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Effects of Neighborhood Labeling on Student Performance and Sorting*

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Abstract

We study how labeling of neighborhoods affects the performance and sorting of young residents at the junction of enrolling in high school. Exploiting the release of a much publicized official situation report on carefully delineated troubled neighborhoods in Sweden we estimate the effect of this unanticipated and well-documented negative information shock of neighborhood quality. Our research strategy compares the change in outcomes of students living in neighborhoods that were listed in the report with those residing in non-listed ones. To shed light on mechanisms, we focus on one hand on self reported perceptions and beliefs from national school survey data while on the other hand empirically ruling out any potential simultaneous place-based investments by using geocoded longitudinal employer-employee data covering the entire Swedish population. We show that the negative information shock: *(i)* does not affect compulsory school performance of students living in a neighborhood listed as "troubled" in the report, *(ii)* increases sorting of students living in these troubled neighborhoods into lower quality high schools located further away from their homes, *(iii)* has a greater effect on students from relatively higher socioeconomic background, *(iv)* affects treated students' perception of fairness and the extent to which effort pays off. Our results are consistent with models of neighborhood labeling, wherein residents in neighborhoods with a negative public image adjust their behavior in response to social image concerns.

JEL Classification: I24, J13, K42, R23.
Keywords: Spatial stigmatization; Social image; Neighborhood effects; Student performance; Student mobility.

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"Shared perceptions of disorder rather than systematically observed disorder appear to be a mechanism of durable inequality"

– Robert J. Sampson, *Great American City: Chicago and the Enduring Neighborhood Effect*

1 Introduction

In every social arena, people care about how they are perceived by those around them. A recent body of work within economics documents how social image concerns impact individuals' behaviors and decision-making (Ager et al., 2021; Becker, 2022; Bursztyn and Jensen, 2017) as well as highlights the role of perception re-calibration about other individuals, both within and outside the group (Bursztyn and Yang, 2022).[1] A related literature in sociology suggests that such perceptions may also form with regard to places, and contribute to behavioral responses that exceed those that might be justified by the observed characteristics (Wacquant, 1993; Sampson and Raudenbush, 2004; Sampson, 2012; Franzini et al., 2008).

If substantiated, the implication of such implicit bias – manifested in place stigma – is that policies aimed at sanitizing perceptions of neighborhoods may prove effective, even independently of traditional social, housing, and policing strategies.[2] However, estimating behavioral responses to neighborhood reputation is complicated by the challenging task of separating the effect of reputation from the very same neighborhood characteristics that may have given rise to the bad "label" in the first place. Therefore, little causal empirical research bears on the theoretical arguments put forth by Wacquant (1993) and Sampson (2012).

This paper examines the effect of neighborhood labeling on student performance and school sorting for children living in highly disadvantaged neighborhoods in Sweden. The context consists of an event in which the police unexpectedly started publishing, in December 2015,

[1]See also e.g. Friedrichsen et al. (2018) for results from lab experiments.

[2]Encouraging examples show that informational interventions seem able to manipulate perceptions within a short time frame, even in domains associated with substantial inertia, such as societal gender norms (Bursztyn et al., 2020).

an erstwhile internal situation report listing so called "troubled neighborhoods".[3] The list of neighborhoods and its publicly available supplement of color-coded maps has since been updated biannually on specific dates and without any prenotification of additions to or removals from the report, which originally included 55 neighborhoods. Its release and subsequent amendments have received much public attention, with the included neighborhoods being discussed as "no-go" zones. Yet, unlike most place-based interventions, the situation report was not initially followed up by any immediate policy changes, such as a surge in local policing, modifications to public resources, or action plans to combat organized crime (National Audit Office, 2020). Under the assumption that listed neighborhoods were not included based on sudden changes in their unobserved underlying characteristics, the release of the report can therefore be interpreted as a negative information shock, priming both insiders and outsiders to the problems associated with these places.

Our analysis uses rich population-based data containing geocoded information on the exact delimitation of the neighborhoods in the police report as well as the boundaries of the students' neighborhood of residence and schools. We focus on cohorts of students completing compulsory school in 2010-2020, thus on the verge of making key educational investments. We adopt a difference-in-differences research design that deals with correlated unobservables by contrasting the change in outcomes among children in listed neighborhoods relative to their counterparts living in non-listed neighborhoods.

To begin, we document that the release of the situation report produced a negative information shock concerning the listed neighborhoods.[4] To this end, we first classified the universe of media articles written in Swedish mentioning these neighborhoods since February 2015 based on their tone (positive vs. negative). We then used this information to construct a tone-of-content index to estimate the impact of the report on the tone of the articles written about listed neighborhoods compared to a random subset of non-listed ones. The findings

[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 (Mulinari and Keskinen, 2022).

[4] The listed neighborhoods are clearly disenfranchized in their own right at baseline, as is described more in detailed in Appendix Section D.1. The negative information shock hence increases public awareness of the existing situation but also generates an artificially discrete categorization of troubled neighborhoods by construction.

reveal a significant increase in negative media coverage (both in terms of number of articles and the negativity of their tone) about listed neighborhoods relative to the non-listed control neighborhoods. This observation aligns with numerous reports affirming that this event played a pivotal role in establishing a narrative of "no-go zones in the Swedish media landscape," potentially contributing to the stigmatization of these places (Backvall, 2019; National Audit Office, 2020). We further demonstrate that the list was compiled based on persistent traits rather than as a reaction to abrupt changes in the social characteristics used by the police to list neighborhoods, and that it was not followed up by other policy interventions. The combination of a negative information shock on listed neighborhoods, the stability of the underlying characteristics, as well as the lack of any remedial public interventions lends support to interpreting this event as an exogenous shifter of stigma.

Our main analysis estimates the effect of neighborhood listing on student performance and sorting. Specifically, our difference-in-differences estimates suggest no significant effect on educational performance, as measured by grade point average (GPA) in ninth grade and the probability of achieving high school eligibility. However, we do observe that students in listed neighborhoods respond to the introduction of the list by enrolling in schools about 6.4 percent further away from their homes. We also find that students in listed neighborhoods attend high schools of 0.043 standard deviation lower quality, measured as an index based on pre-policy performance measures for the student population. The reduction in high school quality is significantly larger for students in listed neighborhoods who come from high socioeconomic (high-SES) backgrounds as compared to those who come from low-SES ones. While we are unable to estimate the long-term effects of the list, we use pre-policy cohorts of students to estimate the implied earnings loss connected to high school quality. Our estimates implies a reduction in earnings at age 28 by 0.34 percent. We find no evidence of differential pre-trends and our results are robust to a battery of specification checks. We conclude that the effect of neighborhood listing on student outcomes is indeed causal with potentially long-lasting consequences for the children.

We then use multiple sources of data to shed some light on the potential mechanisms behind the adverse sorting effect. Once again exploiting our preferred difference-in-differences design combined with geocoded data on workplaces, we empirically verify that the report's release did not lead to any significant change in manpower or hours worked in the police stations nearest to these neighborhoods. This finding is also consistent with the evaluations of both the Swedish National Audit Office and the police themselves, which criticize law enforcement for not redirecting more resources to reducing crime rates and improving safety in troubled neighborhoods (National Audit Office, 2020). We also provide empirical evidence of the release of the list being orthogonal to trends in resources allocated to schools at the neighborhood level. Moreover, we find no evidence that families responded to the introduction of the list by changing neighborhood and there is no significant effect on family economic resources. Taken together, our results suggest that the most likely mechanism through which the effect of the report's public release operates is neighborhood labeling, rather than any change in available resources. Moreover, an analysis of survey data eliciting student beliefs about anticipated discrimination and perceptions about payoff to effort provides additional support for the labeling hypothesis. Specifically, we employ data from repeated cross-sections (5-year intervals) of a nationally representative school survey comprising roughly 5,000 ninth graders in Sweden. Exploiting our difference-in-differences approach, which contrasts changes in responses of students in schools located in listed neighborhoods with those in non-listed ones, we observe a significant negative effect on perceptions of the extent to which effort pays off and of anticipated discrimination.

Our study contributes to two distinct strands of research. First, we build on the literature studying the existence and effects of out-group and neighborhood perceptions (Sampson and Raudenbush, 2004; Franzini et al., 2008; Besbris et al., 2015; Bursztyn and Yang, 2022) and to that on social image concerns (Ager et al., 2021; Becker, 2022). A related emergent body of work shows how place-based policies – such as criteria changes for place-based subsidies and a consequential redrawing of the maps of entitled neighborhoods (Garrouste and Lafourcade, 2022); lending risk maps delineated for a home mortgage refinancing program (Aaronson et al., 2024); and a rental housing policy aimed at increasing the socioeconomic diversity of deprived neighborhoods (Koster and van Ommeren, 2022) – may backfire by making the disadvantages of the concerned neighborhoods more salient.[5] While highlighting

[5]These studies highlight a shaming side effect much in the same way that free school meal programs can

an important potential pitfall of place-based policies (Glaeser and Gottlieb, 2008), all of these studies also focus on policy interventions, each of which affected the targeted neighborhoods and residents therein. A related example of a place-based policy documented to have had a crime-reducing effect is that of a Danish program, which since 2010 has injected additional infrastructure investments and local policing into public-housing neighborhoods (Damm et al., 2024). Though making these neighborhoods widely known, in this case the large investments are thought to have counterbalanced any potential stigma. To our knowledge, ours is the first paper in the literature to adopt a quasi-experimental research design to identify a pure neighborhood labeling effect in a setting where the only "policy" is a negative information signal.

We also add to the large literature showing that neighborhood effects play an important role in shaping children's long-run outcomes, and that a causal interpretation can be made of this relationship (e.g. Åslund et al. 2011; Chetty et al. 2016; Chetty and Hendren 2018a; Chetty and Hendren 2018b; Chyn and Katz 2021; Damm and Dustmann 2014; Kling et al. 2007; Oreopoulos 2003; Jacob 2004; Ludwig et al. 2008; Sampson et al. 2002). While this literature avoids individual sorting on unobservables, less is known about the pathways through which neighborhoods affect outcomes (Chyn and Katz, 2021), primarily due to the fact that neighborhood characteristics are intimately correlated.[6] Our results suggest that place reputation may be a previously little-recognized but potentially important mechanism through which neighborhood effects operate.

The rest of this paper is structured as follows. Section 2 describes the situation report and its public dissemination. Section 3 presents the data and Section 4 the empirical strategy. Section 5 discusses our results and finally, Section 6 offers some concluding remarks.

have stigmatizing side effects at the individual level (Mirtcheva and Powell, 2009).

[6]For instance, Chetty and Hendren (2018a) and Chetty and Hendren (2018b) find that highly segregated neighborhoods are associated with worse outcomes for those who grow up in such places. Other neighborhood-related predictors that stand out include the concentration of poverty, income inequality, school quality, share of two-parent families, crime, and pollution.

2 Conceptual Framework

Most theoretical work on neighborhood stigmatization comes from Sociology. Originally, the conceptual advances of stigma by Goffman (1963) were centered around the individual. Werthman and Piliavin (1967) argued that stigma could equally well be applicable to social groups, such as racial groups. In their example, the police divides up the territories they patrol into readily understandable, and racially shaped, categories. Werthman and Piliavin (1967) call this process *ecological contamination*, whereby all persons encountered in "bad" neighborhoods are viewed as possessing the moral liability of the neighborhood itself. To the extent that this contamination is ingrained among not only the police but the residents themselves, it could lead to stigmatized neighborhoods (Irwin, 1985; Wacquant, 1993; Loury, 2002). This implicit bias of residents of neighborhoods could become self-confirming if residents internalize a "ghetto identity", leading to actions that strengthens the statistical association between in-group characteristics, such as race and observable behavior.

Sampson (2012) rationalizes how perceptions of neighborhood quality are contextually shaped and distinguishes implicit bias from prejudice or statistical discrimination. The former is thought to be independent of individuals' ethnicity, residency, or social status and is likely to hold for the out-group as well as the in-group whereas the latter is likely to be much stronger within the out-group.[7]

In this study, the Swedish police list of troubled neighborhoods can be seen as an shifter of the implicit bias. We hypothesize that this will affect residents' behavior. In the case of adolescent residents, this shift could have short-run implications for educational choices. Students from troubled neighborhoods may trade off school quality against the stigma internalized of attending a school with a high share of out-group students from non-listed neighborhoods. This self-censoring could however be dominated by the desire to escape stigmatized schools with high share of in-group students, even at the expense of school quality. At the end, we consider the behavioral effects to be an empirical question inevitably leading to a two-sided hypothesis test.

[7]For an illustration of this conceptual framework, see Chapter Six and Figure 6.3 (Believing is seeing: The social basis of perceived disorder) in Sampson (2012).

3 Institutional Setting

This section first describes the introduction of the situation report and pertinent list of troubled neighborhoods. It then goes on to document the information shock and shows evidence of the absence of any simultaneous or coordinated policies. Lastly, the section describes relevant features of the Swedish high school enrollment system.

3.1 The Introduction of the Police List

In December 2015, the Swedish police initiated the public dissemination of their 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.[8]

The list was compiled by combining objective statistics with careful assessment by the local police areas (NOA, 2015). Statistical criteria include poor socioeconomic conditions indicated by high unemployment and poverty (Gerell et al., 2022). As shown in Appendix Figure A.2, 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 to assess the extent to which they believed the neighborhood to be infested with organized crime, parallel social structures, religious radicalization, and residential trust in institutions.

[8] 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/

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 defined as a geographically delimited entity throughout these reports, and supplemental maps are included in the report's appendix (based on publicly available shapefiles).

The initial round of the list comprised 55 neighborhoods. Figure 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.[9] 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.

[9] Based on previous weak evidence of neighborhood stigma reversion once the label is removed (Garrouste and Lafourcade, 2022), we consider the neighborhood labeling conferred by the police listing to be an absorbing state that is not reversed during our period of observation.

Figure 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.

3.2 Media Portrayal of Listed Neighborhoods

The police report, which has been widely debated both in national and international media, has played a crucial role in shaping a media discourse of Swedish so called, "no-go zones".[10] Since its introduction, major media outlets have consistently covered the listed neighbor-

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

hoods, often making them the focal point of news coverage (Backvall, 2019).[11] Below we show results from analyses that aims to quantify the magnitude of the coverage.

Figure 2 shows results of textual analysis of mentions of troubled neighborhoods in news articles in Swedish media. The analysis uses data from a media monitoring project called Global Database of Events, Language, and Tone (GDELT), which records instances of events based on articles from a comprehensive, global set of news feeds and is gaining increasing traction in economics (Campante and Yanagizawa-Drott, 2017; Manacorda and Tesei, 2020; Beraja et al., 2023). 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 (which we geocode at the RegSO level), date of the event, and the tone-of-content of the article.[12] The data covers all media articles published online since February 2015. It is clear from the figure that the number of articles discussing troubled neighborhoods starts to increase just after the release of the first 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. 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.

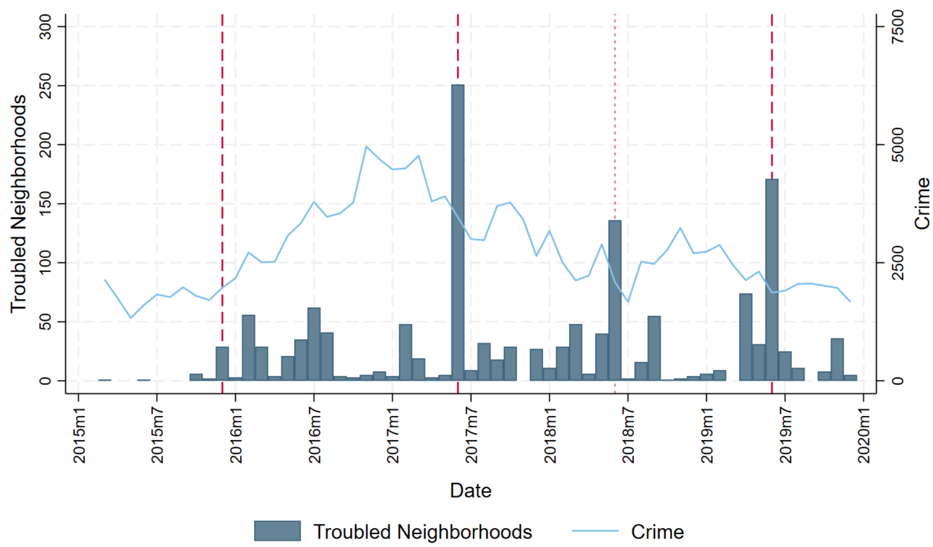
Appendix Figure D.2 depicts the relative number of Google searches for the terms "troubled neighborhoods" and "best restaurants". Clear spikes in searches for "troubled neighborhoods"

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

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

are evident during the months when a new situation report is released and the pertinent list of troubled neighborhoods is updated. After these events, the relative number of searches for troubled neighborhoods even exceeds that of very common phrases like best restaurants.[13] We interpret this as evidence of not only substantial media attention surrounding the report but also significant public interest.

Figure 2: Media Mentions - "Troubled Neighborhoods"



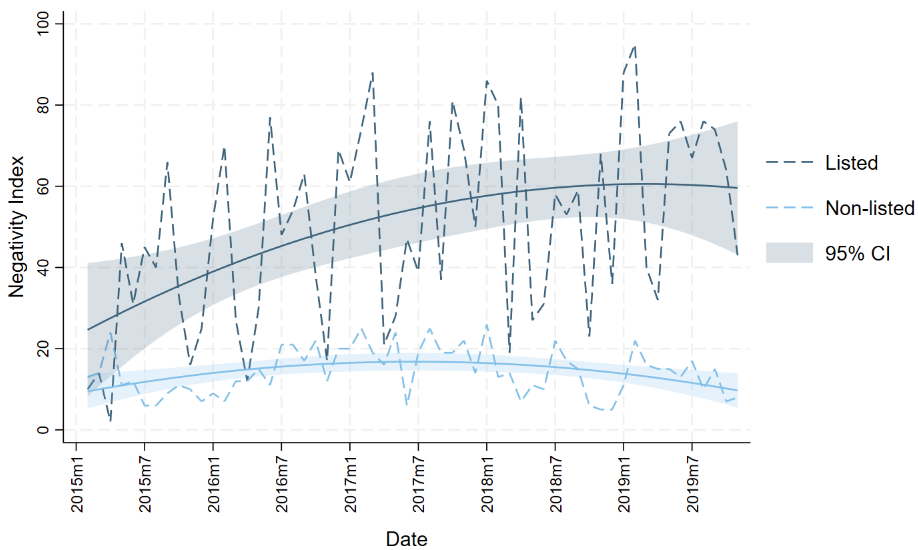
Notes: This figure plots the number of articles in Swedish media including the term "Troubled neighborhoods" (in Swedish, Utsatta områden). Red dash vertical lines indicate months in which the list was released or updated. The lighter red dotted line in 2018 indicates an update that was expected and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

Capitalizing on the the fact that GDELT provides for all articles published in online media a measure of tone, Figure 3 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

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

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 3 reveals that, as expected, 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 by the end of 2019.

Figure 3: Media Tone - Listed and Non-Listed Neighbourhoods



Notes: This figure plots the average tone of the articles in Swedish media for neighborhoods included in the Police List, and a random sample 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 rescaling 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 the sign and organizing it into percentiles. 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 fitted local polynomial (using a second order polynomial). The data was obtained from the GDELT project, https://www.gdeltproject.org/

In summary, the significant public interest in the listed neighborhoods, combined with their negative portrayal in the media, likely contributed to their stigmatization (Backvall 2019; National Audit Office 2020). Such spatial stigmatization may have led to concerns about social image and heightened anticipation or experience of discrimination among affected residents (Sampson et al., 2002; Wacquant, 2008).

3.3 Evidence Against Any Policy Response

In this section, we explore the extent to which the introduction of the situation report was linked to remedial policy interventions targeting the listed neighborhoods. We organize our discussion into qualitative and quantitative evidence.

3.3.1 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 affected neighborhoods. An internal report by the police as of 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 particularly questioned whether any increases in local patrols had been directed to troubled neighborhoods (National Audit Office, 2020).[14]

Municipalities also 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

[14] An example of an intervention is the increased presence by the police in the troubled neighborhoods 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.

police's list of troubled neighborhoods did not align with any specific programs for increased inter-agency collaboration. According to a BRÅ report, the Police Authority began providing additional guidance on community policing to local police from 2015 onwards. 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 (BRÅ, 2018).

3.3.2 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.[15] Using this data, we link police personnel to their geocoded stations. For the police station nearest to each neighborhood, we calculate the average headcount, contracted hours, and the proportion of those working overtime.[16] For each of these dependent variables, we estimate a staggered difference-in-differences model of the effect of neighborhood listing on police resources using non-listed neighborhoods in urban areas as controls.[17]

The results in Figure 4 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 staff working overtime in the police stations closest to listed neighborhoods. Most of the point estimates are close to zero and relatively precisely estimated. In Appendix Figure B.1 we show that the results for police staff hold when only considering police officers, or alternatively considering private security guards.

Taken together, both the qualitative and quantitative evidence suggest that the introduction

[15] We discuss the data in more detail in Section 4.

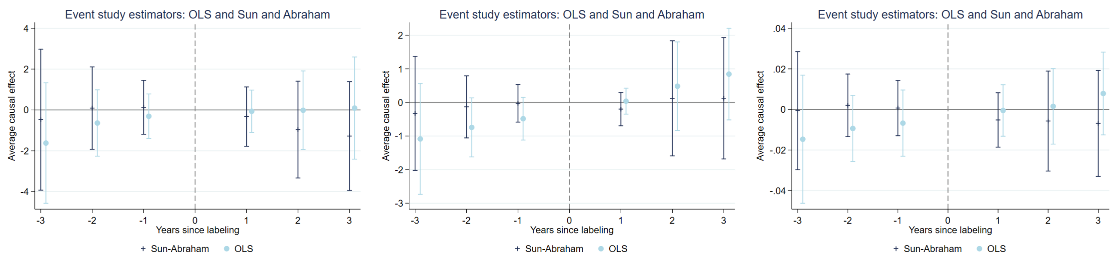
[16] The results are similar also when using the two closest police stations and weighting by the inverse distance.

[17] See details on the estimation strategy in Appendix B.1.

of the list does not seem to have led to any meaningful immediate changes in police resources.

Figure 4: Police Resources - Treatment Effect in Listed Neighborhoods

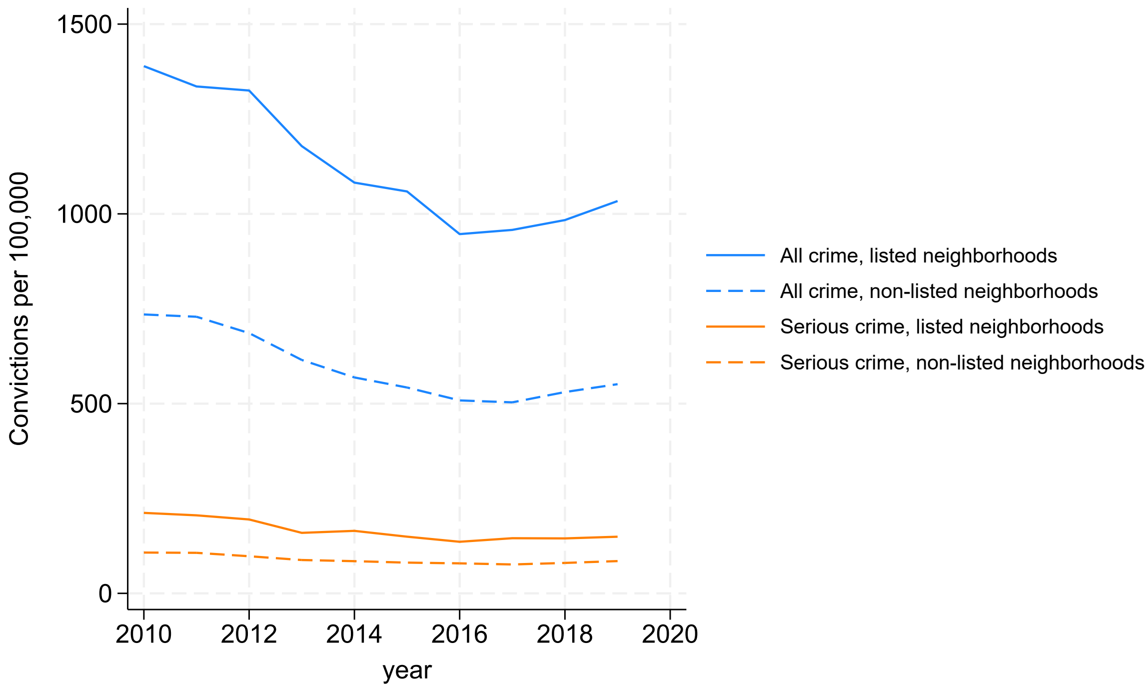
(a) Employees per station (b) Average weekly hours (c) Share working overtime



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 B.1 for the details of the empirical model). TWFE and Sun and Abraham (2021) estimators are presented. Sub-figure (a) shows the effect on number of employees per station, Sub-figure (b) for average weekly contracted hours per police officer, and Sub-figure (c) for share of staff working overtime. 95% confidence intervals are based on standard errors clustered at the neighborhood level.

One potential concern is of course that, abstracting from any changes in the size of police staff, the police force might have engaged in tougher law enforcement due to the release of the report, for example more frequent stop-and-frisking of young men. Such changes in practices could in principle also drive students to apply to high schools further away. To confirm that no such surges occurred, Figure 5 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. Second, we find no discernible shifts in the spread of conviction rates during the complete period of observation (2010-2019). 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.

Figure 5: 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.

3.4 Swedish High School Admission System

After completing compulsory education, which features a broad curriculum with a limited set of elective options, most students proceed to high school.[18] Despite being optional, high school enrollment is very high, with almost 90 percent of students transitioning directly after compulsory schooling in recent years. High school education is organized into several tracks, and students apply to a combination of school and track (hereafter, school-by-track) based on their GPA from compulsory school. Since 1990, local municipalities have managed high school education, primarily funded through local income taxes and government grants, while the central government provides the regulatory framework for compulsory education.

The high school admission process operates as follows: during the final semester of ninth

[18]This descriptive background section draws heavily on the outline presented in Dahl et al. (2023)

grade, students submit a standardized application form, listing up to six preferred schools-by-tracks. These forms are sent to a central administration office, which allocates students based on their GPA and listed preferences. This allocation process, known as "serial dictatorship" ensures that the highest-GPA applicant gets their first-choice, followed by the next highest, and so forth. Importantly, this strategy-proof centralized assignment mechanism ensures that (i) students have no incentive to misrepresent their preferences and (ii) that schools have no possibility to select ("cream-skim") students based on other characteristics than their GPA. Whether a school-by-track becomes oversubscribed depends on the size of the classes, which are typically capped at multiples of 30 students. Based on expected demand, some schools may offer multiple classes for a given track. Consequently, a choice might be oversubscribed in one region but not in another, and this can vary from year to year. In this setting, it is important not to confuse the phenomenon of oversubscription with a competitive environment. There is no salient ranking of school-by-track quality in terms of GPA cutoffs or likelihood of oversubscription. Additionally, average cutoffs, whenever they exist, are generally similar across different choices.

4 Data

In this section, we discuss our population-based micro-level data and describe how we operationalize treatment status based on the police's list and corresponding maps of troubled neighborhoods.

4.1 Swedish Administrative Register Data

The data used in our main analysis come from individual-level administrative registers, encompassing the entire Swedish population of ninth graders from 2010 to 2021. This dataset includes GPA at the completion of compulsory school in grade 9, enrollment in high school and its various tracks, and school identifiers for both compulsory and high school. These data are merged with other administrative registers at Statistics Sweden using individual identifiers. Consequently, the dataset includes sociodemographic characteristics of the working-age population (ages 15-74) for all years, enabling us to characterize neighborhoods. The dataset also uses unique family identifiers to connect ninth graders to their parents. This allows us to link students to their parents' geocoded residential locations and, therefore, to their neighborhood of residence in any given year.

Table A.1 shows descriptive statistics for variables included in the analysis for listed and non-listed neighborhoods.

4.1.1 Main Outcome Variables

Student performance. Grade 9 GPA is average grades in last grade of compulsory school standardized over the observation period with mean zero and unit standard deviation. High School Eligibility is an indicator set to one if the student completed the three core subjects (Mathematics, Swedish, and English) and thus became eligible to apply to high school.

Sorting of students to high schools. High School Quality is an index equal to an unweighted average of two school performance measures: grade 9 GPA among the students who enrolled in the high school in 2014 (i.e pre-policy) and the high school GPA among the students who graduated from that school in the same year, all normalized to Z-score form using control group's distribution (Heller et al., 2017). Distance to High School is the log of the (Euclidean) distance between the centroid of the neighborhood of residence and the exact school location.

4.2 Maps of Listed Neighborhoods

To identify listed neighborhoods we use geocoded maps provided by the Swedish police authority.[19] As the police authority draws its own maps of troubled neighborhoods, the

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

delimitations of these do not correspond to any of the standard breakdowns into statistical regions by Statistics Sweden. We harmonize the listed neighborhoods to the breakdown called Demographic statistical neighborhood units (hereafter, DeSO for the Swedish term Demografiska statistikområden). There are approximately 6,000 DeSOs in Sweden, with population size about the same as U.S. Census Block Groups. In the absence of individual-level information on the geocoded location of the residential building, we define treatment at the neighborhood (DeSO) level. Since the police-drawn maps typically exclude commercial properties, highways, and green areas, they do not perfectly align with DeSO boundaries. To address this, we classify a DeSO as listed if more than 50 percent of its land area falls within the police-drawn map boundaries.[20] Figure 6a shows the geographical distribution of the police drawn maps in Stockholm municipality and Figure 6b zooms in on one of these neighborhoods in Stockholm. We can see that the police drawn map (shown in pink) overlaps relatively well with five DeSOs (boundaries shown in solid black lines), with the exception of some green area. It is also clear that listed neighborhoods are located in the suburban areas of the city.

All listed neighborhoods are located in urban municipalities. For this reason, we restrict the analysis to students living in urban municipalities. We follow (BRÅ, 2018) and define urban municipalities as ones with at least one town larger than 10,000 individuals and whose share of dense neighborhoods (with at least 200 dwellings within a maximum distance of 200 meters) is above the national average.

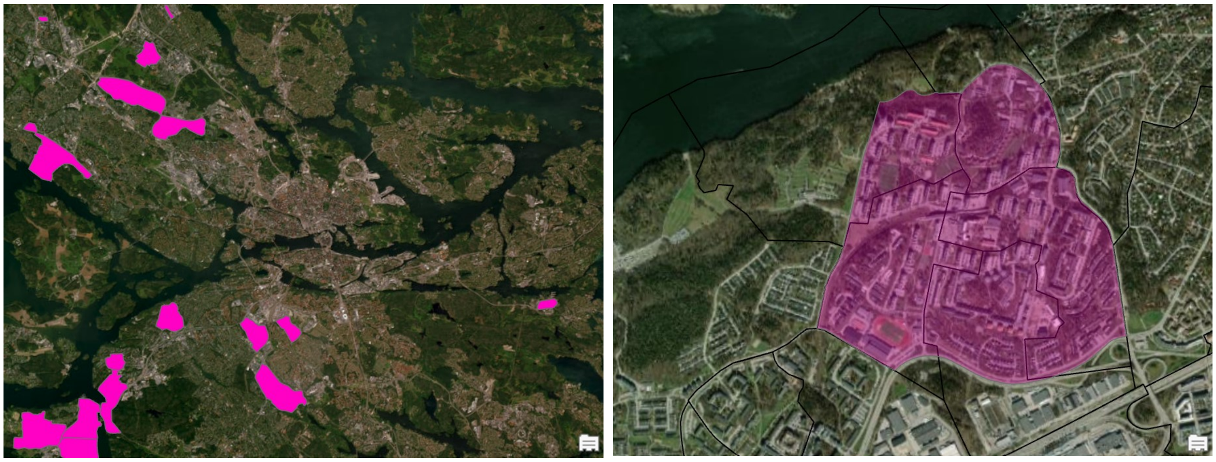
Appendix Table A.1 shows the total number of individuals and neighborhoods included in our sample by treatment status, i.e., listed and non-listed. There are 220 neighborhoods (DeSO) with 45,621 students within the boundaries of the listed troubled neighborhoods as drawn by the police and 3,104 non-listed neighborhoods (521,696 students) located in urban

[20] 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 C.2 by comparing descriptive statistics for age structure and employment of listed neighborhoods based on the 50 percent overlap rule with the definition used by Statistics Sweden based on exact individual-level coordinates of residence and geocoded delimitations of listed neighborhoods provided by the police. The latter descriptives are only available for year 2018 (Statistics Sweden (SCB), 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.

regions.

Figure 6: Map of Listed Neighborhoods in the City of Stockholm

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



Notes: Sub-figure 6(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/.

5 Research Design

To study the effect of the neighborhood being listed in the situation report on student performance and sorting into high schools, we match each ninth grader's neighborhood to the police list of troubled neighborhoods by year and neighborhood identifier.

In the main analysis, we estimate the following conventional event-study specification around the first cohort (c = 0) being treated by the inclusion of its neighborhood in the police list of troubled neighborhoods:

$$Y_{inc} = \gamma + \sum_{k} \theta^{k} \mathbb{1}\left[\mathrm{L}_{in} = c - k\right] + \pi_{mc} + \kappa_{n} + \lambda X_{inc} + \epsilon_{inc}, \tag{1}$$

where Y_{inc} is a student outcome of interest and $\mathbb{1}\left[\mathrm{L}_{in} = c - k\right]$ is an indicator for the listing of individual i's neighborhood n occurring k periods from c (negative k indicating a future event date). The model assumes conditional parallel trends in outcomes for listed and non-listed neighborhoods in the counterfactual state where no police list ever was released, which we are able to verify by testing whether the anticipatory effects at $k = 3, 2, 1$ are statistically significant. We expect the effect of neighborhood labeling to manifest in the subsequent year 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 the first treated cohort is the one graduating from ninth grade in 2016 ($k = 0$), and subsequent treated cohorts ($k = -1, -2, -3$), of ninth graders in listed neighborhoods are considered as treated.

By including neighborhood, cohort, and municipality-by-cohort fixed effects, and neighborhood-specific time trends in our estimating equation (1) our coefficients of interest, θ^k, captures the effect on the cohort member of exposure to the listing of one's neighborhood in ninth grade as compared to cohort members living in unlisted neighborhoods within the same municipality during ninth grade. Controlling for neighborhood specific linear time trends assuages concerns that becoming listed is correlated with other neighborhood-level trends that themselves may affect student performance and sorting, such as local crime patterns or religious radicalization.

We first estimate equation (1) by a two-way fixed-effects model (TWFE). Alternatively, we also use the estimator proposed by Sun and Abraham (2021) to account for the staggered treatment timing and possible treatment effect heterogeneity in implementing the policy (e.g., Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021). These concerns are, however, less pronounced in our setting since: (i) our study sample includes a large share of never-treated neighborhoods (45.9%) and (ii) the staggered adoption was limited in the sense that the majority of the treated neighborhoods were so already in the initial round in 2015.

By simply restricting the analysis to urban neighborhoods, as described in Section 4, narrows down the control group and ensures common trends with regard to key characteristics (Figures 5 and A.2). The comparison units are further narrowed down for the study sample using propensity score matching. We match each treated unit with control units based on unit-specific average baseline characteristics. These characteristics are measured before the release of the first list in 2015, and include many of the factors that are considered by the police when they constructed the list NOA (2015).[21]

6 Results

This section presents the findings from our empirical analysis. We begin by presenting the main results and proceed with robustness checks. Subsequently, we offer estimates detailing the effect of neighborhood listing in relevant population subgroups. We conclude with supplementary analyses aimed at shedding light on the underlying mechanisms.

6.1 Benchmark Model

Table 1 presents our baseline estimates from equation (1) of the effect of neighborhood listing on student performance and sorting. The top panel shows the results for our two performance measures: Grade 9 GPA and High School Eligibility. The bottom panel shows estimates for our two sorting measures: (log) Distance to High School and High School Quality. For each outcome we show estimates of three models: Column (1) shows our DiD estimates of a two-way fixed-effect model excluding neighborhood-specific linear time trends. Our preferred specification in column (2) adds these trends. For comparison we complement our analysis with estimates using the approach proposed by Sun and Abraham (2021). The standard errors reported in parenthesis are clustered at the neighborhood (DeSO) level.

The top panel in Table 1 shows that there is no statistically significant effect of neighborhood listing on student performance in compulsory school. The estimates for both performance measures are small in magnitude and relatively precise. For instance, a 95% confidence interval rules out reductions by more than 0.084 standard deviations for Grade 9 GPA and 4.4 percentage points for High School Eligibility (from a baseline of 88.8 percent). Based

[21] We use a Kernel matching procedure that uses pre-policy information on neighborhood total population, share of foreign-born residents, share in employment, share of single parent households, share of young (ages 15-35) and average income.

on these results it is unlikely that any changes in the sorting pattern of students would be driven by changes in their compulsory school performance. Moreover, as discussed earlier, high school admission system is centralized and only based on compulsory school GPA, implying that high schools have limited possibilities to cream-skim students based on other (unobserved) characteristics.

The bottom panel in Table 1 shows that neighborhood listing significantly increases the distance between the students' place of residence and the high school they enrol in. The point estimate in column (2) suggests a 6.4 percent significant increase in the geographic distance, and therefore their commute. This estimate is essentially invariant to alternative specifications in columns (1) and (3). It is also clear that the there is a tendency of a reduction in the quality of the high school, ranging from 0.046 to 0.05 standard deviations. While the point estimates are all negative the estimates from models that excludes neighborhood trends and those accounting for the staggered design of the policy are statistically insignificant, albeit not qualitatively different from the benchmark estimates.

Appendix Figure A.1 shows the event-study graphs corresponding to those in Table 1. Reassuringly we cannot discern significant differential pre-trends for any of the outcomes.

To summarize, our results suggest that students in listed neighborhoods responded to the introduction of the list by choosing high schools further away from their homes and there is also some evidence that these high schools were of lower quality. There is no evidence that this behavioral effect can be explained by changes in the school performance of these students that placed them in a worse position when choosing high schools.

Table 1: Effect of Neighborhood Listing on Student Performance and Sorting

	School Performance					
	Grade 9 GPA			High School Eligibility		
	(1)	(2)	(3)	(1)	(2)	(3)
Listed x Post	0.021	-0.018	-0.002	0.007	-0.019	-0.014
	(0.022)	(0.034)	(0.055)	(0.009)	(0.013)	(0.020)
Observations	530,905	530,905	530,905	530,905	530,905	530,905
	High School Sorting					
	High School Quality			Distance to High School		
	(1)	(2)	(3)	(1)	(2)	(3)
Listed x Post	-0.016	-0.043*	-0.050	0.077***	0.064***	0.087***
	(0.018)	(0.024)	(0.042)	(0.024)	(0.022)	(0.033)
Observations	472,767	472,767	472,767	440,013	440,013	440,013
Neighborhood FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Municipality-by-Year FE		✓	✓		✓	✓
Neighborhood trends		✓	✓		✓	✓
Estimator	TWFE	TWFE	S&A	TWFE	TWFE	S&A

Notes: This table reports the average treatment effect of the treated (ATT) estimate of the average θ in Equation (1). 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. Columns (1)-(2) and (4)-(5) report two-way fixed-effects estimates of the ordinary least squares (TWFE) estimator and columns (3) and (6) report estimates of the estimator proposed by Sun and Abraham (2021) (S&A). The sample includes all students in urban areas who completed compulsory school in years 2011-2021. Standard errors (in parentheses) are clustered at the neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

6.2 Summary of Robustness Checks

Appendix Table C.1 shows robustness exercises to our main specification. We show in the first three panels that the results are robust to both using a selected (trimmed) sample of control neighborhoods having similar pre-policy characteristics (share of foreign born residents and average monthly labor income, see Figure C.1), and to removing the adjacent non-treated neighborhoods from the control group. The last three panels document robustness to variations of the treatment definition. We consider only those neighborhoods treated in the first release of the situation report and compare them to controls (fourth panel), and also vary the degree to which the delimitations of the troubled neighborhood as drawn by the police authority needs to overlap our regional neighborhood unit (DeSO) for the unit to be considered treated. Our results are robust to both a minimum of 40 percent overlap and a minimum of 30 percent overlap (fifth and sixth panels), as compared to our baseline rule of 50 percent overlap.

6.3 Treatment Effect Heterogeneity

Having shown that our baseline results are robust to changes in specification and sample selection we now proceed with probing heterogeneous treatment effects. We focus on differences across gender, migrant background and family background. For the latter, we split the sample into whether the student is predicted to be low socioeconomic status (SES) or a high SES.[22] We then group those students in the first quartile of this prediction into low-SES students and students in the fourth quartile of this distribution as high-SES students.

Table 2 shows the results from our heterogeneity analysis. We find that neighborhood listing significantly decreases the probability of high school eligibility among boys but not among girls. The point estimate suggest a 2.7 percentage point decrease in high school eligibility for boys (3.0 percent relative to sample mean). The results also show a significant 0.059 reduction in high school quality and a 5.9 percent increase in distance to high school for boys. While the results also show a 6.7 percent increase in distance to high school for girls, there is no significant effect on high school quality among girls; albeit the estimate is not significantly different compared to that of boys.

Table 2 further shows that there is a significant decrease in high school eligibility, high school quality and a significant increase in the distance to high school for students born in Sweden. While the estimates for high school eligibility and high school quality among foreign-born students are not significantly different compared to native students, the estimates are statistically significant. Both for native and foreign-born students, there is an increase in the distance to high school.

[22] The prediction is based on a regression using baseline child and family characteristics to predict compulsory school GPA. These characteristics are children's year of birth, gender, country of origin, parental (mother and father) education attainment, parental year of birth, municipality of residence and calendar year. In this exercise, the strongest predictor is parental educational attainment.

The results in Table 2 also show a significantly larger decrease in high school quality among the high-SES students. While the point estimate for low-SES students is also negative it is not statistically significant. We do, however, find that neighborhood listing significantly increases the distance to high school for these students but not for the high-SES students. One possible explanation could be that low-SES students already attend the schools with the lowest quality and therefore have less margin to re-adjust, hence only rearranging (if anything) the distance to high school while keeping the quality constant. For high-SES, who already had a longer baseline distance to high schools, a possible explanation is the opposite one: they do not adjust on the distance margin because they were already attending schools further away, and instead choose to calibrate (if any) to lower quality schools of similar distance.

Table 2: Effect Heterogeneity Analysis by Student Characteristics

	Grade 9 GPA	High School Eligibility	High School Quality	Distance to High School
Full Sample	-0.018	-0.019	-0.043*	0.064***
	(0.034)	(0.013)	(0.024)	(0.022)
Gender				
Boys	-0.057	-0.027**	-0.059**	0.059***
	(0.036)	(0.014)	(0.025)	(0.023)
Girls	0.017	-0.012	-0.029	0.067***
	(0.035)	(0.014)	(0.025)	(0.024)
Origin				
Natives	-0.011	-0.026**	-0.048*	0.053**
	(0.035)	(0.013)	(0.025)	(0.022)
Foreign born	-0.032	-0.011	-0.034	0.080***
	(0.038)	(0.014)	(0.028)	(0.027)
Family background				
Low-SES	-0.044	-0.023*	-0.034	0.075***
	(0.035)	(0.013)	(0.026)	(0.024)
High-SES	-0.014	-0.021	-0.128***	0.029
	(0.048)	(0.016)	(0.043)	(0.032)
Observations	530,905	530,905	472,767	440,013

Notes: This table reports the average treatment effect of the treated (ATT) estimate of the average θ in Equation (1) by student characteristics: gender, origin of birth (Sweden/abroad) and family socioeconomic (SES) background. 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. Family background is measured as the predicted grade 9 GPA based on a regression including parental educational attainment, gender, birth cohort, and immigrant status. Low-SES are students in the first quartile of the family background score and High-SES are students in the fourth quartile of the family background score. Heterogeneous effects by gender, ethnic origin and SES are based on regressions that pool both sub-groups and refer to the linear combination of coefficients of a joint model with interactions. All models control for neighborhood fixed effects, year fixed effects, municipality-by-year fixed effects and neighborhood-specific linear time trends. Standard errors (in parentheses) are clustered at the neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

6.4 Mechanisms

Our analysis shows that the introduction of the list was neither coordinated with nor followed by any meaningful policy interventions. Moreover, the selection of neighborhoods was based on persistent characteristics, which are effectively accounted for in our research design. This suggests that the list may have influenced student sorting through increased negative media coverage, which amplified social stigma and raised concerns about social image, ultimately affecting perceptions and beliefs (Bursztyn and Jensen, 2017). We have already shown that this change in school sorting cannot be explained by changes in compulsory school grades. Yet, neighborhood labeling could potentially also influence student sorting through other mechanisms such as school-level resource reallocation or via behavioral adjustments by parents. In this section, we use data from multiple sources to shed some light on the potential roles of these different channels.

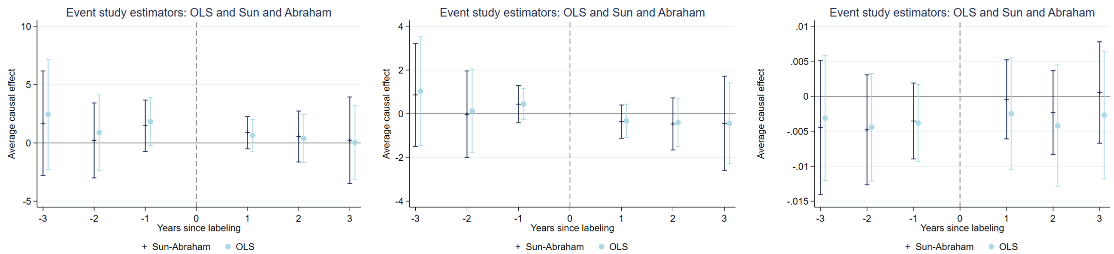
6.4.1 School Resources

As previously discussed, due to the centralized admission system, schools have limited opportunities to select students based on factors other than compulsory school GPA. However, schools can adjust their resource allocation. To explore this potential channel, we estimate a staggered difference-in-differences model to assess the effect of the list's introduction on school resources. In this context, our definition of treatment is based on a school's location. A school is considered 'treated' if it is situated in a listed neighborhood, with schools in non-listed neighborhoods serving as controls. We use occupational data to identify staff in both compulsory and high schools and calculate the average headcount, contracted hours, and the share of overtime at each school. The model is specified in Appendix B.1.

As can be seen in Figure 7 there is no significant effect of neighborhood listing on number of school staff, contracted hours, or the share of overtime. This verifies that schools did not respond in any meaningful way to the introduction of the list by changing resources.

Figure 7: School Resources - Treatment Effect in Listed Neighborhoods

(a) Staff count per school (b) Average weekly hours (c) Share working overtime



Notes: This figure plots the average treatment effect of the police list on neighbourhood school 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. Sub-figure (a) shows effects on staff per school, Sub-figure (b) for average weekly hours, and Sub-figure (c) for share of staff working overtime. 95% confidence intervals are based on standard errors clustered at the neighborhood level.

6.4.2 Parental Responses

One of the most obvious ways in which neighborhood listing could affect students is by directly changing the labor market opportunities of their parents. Changes in economic resources may in turn influence parental investments in their children. We hypothesize that any negative short-term labor market effects for parents of the report would negatively affect children's studying effort and academic aspirations due to financial strain and psychological stress. We acknowledge however that it is a priori unclear to what extent potential freed up time to spend with one's children would balance up these negative effects. We examine this by estimating models similar to Equation (1) where the dependent variable is either an indicator for whether the parents to the children in our sample are not in employment (extensive margin), or annual earnings (extensive and intensive margin). The results in columns (1) and (2) of Table 3 show no significant effect of neighborhood listing on these labor market outcomes.

Parents could also respond to neighborhood listing by moving to another residential neighborhood, potentially mitigating any negative effect on the children. In our sample 7.9 percent of parents change residential neighborhood in any given year. Column (3) of Table 3 shows that there is no significant effect of neighborhood listing on the probability that the family moves.[23]

Table 3: Parental Labor Market and Migration Responses

	Dependent variable:		
	P(Not employed=1)	Log(Earnings)	P(Move=1)
Listed x Post	-0.003	0.003	-0.008
	(0.003)	(0.011)	(0.007)
Mean of dependent variable	0.150	-	0.078
Individual-year observations	7,160,501	6,068,502	7,160,501

Notes: This table reports the average treatment effect of the treated (ATT) estimate of the average θ in Equation (1). These are estimated using a staggered difference-in-differences approach, where the treatment group includes parents whose children graduated from grade 9 in the study period and resided in listed neighborhoods upon graduation, and the control group includes parents whose children graduated from grade 9 in the period under study and lived on non-listed neighborhoods upon graduation, weighted by their propensity of being treated on pre-policy characteristics. Columns (1)-(2) present result for labor market outcomes: probability of not being employed and the natural logarithm of earnings for those in employment. Column (3) reports estimates for the probability of moving (i.e. changing DeSO from one year to another). All models control for individual fixed effects, year fixed effects, municipality-by-year fixed effects and neighborhood-specific linear time trends. Standard errors (in parentheses) are clustered at the individual-neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

6.4.3 Social Image Concerns

To shed light on the role of changes in student perceptions and beliefs, we employ survey data from repeated cross-sections (5-year intervals) comprising roughly 5,000 ninth graders in Sweden. The data come from the Evaluation Through Follow-up (UGU) survey, the largest Swedish longitudinal school survey. It covers a representative sample of ninth graders expected to complete compulsory school in the years 2014 and 2020. This means that we have data from before and after the introduction of the list. One difference compared to our population-based data is that the survey does not contain the neighborhood of residence for the students. Instead, we use geocoded data on the location of schools to define whether a given school is located in a listed neighborhood.

The survey contains several questions where the answers are collected using a 5-step Likert response scale. In the analysis we standardize all variables before calculating the average

[23]We further find no effect of neighborhood listing on residential mobility among all residents, including non-parents (results available from the authors upon request). This is consistent with the results in Andersson et al. (2023) who show that neighborhood labeling is connected with a modest 3.7 percent decline in house prices.

responses across the multiple survey questions within a given domain.[24] Table 4 shows the difference-in-differences estimates of the effect of the neighborhood listing on various measures of student beliefs. To mimic our baseline empirical model as close as possible we continue to weight the regressions using the inverse probability of the propensity score from a regression of the probability of the neighborhood of the school being listed on pre-policy neighborhood characteristics. Besides the main effects (dummies for listed neighborhood and post), the regressions control for baseline student characteristics gender, age parental education (low/high).

We can see from the results reported in Table 4 that neighborhood listing affects in particular high-SES students as proxied by the educational attainment of the parents. For these students, listing leads to a 0.547 standard deviation reduction in one's perception of being fairly treated, a 0.311 standard deviation decrease in one's perception that effort pays off, and a 0.453 standard deviation increase in feelings of despair. These estimates are all statistically significant. In contrast, there is no significant effect of neighborhood listing for low-SES students. One potential explanation for the larger effects among high-SES students is that these students may be better informed about the negative public attention, or alternatively, more quickly update their beliefs as a result of the negative information as compared to their lower-SES peers.

Overall, the evidence presented in this section is consistent with neighborhood listing mainly affecting student sorting through social image concerns.

[24] Whenever needed, we reverse the scale of items to get all items in each domain set on the same orientation (positive or negative).

Table 4: The Effect of Neighborhood Listing on Student Beliefs

	Dependent variable:		
	Fairly treated	Effort pays off	Feeling despair
Panel A: Full sample (N=4,088)			
Listed x Post	-0.128	-0.118	0.486
	(0.209)	(0.123)	(0.297)
Panel B: High-SES parents (N=2,635)			
Listed x Post	-0.547**	-0.311***	0.453*
	(0.268)	(0.140)	(0.427)
Panel C: Low-SES parents (N=1,453)			
Listed x Post	0.258	0.016	0.650
	(0.302)	(0.200)	(0.435)
Individual & neighborhood characteristics	Y	Y	Y

Notes: This table presents the difference-in-differences estimates for the effect of the school being located in a listed neighborhood on various self-reported measures of student beliefs. The data come from the national school (UGU) survey covering two survey waves of ninth grade students expected to finish compulsory school in 2014 and 2020. All dependent variables are based on the responses on a 5-point Likert scale. The variables are standardized and item scales converted to reflect the outcome definition whenever appropriate. The dependent variable in column (1) is the weighted average of the following items: How often do you feel like this at school and in your spare time? "I am treated fairly" and "Feel unfairly treated by teachers?" The dependent variable in column (2) is the weighted average of the following items: If you really made an effort, would you be more smart? "You have not made an effort if you get low marks in school?" and "Even if you are not smart can you get high marks in school if you make an effort". The dependent variable in column (3) is based on the answer to the item: How often do you feel like this at school and in your spare time? "I am unlucky and things just happen." The regressions are inverse probability weighted using the propensity score from a regression of the probability of the neighborhood (DeSO) of the school being listed as a troubled neighborhood on pre-policy (2014) neighborhood characteristics. Besides the main effects, the regressions control for student gender, age, parental education (low/high) where missing values on parental education are included in the full sample and controlled for in the regressions. Neighborhood (DeSO) controls included are (log) population size, share employed, share foreign born, share young individuals, average disposable income, share single parents. In the bottom two panels, Low-SES and High-SES parental sub-samples refer to at least one parent having or not a college degree. Standard errors (in parentheses) are clustered at the school level.*** p<0.01, ** p<0.05, * p<0.1.

6.5 Additional Results

To explore how the effects of neighborhood listing persist throughout high school and beyond, we make us of data on time of high school graduation. Since the situation report was only recently introduced, our ability to examine intermediate and long-term outcomes is currently limited. Nevertheless, we can investigate the effects of neighborhood listing on the probability of completing high school on time (by age 19) for the cohorts affected by the first release of the police report, i.e. those completing compulsory school through 2017. Table 5 presents these estimates. While the estimate for the full sample is statistically insignificant, i t conceals important heterogeneity: a marginally significant positive effect on high school completion for low-SES children, contrasted with a significant negative effect for high-SES students. For high-SES students, the point estimate indicates a 4.8 percentage point reduction in the probability of on-time high school completion, equivalent to a 5.3 percent decrease relative to the mean of the dependent variable.

Table 5: The Effect of Neighborhood Listing on High School Completion

	Dependent variable: P(graduating HS on time=1)			Observations	Mean of dep. var.
	Full Sample	Low-SES	High-SES		
Listed x Post	0.005	0.029*	-0.048**	309,277	0.900
	(0.014)	(0.015)	(0.019)		

Notes: This table reports the average treatment effect of the treated (ATT) estimate of the average θ in Equation (1). 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. Low-SES are students in the first quartile of the family background score and High-SES are students in the fourth quartile of the family background score. Results by SES refer to the linear combination of coefficients from a joint model with interaction terms. All models control for neighborhood fixed effects, year fixed effects, municipality-by-year fixed effects and neighborhood-specific linear time trends. The sample includes all students in urban areas who completed compulsory school in years 2011-2017. Standard errors (in parentheses) are clustered at the neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

To shed some light on the potential long-run effects on earnings we calculated the implied earnings loss at age 28 using children who finished compulsory school in 2008 for whom we can observe their earnings until 2020. The regression adjusted correlation between high school quality and earnings indicates that one standard deviation decrease in school quality decreases earnings at age 28 by 5.8 percent.[25] Multiplying this number with the treatment effect for high school quality in Table 1 reveals that the implied earnings loss at age 28 is 0.34 percent. This may seem like a modest effect in comparison to earnings effects documented for direct placed-based policies. For example, Aaronson et al. (2024) show that the so called "redlining" maps on creditworthiness that were drawn in American cities in the 1930 by the Home Owners Loan Corporation (HOLC) causally depressed annual real wage by 2.9 percent for those who grew up at the lower-graded side of the red line, i.e., the map boundary. Crucially though here, as we document empirically, the implied earnings loss is conferred through labeling only and not through any place-based policy that would directly affect the fundamentals of the neighborhood whereas in the HOLC case, the differential access to credit across the boundaries led to reduced home ownership rates and increased racial segregation among other profound effects for the redlined neighborhood (Aaronson et al., 2021).[26]

7 Concluding Remarks

The 2015 release of a police situation report publicly listing troubled neighborhoods—often referred to as 'no-go zones'—triggered a significant negative information shock, unaccompanied by immediate policy interventions. Using data from various sources and a difference-in-differences research design, we demonstrate that the introduction of the list led to a marked increase in negative media coverage, likely reinforcing the stigmatization of the listed neighborhoods.

Our main findings reveal a significant effect of neighborhood listing on school sorting, independent of any changes in students' compulsory school performance. Students from listed neighborhoods began attending lower-quality high schools located farther from their homes, with the impact being more pronounced for high-SES students. This impact seems to persist throughout high school delaying graduation.

[25]The regression controls for gender, foreign-born background, and municipality of residence.

[26]Note also that our extrapolations on earnings based on our effects on high school quality abstract from any spatial discrimination on the labor market. See Zenou and Boccard (2000) for a model in which both spatial and ethnic discrimination depress labor market outcomes of ethnic minorities.

In our exploration of potential mechanisms, we find no evidence of changes in policing, school resources, family relocation, or household income. Instead, neighborhood labeling emerges as the primary driver of these effects. Survey data from ninth-grade students further corroborate this, showing reduced student effort and heightened perceptions of discrimination, particularly among high-SES students.

Overall, our results suggest that public labeling of disadvantaged neighborhoods can significantly shape student behavior, even without direct policy interventions. This stigmatization may also have a lasting impact on social and economic disparities; however, due to the limited existing evidence, further research is needed to confirm these findings. From a policy standpoint, addressing and mitigating negative perceptions associated with these neighborhoods could be a cost-effective strategy for improving outcomes in disadvantaged areas, without the need for substantial financial investments or major policy changes.

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Appendix For Online Publication

A Additional Tables and Figures

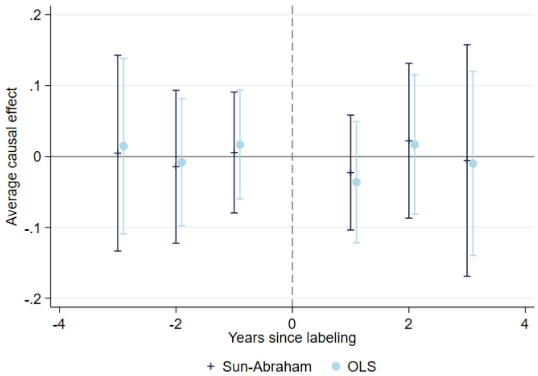
Table A.1: Descriptive Statistics

	Listed	Non-Listed
Panel A: Sampling Frame		
	N	N
Study persons	45,621	521,696
Neighborhoods	220	3,104
Panel B: School Performance and High School Sorting		
	Mean	Mean
Grade 9 GPA (standardized)	-0.588	-0.115
	(1.137)	(0.964)
High school eligibility	0.713	0.888
	(0.452)	(0.315)
High school quality (standardized)	-0.444	-0.055
	(0.949)	(0.782)
Distance to high school (in kms)	6.689	10.235
	(5.052)	(9.503)
Panel C: 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)

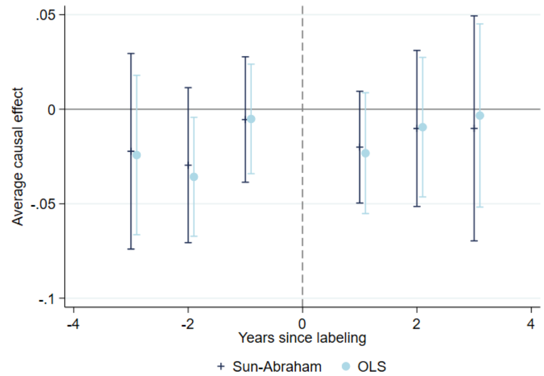
Note: All descripive statistics refer to year 2015, i.e., the final pre-treatment year. Grade 9 GPA and High school quality are standardized standardized over the entire study period (2010-2020) with mean zero and unit standard deviation for the control group. Median earnings is based on earnings for all residents including those with no taxable income. Standard deviations are reported in parentheses.

Figure A.1: Event-Study Estimates of the Effect of Neighborhood Listing on Student Performance and Sorting

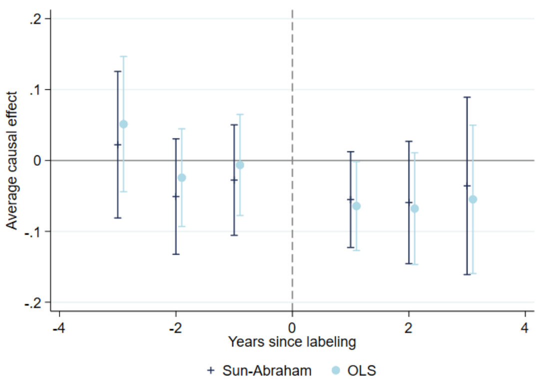
(a) Grade 9 GPA



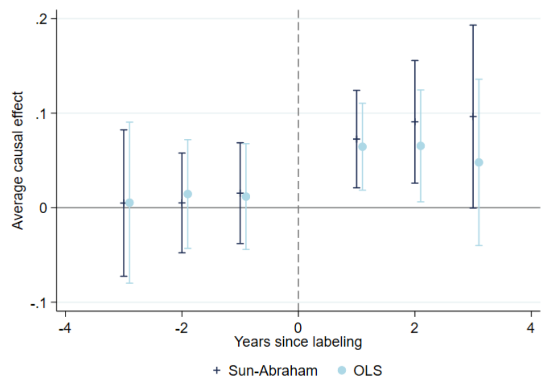
(b) High school eligibility



(c) High school quality

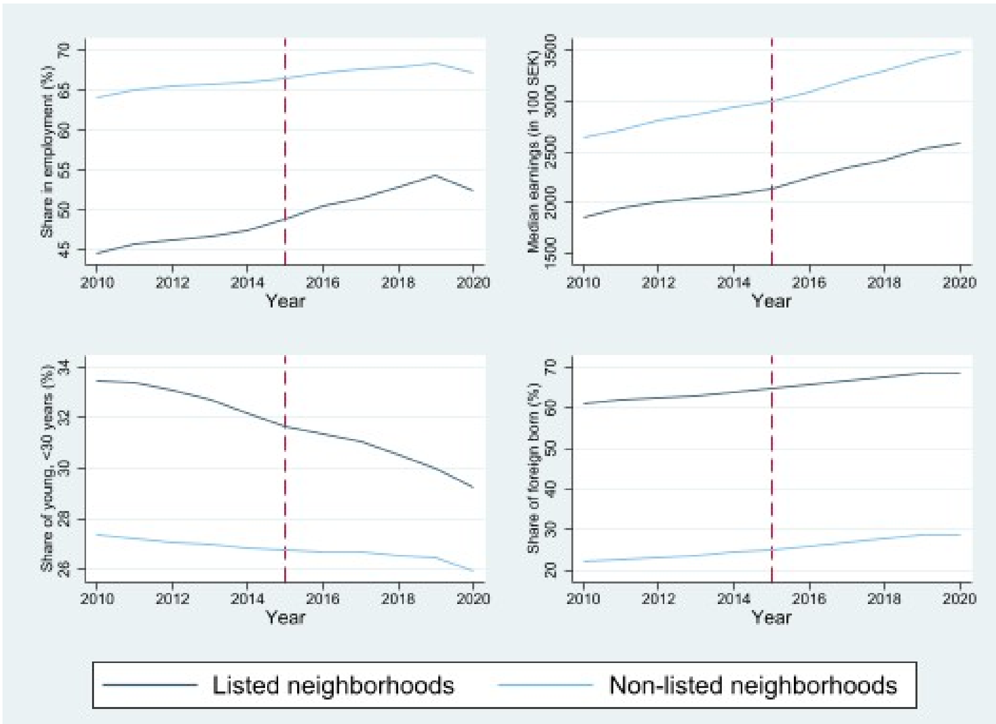


(d) Distance to high school



Notes: This figure plots the event-study estimates corresponding to equation (1) of the effect of the police list on student outcomes. 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. Both estimates of TWFE and Sun and Abraham (2021) estimators are presented using the same error-term structure as in for example column (2) of Table 1. Sub-figures in Panel A report results for compulsory school performance and figures in Panel B for high school sorting. 95% confidence intervals are based on standard errors clustered at the neighborhood level.

Figure A.2: 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.

B Evidence Against Policy Responses

This Appendix Section describes the empirical strategy employed in section 3.3 to shut down any place-based policies by the police authorities or any other coordinated agents simultaneous to or as a response to the situation report issued by the police.

B.1 Event-Study Specification

We estimate a staggered difference-in-differences model of the effect of neighborhood listing on police resources using non-listed neighborhoods in urban areas as controls. Here we describe our regression model used to estimate the effect of neighborhood listing on police resources shown in Figure 4 presented in the main text. The regressions are weighted by the propensity to be treated based on pre-policy characteristics. The estimating equation is as follows:

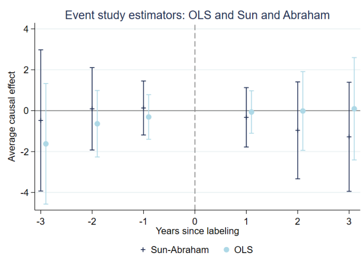
$$Y_{nt} = \alpha + \sum_{k} \beta^{k} \mathbb{1}\left[\mathrm{L}_{n} = t - k\right] + \phi_{mt} + \eta_{n} + \delta X_{nt} + \epsilon_{nt}, \tag{2}$$

where Y_{nt} is an outcome of interest in neighborhood n at year t, and $\mathbb{1}\left[\mathrm{L}_{in} = c - k\right]$ is an indicator for the listing of neighborhood n occurring k periods from t (negative k indicating a future event date). We also control for neighborhood η_n, and municipality-by-year ϕ_{mt} fixed effects, as well as for neighborhood specific linear time trends X_{nt}. The model assumes conditional parallel trends in outcomes for listed and non-listed neighborhoods, which we are able to verify by testing whether the anticipatory effects at $k = 3, 2, 1$ are statistically significant. This strategy provides estimates of the effect of neighbourhood listing on police manpower by year since the first release of the list (year 0). We estimate the model using two way fixed effects ordinary least squares (TWFE) and an estimator proposed by and Sun and Abraham (2021), the results both for which are shown in Figure 4.

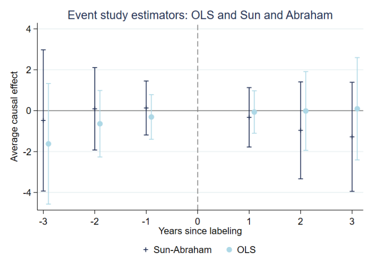
B.2 Additional Empirical Evidence

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

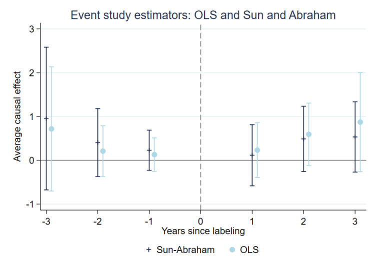
Panel A: Staff Count



(a) Employees per station

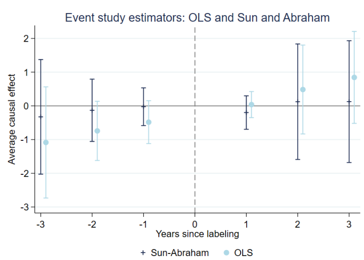


(b) Police officers per station

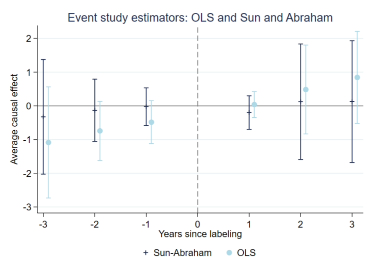


(c) Private security guards

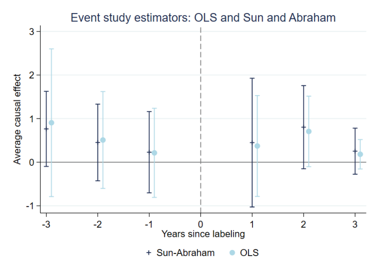
Panel B: Average Weekly Hours



(a) Employees per station

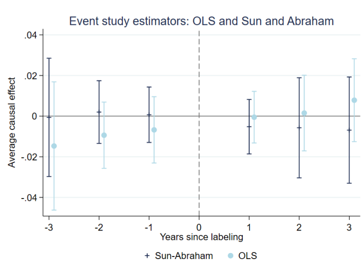


(b) Police officers per station

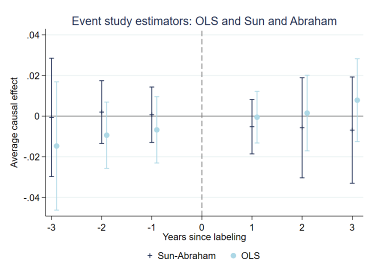


(c) Private security guards

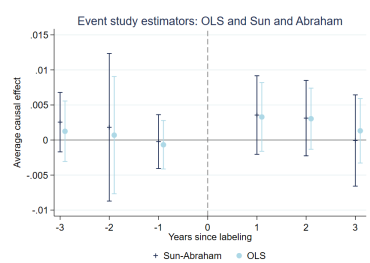
Panel C: Share of Staff Working Overtime



(a) Employees per station



(b) Police officers per station



(c) Private security guards

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

C Robustness

Table C.1: Effect of Neighborhood Listing on Student Performance and Sorting - In Sub-samples and Across Treatment Definitions

	Grade 9 GPA	High School Eligibility	High School Quality	Distance to High School
Panel 1: Baseline				
Listed x Post	-0.018	-0.019	-0.043*	0.064***
	(0.034)	(0.013)	(0.024)	(0.022)
Observations	530,905	530,905	472,767	440,013
Panel 2: Selected controls				
Listed x Post	-0.030	-0.017	-0.039*	0.047**
	(0.029)	(0.011)	(0.022)	(0.019)
Observations	131,032	131,032	113,451	108,030
Panel 3: Dropping adjacent neighborhoods				
Listed x Post	-0.023	-0.014	-0.041	0.065***
	(0.040)	(0.015)	(0.026)	(0.025)
Observations	492,467	492,467	439,000	408,012
Panel 4: First wave only				
Listed x Post	0.030	-0.008	-0.008	0.061**
	(0.035)	(0.014)	(0.029)	(0.026)
Observations	517,734	517,734	461,104	429,032
Panel 5: 40% Overlap				
Listed x Post	-0.018	-0.016	-0.040*	0.064***
	(0.033)	(0.013)	(0.023)	(0.022)
Observations	530,905	530,905	472,767	440,013
Panel 6: 30% Overlap				
Listed x Post	-0.010	-0.018	-0.035	0.066***
	(0.034)	(0.013)	(0.024)	(0.021)
Observations	530,905	530,905	472,767	440,013

Notes: This table reports the TWFE difference-in-differences (DiD) estimate of the average θ in Equation (1). 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. The sample includes all students in urban areas who finished compulsory school in the period 2011-2021. Panel 1 shows baseline estimates as in Table 1, Panel 2 shows estimates for a sample that includes only control neighborhoods matched based on pre-policy characteristics (share of foreign born residents and average income) as in Appendix Figure C.1. Panel 3 shows estimates removing neighboring neighborhoods (DeSO). The last three panels shows results using variations of the baseline treatment definition. Panel 4 shows estimates using only the first wave in a static DiD setting, Panel 5 shows estimates using a 40 percent cutoff, and Panel 6 shows estimates using a 30 percent cutoff. Standard errors (in parenthesis) are clustered at the neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

Table C.2: 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 C.1: Robustness Exercise - 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.

D Institutional Background

D.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, e.g., 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).[27] 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. Roughly 30 percent of the ninth graders in troubled neighborhoods do not complete compulsory school as compared to the 13 percent nationwide (Statistics Sweden (SCB), 2018). 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 though, 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

[27] 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.

Gothenburg (the second largest city in Sweden), saw 298 gun-related homicides in 2020. Even though crime rates have gone down in New York City during the past decades, the Bronx with its 1.5 million inhabitants still has roughly 150 murders annually. In the European context though, the Swedish capital Stockholm is almost an outlier, having for example recorded a 30-fold gun murder rate in comparison to London in 2022 (Engel Rasmussen, 2023).

D.2 Additional Figures

Figure D.1: Examples of Media Coverage in the Main National News Outles

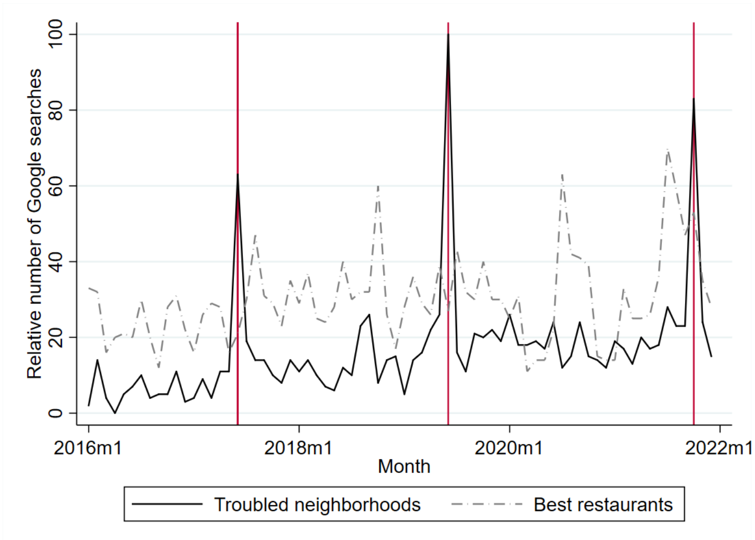
(a) Headline from Dagens Nyheter

(b) Headline from Dagens Nyheter

(c) Headline from Svenska Dagbladet

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 D.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.