Effects of Neighborhood Labeling on Student Performance and Sorting*

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Abstract

Exploiting the release of a much publicized official situation report on carefully delineated deprived neighborhoods we explore how this well-documented negative information shock affected the performance and sorting of young residents at the junction of completing compulsory school and enrolling in high school. 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. While living in a listed neighborhood does not affect student performance, it does increase student sorting. Students from listed neighborhoods choose to enrol in lower quality high schools located further away from their homes. The reduction in high school quality is significantly larger for students in listed neighborhoods who come from high socioeconomic backgrounds. Evidence on complementary survey data shows that these students are more likely to report feeling unfairly treated, that effort does not pay, and sentiments of despair, relative to students in control neighborhoods. Our results are consistent with models of neighborhood labeling, wherein residents in neighborhoods with a negative public image adjust their behavior in response to anticipated or actual discrimination.

JEL Classification: I24, J13, K42, R23.

Keywords: Spatial stigmatization; Social image; Neighborhood effects; Student performance; Student mobility.

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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). 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; 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.[1] 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.[2]

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, an internal situation report listing troubled neighborhoods.[3] The list of neighborhoods and its publicly available supplement of geocoded maps has since been updated biannually on specific dates and without any pre-notification of additions to or removals from the report,

[1] 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).

[2] A large literature shows that neighborhood characteristics, such as poverty, crime, and minority concentration, are causally linked to the outcomes of residents, especially those of children, see 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).

[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).

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, and 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 outside the boundaries of these places.

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 "negativity" 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 reveal a significant increase in negative media coverage 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

[4]The listed neighborhoods are clearly disenfranchized in their own right at baseline, as is described more in detailed in Appendix Section C.1. The negative information shock hence increases public awareness of the existing situation but also generates an artificially discrete categorization of troubled and not-troubled neighborhoods by construction.

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 identify troubled 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 a "stigma shifter".

Our main analysis estimates the impact of the report on student performance and sorting. Specifically, our difference-in-differences estimates, which contrast the change in the outcomes of students in listed versus non-listed neighborhoods after the release of the list, suggest no significant effect on educational performance, as measured by grade 9 GPA 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 and show that our estimates translate into a Y % reduction in earnings at age Y. Our estimates reveal no evidence of differential pre-trends and remain robust after conducting a battery of specification checks.

We also explore 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.[5] 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 attitudes towards school work and perceptions about discrimination provides additional support for the labeling hypothesis. Specifically, we employ data from repeated cross-sections (5-year intervals) of a 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 significant reductions in student effort and heightened feelings of 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.[6] While highlighting

[5]Consistent with this, Andersson et al. (2023) show that neighborhood labeling is connected with a modest 3.7% decline in house prices in the short run.

[6]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 remedial interventions, each of which affected the targeted neighborhoods and residents therein. A related example of a place-based policy documented to have had a positive 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 casual 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). Little, however, is known about the underlying mechanisms that produce these effects (see Chyn and Katz 2021). 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. 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. 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.

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

Section 5 discusses our results and finally, Section 6 offers some concluding remarks.

2 Institutional setting

This section outlines relevant details surrounding the policy as well as discusses media attention and policy responses. We end by placing the Swedish disadvantaged neighborhoods in an international context.

2.1 The introduction of the list

In December 2015, the Swedish police initiated the public dissemination of their internal situation report, identifying "troubled" urban neighborhoods, which was used to identify and monitor these 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 broke out in Sweden. 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.[7]

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). In addition to these social factors, the police authority involved the local police districts (LPOs) 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. The National Operative Unit (NOA) of the police authority then compiled the list into a situation report detailing

[7]See https://www.dn.se/nyheter/sverige/detta-har-hant-1/

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 on the list. It is evident that the majority of the neighborhoods initially listed remain on subsequent revisions, with only three neighborhoods removed from the list.[8] 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.

[8]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 we use in our analysis, i.e., 2015, 2017 and 2019 releases. The data are sourced from the National Operative Unit (NOA) of the Swedish Police Authority.

2.2 Portrayal as "No-go" zones

This 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 "no-go zones".[9] Since its

[9]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 references 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 C.1 for examples of headlines.

introduction, major media outlets have consistently covered the listed neighborhoods, often making them the focal point of news coverage (Backvall, 2019). Below we show results from analyses that aims to quantify the magnitude of the coverage.

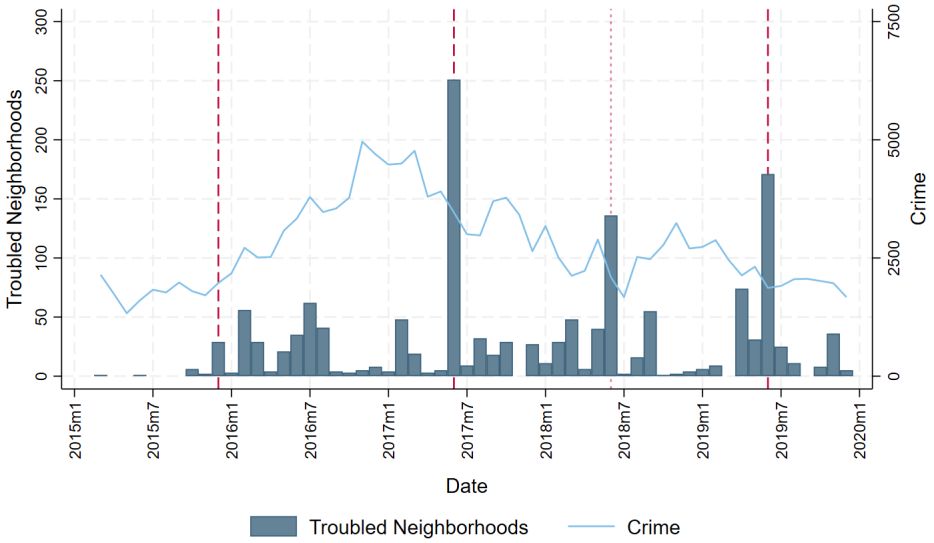
To quantify both the extent and nature of the media discussion, Figure 2 shows textual analysis of mentions of troubled neighborhoods in news articles in Swedish. The analysis uses data from a global news media monitoring project called GDELT[10]. The data covers all media articles published online since February 2015. It is clear from the figure that the number of articles in Sweden discussing "troubled neighborhoods" (utsatta områden) starts to increase just after the release of the first situation report in December 2015. The release of the situation report and each subsequent revision (shown in dashed red vertical lines) coincides with 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.

Appendix Figure C.2 depicts the relative number of Google searches for the terms "troubled neighborhoods" (utsatta områden) and "best restaurants". Clear spikes in searches for "troubled neighborhoods" are evident during the months when the list is updated. During these periods, the relative number of searches even exceeds that of common terms like "best restaurants".[11] We interpret this as evidence not only of significant media attention surrounding this report but also of considerable interest among the general public.

[10]See `www.gdeltproject.org`

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

Figure 2: Media Mentions - "Troubled Neighborhoods"

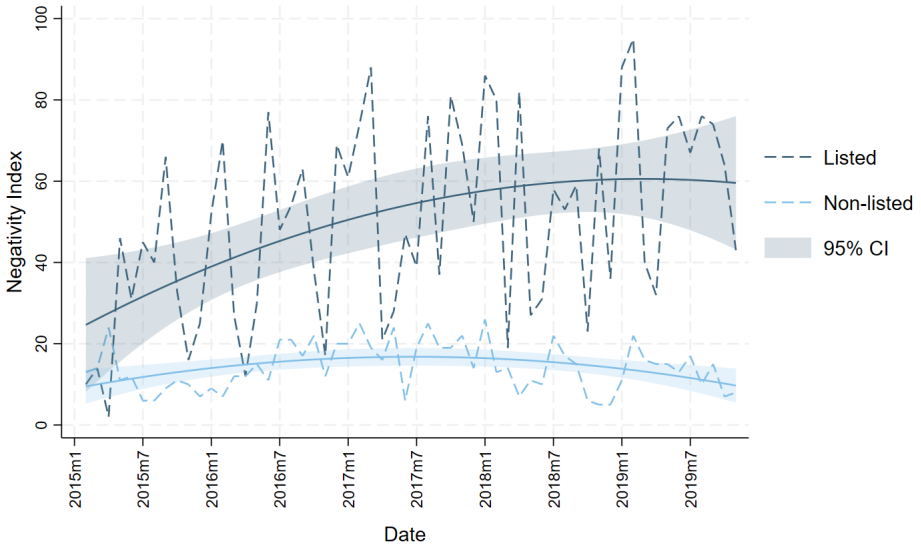


Notes: This figure plots the number of articles in Swedish media including the term "Troubled neighborhoods" (Utsatta områden). Red dash vertical lines indicate months in which the list was released or updated. The lighter red dotted line in 2018 indicates an update that was expected and did not take 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 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. Figure 3 indicates that, as expected, listed neighborhoods are portrayed with a more negative tone than non-listed neighborhoods. Moreover, the gap in negativity between listed and non-listed neighborhoods widens after the police reports are released. In terms of magnitudes, in 2019 while the average negativity index for non listed neighborhoods is close to the 10th percentile, it is close to the 60th percentile for listed neighborhoods. This means that the gap in negative portrayal

between listed and non-listed neighborhoods grew from a statistically indistinguishable 10 percentile difference pre-policy to a significantly 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 large general interest in the listed neighborhoods, combined with their negative portrayal, may have contributed to the stigmatization of these areas (Backvall 2019; National Audit Office 2020). This "spatial stigmatization" may have led to negative peer influences and anticipated or actual discrimination among the exposed residents (Wacquant, 2008; Sampson et al., 2002).

2.3 What policy responses did the list give rise to?

In this section, we discuss the extent to which the negative information shock on listed neighborhoods was related to remedial policy interventions.[12] We organize our discussion into qualitative and quantitative evidence.

2.3.1 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).[13] Such report also shown that s 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.

The municipalities could of course deal in many other ways with the social issues and crime prevention. Much of the crime preventive work is handled by other authorities than the police, in particular by schools. Based on our interviews with officers at the The Swedish National Council for Crime Prevention (BRÅ) it seems clear that the co-operation between

[12] A recent literature demonstrates that place-based policies targeting neighborhoods can help to improve the opportunities for the residents therein, but also has the potential to backfire by making the disadvantages of the concerned neighborhoods more salient to the general public (Garrouste and Lafourcade, 2022; Glaeser and Gottlieb, 2008; Aaronson et al., 2024; Koster and van Ommeren, 2022; Damm et al., 2024)

[13] 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 (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).

the local police and other local agents has improved slowly but steadily during the past two decades. However, the police list of troubled neighborhoods did not coincide with any particular programs of increased cooperation between authorities. According to a report by BRÅ, the Police Authority provided increased guidance of community policing to the local police from 2015 onwards. However, the report evaluates through four case studies the fulfillment of explicitly stated community contracts in which the local police in collaboration with local authorities commit themselves to an idiosyncratic menu of goals. BRÅ finds little or no impact of the community contracts on the police practices. Among the mechanisms rationalizing this null effect, BRÅ highlights low priority of community policing by the local police district as well as poor collaboration between the local police and the other local agents (BRÅ, 2018).

2.3.2 Quantitative evidence

To empirically substantiate the qualitative evidence suggesting a lack of meaningful policy responses—at least during the first three rounds of the report—we use population-based data, as discussed in detail in Section 3, which links police staff to their geocoded workplaces. For the police station nearest to each neighborhood, we calculate the average number of staff, contracted hours, and the share of those doing overtime.[14] For each of these dependent variables, we estimate a staggered difference-in-differences model, where control neighborhoods consist of non-listed neighborhoods and the regressions are weighted by the propensity to be treated based on pre-policy characteristics.[15] The estimating equation is as follows:

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

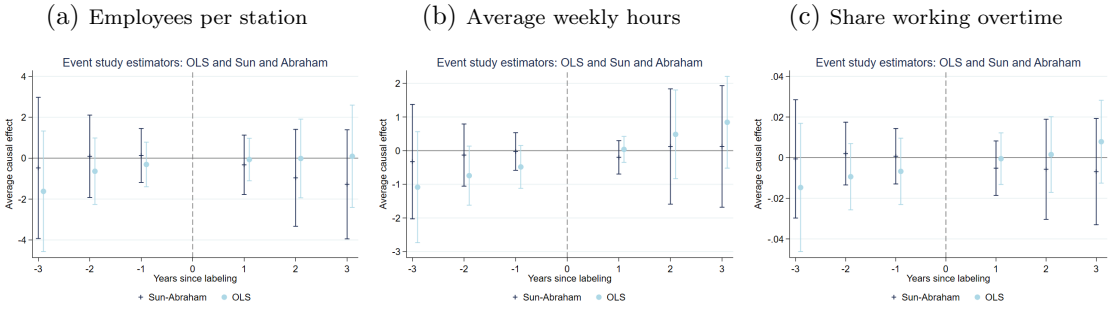
where Y_{nt} is an outcome of interest in neighborhood n at year t, and $\mathbb{1}\left[\mathrm{L}_{in} = c - k\right]$ is an indicator for the listing of neighborhood n occurring k periods from t (negative k indicating

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

[15]This strategy mirrors our empirical design, using micro-level data as discussed in detail in Section 4.

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 OLS and Sun and Abraham (2021) estimators, and results for both are shown in Figure 4.

Figure 4: Police Resources - Treatment effect in Troubled Neighborhoods



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. OLS 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 DeSO (neighborhoods) level.

Our 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.[16] In Appendix Figure C.3 we show that the results for police staff hold when only considering police officers, or alternatively considering private

[16]One can also argue that a modest policy response by the police itself was expected since the list already existed and was used internally by the police before its release.

security guards.[17]

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

2.4 The Swedish School System

2.4.1 School choice

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 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 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. This strategy-proof assignment mechanism means that students have no incentive to misrepresent their preferences and that schools have limited possibilities to "cream-skim" students based on other characteristics than 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

[17]Municipalities have the possibility of contracting private security companies in order to improve public safety.

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

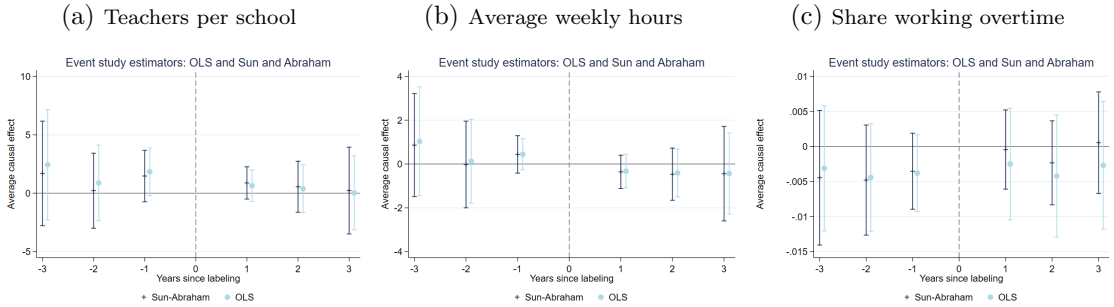
classes for a given track. Consequently, a choice might be oversubscribed in one region but not in another, and this can vary from year to year. In this setting, it is important not to confuse "oversubscription" with "highly competitive". There is no consistent ranking of school-by-track in terms of higher cutoffs or likelihood of oversubscription. Additionally, average cutoffs, whenever they exist, are generally similar across different choices.

After admission decisions are released in July, students may reallocate to different schools and fields of study for various reasons. For instance, in a given school a student initially admitted to Social science might decide to switch to Natural Science, which still has open slots. Such a switch would free up a slot in Social science for another student. While changes can occur at any time, switching tracks becomes more difficult after the first fall semester due to curriculum differences.

2.4.2 Orthogonality of school resources to listing of neighborhood

As discussed earlier, because of the centralized admission system, there are very limited possibilities for schools to select students based on other characteristics than compulsory school GPA. However, schools have the option of adjusting resources. To investigate this we estimate regressions similar to Equation 1 where, instead of studying police resources, we estimate the effect of the introduction of the list on school resources e.g., teachers. We focus on compulsory schools I thought that were looking at high schools. This seems more relevant. Can we do this instead? Perhaps both? And then adjust this text accordingly. Relatedly, do we not want to have (log) number of students as an outcome? or student/teacher ratio? located in listed and non-listed control neighborhoods. We identify teachers using occupational data and use the information to calculate the average number of employees, contracted hours and share of overtime in each workplace/school. As can be seen in Figure 5 there is no significant effect of the introduction of the list on number of teachers, contracted hours, or the share doing overtime. This suggests that schools did not respond to the introduction of the list by changing resources.

Figure 5: School Resources - Treatment effect in Troubled Neighborhoods



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. OLS and Sun and Abraham (2021) estimators are presented. Sub-figure (a) shows effects on teachers per school, Sub-figure (b) for average weekly hours, and Sub-figure (c) for share of teachers working overtime. 95% confidence intervals are based on standard errors clustered at the DeSO (neighborhood) level.

3 Data

In this section, we discuss our micro-level data and how we classify treatment status based on the police's neighborhood list.

3.1 Swedish Administrative Register Data

Our main analysis data come from individual-level administrative registers, covering the entire Swedish population of ninth graders in years 2000-2021 that were merged by Statistics Sweden. The data include grade point averages (GPAs) at the completion of compulsory school in grade nine; enrolment in high school and the tracks therein, school and teacher identifiers for both compulsory school and high school. The data also contain sociodemographic background characteristics of the working age population (16-64 years) in years 2000-2021, allowing us to characterize all neighborhoods but also to link the ninth graders to their parents and hence, through the geocoded location of parents' residence, to the neighborhood of residence in any given year between 2000-2021. Appendix Figure C.1 shows the total num-

ber of individuals and neighborhoods included in our sample by treatment status, i.e., listed and non-listed. There are 220 neighborhoods and 45,621 students in the listed areas and 3,104 neighborhoods and 521,696 students in the non-listed control areas located in urban regions. Appendix Table C.2 shows descriptive statistics for selected variables included in the analysis for listed and non-listed neighborhoods.

3.2 Maps of Listed Neighborhoods

We complement our analysis data with geocoded maps of the listed neighborhoods provided by the Swedish police authority (see Figure 6a). As the police authority draws its own maps of troubled neighborhoods, the 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). In the absence of individual-level information on geocoded location of residence, we define treatment at the DeSO-level. Specifically, if more than 50 percent of a DeSO unit's land area falls within the boundaries of troubled neighborhood as drawn by the police (who avoided including commercial property, highways and green areas, see Figure 6(b)), we identify the DeSO as being listed.[19].

All listed neighborhoods are located in urban municipalities. For this reason, we restrict the analysis to students living in urban municipalities. We follow (Brottsförebyggande rådet (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.

[19]We gauge the feasibility of this operationalization of exposure in Appendix Table B.2 by comparing descriptive statistics for age structure and employment of listed neighborhoods based on the 50 percent overlap rule to the definition used by Statistics Sweden based on exact 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. Moreover, in the robustness checks we show that our results are insensitive to small changes in the cutoff used for the share of overlap.

Figure 6: Map of Listed Neighborhoods of 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 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/.

If we want to describe the outcome variables here instead of in table notes, then the following paragraphs need to be elaborated. BTW, I think paragraphs work better than subsubsections in this case.

3.3 Main Outcome Variables

3.3.0.1 Student performance

Grade 9 GPA is standardized 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 upper secondary education.

3.3.0.2 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 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 (DeSO unit) 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:

$$Y_{inc} = \gamma + \sum_{k} \theta^{k} \mathbb{1}\left[\mathrm{L}_{in} = c - k\right] + \pi_{mc} + \kappa_{n} + \lambda X_{inc} + \epsilon_{inc}, \tag{2}$$

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, 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.[20]

By including neighbourhood, cohort, and municipality-by year fixed effects, nd neighborhood-specific time trends in our estimating equation (2) our coefficients of interest, θ^k, captures the effect on the cohort member of exposure to the listing of one's listed 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 affected student performance and sorting, such as crime patterns or religious radicalization.

We firstly estimate equation (2) by OLS. 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 deregulation policy (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021, e.g.). These concerns are, however, less pronounced in our setting since: (i) our 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 3, narrows down the control group and ensures common trends with regard to key socioeconomic characteristics. The comparison units are further narrowed down for the analytic sample using propensity score matching. We match each treated unit with control units based on unit-specific average baseline characteristics.Magdalena, can you fill in the details in a footnote? These characteristics are measured before the release of the first list in 2015, and include factors that are considered for the labeling of Troubled Neighborhoods according to Nationella Operativa Avdelningen (NOA) (2015).

[20]We consider treatment to be an absorbing state in the sense that in those three neighborhoods listed in 2015 that where removed from the list in reports 2017 or 2019, the labeling effect is assumed to remain. Previous research shows evidence of bad reputation stickiness conveyed by policy labeling (Garrouste and Lafourcade, 2022).

5 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 listing in relevant population subgroups. We conclude with supplementary analyses aimed at shedding light on the underlying mechanisms.

5.1 Main results

Table 1 presents our baseline estimates from equation (2) 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 of more than 0.084 standard deviations for Grade 9 GPA and 4.4 percentage points for High School Eligibility (form a baseline of 88.8%). Based 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 based on school performance, only 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 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). Move Distance to High School to High School Quality. 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. 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).

While we are not able to estimate the long-term effects of neighborhood labeling on the students we calculated the implied earnings loss at age 26? using children who finished compulsory school in 2011 whom we can observe their earnings until 2021. The (unadjusted?) estimated correlation between high school quality and (log?) earnings at age 26 is Y %. Scaling our estimate in Table 1 by multiplying this number with the treatment effect for high school quality suggests that the implied earnings loss amounts to Y % We need to add the numbers and update this text.

Appendix Figure C.4 shows the event-study graphs corresponding to those in Table 1. We cannot discern significant differential pre-trends for any of the outcomes. Place in appendix.

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: Baseline Difference-in Differences Estimates of the Effect of Neighborhood Listing on Student Compulsory School Performance and High School 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	OLS	OLS	S&A	OLS	OLS	S&A

Notes: This table reports the average treatment effect of the treated (ATT) estimate of the average θ in Eq.(2). 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 (OLS) 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. Grade 9 GPA is standardized 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 English) and thus became eligible to apply to upper secondary education. 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. Standard errors (in parentheses) are clustered at the neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

5.2 Robustness Checks

Appendix Table B.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 sample of control neighborhoods based on pre-policy characteristics (share of foreign born residents and average monthly labor income, see Figure B.2), 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 troubledneighborhood 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% overlap and a minimum of 30% overlap (fifth and sixth panels), as compared to our baseline rule of 50% overlap.

5.3 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. The prediction is based on a regression using baseline child and family characteristics to predict compulsory school GPA. These characteristics are, PLEASE ADD. In this exercise, the strongest predictor is parental educational attainment. 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% 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 insignificant. Both for native and foreign-born students, there is a significant increase in the distance to high school.

The results in Table 2 also shows 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 do no have such a margin to adjust, hence only adjusting (if anything) the distance to high school while keeping the quality constant. For high-SES, who already had a larger 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 adjust (if any) to lower quality schools of similar distance.

Table 2: Difference-in-Differences Estimates of the Effect of Neighborhood Listing, 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 Eq.(2) 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. All models control for neigborhood 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.

5.4 Mechanisms

Our analysis in Section 2 indicates that the introduction of the list was neither coordinated with nor followed by any significant policy interventions. Moreover, it was not a response to changes in the underlying characteristics of the neighborhoods. This suggests that the primary mechanism through which the list affected student sorting was an increased negative portrayal in the media, which added stigma to the listed neighborhoods.

Neighborhood stigma could influence student sorting either through behavioral adjust-

ments made by parents or giving rise to social image concerns related to changes in perceptions and beliefs (Bursztyn and Jensen, 2017). In this section, we explore evidence on the potential roles of these channels.

5.4.1 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 examine this by estimating models similar to Equation (2) where the dependent variable is either an indicator for whether the parents to the children in our sample are non-employed (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 changing residential neighborhood, potentially mitigating any negative effect on the children. 7.9% of the parents in our sample change residential neighborhood at any given year. Column (3) shows that there is no significant effect of neighborhood listing on the probability that the family moves.[21]

[21] In unreported regressions, available on request, we also find no effect of neighborhood listing on residential mobility among all residents (i.e. also including non-parents).

Table 3: Parental Labor Market and Annual Moving Responses - Treatment Effect in Troubled Neighborhoods

				Mover by Direction of Move			
	Non-employment	Earnings	Mover	From non-listed		From listed	
				to non-listed	to listed	to non-listed	to listed
Listed x Post	-0.003	0.003	-0.008	-0.001	0.001	0.003**	-0.012**
	(0.002)	(0.002)	(0.007)	(0.003)	(0.002)	(0.001)	(0.005)
Mean of dependent variable	0.142	-0.042	0.079	0.063	0.004	0.009	0.004
Observations	7,160,501	7,160,501	7,160,501	7,160,501	7,160,501	7,160,501	7,160,501

Notes: This table reports the average treatment effect of the treated (ATT) estimate of the average θ in Eq.(2). 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 on 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: unemployment and (standardized) earnings. Columns (3)-(7) report estimates for moving patterns. 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.

5.4.2 Social image concerns

To shed some 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 the year before the introduction of the list and again when the list has been in effect for five years, and after the last release of the list studied in this paper. One difference compared to our micro data is that we do not know 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 and the answers are reported using a 5-step Likert response scale. In the analysis we standardize all variables before calculating the average responses across the multiple survey questions within a given domain.[22] Table 4 shows the difference-in-differences estimates of the effect of the student going to a school in a listed

[22]If needed, we re-orient the scale so that all responses move in the same direction (positive or negative).

neighborhood 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 (DeSO) of the school being listed as a troubled neighborhood on pre-policy (2014) neighborhood characteristics. Besides the main effects (listed neighborhood and post), the regressions control for student gender, age parental education (low/high). Neighborhood (DeSO) controls are (log) population size, share employed, share foreign born, share young individuals, average disposable income, share single parents.

We can see in Table 4 that neighborhood listing influences in particular high SES students as proxied by the educational attainment of the parents. For these students, neighborhood listing leads to a 0.547 standard deviation reduction in reporting being fairly treated, a 0.311 standard deviation decrease in reporting 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 are more informed about the negative attention associated with their neighborhood appearing on the list, or are the ones more "surprised" by the list. Overall, the evidence presented in this section is consistent with neighborhood listing mainly affecting student sorting through social image concerns.

Table 4: Difference-in-Differences Estimates of the Effect of Neighborhood Listing on Student Beliefs

	Fairly treated	Effort pays off	Feeling despair	Parental involvement
Panel A: Full sample (N=4,070)				
Listed x Post	-0.128	-0.118	0.486	-0.477*
	(0.209)	(0.123)	(0.297)	(0.275)
Panel B: High-SES parents (N=2,635)				
Listed x Post	-0.547**	-0.311***	0.453*	-0.342
	(0.268)	(0.140)	(0.427)	(0.300)
Panel C: Low-SES parents (N=1,453)				
Listed x Post	0.258	0.016	0.650	-0.431
	(0.302)	(0.200)	(0.435)	(0.456)
Neighborhood characteristics	Y	Y	Y	Y
Individual characteristics	Y	Y	Y	Y

Notes: The table shows the difference-in-differences estimates for the effect of the school being located in a listed neighborhood on various measures of student beliefs. The data is from the UGU survey covering a sample of 9th 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 questions: 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 questions: "If you really made an effort, would you be more smart?", "You have not made an effort if you get low marks in school?", "Even if you not are 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 question How often do you feel like this at school and in your spare time? "I am unlucky and things just happen." The dependent variable in column (4) is a weighted average of the following questions: "One of your parents takes part in the teaching or other activities in school?", "You get help at home with schoolwork if needed?". 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), and 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 a college degree. Standard errors (in parenthesis) are clustered at the school level.*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

6 Concluding remarks

The 2015 release of a police situation report publicly listing troubled neighborhoods—often referred to as "no-go zones"—created a significant negative information shock, despite not being followed by immediate policy interventions. Using population-based data and a difference-in-differences research design, we show that the introduction of the list led to a significant increase in negative media coverage of the listed neighborhoods, reinforcing their stigmatization. We also show empirically that the introduction of the list was not driven by changes in crime rates or socioeconomic conditions.

While we find no significant effect on students' educational performance, there is a significant impact on school sorting. Students from listed neighborhoods began attending lower-quality high schools further away from their home. These effects are more pronounced for high SES students.

Our exploration of potential mechanisms show no evidence of shifts in policing, school resources, family re-locations or changes in household income. Instead, the primary driver appears to be neighborhood labeling. Survey data from ninth graders further support this, showing reductions in student effort and increased perceptions of discrimination, especially among high SES students.

Overall, the results suggest that public labeling of disadvantaged neighborhoods can have significant effects on student behavior, even in the absence of direct policy interventions. Addressing and sanitizing the negative perceptions associated with a neighborhood could, therefore, be a cost-effective way to improve outcomes for poor neighborhoods without requiring substantial financial investments or changes in public policy.

While we are unable to estimate the long-term consequences of neighborhood labeling, our results suggest that territorial stigmatization has the potential to also persistently change social and economic disparities. Our findings suggest that neighborhood labeling may be a previously unappreciated mechanism through which neighborhood effects operate.

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Appendix For Online Publication

A Additional Tables and Figures **2**

B Robustness **2**

C Institutional Background **5**

C.1 Swedish "no-go" zones in comparison 5

C.2 Institutional Background: Tables and Figures 6

A Additional Tables and Figures

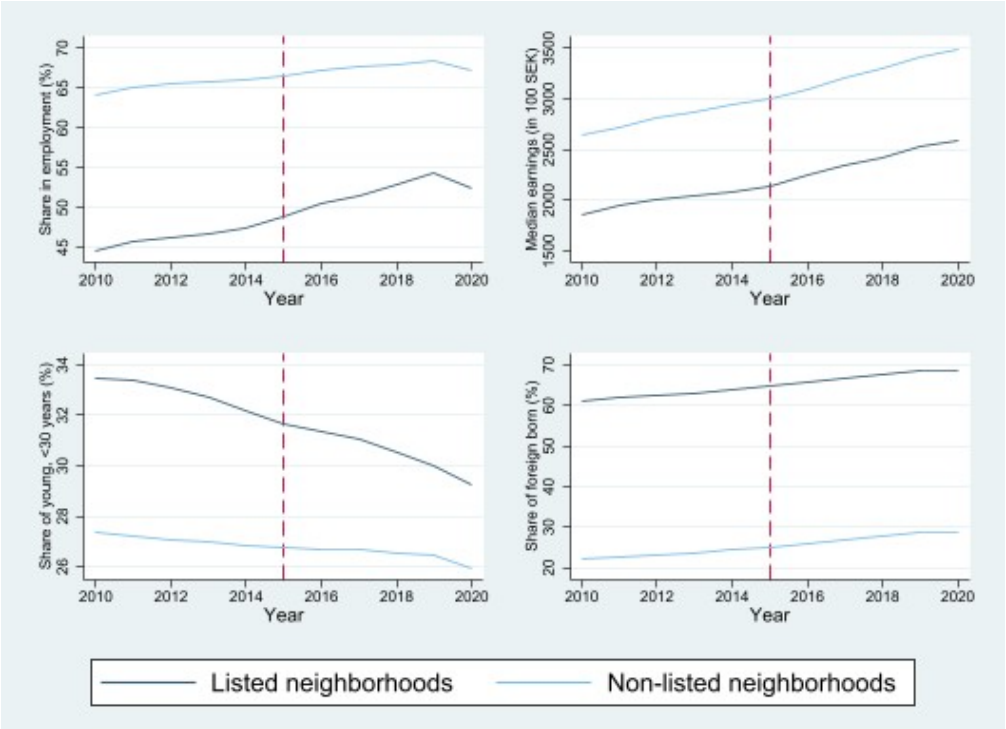
B Robustness

Table B.1: Robustness Exercises of the Effect of Neighborhood Listing on Student Compulsory School Performance and High School Sorting

	Grade 9 GPA	High School Eligibility	High School Quality	Distance to High School
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
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
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
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
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
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 OLS difference-in-differences (DiD) estimate of the average θ in Equation (2). 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. Grade 9 GPA is standardized 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 English) and thus became eligible to apply to upper secondary education. 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. Panel 1 shows baseline estimates as in Table 1, Panel 2 shows estimates using a selected sample of controls using pre-policy characteristics (share of foreign born residentes and average income) , 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% cutoff, and Panel 6 shows estimates using a 30% cutoff. Standard errors (in parenthesis) are clustered at the neighborhood level.*** p<0.01, ** p<0.05, * p<0.1.

Figure B.1: 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 DeSO units except for the listed neighborhoods.

Table B.2: Probing the accuracy of our 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 complete count data of the Swedish working population in 2018. Our operationalization of a "Listed neighborhood" is that 50 percent of the land area of one's statistical unit of residence (DeSO), falls within the boundaries of a troubled neighborhood as listed by the police. The rest of Sweden are all other neighborhoods in Sweden. 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 B.2: 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.

C Institutional Background

C.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%) of the Swedish population live within the boundaries of a neighborhood that by the police qualified as troubled in 2019 (NOA, 2021).[23] 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% of the residents in troubled neighborhoods were employed in 2017 as compared to 80.2% in the rest of the country. There is also considerably more welfare dependence among the households in the troubled neighborhoods (13%) as compared to the rest of the country (4%). In Sweden, social housing does not technically exist, although an indication of the inequitable housing conditions is that 75% of the households in troubled neighborhoods live in rental housing whereas only 27 % of the households in the rest of the country do so. Roughly 30% of the ninth graders in troubled neighborhoods do not complete compulsory school as compared to the 13% 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% of all shootings take place in these neighborhoods (which make up 0.16% 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 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,

[23] 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% of the population.

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).

C.2 Institutional Background: Tables and Figures

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



(a) Headline from Dagens Nyheter



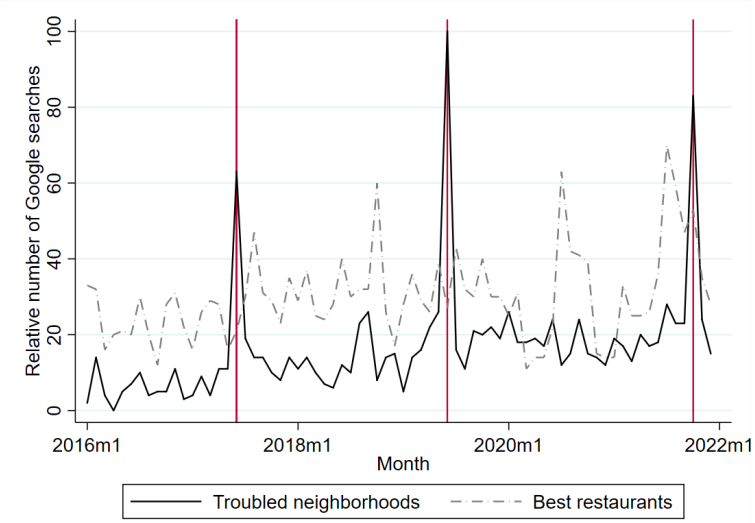
(b) Headline from Dagens Nyheter



(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 (utsatta områden).

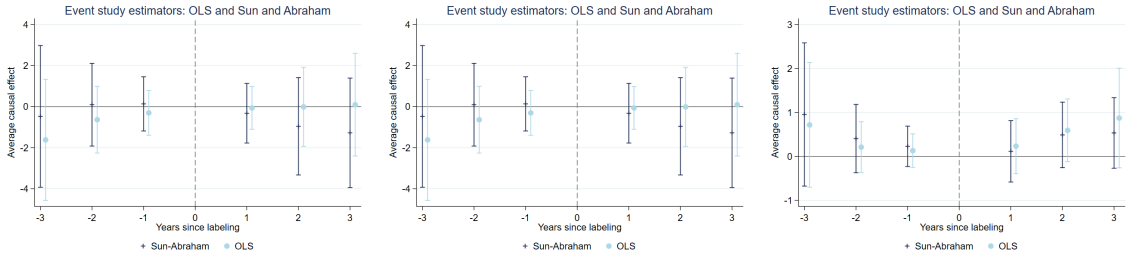
Figure C.2: Google searches for "Troubled neighborhoods and "Best restaurants"



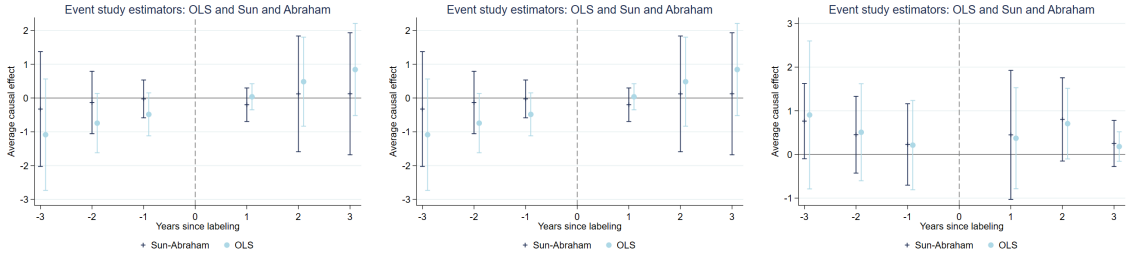
This figure plots the relative number of searches on Google for the terms "Troubled neighborhoods" and "Best restaurants". The data was obtained from Google Trends. Google Trends normalizes search data to make comparisons between terms easier. Search results are normalized to the time and location of 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 to all searches on all topics.

Figure C.3: Security Resources - Treatment effect in Troubled Neighborhoods

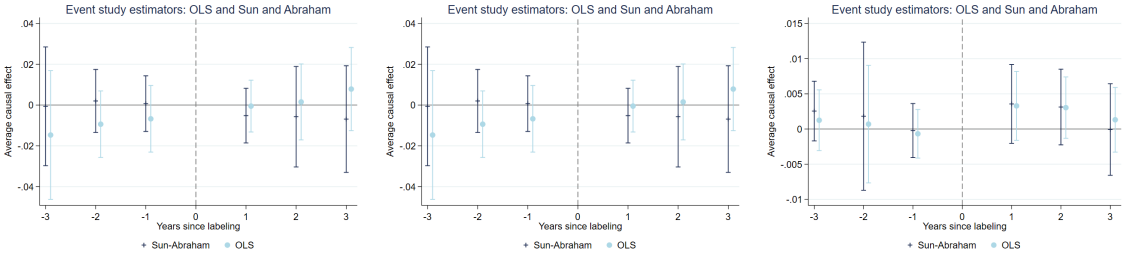
Panel A: Number of Staff (police, police officers, and private security)



Panel B: Average Weekly Hours (police, police officers, and private security)



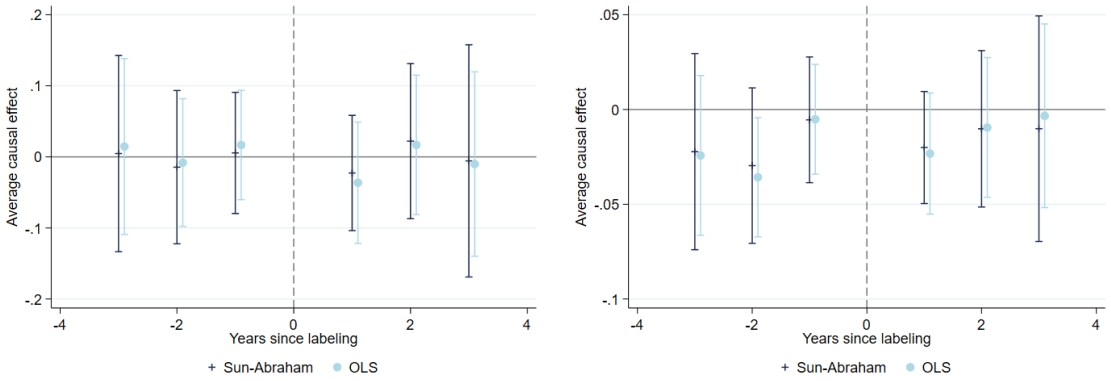
Panel C: Share of Staff doing overtime (police, police officers, and private security)



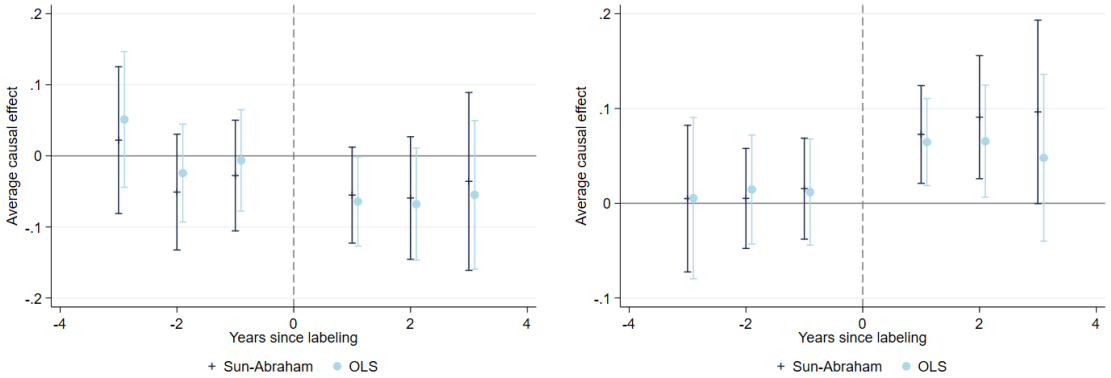
Notes: If this table stays in the manuscript, fix sub-captions in the figure and references to the sub-figures in the table notes.This figure plots the average treatment effect of the police list on neighbourhood resources. These are estimated using a staggered difference-in-differences strategy, where 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. OLS and Sun and Abraham (2021) estimators are presented. Panel A includes results for the total number of staff, 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 DeSO level.

Figure C.4: Event-Study Estimates of the Effect of Neighborhood Listing on Student Performance and Sorting

Panel A: School Performance: Grade 9 GPA (left) and High School Eligibility (right)



Panel B: High School Sorting : High School Quality (left) and Distance to High School (right)



Notes: This figure plots the event-study estimates corresponding to equation (2) 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 OLS 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.

Table C.1: Sampling frame

	Total	Urban		Rural
		Listed	Non-Listed	
	N	N (%)	N(%)	N(%)
Neighborhoods	5,984	220 (3.7)	3,104 (51.8)	2,660 (44.5)
Individuals	1,014,045	45,621 (8.0)	521,696 (51.4)	446,728 (44.1)

Note: This tables presents the sampling frame of this study. The first row presents the number of neighborhoods (DeSO), divided by their classification as urban or non-urban and as listed or non-isted. The second row does the same but for individuals in our sample (those finishing compulsory school between years 2010 and 2021).

Table C.2: Descriptive statistics

	Listed	Non-Listed
Panel A: School Performance		
Grade 9 GPA (standardized)	-0.588	-0.115
High School Eligibility	0.713	0.888
Panel B: High School Sorting		
High School Quality	-0.055	-0.444
Distance to High School (in kms)	6.89	10.235
Panel C: Neighborhood characteristics		
Share Young (16-30)	0.346	0.283
Share Employed	0.606	0.749
Share Foreign	0.653	0.246
Median Income (100 SEK)	1,282	2,353
Population	1,361	1,263

Note: Pre-policy values (2015)