sentence length		
RDD estimate	0.530***	0.519***	0.564***	267.009**	260.513**	235.400**
	(0.162)	(0.176)	(0.181)	(115.745)	(122.725)	(104.918)
Mean dep. var.	4.980	4.980	4.980	378.003	378.003	378.003
Obs.	707	707	1030	707	707	1030
R^2	0.021	0.067	0.088	0.021	0.047	0.047
Matched sample	✓	✓		✓	✓	
Controls		✓	✓		✓	✓
List overlap $\geq 30\%$	✓	✓	✓	✓	✓	✓
Bandwidth	±10 weeks	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triang.	Triang.	Triang.	Triang.	Triang.

Standard errors in parentheses are clustered at the court level. Including controls for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Matched sample is based on the covariates listed above and including 37 indicators for crime type, with nearest-neighbor-matching.

Table 6: RDD outcome year table.

List year:	Stacked	2015	2017 & 2019	2021
Outcome:	Log sentence length			
RDD estimate	0.519***	0.433	0.700***	0.205
	(0.176)	(0.535)	(0.214)	(0.359)
Mean dep. var.	4.980	4.980	4.980	4.980
Obs.	707	196	292	219
R^2	0.067	0.099	0.132	0.083
Matched sample	✓	✓	✓	✓
Controls	✓	✓	✓	✓
List overlap $\geq 30\%$	✓	✓	✓	✓
Bandwidth	±10 weeks	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triang.	Triang.	Triang.

Standard errors in parentheses are clustered at the court level. Including controls for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Matched sample is based on the covariates listed above and including 37 indicators for crime type, with nearest-neighbor-matching.

Table 7: Heterogeneous treatment by judge exposure to troubled areas.

Outcome:	Log sentence length		Sentence length	
RDD estimate (Reference group)	0.596***	0.631	352.944***	475.274
	(0.180)	(0.471)	(127.220)	(300.492)
Jump × High share judges exposed	−0.658**		−463.009**	
	(0.306)		(192.134)	
Jump × Linear share of judges exposed		−0.013		−21.185
		(0.038)		(21.825)
Mean dep. var.	5.052	5.052	467.715	467.715
Obs.	707	707	707	707
R^2	0.099	0.072	0.072	0.059
Matched sample	✓	✓	✓	✓
Controls	✓	✓	✓	✓
List overlap ≥ 30%	✓	✓	✓	✓
Bandwidth	±10 w.	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triangular	Triangular	Triangular

Standard errors in parentheses are clustered at the court level. High share judges exposed is defined as an indicator for being above the 75th percentile of judge exposure (more than 13% of judges have lived in a troubled area between 1982–2015). Including fully interacted models with controls for gender, foreign born, arabic background, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: RDD sentence length, by vulnerable area status (not mutually exclusive).

Vul. area status:	High	Mid	Low
Outcome:		log(Sentence length)	
RDD estimate	0.740***	0.641	0.566
	(0.233)	(0.538)	(0.415)
Mean dep. var.	4.920	5.083	5.042
Obs.	423	130	277
R^2	0.074	0.175	0.099
Matched sample	✓	✓	✓
Controls	✓	✓	✓
List overlap $\geq 30\%$	✓	✓	✓
Bandwidth	$\pm$10 w.	$\pm$10 w.	$\pm$10 w.
Kernel	Triangular	Triangular	Triangular

Standard errors in parentheses are clustered at the court level. Including controls for gender, list year, foreign born, arabic background, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Heterogeneous treatment by predicted risk of reoffending, cooffending status, and foreign background.

Outcome:	Log sentence length		
RDD estimate (Reference group)	−0.244	0.401	0.627*
	(0.305)	(0.242)	(0.344)
Jump × Reoffending risk Q2	0.184		
	(0.421)		
Jump × Reoffending risk Q3	0.540		
	(0.466)		
Jump × Reoffending risk Q4	0.609*		
	(0.303)		
Jump × Cooffending		−0.161	
		(0.517)	
Jump × Foreign bgr.			−0.196
			(0.434)
Mean dep. var.	4.980	5.767	4.947
Obs.	707	707	707
R^2	0.450	0.216	0.092
Matched sample	✓	✓	✓
Controls	✓	✓	✓
List overlap ≥ 30%	✓	✓	✓
Bandwidth	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triangular	Triangular

Standard errors in parentheses are clustered at the court level. Including fully interacted models with controls for gender, foreign born, arabic background, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: RDD labor market and recidivism relative to list release year.

Outcome:	Earnings t+1	Earnings t+3	Employment t+1	Employment t+3	Reoffended t+1	Reoffended t+3
RDD estimate	−75.506	−145.358	−0.089**	−0.163*	−0.012	0.008
	(148.595)	(196.270)	(0.043)	(0.082)	(0.096)	(0.091)
Mean dep. var.	1043.504	1200.641	0.436	0.505	0.297	0.380
Obs.	700	686	700	686	707	707
R^2	0.168	0.178	0.161	0.153	0.193	0.300
Matched sample	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
List overlap $\geq 30\%$	✓	✓	✓	✓	✓	✓
Bandwidth	±10 weeks	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triang.	Triang.	Triang.	Triang.	Triang.

Standard errors in parentheses are clustered at the court level. Including controls for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Matched sample is based on the covariates listed above and including 37 indicators for crime type, with nearest-neighbor-matching. Employment status is defined as having non-zero or non-missing earnings.

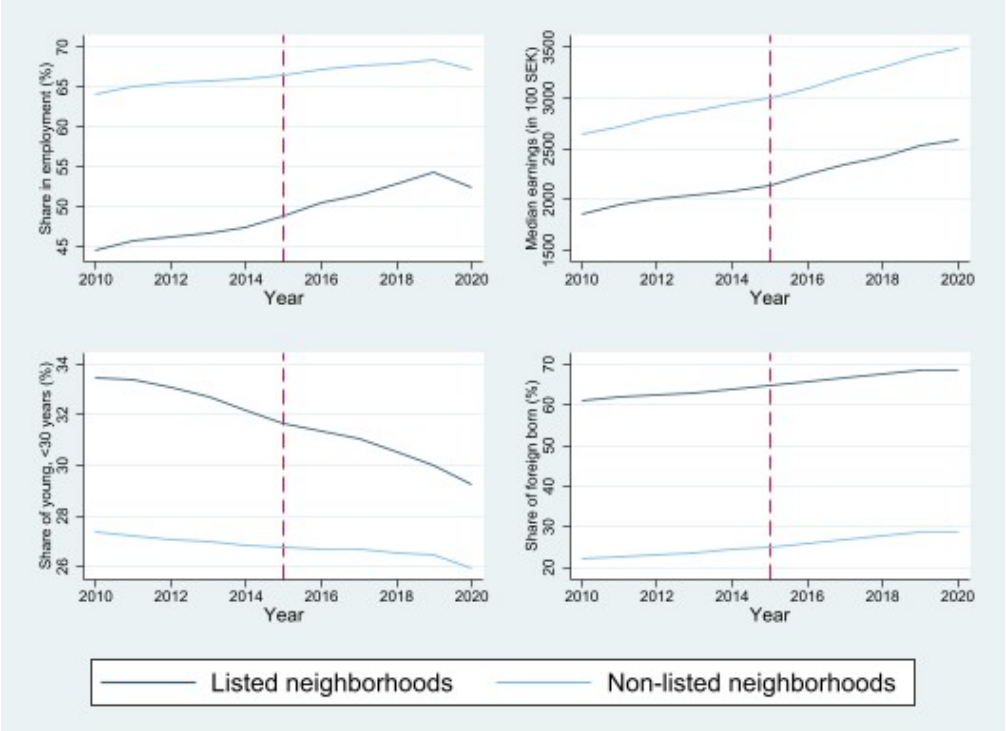
Table 11: RDD health outcome table .

Outcome:	Any drug t+3	Antidep. t+3	Any incare t+3	Antidep. incare t+3	Any outcare t+3	Antidep. outcare t+3
RDD estimate	0.051	−0.040	−0.010	0.005	0.050	0.013
	(0.093)	(0.052)	(0.075)	(0.005)	(0.066)	(0.019)
Mean dep. var.	0.534	0.094	0.123	0.003	0.447	0.015
Obs.	488	488	707	707	707	707
R^2	0.095	0.051	0.063	0.014	0.095	0.034
Matched sample	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓
List overlap $\geq$ 30%	✓	✓	✓	✓	✓	✓
Bandwidth	±10 weeks	±10 w.	±10 w.	±10 w.	±10 w.	±10 w.
Kernel	Triangular	Triang.	Triang.	Triang.	Triang.	Triang.

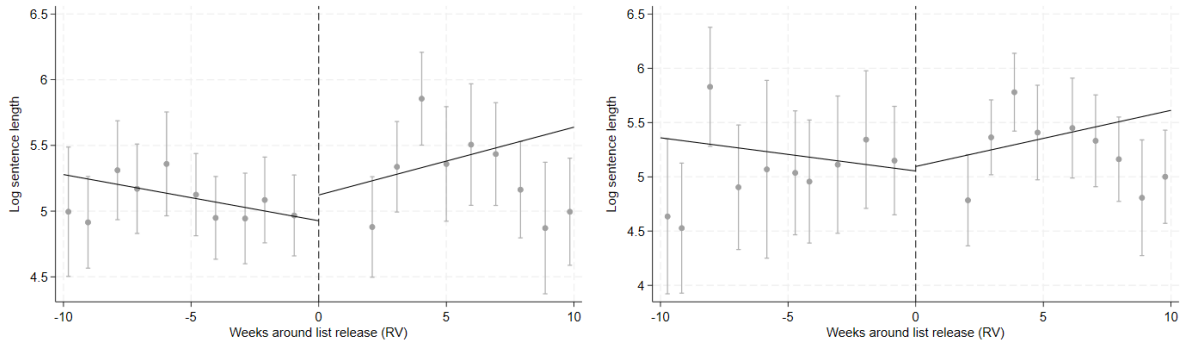
Standard errors in parentheses are clustered at the court level. Including controls for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Matched sample is based on the covariates listed above and including 37 indicators for crime type, with nearest-neighbor-matching.

Appendix A: Figures and tables

Figure A1: Trends in Socioeconomic Characteristics in Listed and Non-Listed Urban Neighborhoods



Notes: Statistics are based on the complete Swedish population aged 16-64 in years 2010-2020. Median earnings refer to the P(50) earnings among positive observations of taxable gross annual earnings. Immigrants are defined as individuals who have a recorded date of migration to Sweden. The statistics are aggregated from individual-level data that come from Statistics Sweden. The control group of non-listed neighborhoods refer to all other urban neighborhoods units except for the listed neighborhoods.



(a) Log sentence length (days).

(b) Log sentence length (days) - Matched sample.

Figure A2: Placebo figure: RDD figure with ±10 weeks bandwidth for placebo years one year prior to the list release.

The panels show RDD figures for troubled areas with placebo years, excluding week 0–1 to remove potential mismatches between the decision date matching to when the sentences are actually decided and the list release date. CI95 are indicated in black and gray. Propensity score nearest-neighbor matching with replacement is based on gender, list year, foreign born, arabic background, high school education, earnings, residence in large city, age, and including 37 indicators for crime type. Note that this is estimated using rdplot with a triangular kernel.

Table A1: RDD robustness table.

Robustness:	Main spec.	Incl. t+1 w.	Bwidth 15 w.	Bwidth 20 w.	Overlap 20%	Overlap 40%	Include non-prison
Outcome:	Log sentence length		Log sentence length		Log sentence length		Sentence length
RDD estimate	0.519***	0.472***	0.710***	0.730***	0.560***	0.493**	41.413**
	(0.176)	(0.131)	(0.179)	(0.173)	(0.184)	(0.236)	(18.047)
Mean dep. var.	4.980	4.980	5.064	5.113	4.935	4.959	55.754
Obs.	707	1053	1476	1561	1167	876	6355
R^2	0.067	0.082	0.082	0.079	0.101	0.088	0.011
Matched sample	✓	✓					
Controls	✓	✓	✓	✓	✓	✓	✓
List overlap	$\geq$ 30%	$\geq$ 30%	$\geq$ 30%	$\geq$ 30%	$\geq$ 20%	$\geq$ 40%	$\geq$ 30%
Bandwidth	$\pm$10 weeks	$\pm$10 weeks	$\pm$15 w.	$\pm$20 w.	$\pm$10 w.	$\pm$10 w.	$\pm$10 w.
Kernel	Triangular	Triang.	Triang.	Triang.	Triang.	Triang.	Triang.

Standard errors in parentheses are clustered at the court level. Including controls for gender, foreign born, arabic background, age, high school education, earnings, residence in large city, and age. All specifications include indicators capturing the list year. Weeks 0–1 are excluded from the analysis (dougnnut). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Matched sample is based on the covariates listed above and including 37 indicators for crime type, with nearest-neighbor-matching. Employment status is defined as having non-zero or non-missing earnings.

6. Online Appendix

Appendix For Online Publication

A Institutional Background of the Police List

1.1 Swedish "No-Go" Zones in Comparison

Most big cities have neighborhoods notorious for their high crime rates, ethnic segregation, and socioeconomic disadvantage. Consequently, these neighborhoods often find themselves ranked unfavorably in reverse desirability lists across various outlets. A relevant question to ask is how comparable the Swedish troubled neighborhoods are to their, oftentimes more infamous, counterparts throughout the major cities in Europe and the US. Roughly 550,000 (5 percent) of the Swedish population live within the boundaries of a neighborhood that by the police qualified as troubled in 2019 (NOA, 2021).[28] Socioeconomically, the residents of these neighborhoods do clearly worse than the national average. Among the adult working-age population (ages 20-64), only 61.4 percent of the residents in troubled neighborhoods were employed in 2017 as compared to 80.2 percent in the rest of the country. There is also considerably more welfare dependence among the households in the troubled neighborhoods (13 percent) as compared to the rest of the country (4 percent). In Sweden, social housing does not technically exist, although an indication of the inequitable housing conditions is that 75 percent of the households in troubled neighborhoods live in rental housing whereas only 27 percent of the households in the rest of the country do so. When it comes to gun violence, the troubled neighborhoods are in a league of their own in Sweden. Roughly 45 percent of all shootings take place in these neighborhoods (which make up 0.16 percent of Sweden's land area) (NOA, 2021).

By an international comparison, the crime rates in these neighborhoods are however not exceptional. In total, 130 shootings were reported in these neighborhoods in 2020 and in the entire Stockholm county, 39 gun related homicides were reported in the same year. Compared to the most notorious neighborhoods in the U.S., these figures are relatively modest. The Cook County's Medical Examiners Office alone confirmed 881 gun-related homicides in 2020 among its roughly 2.3 million inhabitants (including the City of Chicago), roughly the size of Stockholm county. Baltimore with its 600,000 inhabitants, roughly the same size as Gothenburg (the second largest city in Sweden), saw 298 gun-related homicides in 2020. Even though crime rates have gone down in New York City during the past decades, the Bronx with its 1.5 million inhabitants still has roughly 150 murders annually. In the European context though, the Swedish capital Stockholm is almost an outlier, having for example recorded a 30-fold gun murder rate in comparison to London

[28] In metropolitan Stockholm, the share of residents living in a troubled neighborhood is larger. Roughly 260,000 in Stockholm county live in these neighborhoods, which is 11 percent of the population.

in 2022 (Engel Rasmussen, 2023).

1.2 Additional evidence of media coverage

Figure A.1: Examples of Media Coverage in the Main National News Outlets



(a) Headline from Dagens Nyheter: "Vulnerable neighbourhoods become drug shops as gangs take over"



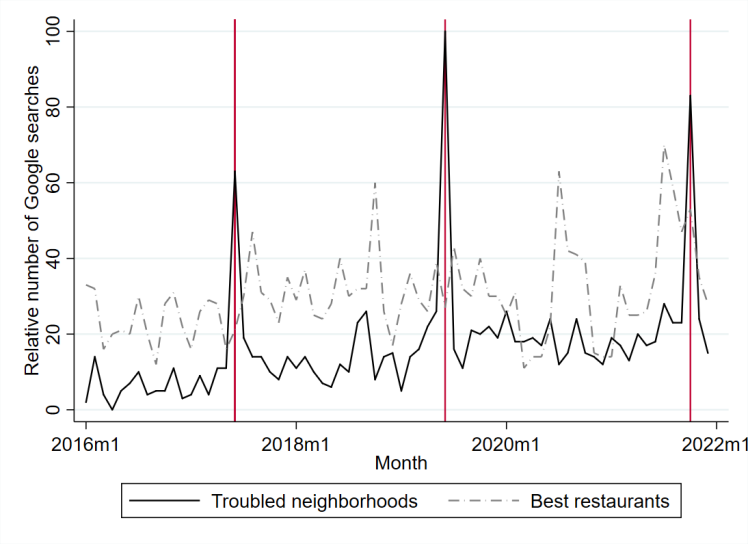
(b) Headline from Dagens Nyheter: "Police: '5000 criminals and 200 networks in vulnerable areas"'



(c) Headline from Svenska Dagbladet: "55 'no go' zones in Sweden"

Notes: The Figures show headlines in the two major Swedish newspapers, Dagens Nyheter and Svenska Dagbladet associating social problems with the troubled neighborhoods using the specific term for these neighborhoods coined by the Swedish police authority, i.e., "utsatta områden".

Figure A.2: Google Searches for "Troubled Neighborhoods" and "Best Restaurants"



Notes: This figure plots the relative number of searches on Google for the terms "Troubled neighborhoods" and "Best restaurants". The data was obtained from Google Trends. Google Trends normalizes search data to make comparisons between terms easier. Search results are normalized to the time and location of a query by dividing each data point by the total searches of the geography and time range it represents. The resulting numbers are then scaled on a range of 0 to 100 based on a topic's proportion of all searches on all topics.

B Evidence Against Policy Responses

This Appendix Section describes the empirical strategy employed to shut down any place-based policies by the police authorities or private security, to or as a response to the situation report issued by the police.

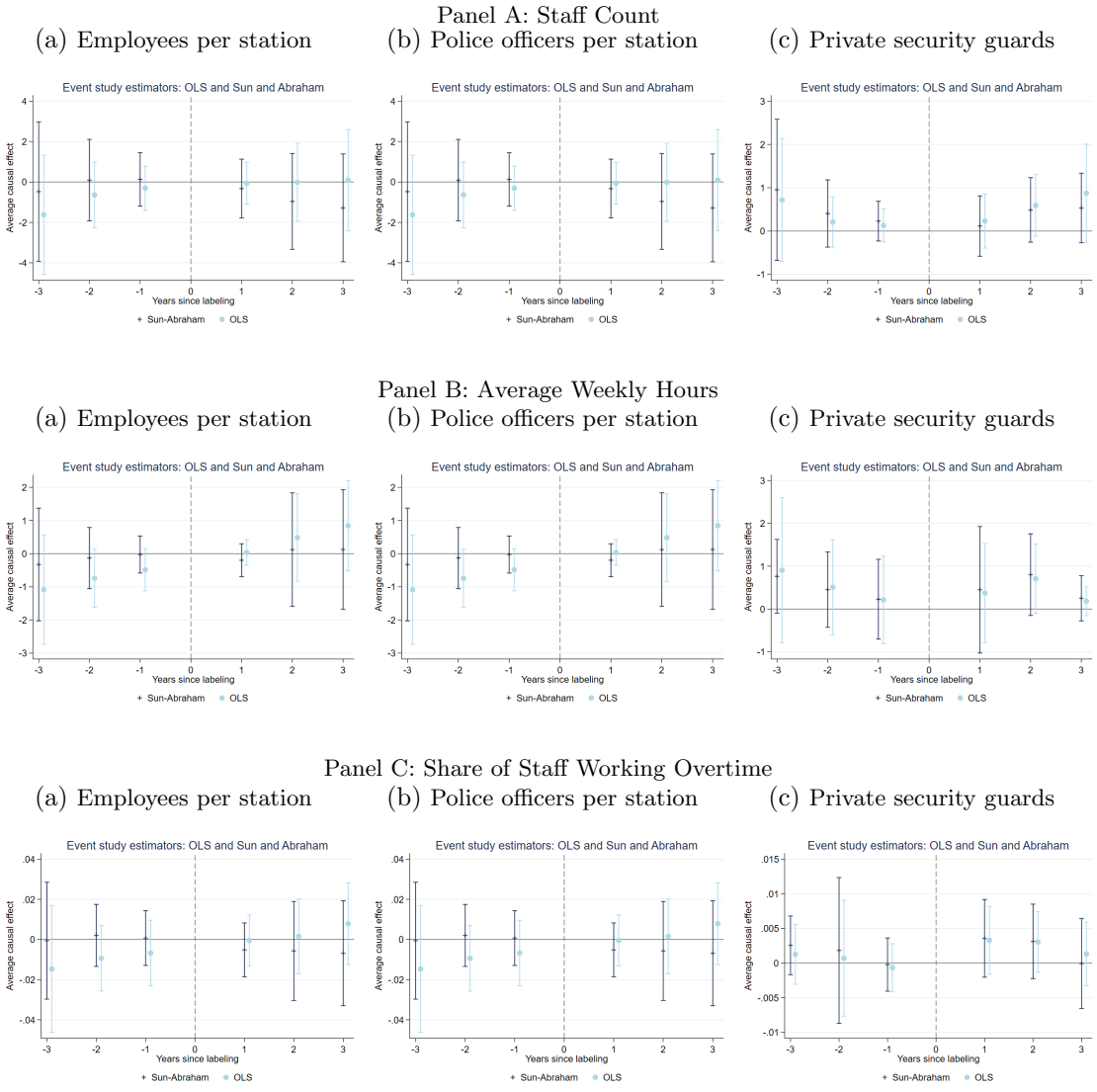
2.1 Event-Study Specification

We estimate a staggered difference-in-differences model of the effect of neighborhood listing on neighborhood resources using non-listed neighborhoods in urban areas as controls. Here we describe our regression model used to estimate the effect of neighborhood listing on its resources shown in Figure 3 in the main text. The regressions are weighted by the propensity to be treated based on pre-policy characteristics. The estimating equation is as follows:

$$Y_{nt} = \alpha + \sum_{k} \beta^{k} \mathbb{1}\left[\mathrm{L}_{n} = t - k\right] + \phi_{mt} + \eta_{n} + \delta X_{nt} + \epsilon_{nt}, \tag{2}$$

where Y_{nt} is an outcome of interest in neighborhood n at year t, and $\mathbb{1}\left[\mathrm{L}_{in} = t - k\right]$ is an indicator for the listing of neighborhood n occurring k periods from t (negative k indicating a future event date). We also control for neighborhood η_n, and municipality-by-year ϕ_{mt} fixed effects, as well as for neighborhood specific linear time trends X_{nt}. The model assumes conditional parallel trends in outcomes for listed and non-listed neighborhoods, which we are able to verify by testing whether the anticipatory effects at $k = 3, 2, 1$ are statistically significant. This strategy provides estimates of the effect of neighbourhood listing on police manpower by year since the first release of the list (year 0). We estimate the model using two way fixed effects ordinary least squares (TWFE) and an estimator proposed by and Sun and Abraham (2021), the results both for which are shown in Figure 3.

Figure B.1: Law Enforcement Resources - Treatment effect in Troubled Neighborhoods



Notes: This figure plots the average treatment effect of the police list on neighbourhood resources. These are estimated using a staggered difference-in-differences strategy, where the treatment group includes all listed neighborhoods, and the control group includes non-listed neighborhoods weighted by their propensity of being treated on pre-policy characteristics. TWFE and Sun and Abraham (2021) estimators are presented. Panel A includes results for the total headcount, Panel B for average weekly hours, and Panel C for share of staff doing overtime. Column 1 shows results for police employees, Column 2 for police officers employees, and Column 3 for private security. 95% confidence intervals are based on standard errors clustered at the neighborhood level.

C Additional Figures and Tables

Figure C.1: Processing of criminal cases

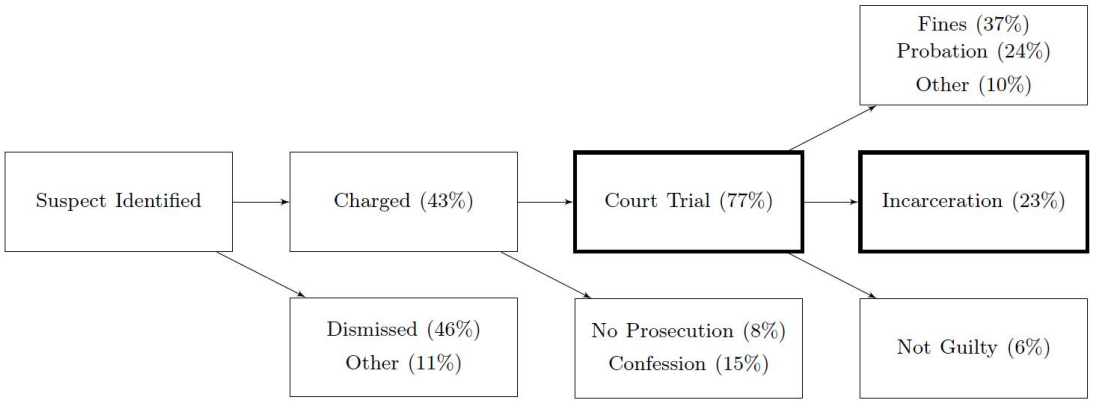


Figure C.2: Cross-country incarceration trends

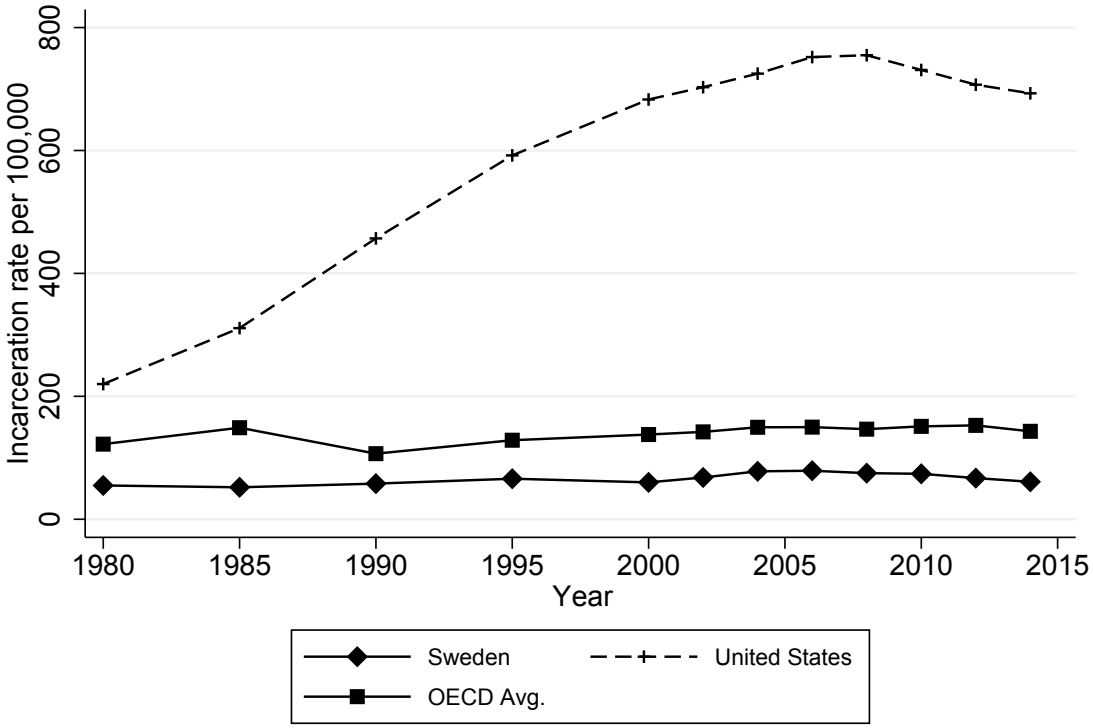


Figure C.3: GDELT Media Analysis

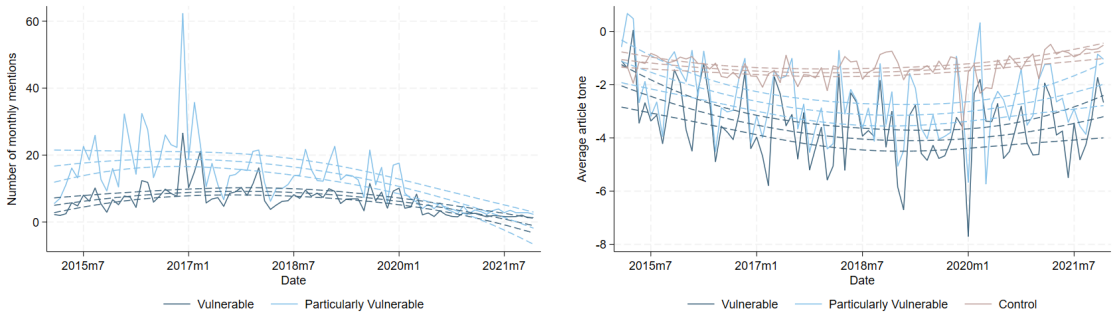


Table C.1: Descriptive Statistics

	Listed	Non-Listed
Panel A: Sampling Frame		
	N	N
Neighborhoods	220	3,104
Panel B: Neighborhood Characteristics		
	Mean	Mean
Young residents (aged 16-30)	0.346	0.283
	(0.047)	(0.091)
Employed	0.606	0.749
	(0.088)	(0.068)
Foreign born	0.653	0.246
	(0.126)	(0.137)
Median earnings (in 100 SEK)	1,282	2,353
	(351)	(661)
Number of residents	1,361	1,263
	(275)	(284)

Note: All descripive statistics refer to year 2015. Median earnings is based on earnings for all residents including those with no taxable income. Standard deviations are reported in parentheses.

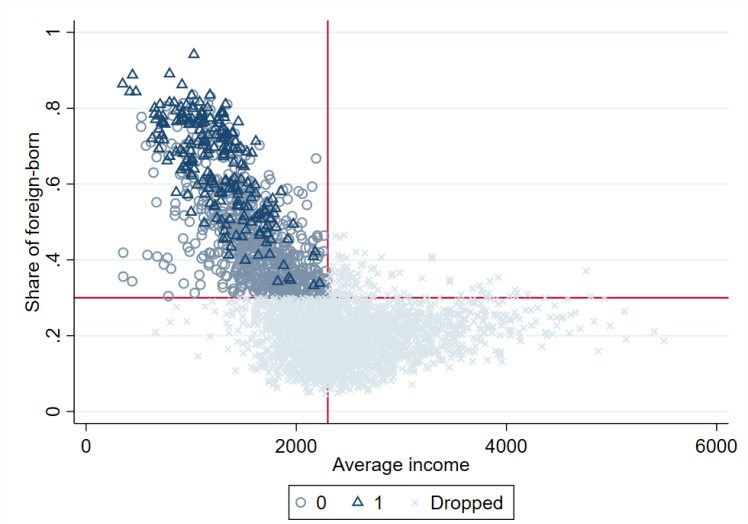
D Robustness

Table D.1: Probing the Accuracy of the Definition of Treatment

	Our definition of treatment		Based on exact location of residence	
	Listed neighborhoods (1)	Rest of Sweden (2)	Listed neighborhoods (3)	Rest of Sweden (4)
Age distribution				
20-24 years	12.1	10.2	12.2	10.2
25-44 years	52.9	46.2	52.6	46.2
45-64 years	35.0	43.6	35.1	42.6
Labor market				
Employment	60.5	80.4	61.4	80.2
Employment -men	63.7	81.7	65.5	81.2
Employment - Women	56.8	79.1	57.0	79.2

Notes: The descriptive statistics in columns (1)-(2) are based on our own complete count data of the Swedish working population in 2018. Our definition of a "Listed neighborhood" is that 50 percent of the land area of one's neighborhood of residence (DeSO), falls within the boundaries of a troubled neighborhood as listed by the police. "Rest of Sweden" in columns (2) and (4) refers to all neighborhoods in Sweden other than listed ones. Columns (3)-(4) report equivalent statistics computed by Statistics Sweden based on exact coordinates of residence and exact delimitations of listed neighborhoods provided by the police authority.

Figure D.1: Sample Selection Based on Pre-Policy Characteristics



Notes: This figure plots neighborhood status (non listed, listed, or dropped) for a robustness exercise in sample selection. Each dot is an urban neighborhood. Triangular dots are listed neighborhoods, circular dots are non-listed neighborhoods with similar pre-policy characteristics (in the share of foreign born residents, and average monthly labor income in Swedish Kronas), and cross dots are dropped neighborhoods, whose pre-policy characteristics are different to those in listed neighborhoods.