The Punishment of Place: Neighborhood Labeling and Criminal Sentencing*

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October 28, 2026

Abstract

In 2015, the Swedish National Police unexpectedly began publishing a formerly internal report listing "troubled neighborhoods." The release of the list and its subsequent updates have played a crucial role in shaping a discourse in both national and international media of Swedish so-called "no-go zones." We verify empirically that the release of the list led to a marked increase in negative media coverage of listed neighborhoods, both in the number of articles and in the negativity of their tone, compared with non-listed neighborhoods. We then combine administrative registers with written court judgments to study whether this salience shock affected criminal sentencing. We implement a regression discontinuity design around each publication date and focus on defendants whose trials had already been scheduled within a narrow window around the release or update. Among defendants sentenced to prison, imposed sentence length increased by approximately 260 days immediately after publication. The effect fades as media attention declines and is smaller in courts where more judges have previously lived in listed neighborhoods. Affected defendants also experience a 16-percentage-point reduction in employment over the three years following sentencing. Drawing on a corpus of more than 270,000 court documents, we then ask how courts responded. We recover the factors courts record at each stage of the statutory sentencing sequence from their written reasons and decompose the sentencing response across them. Recorded case characteristics account for about 30 percent of the response. The remaining 70 percent is additional punishment attached to cases the courts describe no differently. The response is therefore consistent with courts reweighting an unchanged set of inputs when a cue becomes salient, as models of limited attention predict, rather than with updated beliefs about the cases themselves.

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

*We thank Marianne Bitler, Christine Jolls, and Olivier Marie for helpful comments, as well as conference participants at the Chicago/LSE Conference on the Economics of Crime and Justice (Chicago, 2025), the NBER Law and Economics Spring Meeting (Cambridge, MA, 2026), the CESifo Area Conference on Labor Economics (Munich, 2026), the 17th Transatlantic Workshop on the Economics of Crime (Cornell, 2026), the IFS conference on Economic Opportunities by Race and Ethnicity, and the IV Workshop on Public Policies (IEB, Barcelona), and seminar participants at the IFS, Universidad de Montevideo, and the University of the Basque Country. Domínguez, Grönqvist, and Niknami gratefully acknowledge funding from the Swedish Research Council. Hertegård gratefully acknowledges funding from the Jan Wallander and Tom Hedelius Foundation (grant W23-0004). [The project has been approved by the Swedish Ethical Review Authority. The authors used AI tools for proofreading text and code and take full responsibility for the content.

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1. Introduction

Where people live shapes how their lives unfold. A large literature shows that growing up in, or moving to, a disadvantaged neighborhood lowers earnings, schooling, and health and raises criminal involvement.[1] Far less is known about *why*. Most of this work studies what happens within neighborhoods. We study a largely overlooked channel: how outside institutions respond to a neighborhood's reputation once it is made salient. Some neighborhoods come to be seen as dangerous, disorderly, or criminal [Sampson and Raudenbush, 2004, Wacquant, 2007, Besbris et al., 2015], and because an address is observable to anyone who deals with a resident, it can act as a collective reputation in the sense of Tirole [1996]. If institutions treat residents on the basis of their address rather than their conduct, part of the neighborhood penalty arises through reputation rather than through the resources, networks, or crime that residents actually face [Bertrand and Mullainathan, 2004, Aaronson et al., 2021].[2] Causal evidence on this channel is scarce, for two reasons. A neighborhood's reputation is endogenous to the conditions that produced it—poverty, crime, weak institutions, and minority concentration move together [Kling et al., 2007, Chyn and Katz, 2021]. And events that make a reputation salient—redlining maps, zoning designations, official "ghetto" lists, targeted policing—typically change material conditions at the same time.

This paper provides causal evidence that making a neighborhood's reputation salient changes how its residents are treated by a high-stakes institution: the criminal courts. We exploit a setting in which the salience of a set of neighborhoods' reputations changed sharply and publicly on known dates, while resources, enforcement, and the legal facts of individual cases did not. In December 2015, the Swedish police unexpectedly published a formerly internal situation report listing so-called "troubled neighborhoods" (*utsatta områden*).[3] The report had been used internally to monitor areas

[1] See Kling et al. [2007], Ludwig et al. [2013], Chetty et al. [2014], Chetty and Hendren [2018], Chyn and Katz [2021], and Disney et al. [2026]. On mechanisms within neighborhoods, see Billings and Schnepel [2022] and Chetty et al. [2022] on peers and networks, and Sampson [2012] and Sampson [2026] on local institutions.

[2] Bertrand and Mullainathan [2004] find that an address in a better neighborhood raises callbacks for otherwise identical job applicants, and Aaronson et al. [2021] show that the 1930s redlining maps had lasting effects on homeownership, house values, and segregation decades after they were drawn. Zenou and Boccard [2000] model the interaction of race and space in labor markets. See also Tootell [1996] on mortgage redlining, Aaronson et al. [2023] on the long-run effects of redlining maps, and Yamagishi and Sato [2025] on persistent spatial stigma in Japan.

[3] 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?`).

characterized by long-run disadvantage and elevated crime-related concerns. Once public, the list became a highly visible marker of place-based disorder: the listed neighborhoods were repeatedly described in domestic and international media as "no-go zones" and displayed on maps in news reporting.[4] The publication and its subsequent updates therefore constitute a rare, dated shock to the salience of an existing reputation that left the underlying neighborhoods unchanged in the short run.

Courts are a natural place to look for the consequences of such a shock. Judicial decision-making takes place under information complexity, time pressure, and limited attention. In such settings, decision-makers place disproportionate weight on salient and readily observable cues, allowing formally irrelevant signals to influence outcomes [Tversky and Kahneman, 1974, Bordalo et al., 2012, 2015]. Recent evidence confirms that criminal courts respond to such cues: Hanemaaijer et al. [2024] show that Dutch judges imposed markedly longer sentences on defendants of Moroccan descent after a high-profile event raised the salience of that group. A defendant's address is one such cue, and an official label backed by intense media coverage is exactly the kind of event that raises its salience. Courts are also where the stakes of such a cue are highest: a longer prison sentence is costly in itself and has lasting consequences for employment and family life [Mueller-Smith, 2015, Bhuller et al., 2020, Dobbie et al., 2018b]. And because courts adjudicate cases arising from their own jurisdictions, any place-based penalty falls on exactly the communities that already bear the greatest disadvantage, so that salience in sentencing can feed back into the geographic inequality from which it arises [Finlay et al., 2023, Chetty and Hendren, 2018].

Our empirical strategy exploits the exact timing of the public release and subsequent updates of the police list. We implement a regression discontinuity design around each publication date and focus on defendants whose trials had already been scheduled within a narrow 10-week window around the release or update. This design compares otherwise similar defendants from listed neighborhoods whose cases were adjudicated immediately before versus immediately after the public label became salient. Because trials were scheduled before publication, the timing of adjudication is unlikely to reflect changes in defendant behavior, local crime conditions, neighborhood composition, police

[4] See, e.g., coverage in *Newsweek* (`https://www.newsweek.com/sweden-police-vulnerable-areas-no-go-zones-628029`) and the *Wall Street Journal* (`https://www.wsj.com/articles/how-peaceful-sweden-became-europes-gun-murder-capital-a5b50`). These narratives were further disseminated through international alternative media; see, e.g., `https://www.breitbart.com/europe/2017/06/29/sweden-police-commissioner-begs-help-no-go-zones-rises/?`.

resources, or other place-based interventions induced by the report. The design does not require that listed neighborhoods were otherwise unchanged in a broad sense; it requires only that no other factor changed discontinuously at the exact publication dates in a way that differentially affected already-scheduled cases from listed neighborhoods.[5] We use population-based administrative data with geocoded information on defendants' neighborhoods of residence, court outcomes, criminal histories, demographic characteristics, and labor-market trajectories, and link defendants to the neighborhoods identified in each police report.

We first document that publication generated a sharp negative salience shock for the listed neighborhoods. Using the universe of Swedish-language online media articles mentioning these neighborhoods since February 2015, we classify articles by tone and compare coverage of listed and non-listed neighborhoods. Each release and update of the list coincides with a spike in the number of articles mentioning the listed neighborhoods that does not track general crime coverage, and the tone of that coverage turns markedly more negative: the gap in our negativity index between listed and non-listed neighborhoods widens from a statistically insignificant 10 percentiles before publication to roughly 50 percentiles by 2019. This pattern is consistent with contemporary accounts describing the police report as central to the emergence of a Swedish "no-go zone" narrative [Backvall, 2019, Riksrevisionen, 2020]. The listing changed not only the availability of information about these neighborhoods but also the salience and framing of that information in public discourse.

We then show that defendants from listed neighborhoods receive substantially longer sentences immediately after their neighborhood is publicly listed. Average sentence length increases by more than 60 percent, or roughly 260 days, relative to a pre-listing baseline of 378 days. The effect on sentence length appears immediately after publication and fades as media coverage declines, consistent with a salience-based interpretation. It is significantly smaller in courts where a larger share of judges have themselves lived in a listed neighborhood, suggesting that first-hand familiarity attenuates the weight placed on the public label. Ethnic minorities and native Swedes residing in listed neighborhoods are affected to a similar extent, indicating that the cue is the place itself rather than minority status. Finally, the penalty has lasting consequences: affected defendants experience a 16-percentage-point decline in employment over the three years following sentencing.

[5] Nevertheless, in Appendix B we show that observable neighborhood characteristics, police resources, and other local policy responses exhibit no corresponding discontinuities around the publication dates.

We then open the black box of judicial reasoning. Using the full text of written judgments around publication of the 2017 list, we construct a difference-in-differences comparison of 3,661 prison sentences from listed and never-listed neighborhoods across 15 courts. Publication increases the imposed prison term by 2.684 months (s.e. 0.825), about 40 percent of the pre-publication mean, with pre-publication event-study coefficients jointly indistinguishable from zero. We then ask what in the written record accounts for this response. Applying an algorithmic extraction of the statutory sentencing framework according to the Swedish Criminal Code—baseline penal value of the gravest conviction, aggravating circumstances, mitigating circumstances, and other adjustments—and a Gelbach [2016] decomposition on the same sample and specification, we find that measured offense and case characteristics account for roughly 30 percent of the sentencing coefficient, while documented aggravating and mitigating circumstances contribute essentially nothing. Importantly, about 70 percent of the sentence increase remains unaccounted for by anything the courts recorded in their judgments. While the residual cannot be read as a measure of discrimination, it does establish that the sentencing response is not visible in the courts' stated reasoning: judges did not write longer sentences by citing more aggravating circumstances or fewer mitigating ones. The salience shock thus operated largely through the punishment attached to otherwise similarly described cases.

Our results contribute to several literatures. First and foremost, we add to the literature on neighborhood effects. That literature has made substantial progress in identifying causal place effects [Kling et al., 2007, Chetty et al., 2014, Chyn and Katz, 2021], but because neighborhood characteristics are deeply intertwined, it has been far harder to isolate specific mechanisms even when research designs account for sorting on unobservables. Identifying the role of place-based reputation is particularly challenging because even quasi-randomly assigned place-based interventions convey both an intervention and a salient signal that these locations require special attention. A handful of studies have examined place-based stigma in the context of redlining and credit access (Tootell 1996; Aaronson et al. 2021, 2023; Fishback et al. 2023), redrawn zoning maps for subsidized neighborhoods [Garrouste and Lafourcade, 2026], official neighborhood designations in Scandinavia [Govind et al., 2024, Damm et al., 2025, Foged et al., 2025, Andersson et al., 2026], historically stigmatized districts in Japan [Yamagishi and Sato, 2025], and housing values [Koster and van Ommeren, 2023, Besbris et al., 2015]. Address-based treatment has also been documented in labor-market audit studies

[Bertrand and Mullainathan, 2004]. These studies examine how markets and residents respond to a label; none isolates how an outside institution making high-stakes decisions about individuals responds to it. The settings also differ from ours in an important respect. Redlining maps, zoning designations, and ghetto lists are instruments of policy and typically come bundled with changes in credit, subsidies, housing allocation, or enforcement, so estimates in this literature generally capture the combined effect of the label and the measures attached to it. Our setting separates the two: the police list publicly labeled specific neighborhoods without mechanically changing the legal facts of individual cases. Combined with a timing-based design around already-scheduled trials, this provides, to our knowledge, the first causal evidence that the salience of a neighborhood's reputation shapes high-stakes institutional decisions, independently of residents' own behavior and case characteristics.[6] Importantly, our finding that the penalty falls equally on natives and minorities within listed neighborhoods also speaks to the long-standing "race versus space" debate over the sources of persistent minority disadvantage, a question that has been difficult to answer because minorities sort into areas that are themselves associated with worse outcomes [Zenou and Boccard, 2000].[7] Our evidence indicates that place carries a penalty of its own.

Second, we contribute to a small but growing literature on how salient events influence criminal justice decisions. Hanemaaijer et al. [2024], McConnell and Rasul (2021, 2024), and Grosjean et al. [2023] study how heightened media attention affects minority groups; Philippe and Ouss [2018] and Shayo and Zussman [2011] examine how crime salience in France and terrorism in Israel shape judicial behavior; and Stricot [2026] shows that television coverage of violence against women raises prosecution rates for unrelated cases of such violence in the following week. The salient cue in this work is a group, a type of crime, or the general threat environment. We study an event that raised the salience of *places*, and we show that the response was uniform across residents of those places. This distinguishes place-based from group-based salience and suggests that the two can operate

[6] 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 unemployment is high, an early indication that judicial decisions respond to local context beyond the facts of the case.

[7] Zenou and Boccard present a theoretical model in which both racial identity and spatial location matter for explaining high unemployment rates among Black workers. A large empirical literature documents racial and ethnic discrimination within the criminal justice system [Ahrsjö et al., 2024, Arnold et al., 2018, 2022, Abrams et al., 2012, Jordan et al., 2023, Hanemaaijer et al., 2024, Tebes and Fagan, 2022, Yang, 2015, Goncalves and Mello, 2021, Ba et al., 2021, Ang, 2021, Rose, 2021, Bindler et al., 2024, Agan, 2024].

through different channels.

Third, we contribute to the literature on "excess sentencing" and geographic disparities in criminal justice [Topaz et al., 2023]. Even within the same legal system, residents of different areas face sharply different punishment, and these differences persist after controlling for crime severity and criminal history.[8] Proposed mechanisms include judges relying on group-level risk beliefs under imperfect information, beliefs that may vary with local experience or media narratives [Arnold et al., 2022]; differences in judge punitiveness [Kling, 2006, Abrams et al., 2012]; administrative pressure through rules and incentives [Yang, 2014]; and differences in ideology and electoral accountability [Ash et al., 2026, Anwar et al., 2019, Huber and Gordon, 2004, Berdejó and Yuchtman, 2013]. Persistent district-level differences that survive personnel changes point to locally stable institutional equilibria rather than contemporaneous crime conditions [Abrams et al., 2012, Rehavi and Starr, 2014].[9] We identify the salience of neighborhood reputation as a distinct mechanism behind excess sentencing and show that it carries a long-run labor-market penalty for those who bear it.

Finally, we contribute to the growing literature using text as data in economics [Gentzkow et al., 2019, Ash and Hansen, 2023, Ludwig et al., 2026, Modarressi et al., 2025]. Existing work on courts uses judicial text mainly to measure judges' attitudes or ideology [Ash et al., 2026], while decompositions of sentencing disparities across stages of the process rely on administratively coded guideline factors [Rehavi and Starr, 2014, Yang, 2015]. Sweden has no such coding: the factors a court relies on exist only in its written reasons, which the Code of Judicial Procedure requires it to state.[10] We use the statutory sequence of the sentencing decision—penal value, aggravating and mitigating circumstances, and adjustments—to recover those factors from the judgments and decompose a causally identified sentencing response across them. To our knowledge, this is the first

[8] In Chicago, neighborhood incarceration rates range from under 500 to over 4,000 per 100,000 residents, and some neighborhoods send residents to prison at more than three times the rate of neighborhoods with comparable crime levels [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 [United States Sentencing Commission, 2020]. 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 in a given year, compared with 0.08 percentage points in neighborhoods around the 50th percentile.

[9] A large literature documents the effects of incarceration on subsequent outcomes in the United States [Mueller-Smith, 2015, Agan et al., 2023, Garin et al., 2023, Humphries et al., 2023], Norway [Bhuller et al., 2020], and Sweden [Dobbie et al., 2018b, 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. [2018a] on pre-trial detention.

[10] Chapter 30, Section 5 of the Code of Judicial Procedure (*Rättegångsbalken*) requires a criminal judgment to state the reasons for the decision (*domskäl*). Appellate review runs on this written record.

paper to do so. The approach separates three margins on which an institution can respond to a salient cue—how the case is characterized, which circumstances are accepted, and the punishment attached to a given description—and is portable to any system with reasoned judgments and a statutory sentencing structure. In our setting, the response falls almost entirely on the third margin, which also means it leaves no trace that appellate review of the written record could detect.

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 register-based estimation results, the analysis of written judgments, and the long-run consequences of listing. Section 5 concludes.

2. Background

This section discusses relevant institutional details regarding the list of troubled neighborhoods. We 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. We also summarize the features of the Swedish criminal justice system that matter for interpreting our results.

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.[11]

The list was compiled by combining objective statistics with careful assessment by the local

[11]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/

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 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.

The publication of the list was not accompanied by any immediate policy response. Internal police reports and the National Audit Office found no sustained increase in police manpower or patrols in listed neighborhoods, and only three of the 28 affected municipalities adopted an action plan [Hultqvist and Ygge, 2018, Riksrevisionen, 2020]. In Appendix B we corroborate this with matched employer–employee data: staggered difference-in-differences estimates show no change in headcount, contracted hours, or overtime at the police stations nearest to listed neighborhoods, nor in private security staffing, and conviction rates in listed and non-listed neighborhoods move in

parallel throughout 2010–2021. Our research design does not rely on this absence of response, since it compares cases already scheduled for trial within weeks of each publication date, but it rules out the possibility that the list operated in practice as a resource intervention.

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."[12] Since its introduction, major media outlets have consistently covered the listed neighborhoods, often making them the focal point of news coverage [Backvall, 2019].[13]

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, 2018]. 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.[14] 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

[12]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.

[13]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. 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."

[14]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."[15] 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

[15]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; Riksrevisionen 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 The Swedish Criminal Justice System

[16]

Sweden has a three-tier court system in which the vast majority of criminal cases are decided in one of 48 district courts, each responsible for the cases arising in its geographic jurisdiction. Three features of the system matter for interpreting our results.

First, there is no plea bargaining. The prosecutor decides whether to charge, and once a case proceeds to trial both the verdict and the sentence are determined at trial by a professional judge together with three lay jurors (*nämndemän*), who are assigned to cases and judges in a quasi-random fashion [Ahrsjö et al., 2024].[17] Of charged cases, about 77 percent result in a trial; the remainder are typically petty offenses settled on confession. Of tried cases, roughly 23 percent end in incarceration, 37 percent in a fine, 24 percent in probation, 10 percent in other sanctions, and 6 percent in acquittal (Appendix Figure C.2).[18] Sentence length, which is determined by the court within the statutory range for the offense, is therefore the margin on which judicial discretion is greatest. Incarceration rates in Sweden are low and stable by international standards, and more

[16] This section draws on Dobbie et al. [2018b].

[17] Lay jurors are nominated by political parties but are rarely themselves elected representatives; in the roster of lay jurors at Stockholm District Court, only 7.1 percent could be matched by name to individuals elected to municipal, regional, or national assemblies in 2024. Each lay juror works approximately 10 to 15 days per year. Jurors indicate their availability for fixed dates in the upcoming year and are assigned to dates before any knowledge of the cases to be tried; a court administrator then forms juror triplets and assigns them to courtrooms. See Anwar et al. [2019] and Ahrsjö et al. [2024] for details. If the judge and the lay jurors disagree on the verdict, a vote is held in which all votes have equal weight and the judge holds the tiebreaker; if the defendant is found guilty, a second vote determines the sentence, with the least severe option chosen in case of an even split.

[18] 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 pretrial detention decision is made by a randomly assigned judge at the time of arrest, independently of the assignment of the trial judge. Individuals with prison sentences of three months or less can ask the prison and probation authorities to serve their sentence at home under electronic supervision; this decision does not involve the sentencing judge.

than 80 percent of prison sentences are shorter than one year (Appendix 1.2).

Second, the defendant's place of residence is visible to the court. It appears in the criminal investigation file and in any statements from social services, and it is stated in the judgment itself, typically together with the name of the residential area.[19]

Third, district court judges are permanently appointed civil servants with a law degree. Judges are assigned to a section (*rotel*) at the beginning of each year and many remain in charge of the same section for several years. This stability allows us to link judges to courts and to their own residential histories in the administrative registers, which we use when examining heterogeneity by judges' exposure to listed neighborhoods.

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.[20] 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 National Council for Crime Prevention (see `www.bra.se`). The crime data include information on

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

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

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).[21] 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.[22]

Figure 3a shows the geographical distribution of the police drawn maps in Stockholm municipality and Figure 3b zooms in on one of these neighborhoods in Stockholm. It is clear from Figure 3a 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.[23]

Ideally, we would be able to exactly identify if a defendant lives inside the areas identified as a "troubled neighborhood." However, the administrative data only provide their neighborhood

[21]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.

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

[23]We follow [**(alias?)**] 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.

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 3b 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.[24] 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:

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

where Outcome_i is the outcome of interest. The model assumes that the unobserved characteristics

[24] 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 [Stiftelsen The Global Village, 2018]. 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.

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).[25]

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.

As shown in Section 2 and Appendix B, 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, nor with any immediate policy response. 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.

[25] Although we do not observe the charge-filing date in our data, official court statistics indicate that 75 percent of criminal cases in Swedish district courts in 2024 were resolved within 3.1 months of filing, normally defined as the date on which the indictment application was received by the court. This 75th percentile is the Swedish National Courts Administration's standard performance measure for case processing times.

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 4 shows that the relative frequency of cases is similar on both sides of the cutoff, and the McCrary test in Figure 5 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 6 and 7 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, Philippe and Ouss [2018] find that media coverage of crimes in France leads to harsher sentencing the following day, and McConnell and Rasul [2021] 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; McConnell et al. [2024] document a similar

[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.

response by parole boards.

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 8 presents results from an analysis in which we extended the doughnut from –20 to +10 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.4 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 Neighborhood labeling and judicial reasoning

The register results establish that the salience shock lengthened sentences. They do not say *how*. A court can respond to a salient cue on three margins: by characterizing the case differently (what offense is proven, how many counts), by accepting different circumstances (which aggravating and mitigating factors it finds), or by attaching more punishment to a case it describes the same way. The distinction matters for interpretation. If the response ran through the first two margins, it would be consistent with judges updating their assessment of the facts or the defendant; if it runs

[27]A large literature studies racial and ethnic discrimination in the criminal justice system [Arnold et al., 2022, Abrams et al., 2012, 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].

through the third, it is consistent with a salient cue changing the weight on otherwise unchanged inputs, which is what limited-attention accounts predict. Because Swedish courts must state their reasons in writing, we can observe all three margins. We therefore complement the register analysis with the full text of written judgments, extracting the factors the court records at each stage of the statutory sentencing sequence and decomposing the sentencing response across them.

We focus on the police list published on 21 June 2017. The treated group consists of defendants whose residential addresses fall within areas designated *särskilt utsatta områden* on that list. Controls reside in areas that never appear on any of the four lists used in constructing the comparison group. This comparison includes both newly listed and previously listed areas; the coefficient therefore captures the response to publication of the 2017 list, including renewed publicity, rather than the effect of first listing alone. The underlying corpus contains 270,580 court-file documents of different types and dates. The estimation sample is narrower: single-defendant criminal judgments with usable residential addresses, known gender and age, and courts that hear cases from the treated group during the event window. Addresses identifying remand prisons are excluded. For the prison-duration analysis, we retain finite prison sentences for which the sentencing reasoning has been processed and the offense measures required for the decomposition are observed. The main difference-in-differences (DiD), event study, and decomposition all use the same 3,661 sentences, including 450 from listed neighborhoods, across 15 courts. These restrictions differ from the broader register sample, so the estimates need not have the same magnitude as the register-based RD effects.

The substantive sentencing framework is provided principally by Chapters 29 and 30 of the Swedish Criminal Code (*Brottsbalken*), together with offense-specific provisions and case law [Justitieutskottet, 2018, Regeringen, 1988]. We organize the measured factors into four blocks that follow the statutory sequence. The first measures baseline penal value using the empirical tariff and statutory maximum of the gravest conviction, an indicator for an offense against the person, injured persons listed, and the number of counts convicted; the empirical tariff is the mean log prison term for that crime outside the 2017 event window. This block combines offense composition and conviction outcomes, and its components need not be predetermined with respect to publication. The remaining blocks contain documented aggravating circumstances under Chapter 29, section 2; mitigating circumstances under Chapter 29, section 3; and other adjustments under

Chapter 29, sections 4–7 and Chapter 30. The factor extractor uses regular-expression rules to identify circumstances recorded as findings by the court; factors found fewer than 20 times are pooled into a count within their block. These measures concern recorded sentencing factors, not a complete reconstruction of deliberation. Appendix E summarizes the measurement blocks.

Let q index 91-day intervals anchored on 21 June 2017. We retain $q = -3, \ldots, 3$, giving three prepublication and four postpublication intervals, with publication at the start of $q = 0$. For defendant i, tried by court c in calendar year t, we estimate

$$Y_{icq} = \alpha_{ct} + \beta L_i + \delta P_q + \tau^R (L_i \times P_q) + X_i'\gamma + \varepsilon_{icq}, \tag{2}$$

where Y_{icq} is the imposed prison term in months, conditional on a finite prison sentence; L_i identifies the listed group; $P_q = \mathbf{1}\{q \geq 0\}$; and X_i contains gender, age, and age squared. The court-by-calendar-year fixed effects, α_{ct}, absorb changes shared by cases heard in the same court and year, and the listed indicator absorbs the average level difference between the groups. Standard errors are clustered by court, with $t(14)$ critical values for the 15 clusters. Identification requires parallel changes in sentencing for the listed and never-listed groups in the absence of publication, conditional on the included controls and fixed effects, and that changes in case composition, residential-address coverage, and selection into the observed prison-sentence sample do not generate the estimated response. For the event study, we replace $\tau^R (L_i \times P_q)$ in equation (2) with

$$\sum_{k=-3,\, k\neq -1}^{3} \tau_k L_i \mathbf{1}\{q = k\}, \tag{3}$$

retaining the other regressors and normalizing the final prepublication interval, $q = -1$, to zero.

To account for the sentencing coefficient, we apply the decomposition of Gelbach [2016], which attributes the change in τ when the four blocks are added to the specification to each block, using the same sample, controls, and fixed effects throughout.[28]

[28] Let V_{ig} contain the measures in block $g \in \{B, A, M, S\}$: baseline penal value, aggravation, mitigation, and other adjustments. The full model adds all four blocks jointly to equation (2):

$$Y_{icq} = \widetilde{\alpha}_{ct} + \widetilde{\beta} L_i + \widetilde{\delta} P_q + \tau^{\text{full}} (L_i \times P_q) + X_i'\widetilde{\gamma} + \sum_g V_{ig}'\eta_g + u_{icq}. \tag{4}$$

Table 12 reports the main prison-months DiD. Publication increases the imposed prison term by 2.684 months (court-clustered standard error 0.825), with a 95 percent confidence interval of $[0.914, 4.454]$; the prepublication means are 6.603 months in the never-listed group and 6.361 months in the listed group. Appendix Figure F.1 shows the dynamic estimates. The prerelease coefficients are not jointly distinguishable from zero ($p = 0.831$), although their confidence intervals are wide; the postpublication coefficients are positive and jointly significant ($p = 0.004$). The event study does not reproduce the short-lived response found in the register RD: its point estimates remain positive over the displayed postpublication horizon, and the design, sample, and time scale differ. The judgment analysis thus provides an independent replication of the register result, in a different sample and with a different identification strategy.

Table 13 reports the decomposition. Baseline penal-value measures contribute 0.807 months, or 30 percent of the initial coefficient. Aggravating circumstances contribute −0.081 months (−3 percent), mitigating circumstances −0.051 months (−2 percent), and other adjustments 0.139 months (5 percent). The measured contributions together account for 0.814 months, leaving 1.870 months, or approximately 70 percent, in the remaining coefficient. The decomposition is imprecise: the bootstrap 95 percent intervals for each block and for the remaining coefficient include zero, so the 70 percent share is a point estimate rather than a precisely identified quantity, and the first block cannot distinguish a change in the underlying cases from a change in their judicial classification.

The pattern is nevertheless informative about the margins of response. The largest measured contribution lies in baseline penal value, which combines offense composition and conviction outcomes. The aggravating and mitigating circumstances the courts cite contribute essentially

For each added measure j, an auxiliary regression uses the same baseline design:

$$V_{ij} = a_{ct,j} + b_j L_i + d_j P_q + \pi_j (L_i \times P_q) + X_i' \rho_j + \nu_{ij}. \tag{5}$$

The contribution of block g is $\widehat{C}_g = \sum_{j \in g} \widehat{\pi}_j \widehat{\eta}_j$, and because the reduced, full, and auxiliary regressions use identical observations, controls, and fixed effects, $\widehat{\tau}^R = \widehat{\tau}^{\text{full}} + \sum_g \widehat{C}_g$. Contributions are measured in prison months; shares divide each contribution by the initial DiD coefficient and are shares of the coefficient, not of the variance in individual sentences. Uncertainty is estimated with 499 pairs cluster-bootstrap draws at the court level, re-estimating the full decomposition in each draw. The sentencing coefficients in the full model need not identify the causal effects of the measured factors on punishment, so the decomposition is an accounting exercise rather than a causal mediation analysis [Imai et al., 2010]. Conviction characteristics and recorded legal factors may themselves respond to neighborhood labeling, and the remaining coefficient may reflect omitted legal circumstances, incomplete documentation, extraction error, nonlinearities, or changes in the weight placed on measured circumstances. It is not a measure or lower bound of discrimination.

nothing: judges did not reach longer sentences by finding more aggravating or fewer mitigating circumstances. The bulk of the response is additional punishment attached to cases the courts describe no differently. Because both conviction outcomes and legal assessments may respond to labeling, neither the accounted-for portion nor the remaining coefficient can be classified as lawful treatment or discrimination from the decomposition alone. But the response is consistent with courts reweighting an unchanged set of inputs when a cue becomes salient, as limited-attention models predict, rather than with updated beliefs about the cases themselves—and because it is absent from the stated reasons, it is a response that appellate review of the written record cannot detect.

4.5 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.

5. Conclusion

The 2015 release of a police situation report publicly listing "troubled neighborhoods" sharply raised the salience of these neighborhoods' reputations without any accompanying change in local resources, enforcement, or the legal facts of individual cases. Using data from various sources, we show that the introduction of the list led to a marked increase in negative media coverage of the listed neighborhoods, both in the number of articles and in the negativity of their tone.

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 and attenuated in courts where more judges have themselves lived in listed

neighborhoods. Natives and ethnic minorities residing in listed neighborhoods were affected to a similar extent, indicating that the cue is the place itself rather than minority status. This pattern aligns with models of decision-making under limited attention: when exposed to salient shocks, courts may overweight widely publicized information—even when it bears 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.

The analysis of written judgments shows how courts responded. Recovering the factors courts record at each stage of the statutory sentencing sequence from their written reasons, and decomposing a difference-in-differences estimate of 2.684 additional prison months around the 2017 list, we find that measured offense and conviction characteristics account for about 30 percent of the response and that the aggravating and mitigating circumstances courts cite account for none of it. The remaining 70 percent is additional punishment attached to cases the courts describe no differently. The decomposition is imprecise and cannot classify the residual as discrimination. But it establishes that judges did not justify longer sentences by finding the facts or the law to be different: the response is consistent with courts reweighting an unchanged set of inputs when a cue becomes salient, rather than with updated beliefs about the cases themselves. It is also a response that leaves no trace in the stated reasons on which appellate review relies.

These findings demonstrate that making a neighborhood's reputation salient 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 disparities 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 the salience of place-based reputation as a meaningful, and previously underappreciated, mechanism behind neighborhood effects, one that operates outside

the neighborhood itself through the institutions its residents encounter. 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. Because the response we document is invisible in the written reasons, it is also unlikely to be corrected by appellate review; standardized sentencing guidelines that reduce judicial discretion, or judicial training on the effects of cognitive biases, may be more promising remedies.

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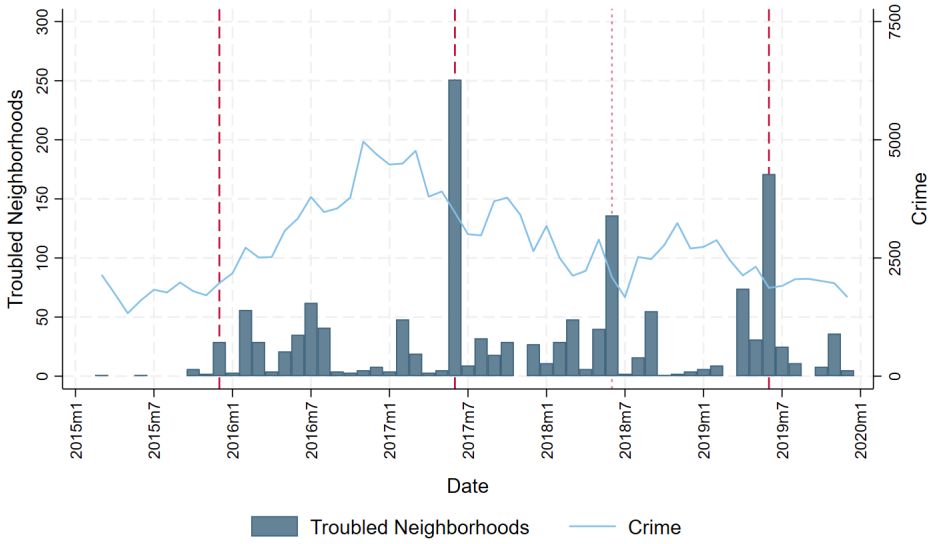
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Figures and tables

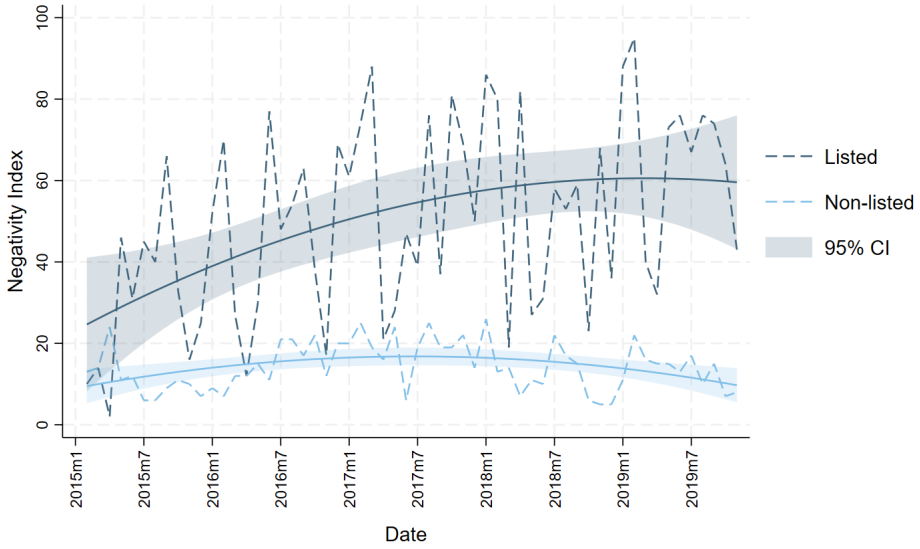
Result figures

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/`.

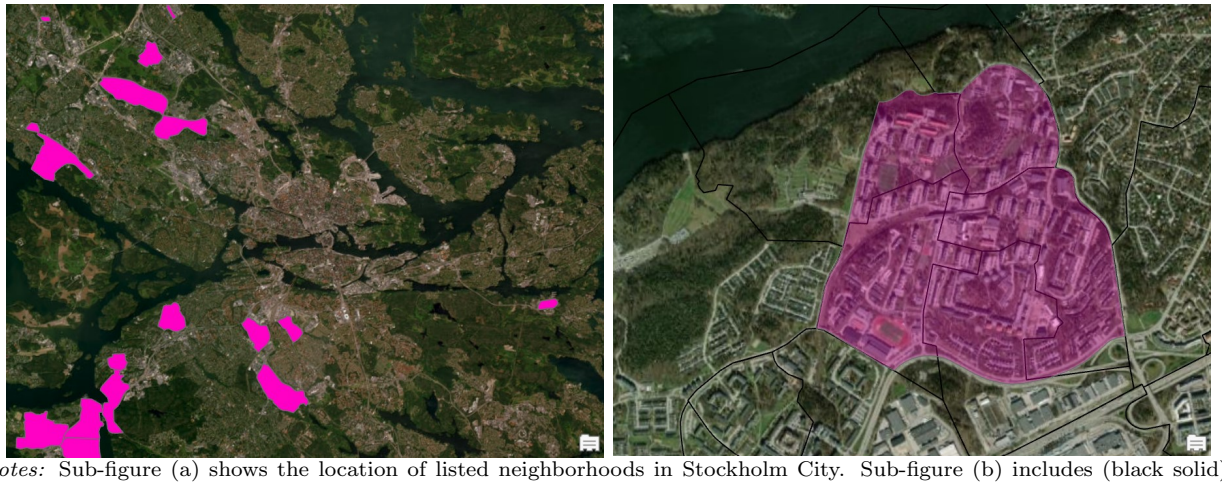
Figure 2: Negativity Index of Media Tone - Listed and Non-Listed Neighbourhoods



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: 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/.

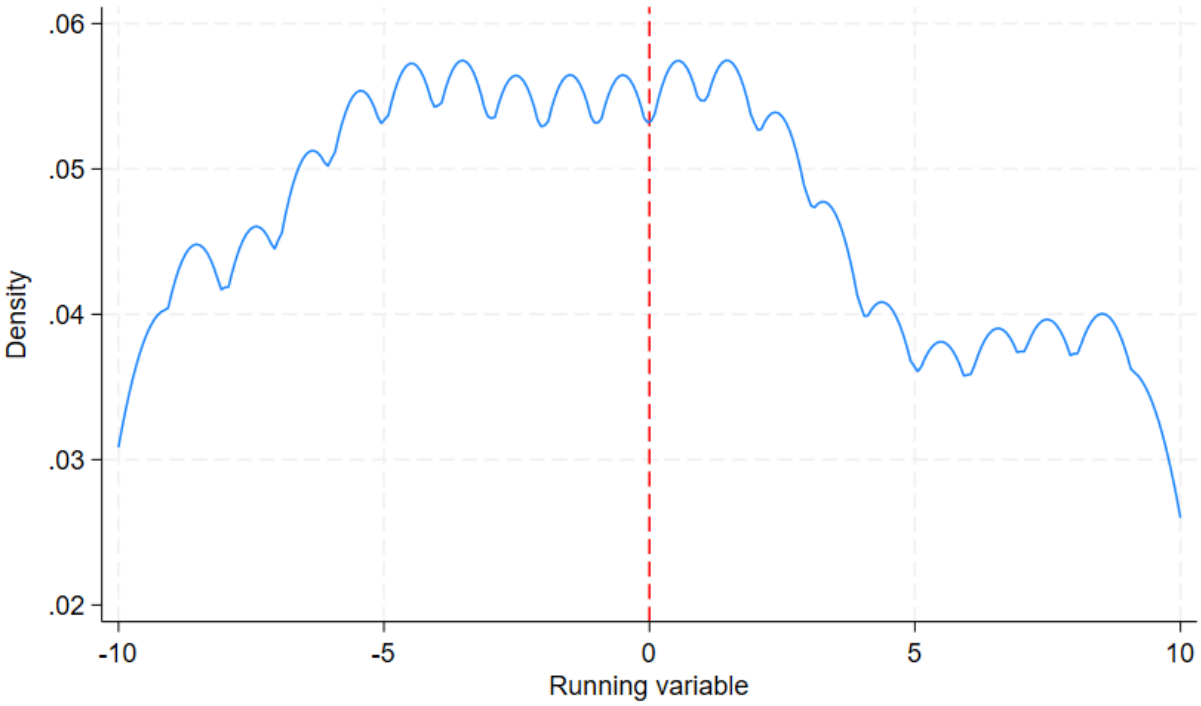
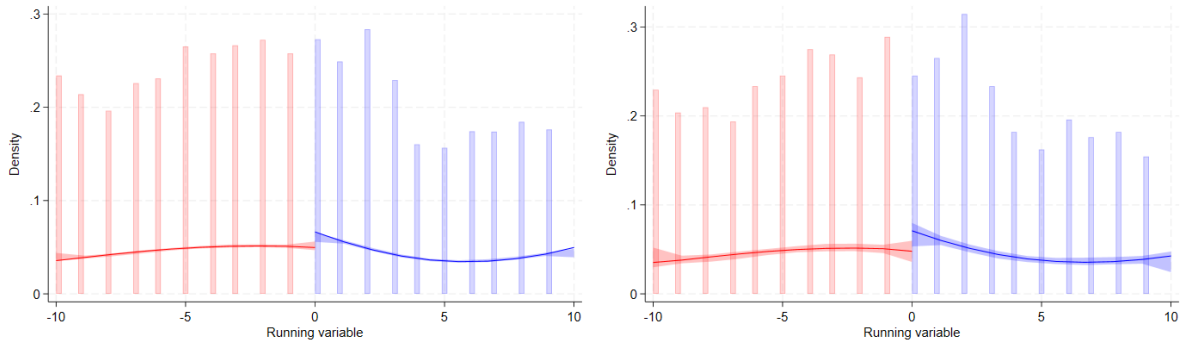


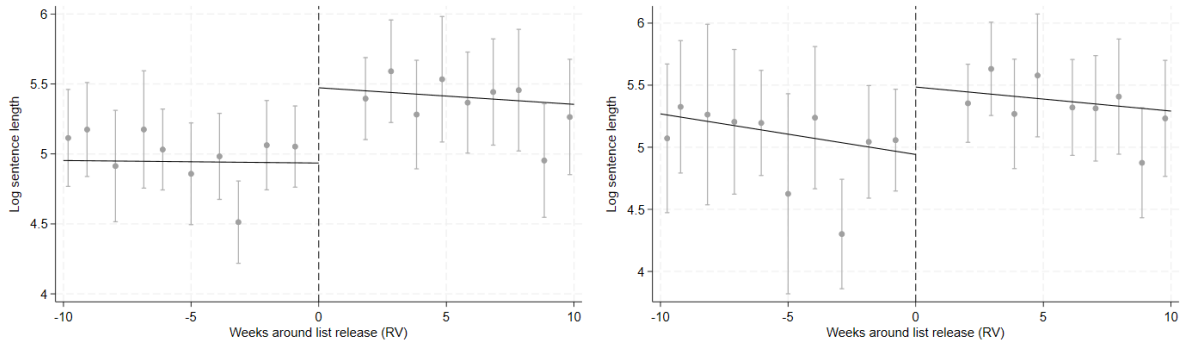
Figure 4: Density of cases around list release.



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

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

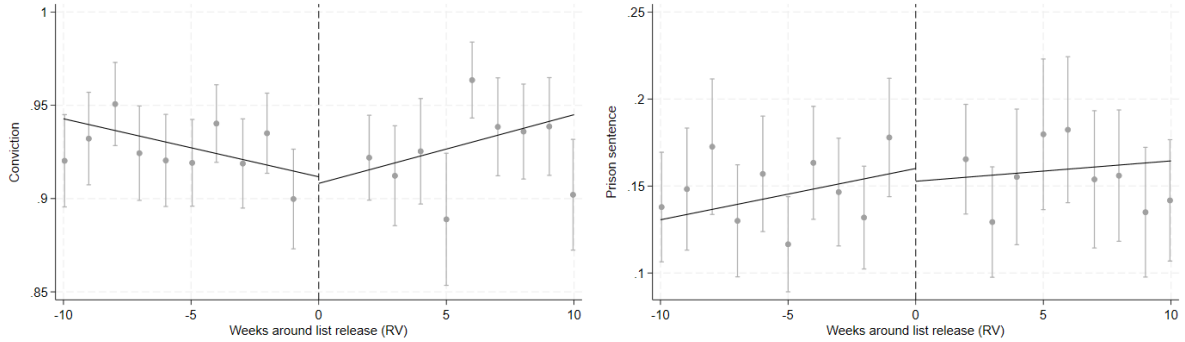
Figure 5: McCrary test figure around list release.



(a) Log sentence length (days).

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

Figure 6: 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 7: 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.

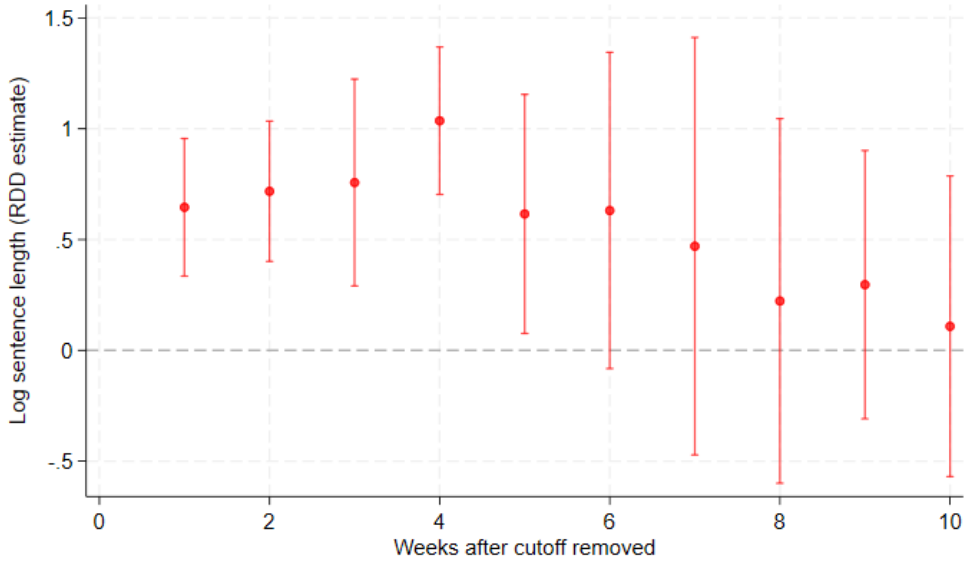


Figure 8: Extended doughnut figure indicating when effects fade. BW -20 up to 10 weeks (moving with doughnut).

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	± 10 w.	± 10 w.	± 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 using the employment registry.

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.

Table 12: Publication of the 2017 police list and prison duration

	Prison months
Listed × postpublication	2.684***
	(0.825)
95 percent confidence interval	[0.914, 4.454]
Never-listed mean before publication	6.603
Listed mean before publication	6.361
Observations	3,661
Observations from listed neighborhoods	450
Courts (clusters)	15
Court-by-calendar-year fixed effects	Yes
Listed and postpublication indicators	Yes
Gender, age, and age squared	Yes

Notes: The outcome is the imposed finite prison term in months, conditional on a prison sentence. The sample covers event quarters −3 through 3 around 21 June 2017, with 91 days per quarter and quarters 0 through 3 designated postpublication. The treated group resides in *särskilt utsatta områden* on the 2017 list; controls reside in never-listed areas. This is the main specification used in the Gelbach decomposition. Standard errors are clustered by court; inference uses $t(14)$ critical values. $^{***}p < 0.01$.

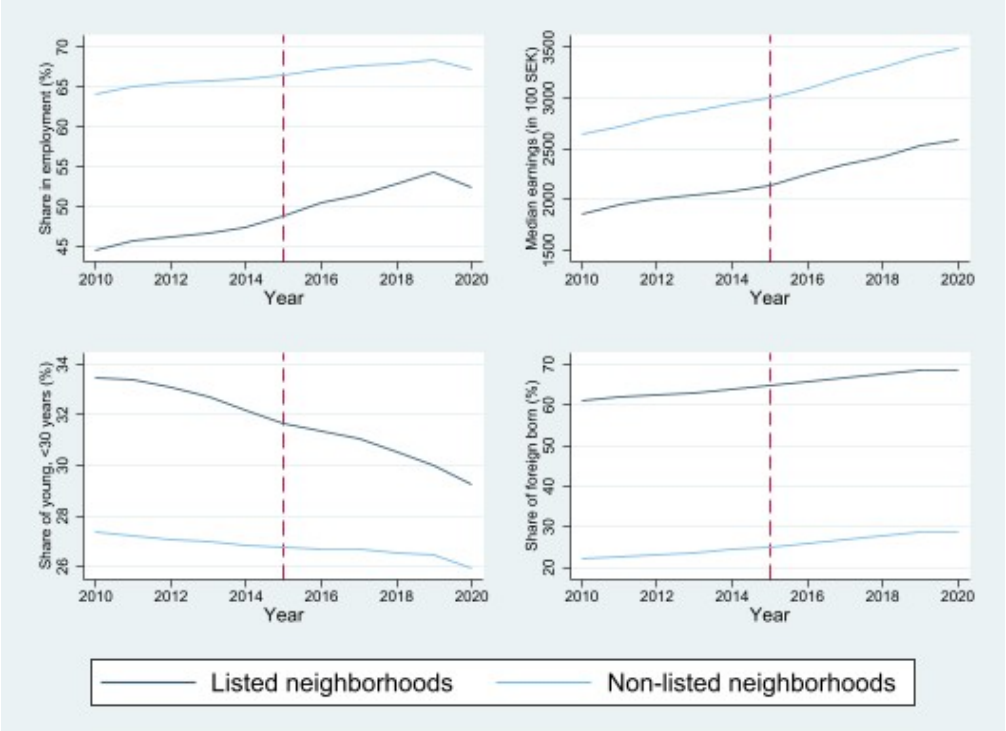
Table 13: Gelbach decomposition of the prison-months response

Component	Months	Bootstrap SE	95% interval	Share (%)
Baseline penal-value measures	0.807	0.634	[−0.098, 2.332]	30
Aggravating circumstances	−0.081	0.211	[−0.335, 0.459]	−3
Mitigating circumstances	−0.051	0.123	[−0.232, 0.161]	−2
Other sentencing adjustments	0.139	0.268	[−0.311, 0.675]	5
Remaining coefficient	1.870	0.887	[−0.315, 3.032]	70
Initial coefficient (main DiD)	2.684	1.053	[0.708, 4.802]	100

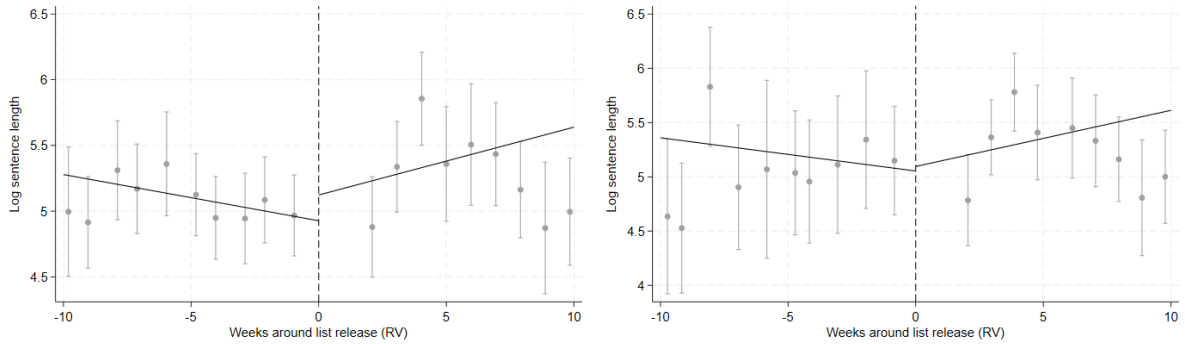
Notes: All models use the same 3,661 finite prison sentences and the main court-by-calendar-year specification in Table 12. Standard errors and percentile 95 percent intervals use 499 pairs cluster-bootstrap draws by court. The bootstrap standard error for the initial coefficient differs from its conventional court-clustered standard error in Table 12; the point estimate is identical. The four block contributions sum to 0.814 months, so $2.684 = 0.814 + 1.870$. Shares are rounded point estimates relative to the initial coefficient; no confidence intervals for the shares are available in the reported output. The remaining coefficient is not a discriminatory share.

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 using the employment registry.

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].[29] 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,

[29] 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.

having for example recorded a 30-fold gun murder rate in comparison to London in 2022.[30]

1.2 Incarceration Rates in Comparison

The criminal justice system in Sweden is broadly similar to many other developed countries, particularly other Nordic countries. Appendix Figure C.3 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.

1.3 Additional evidence of media coverage

[30] Sune Engel Rasmussen, "How Peaceful Sweden Became Europe's Gun-Murder Capital," *Wall Street Journal*, 22 May 2023, `https://www.wsj.com/articles/how-peaceful-sweden-became-europes-gun-murder-capital-a5b500a7`.

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"



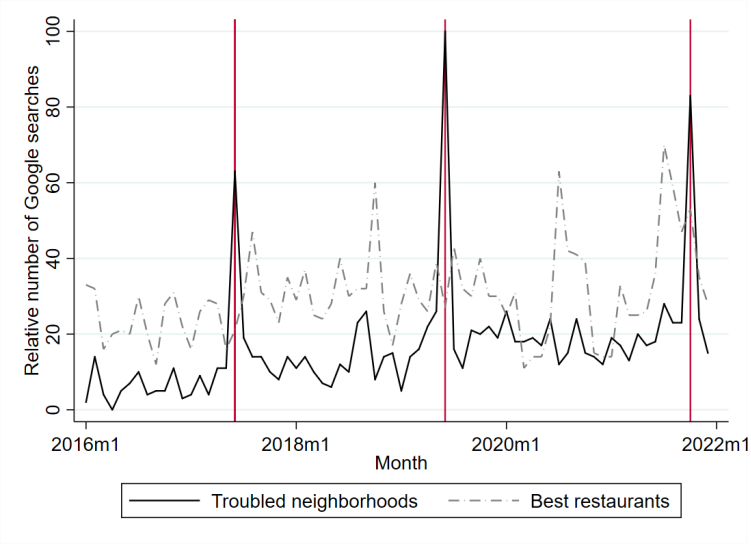
(b) Headline from Dagens Nyheter: "Police: '5000 criminals and 200 networks in vulnerable areas"'



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

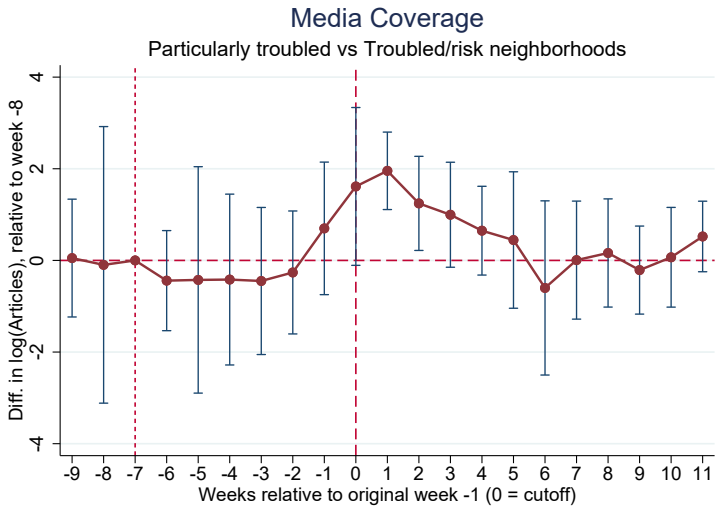
Notes: The Figures show headlines in the two major Swedish newspapers, Dagens Nyheter and Svenska Dagbladet associating social problems with the troubled neighborhoods using the specific term for these neighborhoods coined by the Swedish police authority, i.e., "utsatta områden".

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.

Figure A.3: Media coverage of Particularly Troubled Neighborhoods vs neighborhoods "At risk" or "Troubled"



Notes: The figure shows that the intensity of media coverage was significantly higher for "particularly troubled" neighborhoods than for other neighborhoods on the police list. It plots the difference in the log number of media articles covering neighborhoods classified by the police as "particularly troubled" relative to articles covering neighborhoods classified as "at risk" or "troubled." The data were obtained from Retriever and cover all editorially reviewed media articles, including print media.

B Evidence Against Policy Responses

This Appendix presents the qualitative and quantitative evidence, summarized in Section 2, that the publication of the police list did not trigger any place-based policy response by the police authorities, municipalities, or private security providers.

2.1 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 [Riksrevisionen, 2020].[31]

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 [**(alias?)**], 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 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.

[31] 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.

2.2 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.[32] 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. The estimation strategy is described below.

The results in Figure B.1 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.3 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 B.2 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 (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.

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 the regression model used to estimate the effect of neighborhood listing

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

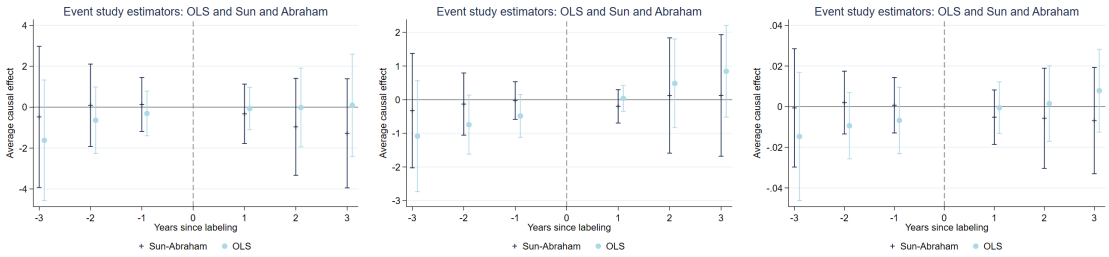
on its resources shown in Figure B.1. The regressions are weighted by the propensity to be treated based on pre-policy characteristics. The estimating equation is as follows:

$$Y_{nt} = \alpha + \sum_{k} \beta^{k} \mathbb{1}[\mathrm{L}_{n} = t - k] + \phi_{mt} + \eta_{n} + \delta X_{nt} + \epsilon_{nt}, \tag{6}$$

where Y_{nt} is an outcome of interest in neighborhood n at year t, and $\mathbb{1}[\mathrm{L}_{in} = t - k]$ 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 B.1.

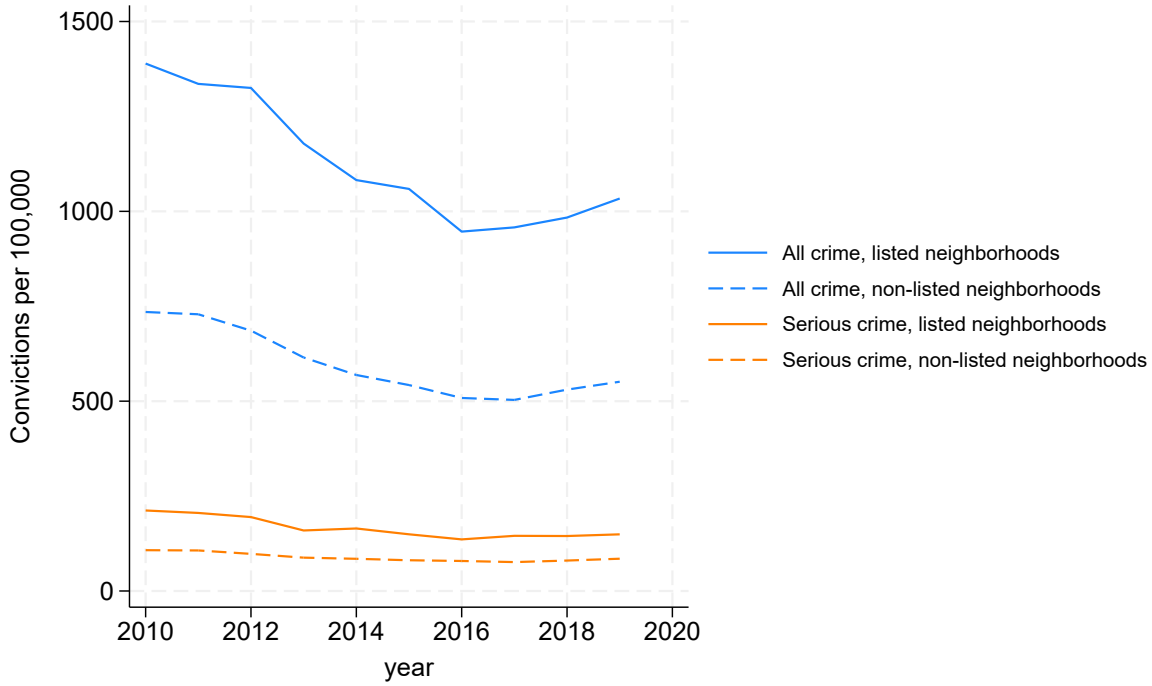
Figure B.1: Police Resources - Treatment Effect in Listed Neighborhoods

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



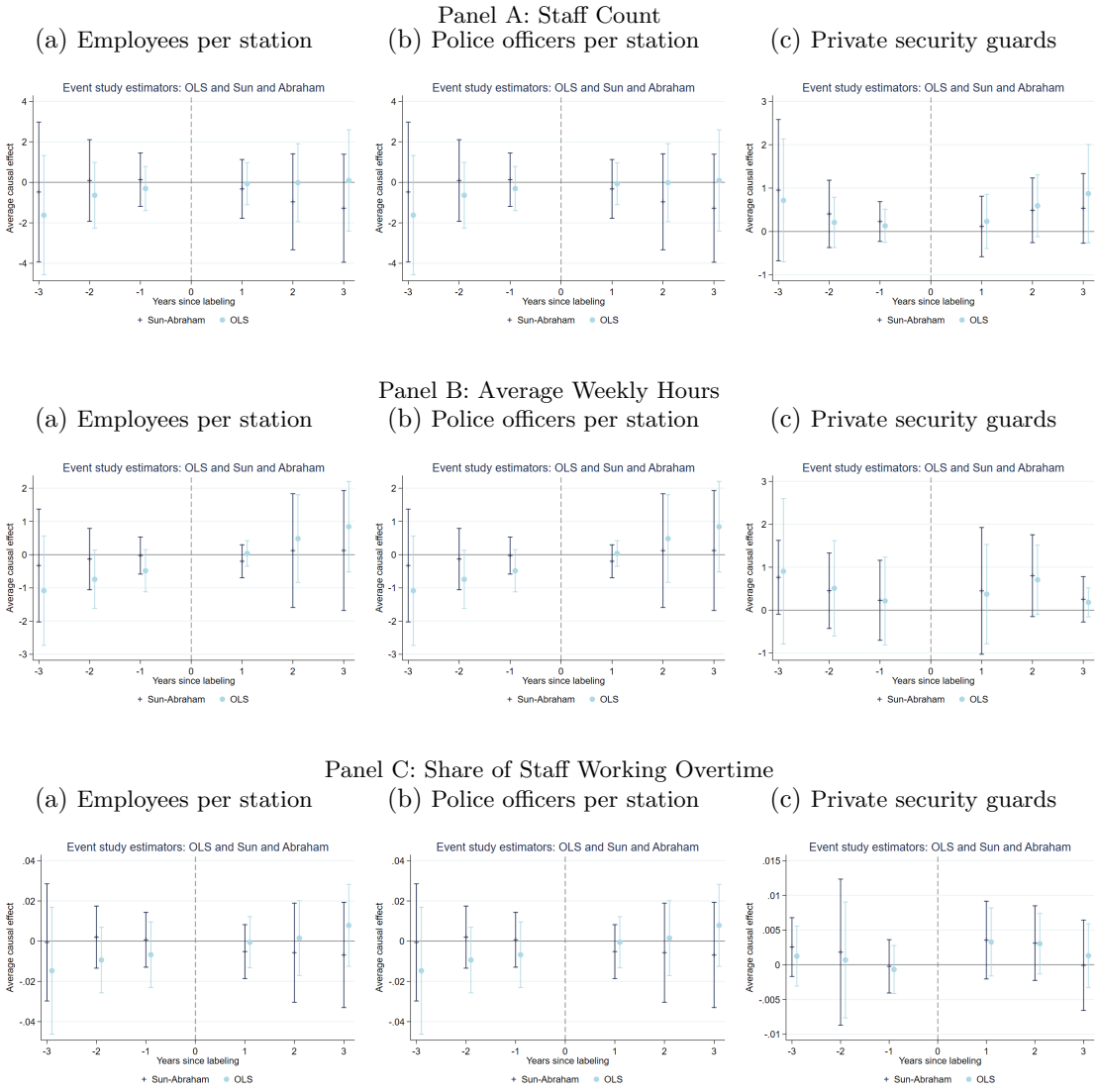
Notes: This figure plots the average treatment effect of the police list on neighbourhood police resources. These are estimated using a dynamic difference-in-differences strategy, where the treatment group includes all listed neighborhoods, and the control group includes non-listed neighborhoods weighted by their propensity of being treated on pre-policy characteristics (see Appendix Section 2.2 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 B.2: Conviction rates for Listed and Non-Listed Neighbourhoods



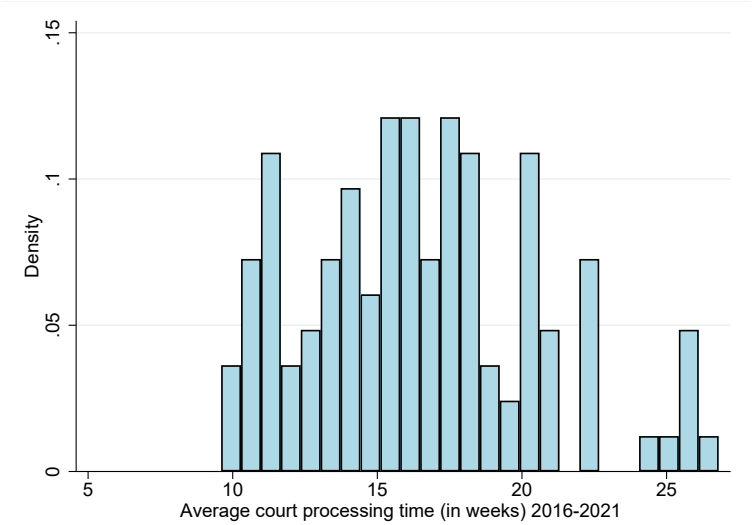
Notes: Statistics are based on the universe of convictions in the Swedish judicial system in years 2010-2019. Serious crime (orange time series) include all prison sentences. The statistics are aggregated from individual-level data that come from Statistics Sweden. The control group of non-listed neighborhoods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

Figure B.3: 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.

Figure B.4: Average court processing time for criminal cases



Notes: The figure plots the average processing time of criminal cases in Swedish district courts for the period 2016-2021 when the data is available. The data is sourced from annual reports by the Courts of Sweden (e.g. https://www.domstol.se/globalassets/filer/gemensamt-innehall/styrning-och-riktlinjer/statistik/2019/domstolsstatistik-2019.pdf). The information refers to the 75th percentile of the court processing time in each district court.

C Additional Figures and Tables

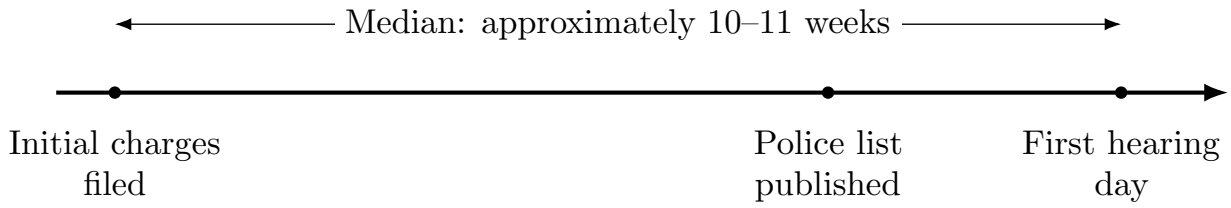


Figure C.1: Illustrative sequence for a case heard shortly after publication of the police list.
Notes. The 10–11-week interval is the published median from filing charges to the first hearing for adult cases in 2009–2012 (Ds 2013:30, Table 19). It includes detained defendants, whereas our analysis excludes cases with ongoing pretrial detention. Hearings for detained defendants are normally expedited: the statutory deadline is two weeks after charges are filed, or three weeks if a preparatory meeting is held, subject to exceptions (Code of Judicial Procedure, chapter 45, section 14). The published median may therefore understate the interval relevant to our sample. An additional 2–3 weeks would imply approximately 12–14 weeks in total, but that adjustment is illustrative; the published data do not identify its size. The position of the police list is illustrative.

Figure C.2: Processing of criminal cases

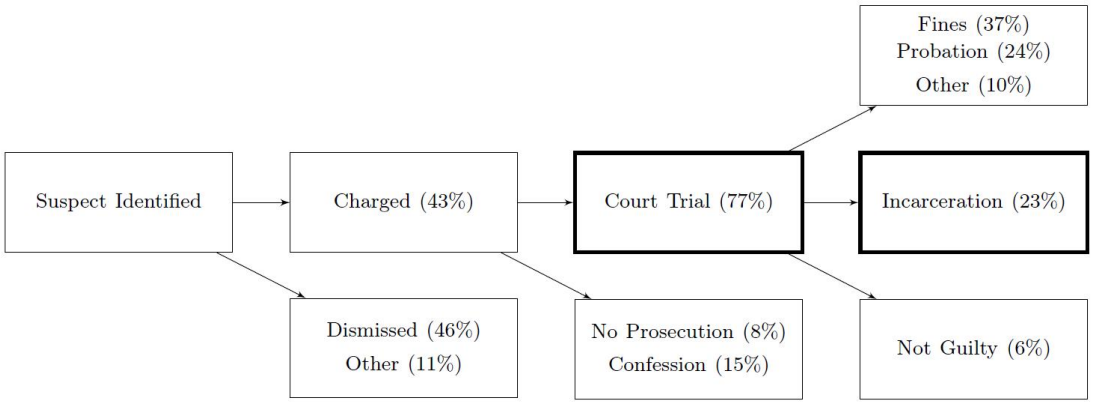


Figure C.3: Cross-country incarceration trends

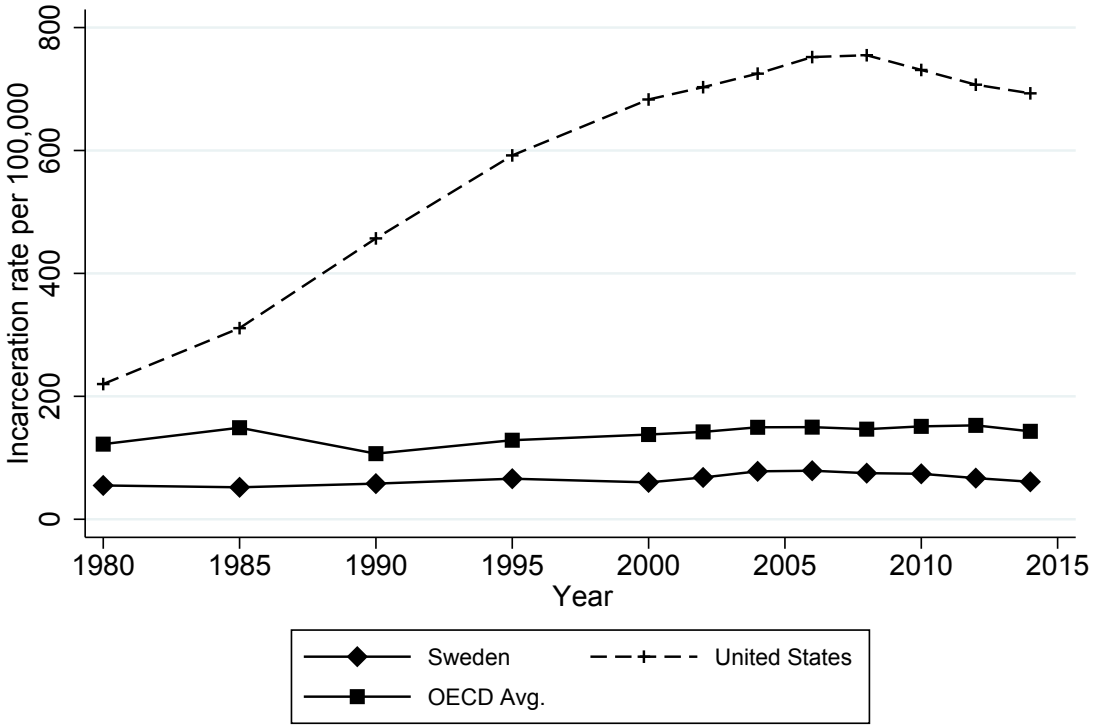


Figure C.4: GDELT Media Analysis

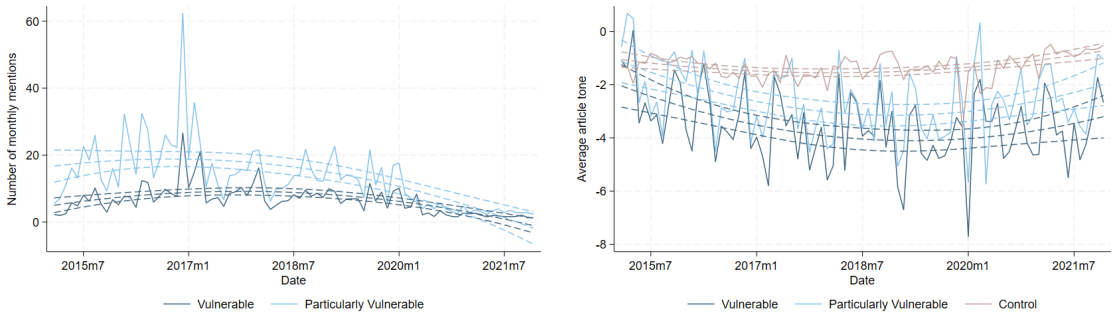


Table C.1: Descriptive Statistics

	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.

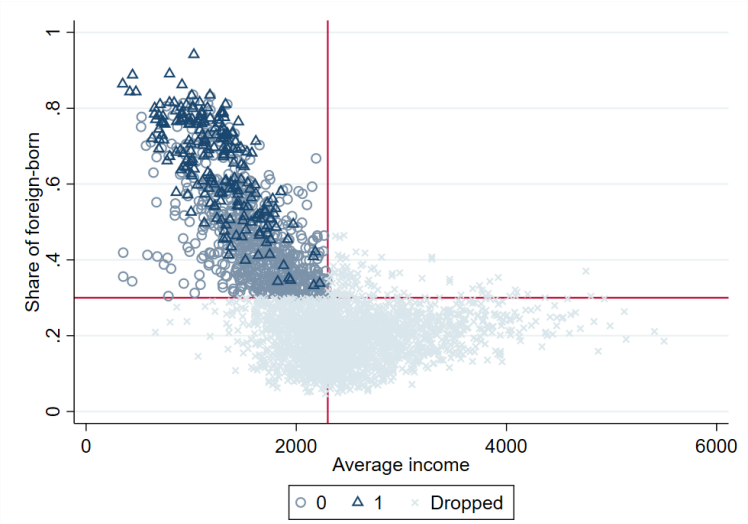
D Robustness

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.

E Measurement of sentencing factors

Table E.1 summarizes the four blocks used in the implemented decomposition. Each regressor belongs to one block. Gender, age, and age squared enter the baseline specification and consequently receive no separate decomposition contribution.

Table E.1: Measurement blocks in the prison-months decomposition

Block	Measures
Baseline penal value	Empirical tariff of the gravest conviction (mean log prison months for that crime outside the event window); log statutory maximum of that conviction; an offense against the person; injured persons listed; number of counts convicted.
Aggravating circumstances	Factors recorded as found by the court under Chapter 29, section 2 of the Criminal Code.
Mitigating circumstances	Factors recorded as found by the court under Chapter 29, section 3.
Other adjustments	Recorded factors under Chapter 29, sections 4–7 and Chapter 30.

The implemented sentencing-factor measures come from a regular-expression extractor applied to the reasoning in the judgment. Factors found in at least 20 cases enter separately; positive counts below that threshold are pooled into a count within the relevant block. The empirical tariff is calculated outside the event window to avoid using the same window's sentencing outcomes to construct it.

The baseline penal-value block uses conviction characteristics rather than a direct numerical penal-value statement by the judge. Its crime-based tariff is a log measure, but the dependent variable and every reported contribution are in prison months. The block may reflect both the offenses reaching court and how those offenses are classified and adjudicated. The decomposition does not contain a separate block of independently dated prepublication case records.

F Additional results on judicial reasoning

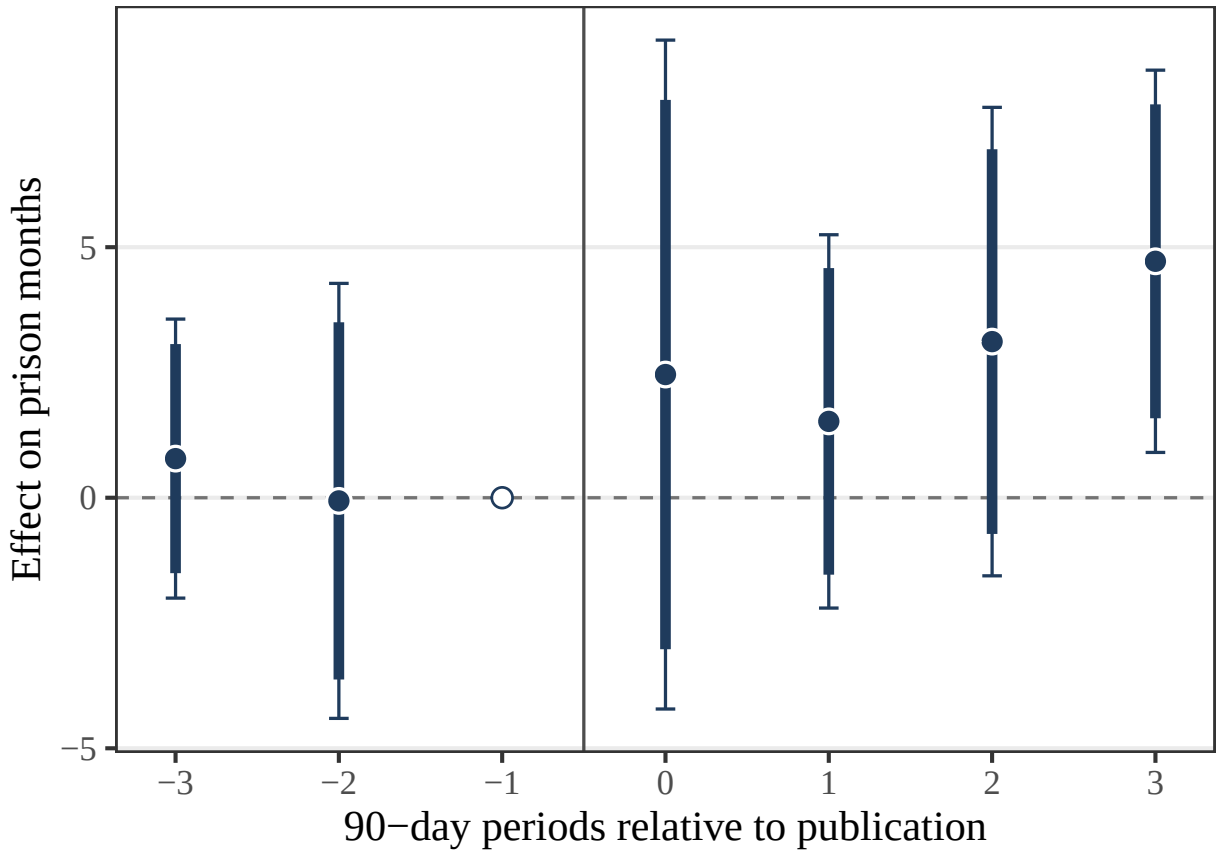


Figure F.1: Publication of the 2017 police list and prison duration: event study

Notes: The figure reports listed-group-by-event-quarter coefficients for imposed prison months, relative to quarter −1 (open circle), using the same 3,661 sentences and court-by-calendar-year fixed effects as Table 12. The model includes listed and postpublication indicators, gender, age, and age squared. Event quarters contain 91 days and are anchored on 21 June 2017. The vertical line separates the prepublication and postpublication intervals; the horizontal dashed line marks zero. Thin whiskers show 95 percent and thick bars 90 percent confidence intervals, based on court-clustered standard errors and $t(14)$ critical values. The right-hand annotation reports the pooled coefficient from equation (2), 2.684 months (SE 0.825, $p < 0.01$), rather than a simple average of the plotted coefficients. Prepublication coefficients are jointly zero with $p = 0.831$; the corresponding postpublication test has $p = 0.004$.