ROCKWOOL Foundation Berlin
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DISCUSSION PAPER SERIES 83/25

The Making of a Ghetto: Place-Based Policies, Labeling, and Impacts on Neighborhoods and Individuals

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OCTOBER 2025

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Reference

JEL Codes: J15, J18, R23, R28

Keywords: residential segregation, place-based policies, migration, neighborhood effects

Recommended Citation: Yajna Govind, Jack Melbourne, Sara Signorelli, Edith Zink (2025): The Making of a Ghetto: Place-Based Policies, Labeling, and Impacts on Neighborhoods and Individuals. RFBerlin Discussion Paper No. 83/25

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The Making of a Ghetto

Place-Based Policies, Labeling, and Impacts on Neighborhoods and Individuals*

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July 2025

Abstract

Policies targeting disadvantaged areas aim to improve their conditions, but the labels they impose carry consequences of their own. In this paper, we examine Denmark's Ghetto Plan, one of the first recent place-based policies explicitly targeting migrant populations. Under this policy, certain public housing deemed "problematic" were officially designated as "ghettos", with minimal additional implications. Using rich administrative data and a Difference-in-Differences approach, we show that the policy backfired, worsening spatial inequality through compositional shifts driven by *native avoidance*. In addition, the policy was particularly detrimental to exposed natives, who accepted a 4% annual income loss to leave stigmatized areas.

Keywords: residential segregation, place-based policies, migration, neighborhood effects

JEL Codes: J15, J18, R23, R28

*We would like to thank Abi Adams, Jan Bakker, Stefano Fiorin, Mette Foged, Teresa Freitas Monteiro, Manon Garrouste, Pamela Giustinelli, Linea Hasager, Murat Kirdar, Eliana La Ferrara, Thomas Le Barbanchon, Stephen Machin, Ismir Mulalic, Pablo Selaya, Guido Tabellini, and Stephane Wolton for their helpful comments, as well as to the participants of the Investigating Social Inequalities using Survey and Register Data IAB 2024, Politics of Residential Mobility Workshop at MZES University of Mannheim 2024, Workshop on Wealth Inequality, Intergenerational Mobility, and Equality of Opportunity in Vienna 2025, Migration in OECD Countries Conference 2024, OECD International Forum on Migration Statistics 2025, the Rockwool Foundation Migration Forum 2025, and the EALE-SOLE-AASLE Labor World Congress 2025. This project was made possible thanks to the generous financial support of LEAP.

1 Introduction

Segregation and ethnic enclaves are common features of cities, with disadvantaged populations often concentrated in certain neighborhoods. Such spatial inequalities create challenges that are sometimes harshly portrayed by both the media and the political discourse, contributing to the stigmatization of the marginalized areas.[1] A vast body of literature has documented that neighborhood characteristics affect individual trajectories and chances of success (Chetty et al., 2016; Chetty and Hendren, 2018a,b), thereby motivating a range of policy interventions aimed at reducing residential inequality. Yet, place-based policies, by design, require the identification and targeting of disadvantaged areas– a process that may inadvertently reinforce stigma and exacerbate the very problems these interventions aim to solve.

A salient way in which the inherent tension between targeting and stigmatization manifests is through geographic labels. Labeling neighborhoods as “disadvantaged” can have unintended consequences: it can alter residents’ behavior (e.g. school choices (Davezies and Garrouste, 2020)), fuel discrimination by outsiders (Besbris et al., 2015), and diminish neighborhood attractiveness, as reflected by falling housing prices (Tootell, 1996; Aaronson et al., 2021; Hynsjo and Perdoni, 2023; Koster and van Ommeren, 2019; Andersson et al., 2023). This paper investigates whether and how a residential place-based policy, where the strength of the label far outweighed modest efforts to improve local conditions, affected targeted neighborhoods and their residents. We contribute to the existing evidence by using longitudinal data that track the entire population of affected individuals across time and space for a wide range of socioeconomic outcomes. Further, to the best of our knowledge, this is the first study to demonstrate that the labels embedded in such policy interventions have asymmetric effects, imposing relatively greater socio-economic costs on the ex-ante less stigmatized groups than on the already more stigmatized and targeted group.

We use the Danish “Ghetto Plan”, one of the first recent place-based policies explicitly targeting migrant populations, as a natural experiment. The policy studied was introduced in 2010 with the intention of increasing residential mixing, as a “fight against parallel societies” where “Danish values are not firmly rooted” (The Danish Government, 2010).[2] It classified public housing areas with over 1,000 residents as “ghettos” if they exceeded specific thresholds for unemployment, crime, and the share of residents with

[1] Examples include the designation of disadvantaged areas in the U.K. and France as "no-go zones" in an opinion piece in the New York Times (https://www.nytimes.com/2002/04/27/opinion/IHT-wake-up-to-the-problem-separate-and-unequal-in-france.html, last accessed on 24/07/2025) and Fox News (https://www.foxnews.com/video/3978888136001 for instance, last accessed on 24/07/2025), and as "lawless zones" (*zone de non-droit*) by several conservative French media. Similarly, in Sweden, media outlets such as The Express, MSN, and The European Conservative have used the term "no-go zones" to describe certain neighborhoods perceived as having high crime rates and limited police presence. In the Netherlands, for example, the term “problem neighborhood” (*probleemwijk*) is used to describe neighborhoods on the *Vogelaarwijken* list.

[2] The exact wording in the policy paper is (authors’ translation from Danish): “We must not accept parallel societies in Denmark. We must change the areas where Danish values are not firmly rooted. We must take action against the areas that close off from the surrounding society. And where a high concentration of immigrants means that many remain more closely tied to the country and culture they or their parents come from than to the Danish society in which they live and work. We must transform these areas so that they become an integral part of Danish society.” The Danish Government (2010), p. 5.

non-Western origins. Importantly, aside from the classification itself, the initial years of the "Ghetto Plan" involved only limited policy interventions, providing a quasi-experimental setting to study the effects of labeling on neighborhoods and individuals.

We leverage rich Danish administrative register data, which provide detailed longitudinal information on individuals' address history, education, labor market status, income sources, country of origin, and criminal records. We match these data to a registry of public housing address identifiers, enabling precise identification of the units monitored by the Danish Ministry as part of the "Ghetto Plan". In the first part of the paper, we examine the effect of the policy on neighborhood characteristics and residential segregation. We estimate the effect of being classified as a "ghetto" by comparing classified neighborhoods to similar never-classified public housing areas (control neighborhoods) in a Difference-in-Difference setup. To ensure comparability, we only keep neighborhoods that are within the common support of the propensity score distribution based on baseline characteristics and we perform several robustness tests on the specification.

Our results show that the "Ghetto Plan" worsened the average characteristics of targeted neighborhoods: the average income dropped by 2% and the share of low educated individuals increased by 5%, resulting in a surge in spatial inequality. Moreover, despite the policy's stated objective of reducing the share of non-Western residents in targeted neighborhoods, we find no significant effects on this outcome. When disentangled by origin, we find that the observed deterioration is entirely driven by residents of Western origin (including Danes). For this group, average income declined by 4%, wage income by 8%, and the share of low-educated individuals increased by 17%. In contrast, we observe no significant changes in these characteristics for residents of non-Western origin. A decomposition of the effects into incumbent residents, in-movers, and out-movers, shows that they are entirely driven by changes in the composition of new entrants, whose characteristics worsen following the implementation of the policy. Taken together, these results suggest that the "ghetto" label triggered *native avoidance* behavior as relatively better-off Danes and Westerners who would otherwise have moved into these neighborhoods been deterred from doing so.

In the second part of the paper, we assess the causal effects of the "Ghetto Plan" on individuals who resided in the targeted areas right before its implementation. We follow both stayers and leavers, regardless of their subsequent location, thereby capturing the overall causal effect of the policy on exposed individuals. We compare their outcomes to those of individuals living at baseline in comparable public housing areas that were never classified (control neighborhoods) using a Difference-in-Differences framework. To further increase comparability between treated and control individuals, we apply inverse propensity score weighting based on baseline income deciles. Here as well, we test that results are robust to a battery of alternative specifications.

Our results show that individuals residing in neighborhoods classified under the "Ghetto Plan" experienced a 3% decline in total income, driven by a 5% drop in wage income, partially offset by a 2% increase in benefit receipts. The decline in wage income is driven by reduced employment rates rather than lower hourly wages. Once again, these effects are entirely concentrated among Danes and Westerners, while no significant changes are observed for individuals of non-Western origin. Since Danes, on average, had higher earnings than non-Western residents in the affected areas prior to the policy, our effects translate into a reduction of ethnic inequality, but through a leveling-down dynamic.

Importantly, we are able to examine the mechanisms underlying these findings. Since the probability of leaving the "ghettos" did not change for Danes relative to controls, we analyze outcomes separately for stayers and movers.[3] Among stayers, we observe an immediate increase in long-term unemployment—defined as not working for the entire year—which fades over time and is primarily driven by the poorest individuals, many of whom were already unemployed prior to the policy. We can credibly rule out negative peer effects as the primary explanation, and interpret the findings as consistent with increased employer discrimination based on residential address, either actual or perceived by job seekers, who may consequently reduce their job search effort.[4]

Individuals who decide to leave classified housing units experience even larger declines in wage income, driven by a reduction in the number of hours worked during the year. This effect intensifies over time and is concentrated among individuals who were relatively better off before the policy. We interpret this pattern as reflecting a combination of housing market discrimination and shifting preferences for residential location. Specifically, the policy appears to have reduced the perceived amenity value of the labeled neighborhoods, increasing residents' willingness to relocate. However, the overall number of movers remains stable, and conditional on moving, individuals tend to relocate to poorer neighborhoods, parishes, and municipalities. This suggests that, while affected residents are motivated to escape the stigma associated with their previous address, they face significant constraints in accessing higher-opportunity areas, often accepting moves to locations with worse labor market prospects. We interpret this as evidence of *native flight* driven by place-based stigma, and we calculate that Westerners/Danes are willing to forgo 4% of their annual income to avoid living in a labeled neighborhood.

This paper is related to the literature that studies the effect of residing in disadvantaged neighborhoods or areas designated as such. The literature has documented that it results in lower call-back rates to job applications (Bertrand and Mullainathan, 2004; Petit et al., 2011) and in customer discrimination in online market ads (Besbris et al., 2015). Additionally, several papers have identified the long-run negative

[3]Given that moving decisions are endogenous choices, we interpret this evidence as suggestive. Reassuringly, we do not find evidence that the characteristics of movers out of "ghettos" differ from movers in the control group.

[4]These effects concern a limited number of individuals relative to the entire neighborhood population, which explains why they are not visible at the aggregate neighborhood level.

effects of red-lining, a 1930s practice in the U.S., in which minority neighborhoods were systematically denied mortgage credit based on racially biased risk ratings, resulting in chronic disinvestment (Tootell, 1996; Aaronson et al., 2021; Hynsjo and Perdoni, 2023).[5] Other papers have explored the effect of more recent policies labeling neighborhoods on housing prices (see Koster and van Ommeren, 2019, 2022 in the Netherlands and Andersson et al., 2023 in Sweden) and crime (Damm et al., 2025). Closely related to our work, Davezies and Garrouste (2020) and Garrouste and Lafourcade (2023) study the effect of programs aimed at increasing investments in public schools within deprived neighborhoods in France on school choices and educational outcomes. They both show that these policies backfired due to the stigma attached to their labeling: wealthier families reacted by shifting their children to private schools or into public schools outside the designated areas. Similarly, Domínguez et al. (2025) estimate the effect of a police list of troubled neighborhoods in Sweden on educational performances and sorting. Unlike prior studies that focus primarily on educational outcomes or housing prices, we examine broader economic effects and shed light on the underlying mechanisms, which is made possible by the richness of our data. We document mobility responses, disentangling the role of inflow-driven compositional changes and quantifying the financial costs of stigma-induced relocation. We also show how stigmatizing labels can fuel discrimination based on place of residence in both labor and housing markets. Finally, while reactions documented by Davezies and Garrouste (2020) and Garrouste and Lafourcade (2023) imply a *move to opportunity* in line with the large-scale relocation experiment in the US (see e.g. Chetty et al., 2016), in our case such a move to opportunity does not seem possible, leading to purely negative consequences.

Importantly, we explore heterogeneity by origin—Western vs. non-Western descendants—linking the neighborhood stigma literature with research on residential sorting and *native flight*. This pattern has mostly been described in the US context (see for example Boustan, 2010, 2013; Shertzer and Walsh, 2019; Schelling, 1971). Stonawski et al. (2021) and Boje-Kovacs et al. (2024) document similar forms of *native flight* in Denmark, and surveys on neighborhood preferences conducted in Europe (and Denmark) reveal preferences consistent with *native flight* or at least *native avoidance* (ESS, 2002) (see Online Appendix Figure C2).[6] In the previous literature, *native flight* was shown to be triggered by an inflow of non-native population, used to identify so-called tipping points beyond which neighborhoods converge to full segregation. In this paper, we show that similar behavior can be triggered simply by a label suggesting high-immigrant shares in neighborhoods, without an increase in actual inflows. This suggests that part of the native flight behavior observed in other studies is driven by concerns of the external perception of neighborhoods.[7]

[5] A related literature has also explored place-based job creation policies or investment incentive schemes in the U.S., see for example (Busso et al., 2013; Corinth and Feldman, 2024; Freedman and Neumark, 2024). These programs are by design quite different from the place-based policy studied in this paper and also do not include stigmatizing labels. Even in the absence of a stigma, these policies have often been found to be non cost-effective.

[6] This literature is complemented by studies highlighting the potential benefits of residential sorting, for instance in terms of social cohesion and public good provision, see for example Cutler and Glaeser (1997); Algan et al. (2016).

[7] Hence, the external perception of one's neighborhood is an important component of the value that individuals attach to their

A complementary literature has examined the impact of discrimination on minority performance (Coate and Loury, 1993; Hoff and Pandey, 2004), showing that when animosity toward a minority group is made salient, members of that group tend to perform worse across a range of outcomes (Glover et al., 2017; Carlana, 2019; Corno et al., 2022). The Danish "Ghetto Plan" was explicitly framed as a response to immigrants' so-called "parallel societies", a framing made particularly salient by the inclusion of a demographic criterion requiring that designated "ghettos" have at least 50% non-Western residents. To the best of our knowledge, this is one of the first recent place-based policies to explicitly target minority neighborhoods. Our findings show that the policy in fact had a more negative economic impact on natives than on non-Western migrants and their descendants, whose economic conditions remained relatively stable—though they have been affected along cultural dimensions (Foged et al., 2025). These results point to the emergence of an "amalgamation stigma", whereby natives living in designated areas are penalized through association with a stigmatized minority group, consistent with evidence from correspondence studies (Petit et al., 2011).

The rest of the paper proceeds as follows: we will first give a short overview of the Danish "Ghetto Plan" institutional context and details in section 2. Then, briefly describe our data sources in section 3, our methodology in section 4. Section 5 presents the results on the neighborhood level, and section 6 presents the individual level results. Finally, section 7 concludes.

2 Denmark's Ghetto Policy

Approximately 20% of the housing in Denmark is publicly subsidized (OECD, 2020). From the 1960s and 1970s, immigrant workers who arrived under Denmark's guest worker scheme settled in public housing (Nannestad, 2004), which increasingly led to *native-flight* and *native-avoidance*; resulting in the increase in spatial segregation between natives and migrant descendants in Denmark (Andersen, 2016; Iversen et al., 2019; Stonawski et al., 2021; Boje-Kovacs et al., 2024). From the 1970s to the early 1990s, Denmark's immigration policy shifted from openness to restriction: a liberal 1983 law expanded rights for asylum seekers and family reunification, but rising immigration soon triggered political backlash and tighter regulations. At the beginning of the 2000s, the Danish Social Democratic government started debating about possible policies to tackle such segregation (The Danish Government, 2004), framing it as a combat against the formation of "parallel societies with a lack of Danish norms and values".[8] The

neighborhood–something that would usually be captured in an amenity parameter in the current theoretical models on residential sorting, see e.g. Gregory et al. (2024); Davis et al. (2024).

[8]See The Danish Government (2004) p. 11 (authors' translation): "It is the government's goal that the residential areas where immigrants, refugees and their descendants live should be places where they meet Danes. Where networks are established across personal and cultural differences, where you hear and learn Danish, and where prejudices about each other are tested and dismantled. The residential areas should be platforms for general integration into society and for increased knowledge of the norms and values that apply here."

government announced the allocation of financial resources to counteract the "ghettoization" trend of certain neighborhoods (The Danish Government, 2004), followed by further strategy plans with political measures to achieve a more balanced composition of inhabitants in public housing areas (The Danish Government, 2010, 2013, 2018). The first concrete policy was the "Ghetto Plan" introduced in 2010. It included three clear criteria and cut-offs to evaluate public housing areas with respect to the share of immigrants and descendants from non-Western countries (> 50%),[9] the share of residents not in employment or education over the previous four years (> 40%), and share of residents convicted of violating the Criminal Code or Weapons Act over the past four years (> 2.7%).[10]

Adjacent public housing buildings with more than 1,000 inhabitants that meet at least two of these three criteria are classified as "ghettos". The list of classified areas is updated once a year and made publicly available, and each publication typically receives a lot of public and media attention.[11] Google trends for the search terms "ghetto" and "ghetto list", for example, spike each year exactly at the publication dates, as shown in Figure 1. This attention was increasingly felt by residents of classified "ghettos" (Stender and Mechlenborg, 2022). A total of 198 public housing areas have been evaluated since 2010, and 29 were identified as "ghettos" in 2010, when the first "Ghetto List" was published. Appendix Figure A1 shows a map of the distribution of targeted areas across the country, located in 17 different municipalities. Despite a concentration of "ghettos" in the biggest cities, the map shows significant geographical variation, with designated areas also present in smaller urban areas.

Publicly subsidized housing in Denmark is administered by housing associations and open to all Danish residents through a waiting list scheme. Housing associations receive public funding, and in turn give municipalities allocation rights for up to 25% of available public housing. The waiting lists are lengthy, with waiting times of at least five years and up to twenty years for the greater capital area. Waiting lists can be bypassed, for example, in the case of a divorce, in order to provide housing for both parties. Since 2000, municipalities and housing associations have been allowed to select and reject candidates from waiting lists to control neighborhood composition in areas with high unemployment. The idea was that neighborhoods "can be improved by attracting more resourceful" inhabitants (The Danish Government (2004), p. 22).[12] This practice of *flexible letting* was tightened in 2010. Specifically, one of the implications of being classified as a "ghetto" was that as of January 2011, vacant public housing apartments under municipal control (which are less than 25% of public housing) were not allowed to be offered to

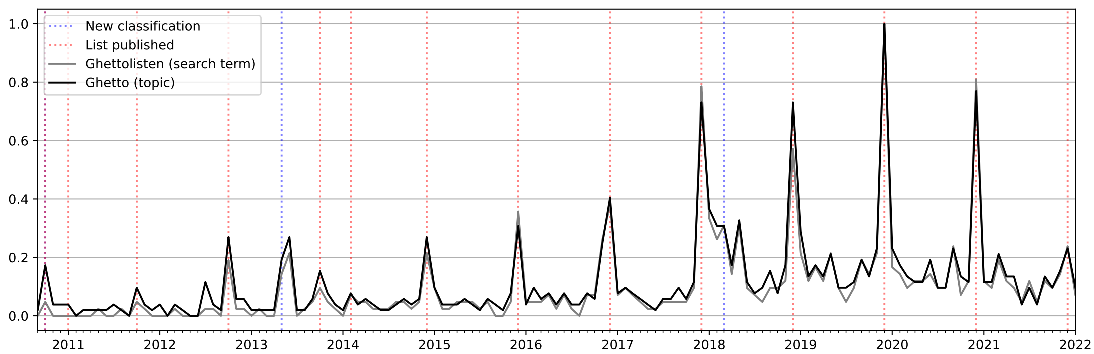
[9] The "non-Western" terminology is the official designation by the Danish government. The government defines a list of countries that are considered Western. Western countries are all EU member states, European countries that are not EU members (Iceland, Liechtenstein, Monaco, Norway, Andorra, San Marino, Switzerland, Vatican, UK, Ukraine (since 2022) and some non-European countries (Canada, USA, Australia, New Zealand). All other countries are categorized as non-Western.

[10] See *lov om almene aliger*, nr. 103, version from February 11, 2011 or *lovforslag* L60 from December 17, 2010.

[11] Criteria and classification categories changed over time. We focus on estimating the effect of classification treatment in 2010. Table C1 in the Online Appendix lists the exact definition of criteria and their changes from 2010 until 2018. Online Appendix C discusses the policy changes in the context of the changing political landscape in Denmark 2010-2018.

[12] Applicants who do not already live in the respective public housing area and who have been dependent on benefits for six consecutive months could be rejected. See §51 b in the Law of Public Housing (*lov om almene boliger*).

Figure 1: Salience of the policy: Google trends in Denmark



Note: Blue dotted lines mark the publication of a new strategy paper, which in general implied new classification criteria and policy implications (The Danish Government, 2010, 2013, 2018). Red dotted lines mark the publications of lists of classified areas. Usually, lists have been published once a year, with two exceptions in 2011 and 2014. In these two years, there are two shorter classification periods. Google Trends are always computed relative to the maximum attention in the selected period. In our case, this was around the publication of the 2019 list when the spike reached one on the vertical axis. The heights of all other spikes can be interpreted relatively to that maximum. For example, attention for the first "ghetto" list in 2010 was only about one-fifth of that nine years later or ca. 0.2 on the vertical axis.

households with at least one member who within the past 6 months had been convicted, had their tenancy in another public housing terminated, is a non-EU citizen (except if enrolled in education), is on disability pension or received unemployment or sickness benefits.[13] The majority of public housing associations were not bounded by these rules, but they were encouraged to apply them.[14] In addition to these moving-in restrictions, existing subsidies for moving out were extended to cover both the moving expenses and the costs of settling into a new home. Additionally, housing associations offered to jump waiting lists in "non-ghettos" when moving out of a "ghetto". This did not seem to matter in our setting as we document that the share of previously "ghetto" residents who move to our comparable non-"ghetto" neighborhoods is not significant (see Table A3). We also show that the policy in general did not trigger increased mobility out of classified public housing units, and that if movers-in were positively selected it was not sufficient to improve the overall neighborhood composition in the policy's intended direction.

Additionally, one of the goals of the policy was to encourage the physical reconstruction of some housing complexes. However, none of the major reconstructions took place during our analysis window, which ends in 2018. Alongside the strategy paper demanded that several socially oriented housing initiatives should be launched, primarily targeting youth employment, education, and community engagement. While specific programs in single neighborhoods showed some promise, evaluations of these initiatives found limited overall impact on broader outcomes (Christensen et al., 2019, 2021). Moreover, these

[13]See § 59, stk 6 of the Law of Public Housing (*lov om almene boliger*), LBK nr. 103 from February 11, 2011. Only if municipalities face the impossibility of finding housing for households meeting one or more of these criteria are they allowed to place them in "ghetto"-designated areas.

[14]On top of that, municipal councils could in coordination with housing associations set their own specific criteria to select tenants. In "ghetto" areas, the municipality had full autonomy over these criteria and did not have to negotiate them with the housing associations. See § 60 of Law of Public Housing (*lov om almene boliger*), LBK nr. 103 from February 11, 2011. Criteria have then to be reviewed at least every four years. It is not specified what these criteria should be other than that they should be designed "with a view to strengthening the resident composition" (own translation).

initiatives were not implemented at scale. Since all these initiatives should increase neighborhood attractiveness and improve average composition, they work in the opposite direction of the “ghetto” label that came with treatment. Hence, our effects should be interpreted as a lower bound of the absolute negative effect of the label.

All in all, in the first eight years following the reform examined in this paper, the policy brought about only modest concrete changes. While it aimed to gradually shift the population composition of these areas and reduce segregation, the measures implemented were arguably mild. At the same time, the public labeling of these neighborhoods as “ghettos” and their heightened visibility likely had a substantial impact on how they were perceived by the broader public. This only changed in 2018, when the “Ghetto Plan” was updated to allow for harsher measures such as demolitions of housing units, including forced displacement, double sentencing on crimes committed in “ghettos” and day-care enrollment demands for small children. All these changes are outside our period of analysis.

3 Data

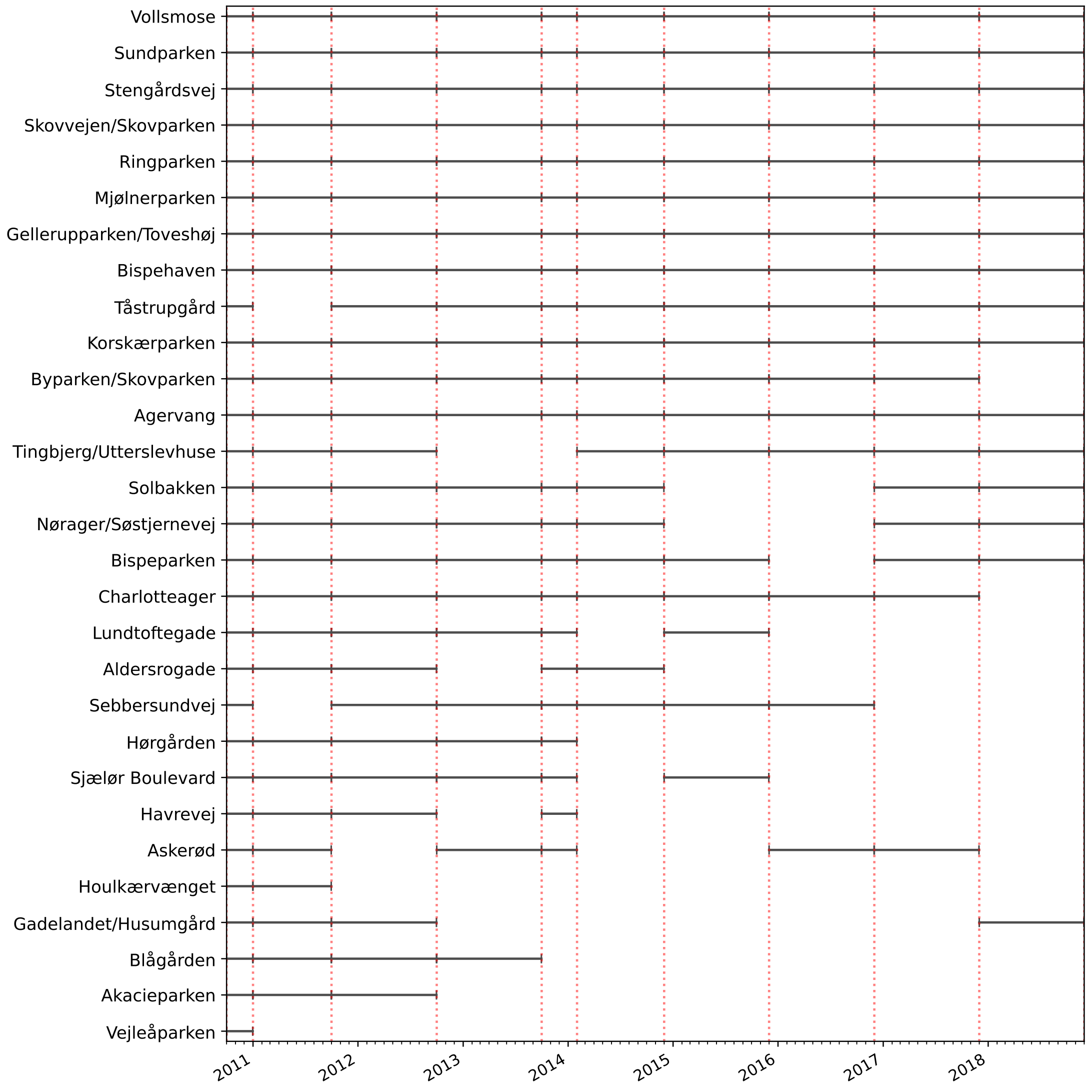
We use register data from 2006 to 2018, containing information on all individuals living in Denmark. This data includes all residential moves, with corresponding addresses and dates, as well as the relevant information with respect to the evaluation criteria of the “Ghetto Plan”, i.e., country of origin, education levels, criminal activity, income, and labor market participation. Unfortunately, without access to the exact coding used by policymakers to calculate eligibility criteria, we are unable to precisely construct the assignment variable. Instead, we combine the register data to the neighborhood identifiers established by the Ministry of Housing for the "Ghetto Plan", which allows us to track whether and in which year a public housing unit is listed. This allows us to identify the treated neighborhoods and comparable social housing units that are not listed.[15] There are in total 198 public housing areas in the country, which are composed of over 190,000 different addresses,[16] with approximately 1,125,000 individuals living in these addresses at any point in time between 2006 and 2018. This represents approximately 20% of the Danish population in 2018. 29 neighborhoods were classified as “ghettos” in 2010. Figure 2 shows their treatment spells. Many of them are repeatedly classified on consecutive lists and only disappear from the classification for one or two years before appearing again.

We combine all this information to produce two main datasets. The first is a yearly panel of public housing neighborhoods going from 2006 to 2018, containing a series of variables on socio-economic

[15]The Ministry has since 2010 defined public housing areas as geographically connected land built-up with public housing, or physically coherent social housing estates (in Danish: *’fysisk sammenhængende almene boligafdelinger’*). Taking 2010 as the starting point means that non-public housing established in the period from 2010 to 2018 as part of the ghetto-plan are included in the data.

[16]We have to drop 0.2% of our observations because we are not able to match address identifiers with the register data.

Figure 2: Treatment spells of public housing areas classified in 2010



Note: Plotted are classifications for the years 2010-2018. Vertical lines in red mark the dates when a new list was published. Areas on the vertical axis are sorted by the date they were first listed and the total number of times listed.

characteristics and crime rates, as well as an indicator for whether and for which years each area was listed as a "ghetto" by the Danish government. We can further decompose the characteristics of residents into sub-groups defined based on whether the household was already residing in the area before the policy introduction and remains a resident after, whether it is a new entrant after the policy change, or whether it moves out after the policy change. This decomposition allows us to understand whether the total effects observed at the neighborhood level are driven by changes within incumbent households or by changes in composition. The second dataset that we produce is at the individual level, and consists of a yearly panel of individuals who were residing in the same public housing neighborhood consecutively between 2007 and 2009.[17] These individuals are then followed over the entire period from 2006 to 2018, wherever they

[17] We impose this restriction to make sure that we only estimate the effect on individuals who reside in a relatively stable way in

move in the country, including if they exit the public housing sector. This dataset allows us to test the causal impact of the policy on exposed individuals, and to disentangle whether the individual-level effects are driven by people remaining in targeted areas or by people moving out.

One of the main outcomes of interest is how income evolves in the neighborhoods listed as "ghettos" compared to other areas, and how the effect differs across Western and non-Western citizens. Appendix Table A1 summarizes how total income and its main sub-components – wage income, capital income, and benefits – differ across neighborhood types in the four years preceding the policy, distinguishing between the average income of Western citizens and non-Western citizens. On average, Western citizens in "ghettos" earn 32% less than in private housing, while Western citizens in other social housing earn 23% less than in private housing. Spatial segregation is even greater for non-Western citizens, who earn 38% less in "ghettos" and 28% less in other social housing relative to private housing. These figures underscore the extent to which listed neighborhoods concentrate some of the country's most vulnerable populations. Spatial inequality in wage income and capital income is even more severe, and partly compensated by differences in benefits that go in the opposite direction. Finally, non-Western citizens earn less than their Western counterparts in all types of residential areas, but ethnic inequality is the most pronounced within "ghettos", where the first earn 33% less on average. In our analysis of the effect of the policy, we will benchmark coefficients by looking at how they impacted both residential and ethnic inequality.

4 Methodology

Our main empirical methodology relies on a Difference-in-Differences approach where social housing neighborhoods classified in 2010 are considered treated, and social housing neighborhoods that are never classified over the period are considered controls. This approach has several advantages. First, it allows us to look at the dynamic effect of the reform over many years, up to 2018, when the policy introduced stronger measures.[18] Second, by focusing on 2010, the first wave of the "Ghetto Plan", we eliminate the risk of anticipation effects. Finally, given that we limit our comparison of treated units in 2010 to never treated units, we do not incur problems of biases due to dynamic treatment effects largely described in contexts where previously treated units are used as controls for subsequently treated units.[19]

We prefer a Difference-in-Differences approach over a regression discontinuity design for several reasons. First, our primary interest lies in the dynamics of average treatment effects over time, tracking neighborhoods and individuals in the years before and after the policy. Second, the limited number of

public housing and are not transient.

[18] We do not consider the period after 2019, as the measures of the "Ghetto Plan" became more severe, including demolitions, for instance.

[19] See for instance Goodman-Bacon (2021); de Chaisemartin and D'Haultfœuille (2020); Callaway and Sant'Anna (2021); Borusyak et al. (2024).

treated neighborhoods—only 29, some of which are far from the threshold—makes it difficult to identify a sufficiently smooth distribution of observations around the cutoff. In addition, without access to the exact coding used by policymakers to calculate eligibility criteria, we are unable to replicate the assignment variables with the precision required for the RDD. Finally, we are interested in the average treatment effect on the treated, rather than the local treatment effect close to the eligibility threshold.

Given the likely persistent effect of stigma, we consider all social housing units listed in 2010 as being treated throughout the period.[20] The main caveat of the Difference-in-Differences approach resides in the fact that, on average, the 140 never listed social housing units are different from the 29 units listed in 2010. The two left columns of Appendix Table A2 show the summary statistics of treated and control units observed between 2006 and 2009, thus prior to the reform. Never-listed units have higher incomes and wages, smaller family size, a much smaller share of population with low levels of education and not employed, a much smaller share of people with non-Western origins and a much smaller number of crimes committed. While differences in levels do not necessarily preclude unbiased estimates in Difference-in-Differences analyses, where the identification relies solely on the assumption of parallel trends, such large differences in baseline characteristics clearly cast doubt on the validity of identification.

In order to increase the comparability of treated and control neighborhoods, we estimate the following propensity score model on 2006 neighborhood data using the probit estimator:

$$P(treat_i) = \beta_0 + \beta_1 \; lwage_i + \beta_2 \; lcrime_i + \beta_3 \; hhsize_i + \beta_4 \; loweduc_i + \beta_5 \; notemp_i + \epsilon_i \tag{1}$$

The model predicts the probability for a neighborhood i to be classified into a "ghetto" in 2010 based on the log of average wage income, the log number of convicted crimes, the average household size, the share of low educated individuals–defined as having only compulsory education–, and the share of active population that is not employed. These criteria are not exactly equal to those chosen to define the classification, but they are closely related to them.[21] In the main specification, we measure characteristics in 2006 to ensure the complete absence of anticipation and to be able to test pre-trends between 2006 and 2009. In robustness analyses, we show that our results are unchanged if we run the propensity score model on 2009 characteristics or if we change the variables used in the prediction model.[22]

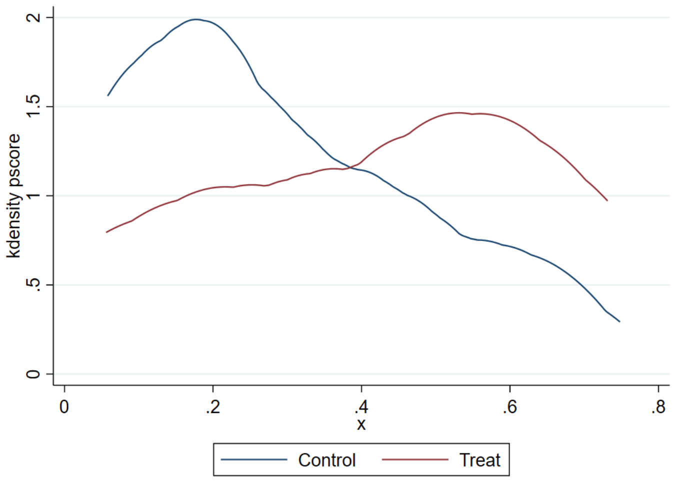
[20] In practice, 11 of the 29 units listed in 2010 remain listed in all following lists up until 2018, and on average, those listed in 2010 remain so for almost three-quarters of the period 2010-2018 (Figure 2). In a robustness test, we refine this definition by only considering treatment during the years when a neighborhood actually appears on the "Ghetto list".

[21] The "Ghetto List" of 2010 selects neighborhoods with more than 1000 inhabitants that possess at least 2 of the following characteristics: i) the share of immigrants and descendants from non-Western countries exceeds 50%, ii) the share of individuals between 18 and 64 years old who are neither in education nor in employment exceeds 40%, iii) the number of criminal convicts exceeds 270 per 10,000 residents.

[22] A model predicting treatment based on average household income expressed in per-adult equivalent and share of non-Western descendants give rise to very similar results.

As expected, the probability distributions obtained from this model are skewed in different directions for treated and control areas: the average probability of being listed, among controls, is 7%, and more than half of never listed neighborhoods have a propensity score below 1%. The average probability of being listed among treated areas is 67%, with more that a quarter of observations showing a propensity score above 90%. We thus decide to exclude all observations laying outside the common support area, i.e. the range of probabilities where both treated and control observations can be found. This procedure brings the average propensity scores much closer to each other, with an average probability of being listed of 28% among controls and of 41% among treated. Figure 3 depicts the distribution of the propensity score among the two groups after the exclusion of the observations laying outside the common support, and the last two columns of Appendix Table A2 compare the summary statistics among these two groups, showing much higher balance.

Figure 3: Distribution of propensity scores inside the common support area



The figure shows the distribution of the propensity score obtained from estimating equation 1 for both treated and control neighborhoods, after excluding the observations laying outside the common support probability range.

The exclusion of the observations outside common support reduces the number of control neighborhoods from 140 to 33 units and the number of treated neighborhoods from 29 to 14 units. This means that we are only able to estimate the treatment effect for this sub-sample of affected neighborhoods, which arguably are those that did not present the worst conditions to begin with. Our estimated results should be interpreted as a potential lower bound of the effects on all treated neighborhoods.[23] Additionally, we refrain from directly controlling for the propensity score, as doing so could artificially induce common pre-trends in the outcomes.[24] In a robustness exercise, we keep all public housing units in the analysis

[23] In an heterogeneity analysis we find worse effects for the neighborhoods in our sample that have the highest levels of p-score (within the common support), suggesting that effects might be even more dire on the social housing units for which we do not have comparable controls.

[24] Given that many of the control variables included in equation 1 are outcomes on which we want to test the effect of the reform, in our main specification we do not want to control for their baseline level because it would increase the likelihood of finding common pre-trends. In a robustness test, we show that the coefficients remain unchanged if we control for the propensity score interacted with year fixed effects.

combined with inverse propensity score weighting, and show that the magnitude of the effects remains comparable.

To verify that characteristics are balanced once we select the sample lying within the common support, we regress the treatment dummy on the main neighborhood characteristics over the period 2006 to 2009, and we compare the results with what we obtain if we do not apply any sample selection. Results are presented in Table 1. As already visible from Appendix Table A2, the differences in the overall sample are large in magnitude and always statistically significant. However, the simple exclusion of observations outside the common support brings such differences to much smaller magnitudes, and none of them remains significant. These results comfort us on the comparability of the selected treated and control groups for the Difference-in-Differences analysis.

Table 1: Balancing test

	Treatment dummy			
	Without selection		With selection	
Log wage income	-0.387***	(0.0501)	0.0101	(0.0646)
Log total income	-0.259***	(0.0274)	-0.0292	(0.0302)
Log household gross income pae	-0.156***	(0.0223)	0.0172	(0.0327)
Household size	0.719***	(0.103)	0.0484	(0.140)
Sh. with low education	0.183***	(0.0179)	0.0255	(0.0196)
Sh. not employed	0.134***	(0.0132)	0.0148	(0.0184)
Sh. not employed and not in educ	0.121***	(0.0117)	0.0158	(0.0166)
Share of non-western migrants	0.342***	(0.0328)	0.0664	(0.0411)
N. of crimes committed	0.603***	(0.128)	0.0619	(0.160)
N. obs.	169		47	

The table shows the outcome of regressions of the treatment dummy on different neighborhood characteristics. Each line is a separate regression. Columns (1) and (2) show the coefficient and standard error obtained when all treated and controls are kept in the sample, while Columns (3) and (4) restrict the sample to the base of common support.

Our main estimation model is dynamic, allowing us to measure the complete evolution of outcomes before and after the introduction of the "Ghetto List" in 2010 :

$$Y_{it} = \sum_{t=-4}^{t=8} \alpha_t (year - 2010 = t) + \sum_{t=-4}^{t=8} \beta_t (year - 2010 = t) \times treat_i + \gamma_i + \epsilon_{it} \tag{2}$$

Where the β_t with $t \in [-4, -1]$ allow us to test the presence of parallel pre-trends and the β_t with $t \in [0, 8]$ show the dynamic effects for the eight years following the reform. We omit $t = -1$ as our reference year. γ_i are neighborhood fixed effects, and standard errors are clustered at the neighborhood level. We also show regression tables with the following static model:

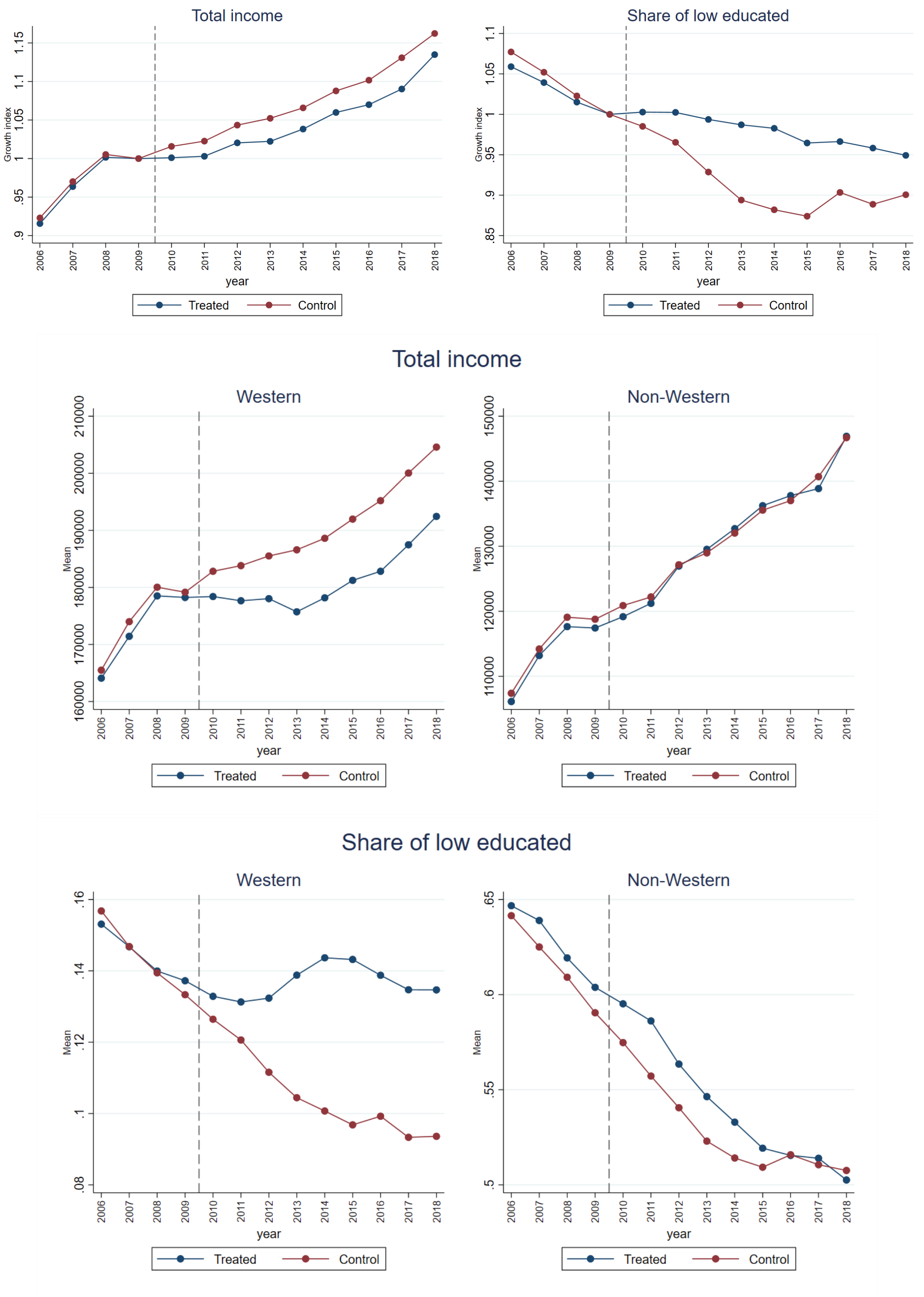
$$Y_{it} = \beta_1 Post_t \times treat_i + \gamma_i + \gamma_t + \epsilon_{it} \tag{3}$$

where we restrict the post-reform period to 2015 and thus β_1 captures the average effect over the first five years following the policy change. γ_i and γ_t are neighborhood and time fixed effects, respectively. Finally, one may argue that if many individuals move from treated to control neighborhoods over the period, this could violate the SUTVA assumption. Appendix Table A3 shows the share of the population in control public housing that previously resided in "ghetto" areas. This share increases over time, by construction, but only reaches 1.8% in 2018, thus making it unlikely to be driving any of the results.

Before moving to the main results, we can look descriptively at how the average outcomes have evolved in treated and control areas over the period. Figure 4 shows that both the average total income and the share of low-educated individuals in the neighborhood were growing at a very similar pace between control and treated areas before 2010, while they start diverging afterwards. In the neighborhoods listed as "ghettos", we observe both a slowdown in income growth and a slowdown in the decline of the share of low-educated individuals, both signaling that the average conditions of citizens living in the area are worsening. In the bottom two panels, we distinguish outcomes between non-Western descendants and Westerners, which include Danes and Western descendants. Interestingly, all of the effects that are visible in the overall sample are driven by Danes and Western descendants. Appendix figures A2 and A3 show the same pictures for additional outcomes, overall and by origin group, respectively. Similarly, we observe a slight increase in the share of the active population not employed and a decrease in average wage income that is driven by Danes and Western descendants. The graphs for the number of crimes are noisier and it is harder to identify a clear trend.

Overall, these descriptive figures suggest that conditions have been worsening in the neighborhoods after being listed as “ghettos”, contrary to the primary aim of the policymakers, and that this worsening is primarily driven by Danes and Western descendants. In the result section, we will test whether these descriptive effects are robust to our regression analysis, we will quantify them, and disentangle the extent to which these are driven by changes in the composition of neighborhood inhabitants versus worsening of the conditions of incumbent inhabitants. We then move to the individual level analysis, defining treatment based on residence prior to the reform, and following people wherever they move (more details on the methodology are reported in section 6).

Figure 4: Evolution of outcomes in treated and control neighborhoods



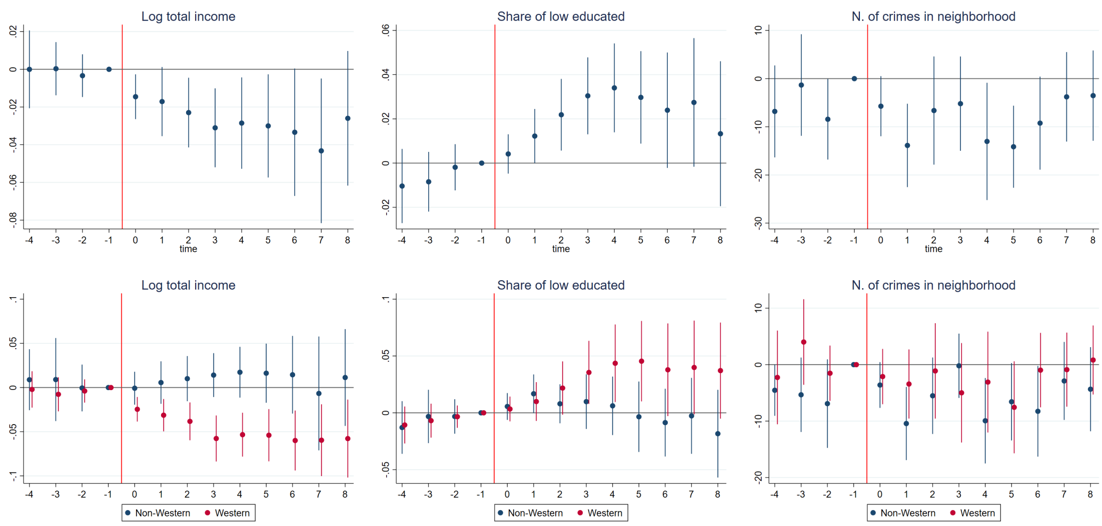
Note: Descriptive evolution of outcomes within treated and control neighborhoods over the period. The top two panels have on the Y-axis the growth rate index, set to 1 in 2009. The bottom two panels distinguish between Non-Western and Western citizens, which includes Danes and migrants from Western countries, and have on the Y-axis the level of the outcome.

5 Results on neighborhoods

5.1 Overall effect

Figure 5 shows the dynamic graphs obtained from estimating equation 2. The vertical lines are the 95% confidence intervals around the estimated coefficients. The top three panels show the outcomes for the overall neighborhood population, while the bottom three panels distinguish between Western and non-Western citizens.[25] Table 2 quantifies the effect using the static regression presented in equation 3, which informs us on the average effect of the policy over the first five years following the publication of the "Ghetto List", until 2015. The table further reports the baseline average among treated neighborhoods computed on the pre-policy period for all outcomes, and calculates the effect in terms of percentage change relative to baseline.

Figure 5: Overall effect of the "Ghetto Plan" on the neighborhoods



Note: The figure reports the estimated difference in trends between treatment and control neighborhoods over the period with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals.

Overall, these figures and the table confirm what was already visible from the descriptive graphs: pre-trends are parallel across all outcomes, and after the introduction of the "Ghetto List", affected neighborhoods saw a drop in income (-2% on average in the first five years) and an increase in the share of low-educated individuals (+5% on average in the first five years). No other effect is significant at the overall neighborhood level, even if we observe a noisy reduction in the number of crimes committed per 10,000 inhabitants.

However, this masks the already described differences in effect among Western and non-Western popula-

[25] Appendix figure A4 shows the same results for additional outcomes: log wage income, share of non-employed, and share of non-Western migrants. Appendix table A4 presents the coefficients for the full set of dynamic effects taking place after the policy.

tions that partly cancel each other out. Among Westerners, total income decreases by 4%, wage income decreases by 8%, the share of low-educated individuals grows by 17%, and the number of property crimes committed– the most common felony among all types of crimes –decreases by 20%. On the contrary, we observe no significant effect among the non-Western population, nor do we observe a significant change in the overall composition between Western and non-Western. These results, except for the crime reduction, go against the intended effects of the policy. At this stage, it is not yet clear whether these are driven by a change in composition, by a negative effect on income of incumbent Western individuals, or by both.

Table 2: Overall effect of the "Ghetto Plan" on the neighborhoods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Share of non-western citizens	Log total income	Log wage income	Share not employed	Share low educ	N. crimes	N. property crimes
Panel A : All citizens							
treat * post	0.0105	-0.0203*	-0.0406	0.00707	0.0221**	-4.395	-2.892
	(0.0124)	(0.0105)	(0.0293)	(0.0110)	(0.00912)	(3.446)	(2.199)
Observations	470	470	470	470	470	470	470
R-squared	0.959	0.951	0.948	0.926	0.928	0.883	0.824
Baseline mean	0.564	11.80	11.12	0.506	0.405	66.21	31.93
Effect (%)	2%	-2%	-4%	1%	5%	-7%	-9%
Panel B : Western citizens							
treat * post	-	-0.0371***	-0.0757***	0.0161	0.0268**	-2.949	-2.648*
		(0.0113)	(0.0275)	(0.0122)	(0.0132)	(2.583)	(1.382)
Observations		470	470	470	470	470	470
R-squared		0.929	0.954	0.922	0.685	0.834	0.763
Baseline mean		12	11.36	0.446	0.155	25.76	13.50
Effect (%)		-4%	-8%	4%	17%	-11%	-20%
Panel C : Non-western citizens							
treat * post	-	0.00759	0.00588	0.000938	0.0101	-1.447	-0.244
		(0.0137)	(0.0464)	(0.0150)	(0.00818)	(1.913)	(1.351)
Observations		470	470	470	470	470	470
R-squared		0.958	0.919	0.878	0.921	0.857	0.776
Baseline mean		11.60	10.90	0.546	0.628	40.45	18.43
Effect (%)		0%	0%	0%	2%	-4%	-1%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the neighborhood level. All regressions control for neighborhood and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. Panel A) shows the effect for the entire neighborhood population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Baseline mean reports the mean value of the outcome for treated neighborhood prior to the "Ghetto Plan", and serves to compute the effect in terms of growth rate relative to baseline.

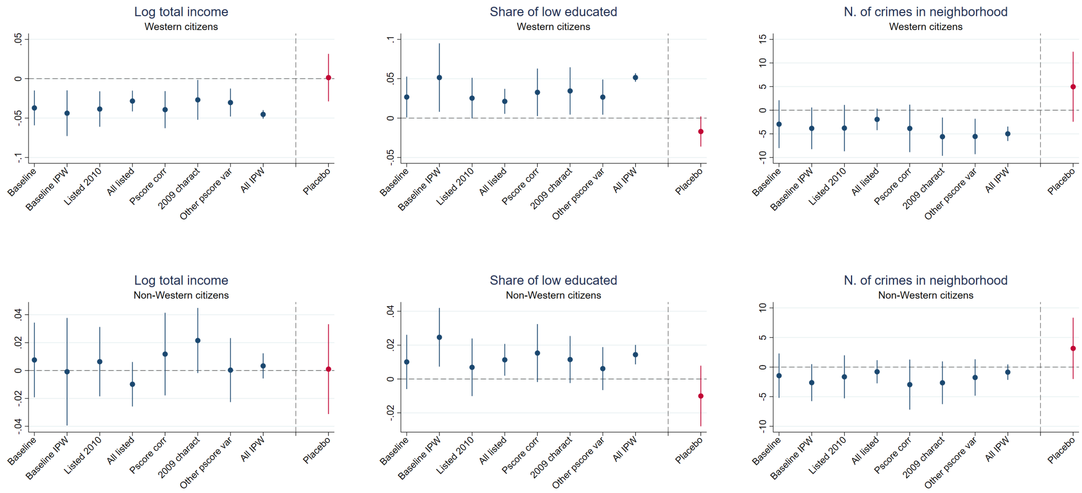
To put these effects in perspective, Appendix Table A5 presents the predicted impact of the policy on spatial and ethnic inequality. Spatial inequality is measured by the income gap between residents in “ghettos” and those in private housing, while ethnic inequality is measured by the income gap between Western and non-Western citizens within “ghettos”. The policy worsens spatial inequality for Western citizens: their total income, relative to private housing residents, drops from -29% to -32%, and their wage income gap widens from -39% to -44%. At the same time, the policy reduces ethnic inequality within listed neighborhoods through a leveling to the bottom effect: the total income gap between Western and non-Western citizens narrows from -33% to -30%, and the wage income gap shrinks from -37% to -32%.

Robustness: To verify the robustness of these results, we perform several tests. First, we introduce inverse propensity score weighting in the baseline model to increase further the comparability of treated and controls. Second, we consider a public housing area to be treated by the policy only as long as it is listed, and we drop observations in the years when the area is taken out of the official "Ghetto list".[26] Third, we extend our analysis to areas listed as "ghettos" after 2010, always using the never treated areas as controls. For this test, we re-estimate our propensity score model and re-apply the restriction on the common support, which brings the number of control areas from 140 to 58 units and the number of treated areas from 57 to 45 units. Fourth, we go back to our baseline model but we include as a control the propensity score measure interacted with year fixed effects, which is another way to increase comparability between treated and control trends. Fifth, we estimate our baseline model on neighborhood characteristics measured in 2009 rather than in 2006. Sixth, we change the variables included in the propensity score model and estimate the probability to be listed only based on average household income expressed in terms of per-adult equivalent and on the share of non-Western migrants and descendants. Seventh, we keep all social housing units and estimate the model with inverse propensity score weighting, to gauge whether the effect measured at baseline are very different from the one obtained on all listed areas. Finally, we perform a placebo test consisting in splitting the never treated areas into a pseudo-treated group and a control group along the propensity score dimension. With this exercise, we test whether control areas that were closer to the "ghetto threshold" with their characteristics experience different trends post 2010 relative to control areas that were further away from it. If the answer is affirmative, this would raise concerns that there might be some confounding factors linked to pre-existing characteristics biasing our main coefficients.

Figure 6 shows the results obtained from these tests. In particular, the figure reports the coefficients and 95% confidence intervals obtained from running the static regression reported in equation 3 on the different samples. The first coefficient from the left reports the baseline specification from Table 2 for comparison. The top three panels show the outcomes for Western citizens and the bottom three show the same outcomes for Non-Western citizens. Appendix Figure A5 shows the same graphs for the other outcomes reported in Table 2. Overall, we can see that the coefficients are very stable across the different robustness tests. If anything, when we keep all listed "ghettos" in the regression with inverse propensity score weighting, we obtain slightly worse effects on income and share of low educated Western individuals. Finally, the effect is almost always non-significant and close to zero on the placebo test. The only

[26]We decide to drop the observations in years after 2010 when a given treated public housing area is not treated anymore, instead of keeping it in the analysis but changing its treatment status, because we want to avoid using previously treated observations as controls, given the biases highlighted by the recent literature (de Chaisemartin and D'Haultfœuille, 2020; Callaway and Sant'Anna, 2021; Borusyak et al., 2024).

Figure 6: Robustness tests



Note: The figure reports the coefficients and the 95% confidence intervals obtained from estimating the static equation 3 in different contexts. The first coefficient from the left reports the baseline estimates for comparison. The second introduces inverse propensity score weighting. The third only considers treatment as long as the neighborhood is listed as "ghetto" in a given year, and drops the observation in the years when it is no longer listed. The fourth extends the analysis to areas listed as "ghettos" after 2010, still restricting the period of analysis to four years pre-listing and five years post-listing. The fifth coefficient goes back to the baseline sample, but includes as a control the propensity score measure interacted with year fixed effects. The sixth coefficient estimates the propensity score model on 2009 characteristics. The seventh estimates an alternative propensity score model based solely on average household income in per-adults equivalent and on the share of non-Western descendants. The eighth coefficient keeps all social housing units in the analysis, even if outside of the common support, with inverse propensity score weighting. Finally, the ninth coefficient, reported in a different color, shows the placebo test where a placebo treatment is assigned to the half of the never-treated areas that have the highest levels of propensity score.

exception is the share of low-educated individuals among Western citizens, where the placebo test shows a negative and marginally significant coefficient. Given that our main result on this outcome is positive, we can conclude that if anything, our magnitude of effect is a lower bound.

Heterogeneity: Appendix Table A6 tests the heterogeneity of the effect across treated neighborhoods with mild and severe pre-policy conditions. In practice, we split the treatment group in two along the propensity score distribution, where *treat severe* corresponds to the listed areas with worse initial conditions in terms of wage income, education, employment rate, and crime. In general, we find that the magnitude of the effect is larger within areas with more severe initial conditions. Given that in our baseline analysis we exclude the listed areas with initial conditions so severe that they lie outside the area of common support of the propensity score, we can conclude that our effects are a lower bound relative to what would be observed there if a comparable control group were available, consistent with the results obtained when all of the “ghettos” are kept in the analysis with inverse propensity score weighting.

In the next section, we investigate the mechanisms behind these effects, focusing on disentangling changes driven by composition from changes driven by the evolution of conditions among incumbents.

5.2 Neighborhood level mechanisms

The main mechanism we test is whether our effects are the result of composition effects versus changes among incumbent individuals. Given that the policy was discussed extensively in the media and that the listed neighborhoods were made widely known in the Danish population, we can hypothesize that the demand for moving into (out of) listed social housing might have decreased (increased), especially among individuals with better outside options. The fact that a lot of the results presented in the previous section are driven by Danes and Western descendants might suggest the presence of native flight and native avoidance, worsening the composition of the households remaining in the areas.

First of all, we test whether the size of the flows in and out of the listed neighborhoods changes after the policy. We compute two different indicators: the number of people moving in and out of the target areas, which we call *flow number*, and the number of people moving in and out of the target areas divided by the incumbent population, which we call *flow rate*. Results obtained from the dynamic specification are reported in Appendix Figure A6. We can see that there is no significant effect on the size of flows, neither in number nor in rate, neither overall nor among specific origin groups. These figures suggest that, if there are composition effects taking place, they are entirely driven by changes in the type of people moving in and out, keeping the number of people moving constant. This also means that moving restrictions aiming at restricting the number of non-Western descendants arriving in these areas have been ineffective, since the share of non-Westerners remains constant.

The second test that we perform to assess the presence of composition effects is to manually replace the values of outcomes within a given household with its mean observed over the entire period. By doing so, we effectively eliminate any variation in outcomes coming from changes within households, thus leaving as the only mechanism possible the change in composition of households. Results for the main outcomes are reported in Appendix figure A7 and table A7. From the figures we see that all of the main effects described in the previous section– i) the decrease in average income, ii) the increase in the share of low educated individuals, and iii) the decrease in number of crimes –remain visible here and continue to be driven by Danes and Western descendants. This suggests that compositional changes play an important role in explaining our main results.

To quantify how much of the total effect is driven by composition rather than by changes observed within existing households, we move to our third test, which consists of disentangling the average outcome within a given neighborhood into three components, as follows:

$$Y_{it} = \sum_{g=1}^{3} \omega_{igt} Y_{igt} \tag{4}$$

Where Y_{it}– the average outcome in neighborhood i at time t –is decomposed into the average outcome observed within three mutually exclusive groups g (Y_{igt}) multiplied by the weight that each group has in the total neighborhood population (ω_{igt}). In our context, the mutually exclusive groups in which the neighborhood population is divided are the following :

– Incumbents: individuals present in the neighborhood at the beginning of the period (2006) and remaining until the end (2018).

– Entrants: individuals who enter the neighborhood after 2006.

– Leavers: individuals present in the neighborhood at the beginning of the period (2006) that leave the area before 2018.

According to this definition, each individual belongs to a unique type within a given neighborhood, but can change type by changing neighborhood (e.g. can be a leaver and an entrant). Additionally, even if entrants leave before 2018, they are only classified as entrants, since we need mutual exclusiveness between types. Finally, we can further define these groups within a given origin type: Western and non-Western, as these categories are themselves mutually exclusive. Results are presented in Table 3, where columns (1) to (4) show the different components of the effect for Westerner citizens, while columns (5) to (8) do the same for Non-Western descendants. Each panel corresponds to a different outcome. An interesting feature of this methodology is that the total effect is equal to the sum of the three different components. All outcomes are entered in levels and not in their logarithmic transformation in order to preserve the decomposition.

The total negative effect on Western total income (-4%) is entirely driven by new entrants after 2010 having lower levels of income than their counterfactual controls (-19%), while incumbents see no change in their level of income. This indeed supports the hypothesis of native avoidance: given the bad reputation gained by these neighborhoods after having been labeled as “ghettos”, the type of natives accepting to move in deteriorates in terms of income. Similarly, the increase in the share of low educated among Westerners (+17%) is entirely driven by an increase in low educated individuals moving into the neighborhood (+80%), while nothing happens on incumbents and leavers. Finally, the decrease in crime observed in the neighborhood is driven by Western individuals who leave the neighborhood at some point during the period. Figure A8 in the Appendix shows the dynamic graphs for the decomposition of the total income and the share of low education effect among Western citizens. This figure allows us to evaluate the presence of parallel pre-trends and the timing of the effect: the worsening of the incoming population picks up slowly after the "Ghetto Plan" and keeps getting worse for average income, while it peaks at t+5 for the share of low-educated and then remains constant.

Table 3: Decomposition of neighborhood effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Western citizens				Non-western citizens			
VARIABLES	Incumbents	Entrants	Leavers	Total	Incumbents	Entrants	Leavers	Total
Panel A) Total income								
treat * post	218.8	-8,297***	1,396	-6,681***	-797.2	1,284	645.4	1,132
	(1,462)	(3,084)	(2,928)	(1,944)	(1,953)	(3,219)	(2,560)	(1,684)
Observations	423	423	423	423	423	423	423	423
R-squared	0.966	0.942	0.959	0.930	0.946	0.934	0.904	0.957
Baseline mean	46023	44259	76508	166789	45852	21025	45160	112037
Effect (%)	0.5%	-18.7%	1.8%	-4.0%	-2%	6%	1%	1%
Panel B) Share not employed								
treat * post	0.00493	0.00933	0.000839	0.0151	-0.00283	0.00418	-0.00392	-0.00256
	(0.00386)	(0.0136)	(0.00739)	(0.0114)	(0.00825)	(0.0173)	(0.0113)	(0.0146)
Observations	423	423	423	423	423	423	423	423
R-squared	0.897	0.937	0.927	0.928	0.914	0.919	0.906	0.890
Baseline mean	0.120	0.118	0.211	0.449	0.218	0.106	0.218	0.542
Effect (%)	4%	8%	0%	3%	-1%	4%	-2%	0%
Panel C) Share low educ								
treat * post	0.00122	0.0203*	0.00365	0.0252**	-0.0113	0.0211	-0.000675	0.00915
	(0.00242)	(0.0108)	(0.00369)	(0.0124)	(0.00868)	(0.0171)	(0.0159)	(0.00829)
Observations	423	423	423	423	423	423	423	423
R-squared	0.929	0.784	0.906	0.694	0.927	0.902	0.921	0.911
Baseline mean	0.0577	0.0255	0.0671	0.150	0.273	0.109	0.232	0.614
Effect (%)	2%	80%	5%	17%	-4%	19%	0%	1%
Panel D) N. crimes								
treat * post	-9.49e-05	-0.000142	-0.00175*	-0.00199	-0.000382	2.97e-05	-0.00221	-0.00256
	(0.000371)	(0.00253)	(0.00103)	(0.00265)	(0.000981)	(0.00194)	(0.00153)	(0.00224)
Observations	423	423	423	423	423	423	423	423
R-squared	0.531	0.555	0.642	0.590	0.599	0.586	0.431	0.527
Baseline mean	0.00343	0.0124	0.0112	0.0270	0.00580	0.00708	0.0126	0.0255
Effect (%)	-3%	-1%	-16%	-7%	-7%	0%	-18%	-10%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the neighborhood level. All regressions control for neighborhood and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. Columns (1) to (4) show the different components of the effect for Westerners, while columns (5) to (8) do the same for non-Western descendants. Each panel corresponds to a different outcome. Baseline mean reports the mean value of the outcome for the treated neighborhood before the "Ghetto Plan", and serves to compute the effect in terms of growth rate relative to the baseline.

All in all, these results show that the worsening composition of new entrant Westerns is driving the bulk of the effect on neighborhoods, strongly suggesting the presence of native avoidance behavior. In the next section, we move away from the neighborhood level perspective to focus on the causal effect of the reform on affected individuals, defined as those people living in the neighborhoods at the time the "Ghetto Plan" was introduced.

6 Results on exposed individuals

6.1 Overall effect

In this section, we estimate the causal impact of the 2010 "Ghetto Plan" on individuals who were exposed to the policy, that is, living in "ghetto" neighborhoods before they were listed. To do so, we compare individuals who resided in the above-defined treated and control neighborhoods between 2007 and 2009.[27] Although the neighborhoods are similar on average as seen in Table 1, individuals living in the neighborhoods are not necessarily similar.[28] Columns (1) and (2) in Appendix Table A8 report the coefficients and the robust standard errors regressing the treatment dummy on various individual-level characteristics in the baseline period (2006 to 2009). These reflect the differences between individuals living in treated compared to control neighborhoods. To ensure comparability, we estimate the following linear propensity score model at the individual level in 2009:

$$P(treat_i) = \beta_0 + \beta_1 Perc_i + \epsilon_i \tag{5}$$

where *treat* is the treatment status of an individual *i*, and *Perc* is the percentile of total income to which individual *i* belongs. We choose to match treated and control individuals only on one dimension to be parsimonious: their position in the total income distribution. We show that the results are robust to matching on other baseline characteristics. The distribution of the propensity scores is shown in Figure A9. We then apply inverse probability weighting (IPW) to generate weights used to reweight individuals in the treated and control neighborhoods to increase their comparability. Columns (3) and (4) in Appendix Table A8 show the balancing test when the estimations are reweighted using the IPW. There are still some significant differences, despite reweighting, that are small in magnitude: the level of total income, wages, benefits, and the probability of unemployment, while the other dimensions, including these measures in log, become insignificantly different from zero.

We then estimate the dynamic and static models from section 4 (eq. 2 and 3) at the individual level, where *i* identifies individuals instead of neighborhoods. Individuals belong to the treated (control) group if they resided in a given treated (control) neighborhood in the baseline period (2007-2009). We follow individuals irrespective of whether they remain in the same neighborhood or not– they may move to a different social housing or to private housing. We also construct an indicator for whether the individual has left Denmark. Similarly to the results at the neighborhood level, we restrict the post-reform period in the static regressions to up to 2015.

[27] We impose this restriction of a minimum of three years of residence to exclude individuals changing housing very frequently.

[28] In the neighborhood analysis, each area counts as one, while in the individual level analysis, each neighborhood has a weight corresponding to the number of residents. The sample restriction to individuals residing in neighborhoods for at least three years

Figure 7: Overall effect of the "Ghetto Plan" on exposed individuals

Note: The figure reports the estimated difference in trends between treatment and control individuals over the period with respect to the last pre-reform year obtained from estimating equation 2 with γ_i being individual fixed effects. The results are decomposed by origin. Vertical bars represent the 95% confidence intervals.

Figure 7 plots the individual-level β_t from equation 2. It shows the difference in outcomes between individuals from the treated and control groups compared to their differences in the pre-treatment period, controlling for time-invariant individual characteristics. The figure focuses on total income and on its two main components: labor income and benefits. Appendix Figure A11 shows the same plot for capital income and for the probability of committing a crime. Table 4 shows the static coefficients on all of these outcomes, also disaggregated by origin. We observe parallel trends up to 2010, when the introduction of the "Ghetto Plan" led to a significant negative effect on total income (-3%) and an even more severe decline in wage income (-7%) for Western citizens, partially mitigated by an increase in social benefits receipts (+6%). The effect for non-Western citizens is much smaller in magnitude and short-lived. The overall probability of committing a crime is not affected, while the probability of committing a narco-related crime appears to drop significantly from the static regressions, but the patterns are very noisy and not clear-cut if we look at the dynamic effects, where none of the individual year effects is significant.

Overall, these results show that not only has the composition of listed neighborhoods worsened in terms of socio-economic outcomes, but also that affected Western individuals suffer an income loss causally driven by the policy. To put these results in perspective, we can calculate the implied effect on ethnic inequality for exposed individuals. Appendix Table A9 shows that before the policy, exposed non-Western "ghetto" residents were earning 12% less in total income and 30% less in wage income relative to Western "ghetto" residents. The policy shrinks their disadvantage to 10% in total income and 25% in wage income. Thus, at the individual level as well, we find that the policy decreased the level of ethnic inequality through a leveling to the bottom. Several reasons can reconcile these results with the absence of visible effects on incumbents shown in the previous section. For instance, the effect might be driven by individuals who have left the "ghetto". Additionally, the effect might be stronger for individuals in large "ghettos", which were weighted equally in the neighborhood level analysis but differently in the individual level analysis. The mechanisms section investigates this further.

(2007 to 2009) further differentiates the two datasets.

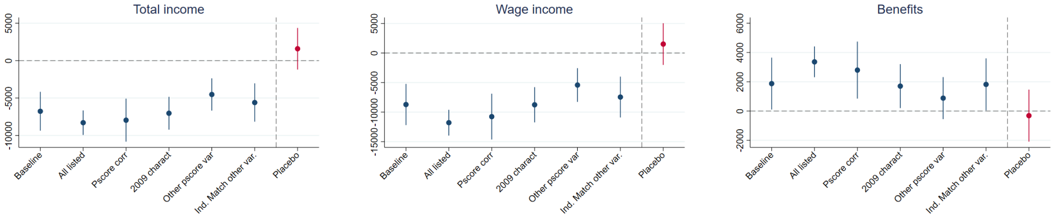
Table 4: Overall effect of the "Ghetto Plan" on the individuals

	(1)	(2)	(3)	(4)	(5)	(6)
	Income components				Crime	
	Total income	Wage income	Capital income	Benefits	P. of committing any crime	P. of committing narco crime
Panel A: All individuals						
Treat * post	-6,500***	-8,685***	-74.4	1,874**	-0.00067	-0.00166*
	(1,299)	(1,778)	(104.5)	(904.5)	(0.00148)	(0.000847)
Observations	168,400	168,400	168,400	168,400	168,400	168,400
R-squared	0.739	0.782	0.298	0.768	0.312	0.242
Baseline mean	240169	160270	428.3	78346	0.0211	0.00690
Effect (%)	**-3%**	**-5%**	-17%	**2%**	-3%	**-24%**
Panel B: Western citizens						
Treat * post	-7,949***	-12,505***	-46.23	4,216***	-0.00150	-0.00259**
	(1,795)	(2,504)	(179.9)	(1,248)	(0.00230)	(0.00130)
Observations	90,340	90,340	90,340	90,340	90,340	90,340
R-squared	0.754	0.795	0.321	0.785	0.315	0.216
Baseline mean	256688	181778	471.9	69265	0.0254	0.00904
Effect (%)	**-3%**	**-7%**	-10%	**6%**	-6%	**-29%**
Panel C: Non-Western citizens						
Treat * post	-3,394*	-3,028	-49.35	-652.5	0.000165	-0.00091
	(1,869)	(2,523)	(105.5)	(1,318)	(0.00192)	(0.00110)
Observations	78,060	78,060	78,060	78,060	78,060	78,060
R-squared	0.698	0.744	0.198	0.744	0.308	0.291
Baseline mean	225784	137949	390.3	86254	0.0173	0.00503
Effect (%)	**-2%**	-2%	-13%	1%	1%	-18%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the individual level. All regressions control for individual and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. Panel A) shows the effect for the whole population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Baseline mean reports the mean value of the outcome for treated individuals prior to the "Ghetto Plan" and serves to compute the effect in terms of growth rate relative to baseline.

Robustness : To verify the robustness of the main individual-level results, we perform several tests, similarly to those performed at the neighborhood level. Figure 8 reports the coefficients and the 95% confidence intervals obtained from estimating the static equation 3 in different contexts. The first coefficient from the left reports the baseline estimates for comparison. The second coefficient includes all public housing units, assigned to either treatment or control, and performs the estimation using inverse propensity score weighting based on the neighborhood-level propensity score. The third coefficient corresponds to the baseline specification controlling for propensity score interacted with year fixed effects. The fourth coefficient estimates the propensity score model at the neighborhood level using 2009 characteristics, mimicking the robustness test presented in section 5. The fifth coefficient estimates an alternative propensity score model at the neighborhood level based solely on average household income in per-adult equivalent and on the share of non-Western descendants. The sixth coefficient uses alternative variables for the individual matching procedure. Finally, the seventh coefficient, reported in a different color, shows the placebo test where a placebo treatment is assigned to half of the never-treated areas that have the highest levels of propensity score. Overall, the negative effect on total and wage income and the increase in benefits are robust across all tests, and is insignificant and close to zero in the placebo test.

Figure 8: Robustness tests



Note: The figure reports the coefficients and the 95% confidence intervals obtained from estimating the static equation 3 in different contexts. The first coefficient from the left reports the baseline estimates for comparison. The second includes all public housing units, assigned to either treatment or control. The third is the baseline specification controlling for the propensity score interacted with year fixed effects. The fourth estimates the propensity score model at the neighborhood level using 2009 characteristics. The fifth estimates an alternative propensity score model at the neighborhood level based solely on average household income in per-adults equivalent and on the share of non-Western descendants. The sixth uses alternative variables for the individual matching procedure. Finally, the seventh coefficient, reported in a different color, shows the placebo test where a placebo treatment is assigned to half of the never-treated areas that have the highest levels of propensity score.

Drivers of wage income effect : The main driver of the drop in income of Western citizens is wage income. We thus explore further which labor market effect is driving it. Table A10 disentangles wage income into wage income if working and probability of working, both defined on a yearly level.[29] We further divide the wage income if working into number of hours worked over the year and hourly wage. Finally, we explore the effects on the probability of switching employer relative to the one reported in 2009, on the probability of switching industry relative to the one reported in 2009, and on the commuting distance between home and work. Results show that all of the effect on wage income of Western citizens is coming from working less, rather than by a drop in hourly wage. Specifically, we observe a 5% drop in the probability of working at all during the year, which we can qualify as long term unemployment, and a 5% drop in hours worked in the year conditional on positive hours, which we can qualify as either short term unemployment or reduction in working time (we cannot disentangle these two). Finally, we do not see any significant effect on employer or sector change, nor on commuting distance.

Effect on moving patterns : Similarly to the neighborhood level analysis, we explore the impact of the policy on residential mobility. At the neighborhood level, we find no significant effects on the sizes of inflows or outflows from "ghettos". However, the individual-level analysis may yield different results due to key differences in design: the sample is restricted to individuals who remained in the same location between 2007 and 2009, and more populous neighborhoods are given greater weight, unlike the neighborhood-level analysis where each area is weighted equally which might reveal patterns that are not visible in the aggregate analysis.

[29]Not working implies that the individual had zero labor income over the year, while any positive level of wage income would be counted as wage income if working, even if the individual worked for a short period of time during the year.

Appendix Figure A12 and Table A11 report the results obtained on the probability of changing building, of changing parish, and of changing municipality relative to 2009.[30] Interestingly, the negative effect on Western individuals is not accompanied by a change in the probability of leaving the “ghetto” relative to controls. However, conditional on leaving, Western individuals exposed to the policy are less likely to switch municipality relative to controls (-7%), thus signaling that they tend to move closer relative to their starting point and perhaps signaling that they would not have moved absent treatment. We discuss these effects further in the mechanisms section, as we think they suggest the presence of discrimination on the rental market, limiting the outside options of individuals who want to leave the "ghetto". Consistent with this explanation, Non-Western citizens are less likely to leave the "ghetto" relative to controls (-8%), and conditional on leaving, they are less likely to switch parish and municipality.

Importantly for us, the absence of effects on the probability of moving for Western citizens allows us to explore mobility as a potential channel behind the negative wage effects in the next section. In the Online Appendix B, we analyze the decision to move out in more details, and we present additional results.

6.2 Individual level mechanisms

We focus the exploration of mechanisms mainly on Western citizens, given that non-Westerns appear unaffected by the policy, as shown in the previous section. There are three main channels through which the policy can affect exposed individuals: i) discrimination based on residence by employers and/or landlords, ii) negative peer effects resulting from a worsening of the characteristics of “ghetto” residents, and iii) a decline in the utility of residing in “ghettos”, which could lead costly relocation decisions.

Discrimination based on place of residence can operate through two channels. External discrimination occurs when employers or landlords are less willing to hire or rent to individuals from areas labeled as “ghettos”. Internalized discrimination arises when residents anticipate such bias and adjust their behavior accordingly—by for example reducing job search effort or narrowing their housing search to avoid likely rejection. These mechanisms highlight how both direct bias and the expectation of discrimination can shape individual outcomes. While these mechanisms may seem especially relevant for the non-Western population, it might also be plausible that discrimination against non-Western individuals is so widespread that it affects them even in control areas —- unlike Western residents, who might only experience discrimination when associated with a “ghetto” address. Negative peer effects are also likely to affect Western residents more strongly, since they appear to be the primary drivers of the observed compositional decline. Lastly, the amenity value of living in listed neighborhoods may decline due to the policy, thus triggering relocation decisions. Employer discrimination and peer effects primarily affect individuals who remain

[30] The data contains approximately 21 thousand buildings, 1400 parishes and 652 municipalities.

in the “ghettos”, whereas landlord discrimination and changes in residential preferences predominantly influence those who move out.

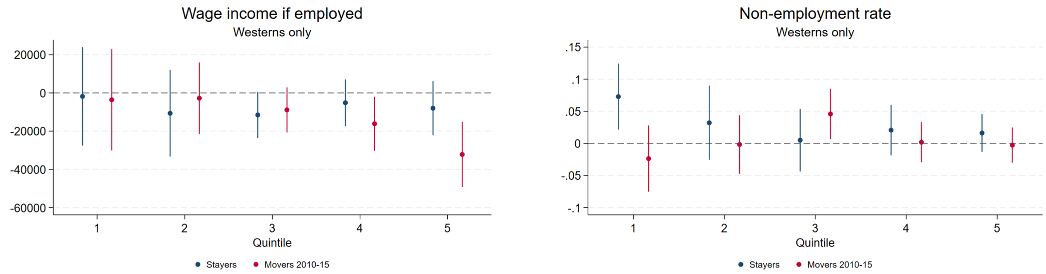
We start by exploring whether the negative effect on wage income of Western citizens is primarily driven by individuals leaving their neighborhood or by individuals staying after the policy. Given that moving is an endogenous decision, for this analysis to retain a causal interpretation, we need to assume that the policy did not generate a significant selection on who leaves. We believe that this assumption is plausible since the number of Western movers remains stable after the policy, as shown in the previous section and in Online Appendix Section B on individual level data, while non-Westerners are less likely to move out of “ghetto” than out of control areas.[31] In addition, Appendix Table A13 compares the average characteristics of Western leavers from control and treated areas and shows that the two groups are similar on observables.

Appendix Figure A13 shows the event study graphs on wage income, wage income if working and employment rate of Western citizens by three separate groups: stayers– the people that never leave the “ghetto” –, movers leaving shortly after the policy (2010-12), and movers leaving between 2013 and 2015. The control group consists of individuals residing in control areas at baseline that follow the same moving patterns. Here we limit the analysis to the 2006-2015 period. Appendix Table A12 reports the underlying static coefficients as well as the effect in percentage terms relative to the baseline. Results show that the negative effect on the annual employment rate is entirely driven by stayers (+11%), while the negative effect on wage income while working is driven by leavers to a much larger extent (-7% to -8% versus -3% for stayers). This suggests that the impact operates through different margins for the two groups of stayers and leavers. Figure 9 further disentangles the static effect on Western stayers and leavers by quantiles of household income before the shock, showing that the effect on the non-employment of stayers comes from the bottom of the income distribution, while the effect on the wage income of leavers comes from the top. We take these results as evidence that separate channels may be at play for individuals who stay in the “ghetto” and for individuals who leave.

The rise in long-term non-employment among low-income Western stayers is concentrated among individuals who were already largely out of the labor force before the policy—70% were not employed in 2009—and occurs immediately following the policy’s introduction. Since neighborhood composition deteriorated only gradually, negative peer effects (channel ii) are unlikely to explain this pattern. Instead, the timing and concentration of the effect point to residential discrimination on the labor market (channel i) as the most plausible mechanism, as it likely hindered job seekers and prolonged their unemployment spells. We cannot disentangle whether the effect is due to external discrimination by employers or inter-

[31] This might be an indicator of discrimination on the housing market, but also the result of an identity backslash where individuals feels closer connected to their neighborhood and prefer to stay, see Foged et al. (2025).

Figure 9: Heterogeneity of the effect by moving status and income quantile



Note: The figure reports the estimated treatment effect of the policy on wage income conditional on working and on non-employment rate, only considering Western citizens and dividing the sample between stayers and leavers and between quantiles of pre-policy household income.

nalized expectations of rejection among residents. Anecdotal evidence suggests that job seekers living in “ghetto” areas— including young and educated individuals—often believe their applications are less likely to be considered if they disclose their addresses.[32]

To further rule out the possibility that peer effects are driving the increase in non-employment among Western stayers, Appendix Figure A14 examines changes in the neighborhood composition of low-educated residents, comparing two groups of neighborhoods: those with a mild increase versus those with a sharp increase in the share of low-educated residents. Although the latter group shows a significant change as early as the first year, the full effect takes four years to materialize and then stabilizes at a high level. In contrast, the impact on non-employment among Western stayers peaks within the first two years and declines thereafter —- regardless of whether they live in neighborhoods with mild or sharp compositional changes. This timing mismatch further supports that peer effects are unlikely to explain the observed pattern, reinforcing residential discrimination as the most plausible mechanism.

We now explore the channel behind the negative effect of the policy on the wage income of employed individuals, which is greater among those who move out of the listed areas that were relatively better off at baseline. The most plausible explanation behind these patterns is that individuals have a shift in preferences, and value leaving the stigma associated with "ghettos" to the point of accepting a loss in income. However, if it was only the shift in preferences that was at play, we would expect a surge in the number of people leaving the “ghettos”, while the latter remains stable. This suggests that while affected residents are motivated to escape the stigma associated with their previous address, they may face significant constraints in accessing higher-opportunity areas because of higher prices and discrimination

[32] See for example: Gulis et al. (2020) Table 1: *The "ghetto" categorization is stigmatizing; after obtaining a good education, people cannot get jobs because they live in a "ghetto" area*; or Jonas Strandholdt Bach (Aarhus University): *Some people find that, because they live in Gellerup, they end up in the wrong pile of job applications. Of course, it's hard to know why you're rejected, but there are many young men in particular who find that their job application never gets any further because it says Gellerup in the corner. Some therefore write a different postcode on the application* (own translation), quoted in Fagbladet Boligen, Ole Ellekrog (2017, November 30): ***Forskere: Ghettolisten stigmatiserer og tjener intet praktisk formål***, `https://fagbladetboligen.dk/alle-nyheder/2017/november/forskere-ghettolisten-stigmatiserer-og-tjener-intet-praktisk-formal/`, last accessed June 13, 2025.

on the rental market.

For this analysis, we restrict the sample to individuals who move out of treated and control neighborhoods after 2009, and we consider the year of move as the time of the first move out of the neighborhood after the policy introduction in 2010. Finally, we adopt an event study framework where we follow treated and control individuals before and after moving. It is important to note that this analysis is not causal since moving is an individual choice and thus its timing is endogenous, contrary to the timing of the reform. Nonetheless, this approach can still be informative of the different conditions accepted by movers in treated neighborhoods as compared to controls, especially since the policy did not generate differential outflows among Western citizens. Appendix Table A13 shows the summary statistics of individual conditions the year before moving, across treated and control individuals, and different moving times. While not identical, the average characteristics of leavers are economically similar between treated and control areas.

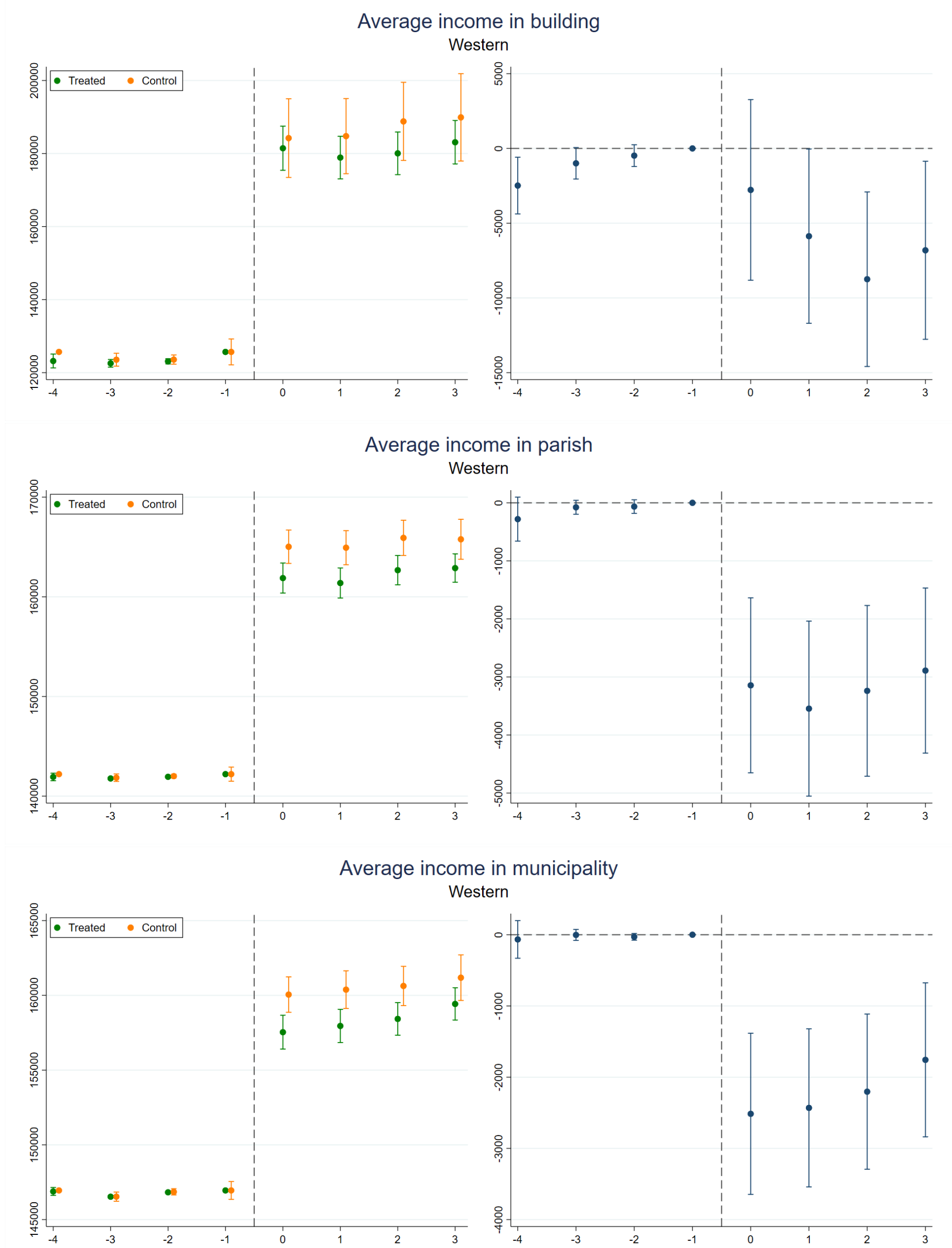
We start by testing whether the policy affected the quality of the neighborhood chosen by Western citizens at the time of moving. We perform the event study around the time of departure, adding controls for the decile of neighborhood quality before leaving, interacted with year fixed effects.[33] Figure 10 shows the effect of the policy on the average income of the building, parish and municipality of destination. The left-hand panels of each figure present the effects for individuals leaving treated and control areas separately, which confirms that our additional decile controls made them perfectly comparable at the start. The right-hand panels show the dynamic Difference in Differences coefficients. Appendix Figure A16 shows similar pictures for the share of non-Western in the building, parish and municipality, while Appendix Table A14 summarizes the static regression coefficients. All in all, we see that Western individuals leaving "ghettos" move to areas with worse average income relative to the destination chosen by controls, even though for both groups they choose areas with higher income relative to their origin. In particular, they move to a building with 3% lower income than controls, in a parish with 2% lower income, and 7% additional share of non-Western relative to controls, and in a municipality with 1% lower income and 5% higher share of non-Western than controls.[34] We take these results as indirect evidence that individuals face discrimination in the housing market pushing them to worse quality neighborhoods.

Finally, we compare the income trajectory of movers out of listed neighborhoods with the one of movers out of control areas to get a sense of the willingness to pay that people have to escape the "ghetto" label,

[33]The reason is that we want to compare treated and control individuals leaving from perfectly comparable neighborhoods to test whether the labeling of "ghettos" had an impact on destination decisions.

[34]Lawrence Katz, interviewed in *The New York Times* regarding the "ghetto plan," expressed concern about the potential harms of relocations. Drawing on his research with co-authors on the *Moving to Opportunity* experiment in the U.S., Katz emphasized that relocation only improves outcomes—-particularly for young children—-when families move to significantly better neighborhoods (Chetty et al., 2016). Otherwise, he warned, such policies risk "creating trauma without creating opportunity" (Erdbrink, 2023). Our findings align with these concerns: individuals leaving designated "ghetto" areas in Denmark were, on average, not able to relocate to wealthier neighborhoods. To the extent that U.S. evidence is applicable in the Danish context, this supports Katz's cautionary stance and might especially hurt young children in the longer run.

Figure 10: Event studies along the time of moving on the average income of the destination



The figure reports the estimated difference in trends between treatment and control individuals over the period with respect to the last year of residence in the neighborhood. Contrary to the rest of the paper, here the timing is defined by the year of the move rather than by the reform. Vertical bars represent the 95% confidence intervals. Here we also add controls for deciles of the average income in the neighborhood of departure, interacted with year fixed effects, to compare treated and control individuals leaving from comparable origin characteristics.

which is a proxy measure of the dis-utility associated with remaining in these areas after the policy. Table 5 presents the static coefficients, and Appendix Figure A15 presents the event study graphs. While we are mostly interested in the effect on Western citizens, we also show results for non-Westerners for comparison.[35]

Table 5: Willingness to pay analysis

	(1) total income	(2) Wage income	(3) Wage income if working	(4) Non-employment
Panel A: All individuals				
Treat *post	-7,226***	-8,653***	-12,653***	0.00902
	(2,126)	(2,726)	(3,294)	(0.00778)
Observations	62,113	62,129	44,045	61,704
R-squared	0.751	0.799	0.729	0.674
Baseline mean	260869	179490	250391	0.377
Effect (%)	-3%	-5%	-5%	2%
Panel B: Western citizens				
Treat *post	-9,786***	-10,419***	-14,294***	0.00731
	(2,685)	(3,562)	(4,034)	(0.0100)
Observations	39,129	39,135	29,657	38,957
R-squared	0.757	0.800	0.729	0.674
Baseline mean	272210	197460	259954	0.340
Effect (%)	-4%	-5%	-5%	2%
Panel C: Non-Western citizens				
Treat *post	-2,018	-4,011	-7,056	0.00937
	(3,442)	(4,214)	(5,668)	(0.0125)
Observations	22,984	22,994	14,388	22,747
R-squared	0.720	0.779	0.712	0.663
Baseline mean	237310	151141	227689	0.423
Effect (%)	-1%	-3%	-3%	2%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the neighborhood level. All regressions control for individual and year fixed effects, as specified in the static regression equation 3. Panel A) shows the effect for the whole population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Here the event is the move out of the neighborhood and not the introduction of the policy. Individuals are followed from 4 years prior to the move to 3 years after. Baseline mean reports the mean value of the outcome for treated neighborhood prior to moving and serves to compute the effect in terms of growth rate relative to baseline.

We find that, on average, the income of Danes moving out of "ghettos" drops by 9,800 DK per year (about 1,500 USD) relative to individuals moving out of control neighborhoods. This corresponds to a drop of 4% relative to their annual income before moving. This is mostly driven by a drop in wage income among employed individuals. Income and employment trends were parallel between treated and control individuals before the move, which reassures us of the validity of this analysis despite the endogeneity of the moving decision. We find no effect on the income of non-Western citizens after moving, but these results are likely to be biased by selection, given that less non-Western citizens leave

[35] As can be seen in the more extensive analysis of mobility in section B, for non-Westerners we find a reduction in their probability to leave "ghettos" towards the end of our period of observation. This probability is not different for treated vs. control Western citizens which makes the comparison of post-moving trajectories for Western treated and control individuals clearner than for non-Western.

their baseline neighborhood after the policy. All in all, these results give us a monetary value of the dis-utility of remaining in the neighborhood after it has been publicly designed as a “ghetto”. While in control neighborhoods people may mostly move to follow career opportunities, in treated neighborhoods people may decide to move to escape the stigma, even to the cost of having to change to a worse job, wait longer for a promotion, or to spend some time in unemployment. This is supported by anecdotal evidence from people moving out of “ghettos” emphasizing that they did not want to move had it not been for the policy.[36]

To conclude, a back-of-the-envelope calculation suggests that 66% of the decline in wage income among Western individuals is attributable to leavers, and 34% to stayers. This implies that two-thirds of the overall effect is driven by a reduced utility of remaining in the “ghetto” combined with discrimination on the rental market, which prompted individuals to move despite facing poorer residential options. We take this result as evidence of native flight. In our case, this is triggered by wanting to avoid the stigma associated with the neighborhood rather than from the number of non-Western population, given that they move to buildings with a similar share of non-Western immigrants and descendants located in parishes with a higher share of non-Westerners relative to the destination of controls. The remaining one-third of the effect appears to be mostly the result of labor market discrimination, either external or internalized, that affects those who stayed.

7 Conclusion

This paper evaluates the impact of labeling geographic areas on neighborhood characteristics and the extent to which it can generate behavioral responses. We do this by evaluating the “Ghetto Plan”, a Danish policy classifying public housing units as “ghettos” without much simultaneous interventions to improve neighborhood composition and individual livelihoods. Our findings reveal that this labeling affected outcomes in the opposite direction to its intended objectives: In targeted areas, average income and educational attainment declined after the reform, and individuals of Western origin experienced significant income losses that can be causally attributed to the policy—regardless of whether and where they relocated.

[36]The housing association Bo Vita, for example, administers public housing areas in Copenhagen. One is Mjølnerparken in Nørrebro which has been on every "ghetto" list since the first Plan was introduced. Another one is in Christianhavn which is much closer to the city center and has never been listed. The contact person for flats in Christianshavn said in an interview that they thought there would be a strong demand for their flats when offered to people in Mjølnerparken. This turned out not to be the case. He said many people are happy to live in Mjølnerparken, mainly because of their social networks. See https://www.arte.tv/en/videos/100300-027-A/denmark-s-immigration-hardline/, last accessed January 2023. In another interview with someone who made the decision to leave Mjølnerparken, the primary reason for moving was highlighted as the uncertainty surrounding the conditions for their continued residence and livelihood under the "Ghetto Plan". It was crucial for them to find alternative housing in close proximity since that was the person’s childhood neighborhood and where their family resides. See https://www.dr.dk/nyheder/indland/antallet-af-ghettoer-er-naesten-halveret-hvis-jeg-ikke-var-noedt-til-flytte-saa, last accessed January 2023.

Our analysis uncovers multiple mechanisms through which the “Ghetto Plan” affected both neighborhoods and their residents. First, the public labeling triggered a stigmatization effect that significantly altered residential preferences and mobility patterns. We estimate that Danes and Western residents were willing to pay a cost equivalent to 4% of their annual income to avoid living in labeled areas. Second, the policy led to strong composition effects by deterring better-off Danes and Westerners from moving into these neighborhoods, thereby substantially worsening the socioeconomic profile of new entrants. Third, the policy amplified discrimination against residents of labeled areas through distinct mechanisms for different groups. Among those who stayed, increased labor market discrimination contributed to worse employment outcomes, explaining 34% of the wage income decline among Western residents. Among those who moved, a combination of housing market discrimination and shifting preferences accounted for the remaining 66% of the income loss, as movers faced barriers to accessing higher-opportunity neighborhoods and often settled in lower-quality areas instead. Finally, the policy failed to achieve its stated goal of reducing non-Western concentration: this group did not increase its mobility out of targeted areas. We also find little evidence of changes in the characteristics, outcomes, or behaviors of non-Western descendants, the population most explicitly targeted by the reform, apart from a reduced likelihood of residential mobility, which may reflect either discrimination in the housing market or identity-based backlash, as analyzed by Foged et al. (2025). Given the behavioral responses driving all these mechanisms, it appears that the label itself prevented the policy from producing meaningful improvements, either for the neighborhoods as a whole or for their residents.

The mechanisms we identify extend beyond this specific policy context. Even when labeling is not ingrained in a formal policy, it may emerge through media discourse and political rhetoric, causing the sorting patterns and residential mobility responses that we document to appear. Finally, our estimated effects should be interpreted as a lower bound, since the modest policy interventions likely worked in the opposite direction, implying that the true effect of the label alone may be even larger.

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Appendix

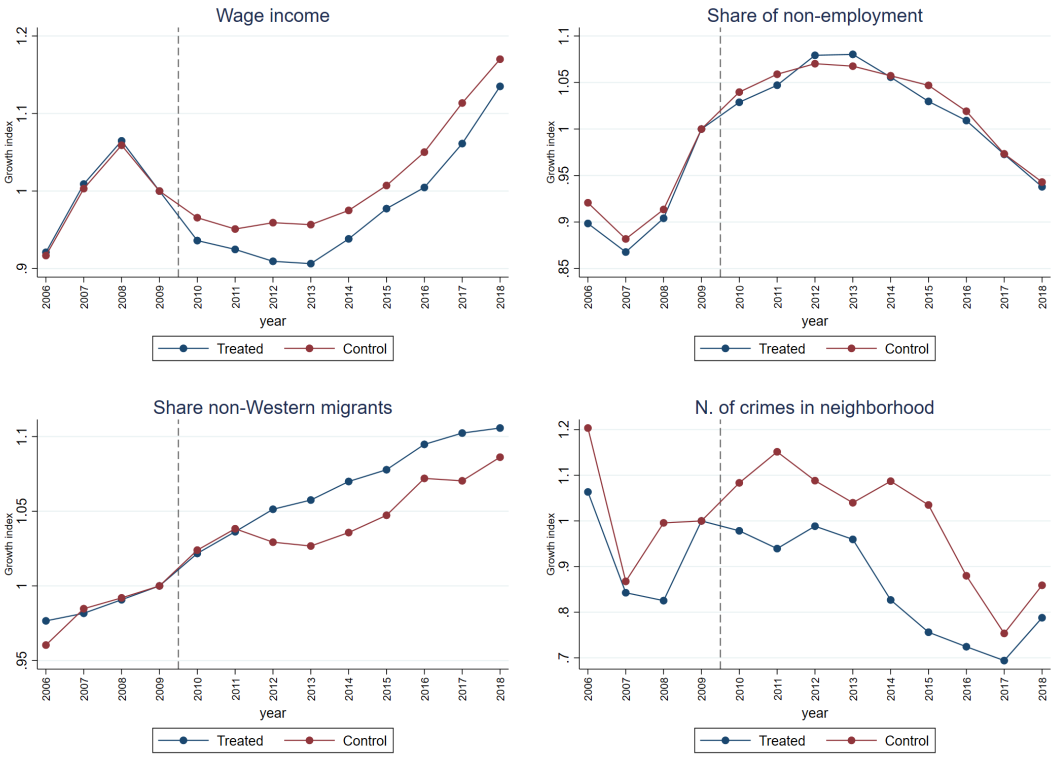
A Additional Tables and Figures

A.1 Figures

Figure A1: Map of listed "Ghettos" in 2010

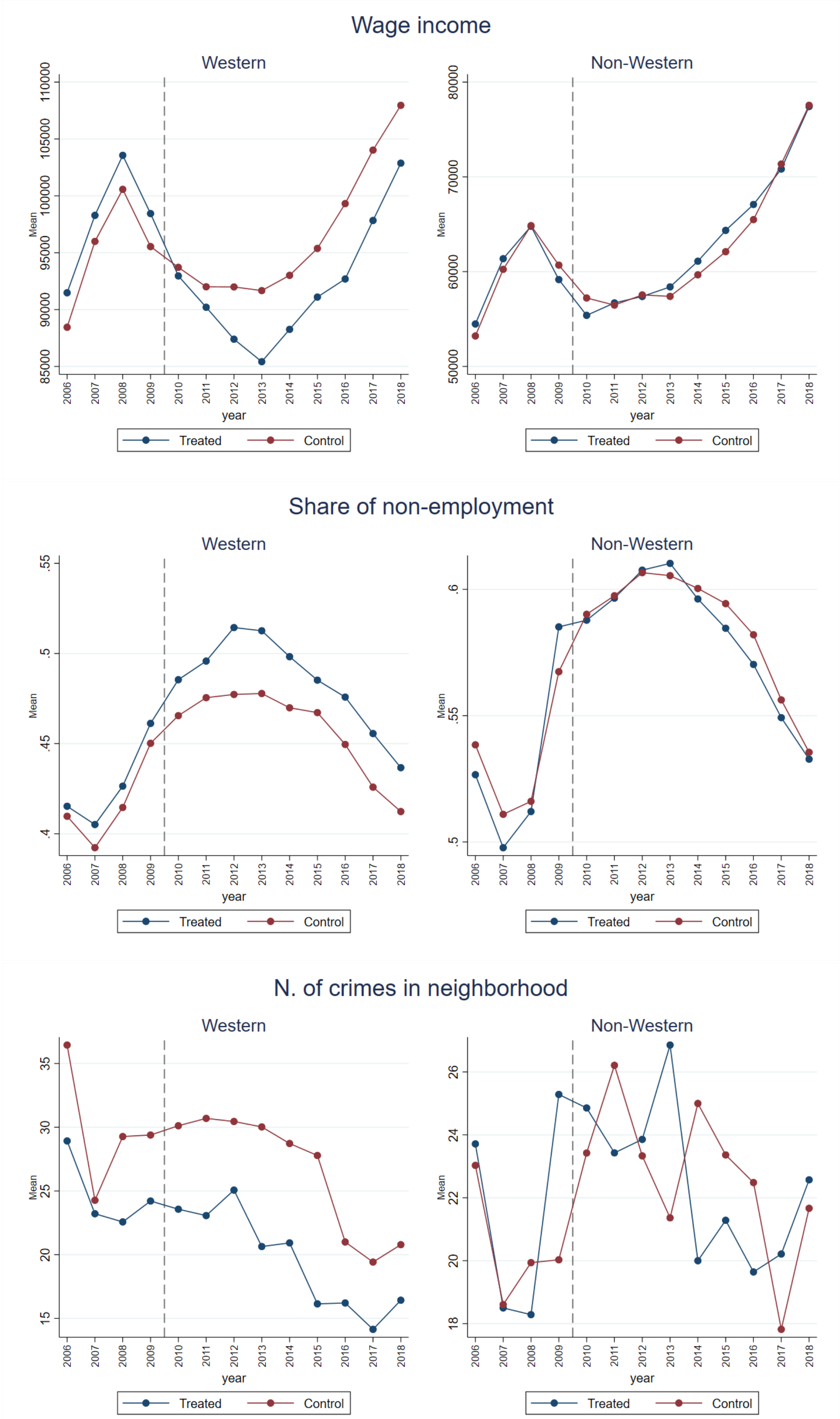
This map shows the location of the neighborhoods listed as “ghettos” in 2010. Source: Denmark 2010 Ghetto Strategy Plan.

Figure A2: Evolution of additional outcomes in treated and control neighborhoods



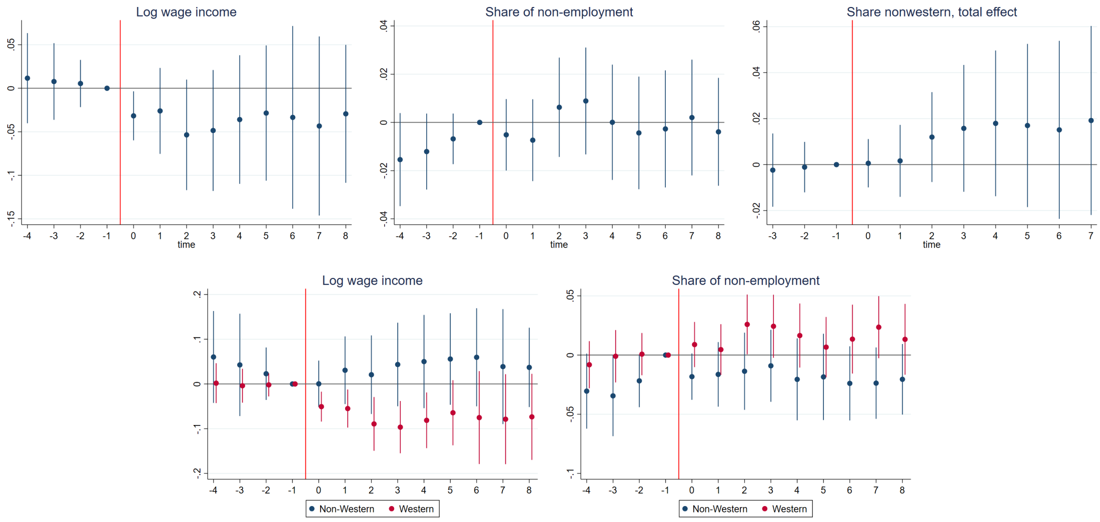
Descriptive evolution of outcomes within treated and control neighborhoods over the period. The Y-axis represent a growth rate index, set to 1 in 2009.

Figure A3: Evolution of additional outcomes in treated and control neighborhoods by nationality



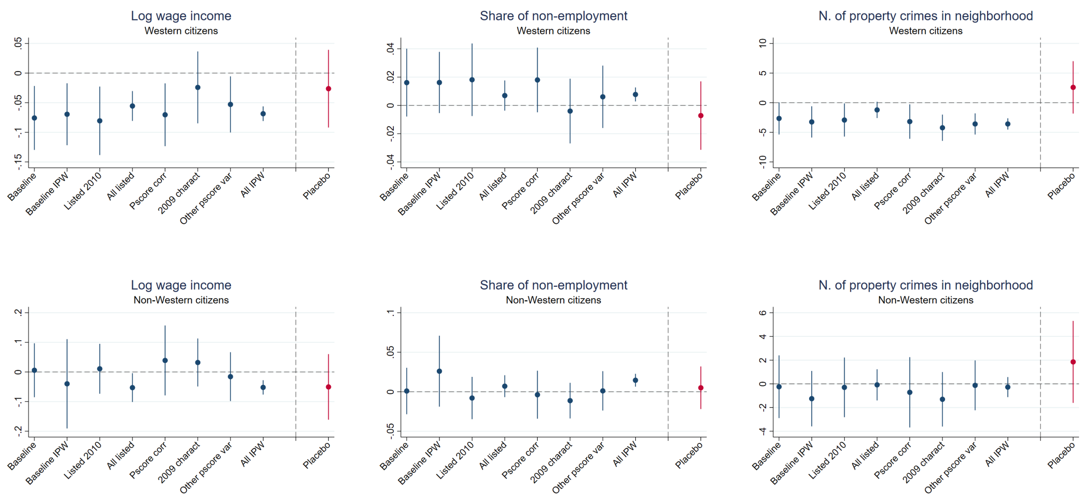
Descriptive evolution of outcomes within treated and control neighborhoods over the period, distinguishing between Non-Western migrants and Western, which includes Danes and migrants from Western countries. The Y-axis captures the level of the outcome.

Figure A4: Overall effect of the "Ghetto Plan" on the neighborhoods - additional outcomes



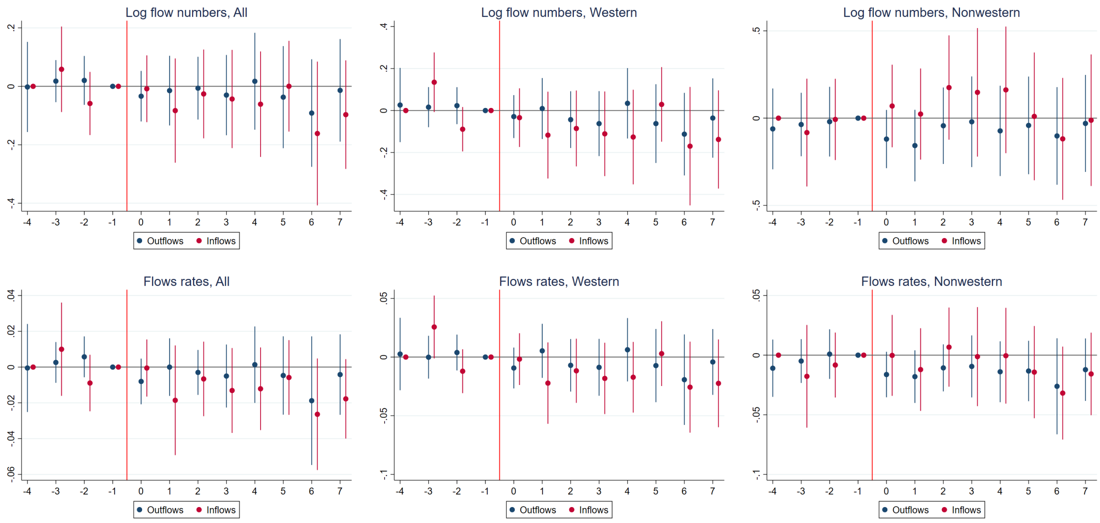
The figure reports the estimated difference in trends between treatment and control occupations with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals.

Figure A5: Robustness tests



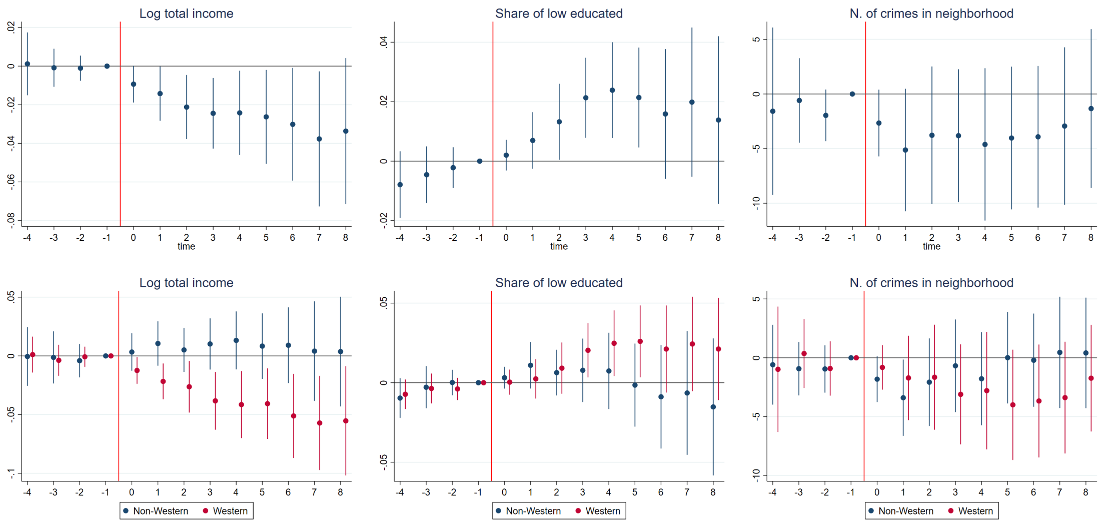
Note: The figure reports the coefficients and the 95% confidence intervals obtained from estimating the static equation 3 in different context. The first coefficient from the left reports the baseline estimates for comparison. The second only considers treatment as long as the neighborhood is listed as "ghetto" in a given year, and drops the observation in the years when it is no longer listed. The third extends the analysis to areas listed as "ghetto" after 2010, still restricting the period of analysis to four years pre-listing and five years post-listing. The fourth coefficient goes back to the baseline sample, but includes as a control the propensity score measure interacted with year fixed effects. The fifth coefficient estimates the propensity score model on 2009 characteristics. The sixth estimate an alternative propensity score model based solely on average household income in per-adults equivalent and on the share of non-Western descendants. Finally, the eighth coefficient, reported in a different color, shows the placebo test where a placebo treatment is assigned to the half of the never treated areas that have the highest levels of propensity score.

Figure A6: Size of flows in and out of the neighborhoods



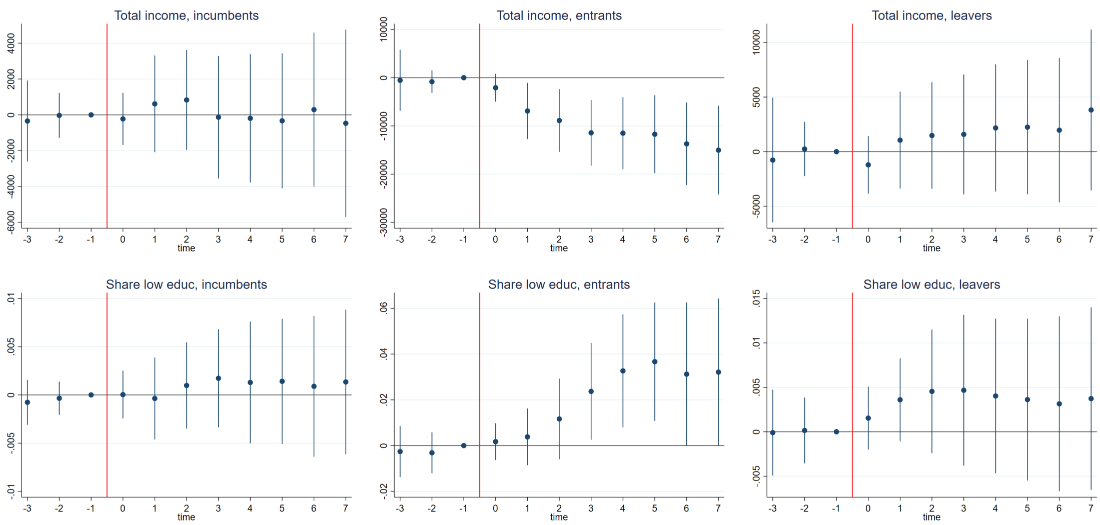
The figure reports the estimated difference in trends between treatment and control neighborhoods over the period with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals. The outcomes compute the size of flows in and out of the areas, measured in number of people, and the flow rates, computed as the number of people moving in or out divided by the incumbent population.

Figure A7: Pure composition effects



The figure reports the estimated difference in trends between treatment and control neighborhoods over the period with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals. The outcomes are transformed such that each household takes the average outcome value observed within the household over the entire period. As such, there is no effect coming from changes within households and all of the effect captured is driven by composition.

Figure A8: Decomposition of effects among Western citizens



The figure reports the estimated difference in trends between treatment and control neighborhoods over the period with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals. The outcomes are transformed following the decomposition presented in equation 4. Here we only present the two main outcomes for the nationality group including Danes and Western migrants.

Figure A9: Distribution of propensity scores at the individual level

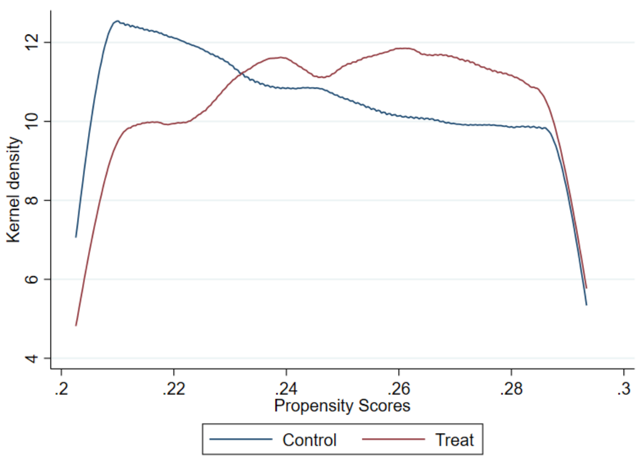
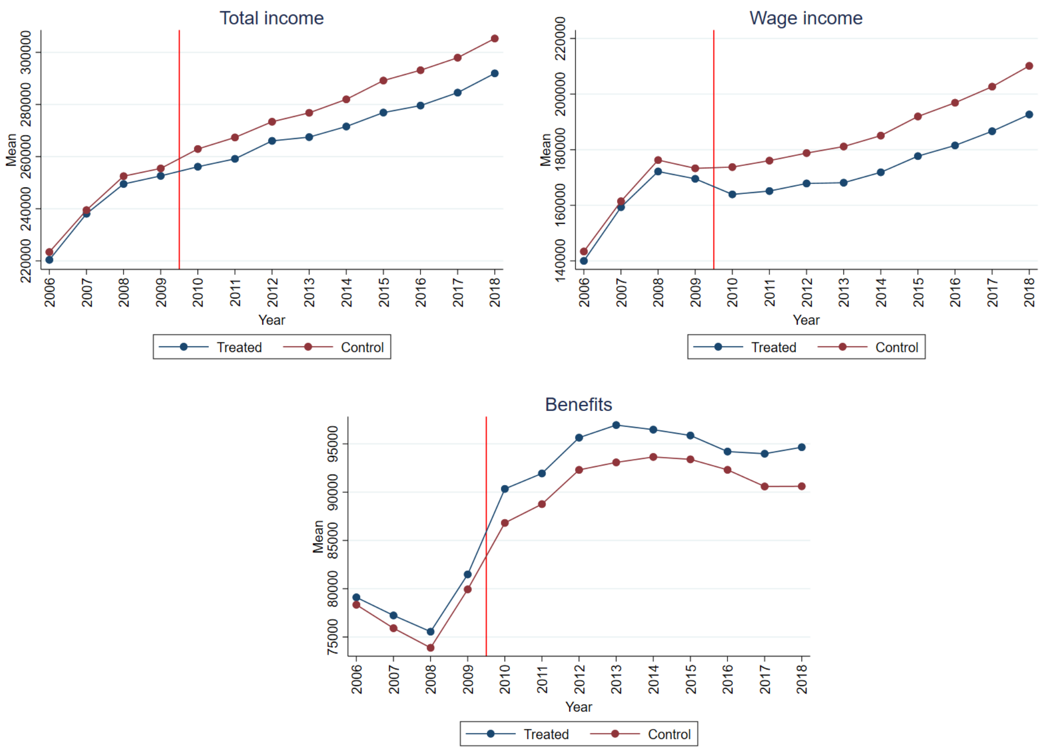
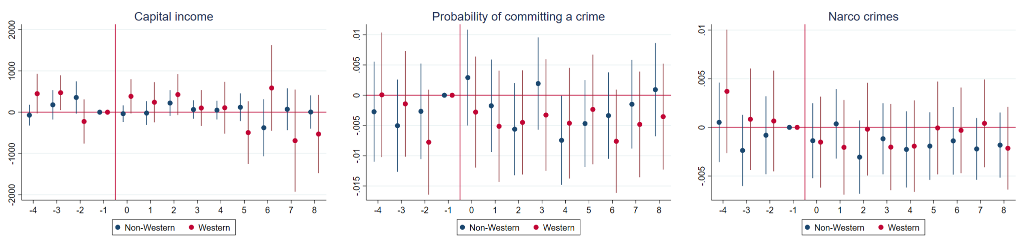


Figure A10: Descriptive effect of the "Ghetto Plan" on exposed individuals



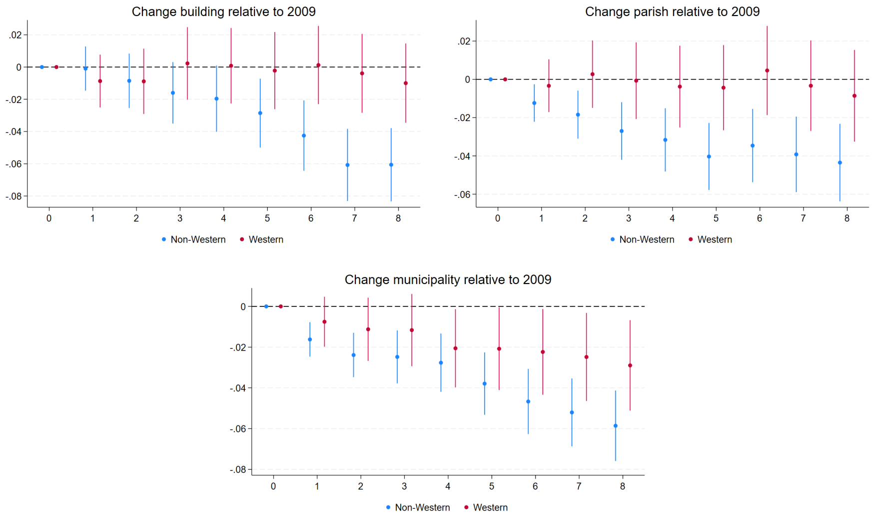
Note: Descriptive evolution of outcomes for treated and control individuals over the period.

Figure A11: Overall effect of the "Ghetto Plan" on individuals - additional outcomes



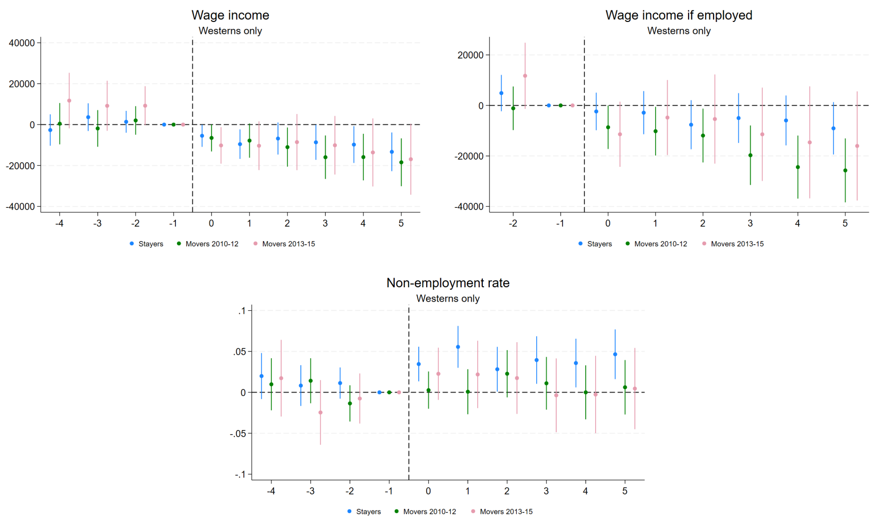
Note: The figure reports the estimated difference in trends between treatment and control individuals over the period with respect to the last pre-reform year obtained from estimating equation 2 with γ_i being individual fixed effects. The results are decomposed by origin. Vertical bars represent the 95% confidence intervals.

Figure A12: Effect of the policy on individual probability of moving



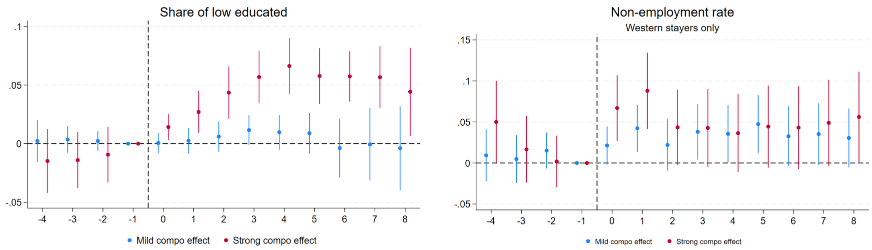
Note: The figure reports the estimated difference in trends between treatment and control individuals on the probability of remaining in the same building, parish and municipality. By definition, all treated and controls have remained in the same place between 2007 and 2009, so the event study captures the differential probability of moving out from 2010 onward. The data contains approximately 21 thousand buildings, 1400 parishes and 652 municipalities. The results are decomposed by origin. Vertical bars represent the 95% confidence intervals.

Figure A13: Effect of the policy on income and employment by moving status



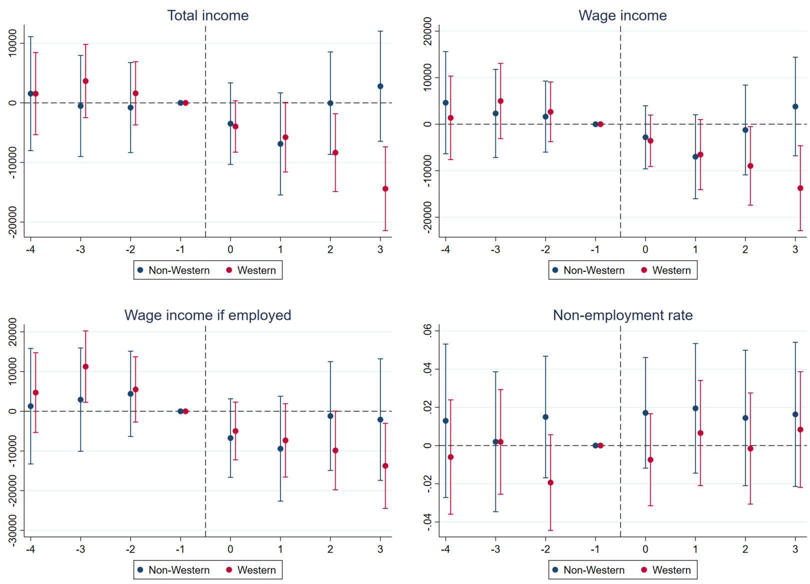
Note: The figure reports the estimated difference in trends between treatment and control individuals over the period with respect to the last pre-reform year obtained from estimating equation 2 with γ_i being individual fixed effects. The results are decomposed by stayers in the neighborhood versus movers at different times and only includes Western citizens. Vertical bars represent the 95% confidence intervals.

Figure A14: Heterogeneity of the effect on employment rate by severity of composition effect



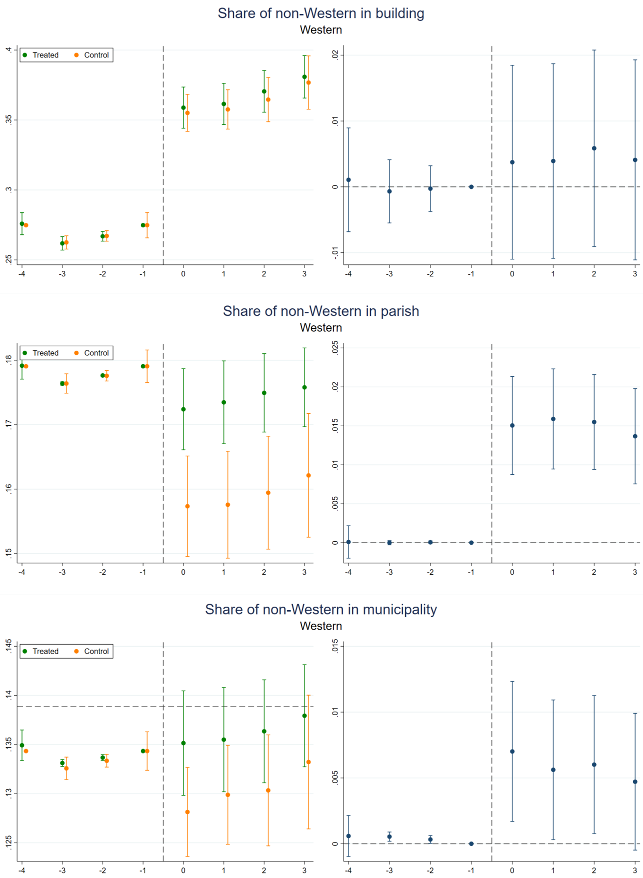
Note: The left panel shows the neighborhood level regression on the share of low educated residents (driven by composition), dividing the sample between neighborhoods with a severe worsening of composition and mild worsening of composition. The right panel presents the individual level effect on non-employment rate on Western stayers divided between treated individuals in neighborhoods that experience a severe worsening of composition and mild worsening of composition.

Figure A15: Event studies along the time of move out of the neighborhood



The figure reports the estimated difference in trends between treatment and control individuals over the period with respect to the last year of residence in the neighborhood. Contrary to the rest of the paper, here the timing is defined by the year of move rather than by the reform. Vertical bars represent the 95% confidence intervals.

Figure A16: Event studies along the time of move out of the neighborhood on new residence share of non-Western individuals



The figure reports the estimated difference in trends between treatment and control individuals over the period with respect to the last year of residence in the neighborhood. Contrary to the rest of the paper, here the timing is defined by the year of move rather than by the reform. Vertical bars represent the 95% confidence intervals. Here we add controls for deciles of the share of non-western individuals in the neighborhood of departure interacted with year fixed effects, to compare treated and control individuals leaving from comparable origin characteristics.

A.2 Tables

Table A1: Summary statistics on income across housing types

		Ghettos	Other social housing	Private housing
Total income, Western	level (average)	164103	186354	242450
	relative to private housing	68%	77%	
Total income, Non-Western	level (average)	109630	128073	177432
	relative to private housing	62%	72%	
Wage income, Western	level (average)	88555	110125	160134
	relative to private housing	55%	69%	
Wage income, Non-Western	level (average)	55854	75262	129618
	relative to private housing	43%	58%	
Capital income, Western	level (average)	1001	1853	12373
	relative to private housing	8%	15%	
Capital income, Non-Western	level (average)	162	356	2522
	relative to private housing	6%	14%	
Benefits, Western	level (average)	67433	62413	48762
	relative to private housing	138%	128%	
Benefits, Non-Western	level (average)	51684	50242	40507
	relative to private housing	128%	124%	
N. obs		116	560	6791588
N. neighborhoods		29	140	1697897

The table summarizes the average income of neighborhood residents across different housing types: i) social housing listed as "ghettos" in 2010, ii) other social housing, iii) private housing. The Table distinguishes between the average income of Western and non-Western citizens and between the three most important income components: i) wage income, ii) capital income, and iii) benefits. The averages are taken over the period 2006 to 2009, just before the policy announcement.

Table A2: Summary statistics on neighborhoods

	Before selection		After selection	
	Control	Treated	Control	Treated
Wage income	102201	69751	80963	79979
	(21644)	(16967)	(21419)	(13867)
Total income	173322	134028	151423	146460
	(20467)	(18242)	(19085)	(12603)
Household size	2.5	3.2	2.8	2.9
	(0.43)	(0.52)	(0.52)	(0.40)
Sh. with low education	0.22	0.40	0.31	0.34
	(0.08)	(0.09)	(0.07)	(0.06)
Sh. not employed	0.37	0.51	0.46	0.48
	(0.08)	(0.07)	(0.07)	(0.06)
Sh. not employed and not in educ	0.33	0.45	0.41	0.43
	(0.07)	(0.06)	(0.07)	(0.05)
Share of non-western migrants	0.22	0.56	0.38	0.45
	(0.14)	(0.17)	(0.13)	(0.13)
N. of crimes committed	34.1	66.2	50.3	46.2
	(25.89)	(63.07)	(38.94)	(25.29)
N. obs (neighborhoods x years)	560	116	132	56
N. neighborhoods	140	29	33	14

The table summarizes the main characteristics of social housing units classified into treated (listed in 2010), and control (never listed). The two columns on the left consider all treated and control units while the two columns on the right restrict the sample to the units inside the common support area obtained from the propensity score model. The period considered is the one preceding the reform (2006-2009).

Table A3: Share of people in control neighborhoods that previously resided in a treated neighborhood

year	**share of pop.**
2006	0
2007	0.1%
2008	0.3%
2009	0.5%
2010	0.7%
2011	0.9%
2012	1.1%
2013	1.3%
2014	1.5%
2015	1.6%
2016	1.6%
2017	1.7%
2018	1.8%

The table summarizes the share of residents in control neighborhoods that have previously resided in a treated neighborhood since 2006. By definition this share is 0 in 2006 and slowly increases over time.

Table A4: Dynamic effect of the "Ghetto Plan" on the neighborhoods

VARIABLES	(1) Log total income	(2) Log wage income	(3) Share not employed	(4) Share low educ	(5) N. crimes	(6) N. property crimes
Panel A : All citizens						
Year = 2010	-0.0131*	-0.0375*	0.00324	0.00871	-1.579	-1.394
	(0.00758)	(0.0217)	(0.00889)	(0.00650)	(3.151)	(2.589)
Year = 2011	-0.0157	-0.0319	0.00100	0.0168**	-9.725**	-4.732
	(0.0101)	(0.0307)	(0.0111)	(0.00779)	(3.912)	(2.889)
Year = 2012	-0.0215*	-0.0594	0.0147	0.0264**	-2.482	-3.185
	(0.0108)	(0.0392)	(0.0128)	(0.00992)	(5.179)	(3.214)
Year = 2013	-0.0296**	-0.0544	0.0173	0.0350***	-1.054	-1.751
	(0.0122)	(0.0424)	(0.0135)	(0.00975)	(4.609)	(3.228)
Year = 2014	-0.0271*	-0.0418	0.00842	0.0386***	-8.892	-5.857**
	(0.0135)	(0.0431)	(0.0134)	(0.0111)	(5.728)	(2.906)
Year = 2015	-0.0286*	-0.0344	0.00401	0.0343***	-9.995**	-5.713*
	(0.0146)	(0.0449)	(0.0137)	(0.0107)	(4.159)	(3.165)
Year = 2016	-0.0320*	-0.0393	0.00568	0.0285**	-5.094	-3.959*
	(0.0182)	(0.0594)	(0.0142)	(0.0138)	(4.281)	(2.181)
Year = 2017	-0.0418**	-0.0492	0.0104	0.0320**	0.362	-0.647
	(0.0206)	(0.0579)	(0.0135)	(0.0149)	(3.925)	(2.231)
Year = 2018	-0.0219	-0.0339	0.00362	0.0154	0.617	1.830
	(0.0188)	(0.0410)	(0.0115)	(0.0167)	(4.145)	(2.017)
Observations	611	611	611	611	611	611
R-squared	0.938	0.939	0.924	0.890	0.884	0.832
Baseline mean	11.80	11.12	0.506	0.405	66.21	31.93
Panel B : Western citizens						
Year = 2010	-0.0209***	-0.0497**	0.0107	0.00832	-2.168	-1.427
	(0.00747)	(0.0199)	(0.0101)	(0.00920)	(2.277)	(1.811)
Year = 2011	-0.0276**	-0.0540**	0.00646	0.0149	-3.489	-3.870**
	(0.0109)	(0.0247)	(0.0119)	(0.0120)	(2.285)	(1.703)
Year = 2012	-0.0346***	-0.0884***	0.0277**	0.0267*	-1.169	-2.921
	(0.0118)	(0.0322)	(0.0138)	(0.0148)	(3.519)	(1.893)
Year = 2013	-0.0540***	-0.0956***	0.0262*	0.0405**	-5.048	-3.039
	(0.0157)	(0.0329)	(0.0151)	(0.0167)	(3.638)	(2.179)
Year = 2014	-0.0497***	-0.0804**	0.0183	0.0486**	-3.149	-2.069
	(0.0152)	(0.0339)	(0.0157)	(0.0199)	(3.772)	(1.761)
Year = 2015	-0.0503***	-0.0634*	0.00854	0.0504**	-7.618*	-5.056*
	(0.0165)	(0.0365)	(0.0144)	(0.0200)	(3.887)	(2.578)
Year = 2016	-0.0563***	-0.0742	0.0153	0.0428*	-1.030	-1.952
	(0.0192)	(0.0529)	(0.0161)	(0.0223)	(2.529)	(1.614)
Year = 2017	-0.0560**	-0.0778	0.0254*	0.0449**	-0.949	-1.375
	(0.0227)	(0.0513)	(0.0141)	(0.0222)	(2.545)	(1.724)
Year = 2018	-0.0532**	-0.0729	0.0140	0.0409*	0.757	1.452
	(0.0230)	(0.0475)	(0.0148)	(0.0224)	(2.560)	(1.602)
Observations	611	611	611	611	611	611
R-squared	0.912	0.939	0.912	0.654	0.838	0.771
Baseline mean	12	11.36	0.446	0.155	25.76	13.50
Panel C : Non-western citizens						
Year = 2010	-0.00431	-0.0306	0.00321	0.0100	0.589	0.0325
	(0.0119)	(0.0462)	(0.0134)	(0.00643)	(2.208)	(1.678)
Year = 2011	0.00210	-0.000214	0.00513	0.0212***	-6.236**	-0.863
	(0.0135)	(0.0499)	(0.0149)	(0.00773)	(2.668)	(2.025)
Year = 2012	0.00653	-0.0102	0.00779	0.0124	-1.312	-0.265
	(0.0158)	(0.0595)	(0.0167)	(0.00837)	(3.133)	(2.167)
Year = 2013	0.0105	0.0126	0.0124	0.0143	3.994	1.288
	(0.0155)	(0.0626)	(0.0167)	(0.00961)	(2.987)	(2.182)
Year = 2014	0.0138	0.0192	0.000921	0.0106	-5.743*	-3.788**
	(0.0168)	(0.0654)	(0.0178)	(0.0121)	(3.277)	(1.860)
Year = 2015	0.0127	0.0249	0.00300	0.000948	-2.377	-0.657
	(0.0189)	(0.0678)	(0.0202)	(0.0138)	(2.563)	(1.728)
Year = 2016	0.0109	0.0288	-0.00246	-0.00421	-4.064	-2.007
	(0.0265)	(0.0737)	(0.0171)	(0.0156)	(3.434)	(1.620)
Year = 2017	-0.0102	0.00791	-0.00226	0.00175	1.311	0.728
	(0.0370)	(0.0834)	(0.0162)	(0.0179)	(2.355)	(1.274)
Year = 2018	0.0110	0.00841	9.72e-05	-0.0157	-0.140	0.378
	(0.0311)	(0.0560)	(0.0151)	(0.0194)	(2.945)	(1.345)
Observations	611	611	611	611	611	611
R-squared	0.936	0.917	0.877	0.890	0.857	0.783
Baseline mean	11.60	10.90	0.546	0.628	40.45	18.43

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the neighborhood level. All regressions control for neighborhood and year fixed effects, as specified in the dynamic regression equation 2. Period of analysis: 2006 - 2018. Panel A) shows the effect for the entire neighborhood population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Baseline mean reports the mean value of the outcome for treated neighborhood prior to the "Ghetto Plan".

Table A5: Effect on income in levels and implications for neighborhood inequality

	(1)	(2)	(3)	(4)
	Total income		Wage income	
	Western	Non-Western	Western	Non-Western
treat * post	-6,747***	1,105	-6,542***	286.8
	(1,997)	(1,731)	(2,387)	(2,308)
Observations	470	470	470	470
R-squared	0.932	0.955	0.955	0.927
Baseline mean	164103	109630	88555	55854
Predicted effects on spatial inequality				
Income ratio “ghetto” to private housing at baseline	71%	64%	61%	46%
Predicted ratio after the policy change	68%	64%	56%	46%
Predicted effects on ethnic inequality				
Income ratio Non-Western to Western at baseline	67%		63%	
Predicted ratio after the policy change	70%		68%	

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the neighborhood level. All regressions control for neighborhood and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. The first exercise computes how the effect translates into changes in spatial inequality relative to private housing, while the second exercises computes how the effect translates into changes in ethnic inequality within "ghetto" neighborhoods.

Table A6: Heterogeneity of overall effect of the "Ghetto Plan" by severity of initial conditions

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Log total income	Log wage income	Share not employed	Share low educ	N. crimes	N. property crimes
Panel A : All citizens						
Treat severe * post	-0.0240*	-0.0587	0.0126	0.0326**	-5.729	-4.547**
	(0.0142)	(0.0389)	(0.0125)	(0.0129)	(3.817)	(1.874)
Treat mild * post	-0.0166	-0.0382	0.00157	0.0117	-3.062	-1.238
	(0.0112)	(0.0375)	(0.0160)	(0.00943)	(4.427)	(3.055)
Observations	470	470	470	470	470	470
R-squared	0.951	0.948	0.926	0.930	0.883	0.824
Baseline mean treat severe	11.85	11.22	0.485	0.373	50.82	22.86
Baseline mean treat mild	11.93	11.33	0.467	0.305	41.54	19.71
Effect treat severe (%)	-2.4%	-5.9%	3%	9%	-11%	-20%
Effect treat mild (%)	-1.7%	-3.8%	0%	4%	-7%	-6%
Panel B : Western citizens						
Treat severe * post	-0.0363**	-0.0704***	0.0126	0.0327	-3.193	-3.452**
	(0.0146)	(0.0255)	(0.0101)	(0.0235)	(3.182)	(1.483)
Treat mild * post	-0.0379***	-0.0810*	0.0196	0.0208**	-2.705	-1.845
	(0.0135)	(0.0427)	(0.0204)	(0.00914)	(3.041)	(1.617)
Observations	470	470	470	470	470	470
R-squared	0.929	0.954	0.922	0.687	0.834	0.763
Baseline mean treat severe	12.03	11.43	0.430	0.151	25.25	12
Baseline mean treat mild	12.08	11.50	0.424	0.137	24.21	12.11
Effect treat severe (%)	-3.6%	-7.0%	3%	22%	-13%	-29%
Effect treat mild (%)	-3.8%	-8.1%	5%	15%	-11%	-15%
Panel C : Non-western citizens						
Treat severe * post	0.00877	-0.0256	0.0210	0.0203*	-2.536	-1.096
	(0.0202)	(0.0691)	(0.0192)	(0.0107)	(1.622)	(1.256)
Treat mild * post	0.00642	0.0374	-0.0191	-0.000105	-0.358	0.607
	(0.0144)	(0.0439)	(0.0179)	(0.00898)	(2.832)	(1.974)
Observations	470	470	470	470	470	470
R-squared	0.958	0.919	0.882	0.922	0.857	0.777
Baseline mean treat severe	11.65	10.98	0.540	0.613	25.57	10.86
Baseline mean treat mild	11.62	10.97	0.521	0.642	17.32	7.607
Effect treat severe (%)	0.9%	-2.6%	4%	3%	-10%	-10%
Effect treat mild (%)	0.6%	3.7%	-4%	0%	-2%	8%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the neighborhood level. All regressions control for neighborhood and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. Panel A) shows the effect for the entire neighborhood population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. The regressions divide treatment into two intensities: mild and severe, corresponding to the bottom and top half of the propensity score distribution within the treatment group. Baseline mean reports the mean value of the outcome for treated neighborhood prior to the "Ghetto Plan", separately for mild and severe conditions, and serves to compute the effect in terms of growth rate relative to baseline.

Table A7: Comparing overall effect to pure neighborhood composition

VARIABLES	(1) Log total income	(2) Log wage income	(3) Share not employed	(4) Share low educ	(5) N. crimes	(6) N. property crimes
Panel A : All citizens						
treat * post	-0.0198**	-0.0273	0.000636	0.0185***	-2.972	-6.161
	(0.00832)	(0.0223)	(0.00673)	(0.00681)	(3.228)	(4.814)
Observations	470	470	470	470	470	470
R-squared	0.965	0.963	0.949	0.930	0.952	0.967
Baseline mean	11.97	11.25	0.550	0.295	80.20	135.6
Effect (%)	-2.0%	-2.7%	0.1%	6.3%	-3.7%	-4.5%
Panel B : Western citizens						
treat * post	-0.0293**	-0.0395	0.00127	0.0175*	-1.965	-3.807
	(0.0114)	(0.0259)	(0.00865)	(0.00971)	(2.209)	(3.687)
Observations	470	470	470	470	470	470
R-squared	0.928	0.954	0.945	0.801	0.920	0.945
Baseline mean	12.14	11.45	0.510	0.138	23.99	48.82
Effect (%)	-2.9%	-3.9%	0.2%	12.7%	-8.2%	-7.8%
Panel C : Non-western citizens						
treat * post	0.00992	0.0256	-0.00379	0.00877	-1.006	-2.347
	(0.0112)	(0.0311)	(0.00701)	(0.00801)	(1.755)	(2.335)
Observations	470	470	470	470	470	470
R-squared	0.969	0.957	0.947	0.892	0.962	0.976
Baseline mean	11.80	11.08	0.574	0.416	56.25	86.81
Effect (%)	0.0%	2.6%	-0.7%	2.1%	-1.5%	-2.7%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
Standard errors clustered at the neighborhood level. All regressions control for neighborhood and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. Panel A) shows the effect for the entire neighborhood population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Outcomes are transformed such that each household takes the average outcome value observed within the household over the entire period. As such, there is no effect coming from changes within households and all of the effect captured is driven by composition.

Table A8: Balancing test: individual-level analysis

	(1)	(2)	(3)	(4)
	Treatment dummy			
	Without PSM weights		With PSM weights	
	coef	se	coef	se
Total income	-13,645***	(932.6)	-2,558***	(958.5)
Log total income	-0.0536***	(0.0060)	0.0009	(0.0057)
Wage income	-18,288***	(1,333)	-3,341**	(1,375)
Log wage income	-0.108***	(0.0155)	-0.0187	(0.0149)
Pr. not employed	0.0448***	(0.0044)	0.0084*	(0.0043)
Benefits	5,285***	(697.4)	1,327*	(700.1)
Log benefits	0.0489***	(0.0118)	0.0007	(0.0120)
Pr. crime	0.0026**	(0.0013)	0.0007	(0.0013)

The table shows the outcome of regressions of the treatment dummy on different individual-level characteristics. Each line is a separate regression. Columns (1) and (2) show the coefficient and standard error without reweighting with PSM weights, while Columns (3) and (4) use PSM weights.

Table A9: Individual effects in terms of ethnic inequality

	Total income		Wage income		Benefits	
	Baseline mean	Effect	Baseline mean	Effect	Baseline mean	Effect
Western citizens	255941	**-8266**	181778	**-12139**	69831	**4231**
Non-Western citizens	224747	**-3060**	127001	-689,8	87132	-421,4
ratio income NW/W	88%	90%	70%	74%	125%	117%
ratio income NW/W, only significant	88%	90%	70%	75%	125%	118%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table reports the baseline means in income across Western and non-Western citizens as well as the static level effects reported in Table 4. It then computes how the effect translates into changes in ethnic inequality within baseline "ghetto" residents.

Table A10: Effect of the "ghetto Plan" on individual labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Wage income determinants						
	Wage income if working	Non-empl prob.	Hourly wage	N. hours worked	Prob. firm change since 2009	Prob. sector change since 2009	Commuting distance
Panel A: All individuals							
Treat * post	-8,929***	0.00966*	-1.314	-22.95**	0.00370	0.000116	-511.6
	(1,924)	(0.00514)	(2.945)	(10.41)	(0.00796)	(0.00409)	(455.6)
Observations	110,685	177,490	87,698	92,175	90,358	90,358	80,865
R-squared	0.718	0.696	0.229	0.684	0.637	0.505	0.548
Baseline mean	229288	0.338	164.9	717	0	0	13520
Effect (%)	**-4%**	**3%**	-1%	**-3%**	-	-	-4%
Panel B: Western citizens							
Treat * post	-12,900***	0.0158**	-0.964	-36.30***	0.0173	-0.000405	460.0
	(2,594)	(0.00683)	(4.393)	(11.75)	(0.0113)	(0.00552)	(742.4)
Observations	66,076	92,830	52,424	67,331	66,573	66,573	47,656
R-squared	0.729	0.711	0.205	0.789	0.565	0.420	0.541
Baseline mean	248034	0.267	172.4	762.5	0	0	15158
Effect (%)	**-5%**	**6%**	-1%	**-5%**	-	-	3%
Panel C: Non-Western citizens							
Treat * post	-2,960	0.00234	-1.147	48.16***	-0.0148	-0.00188	-1,360***
	(2,898)	(0.00766)	(2.946)	(14.80)	(0.0116)	(0.00572)	(504.5)
Observations	44,609	84,660	35,274	48,890	47,831	47,831	33,209
R-squared	0.680	0.667	0.321	0.728	0.578	0.455	0.556
Baseline mean	210379	0.396	157.4	673.6	0	0	11932
Effect (%)	-1%	1%	0%	**7%**	-	-	**-11%**

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the individual level. All regressions control for individual and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. Panel A) shows the effect for the whole population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Baseline mean reports the mean value of the outcome for treated individuals prior to the "Ghetto Plan" and serves to compute the effect in terms of growth rate relative to baseline.

Table A11: Effect of the policy on individual probability of moving

	(1) Prob. Building change since 2009	(2) Prob. Parish change since 2009	(3) Prob municipality change since 2009
Panel A: All individuals			
Treat *post	-0.0223***	-0.0244***	-0.0338***
	(0.00665)	(0.00581)	(0.00521)
Observations	165,721	165,721	165,721
R-squared	0.761	0.757	0.775
Mean in controls	0.432	0.282	0.218
Effect (% relative to controls)	-5%	-9%	-16%
Panel B: Western citizens			
Treat *post	-0.00399	-0.00231	-0.0185**
	(0.0101)	(0.00921)	(0.00843)
Observations	86,306	86,306	86,306
R-squared	0.716	0.725	0.754
Mean in controls	0.496	0.343	0.271
Effect (% relative to controls)	-1%	-1%	-7%
Panel C: Non-Western citizens			
Treat *post	-0.0290***	-0.0305***	-0.0356***
	(0.00882)	(0.00719)	(0.00628)
Observations	79,415	79,415	79,415
R-squared	0.711	0.707	0.744
Mean in controls	0.355	0.209	0.154
Effect (% relative to controls)	-8%	-15%	-23%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. The table reports the effect of the reform on the probability of remaining in the same building, parish and municipality. By definition, all treated and controls stayed in the same place between 2007 and 2009, so the event study captures the differential probability of moving out from 2010 onward. The magnitude of the effect is thus compared to the average moving out probability observed within controls.

Table A12: Effect of the policy on Western labor market outcomes by moving status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Wage income (Western only)			Wage income if employed (Western only)			Non-employment rate (Western only)		
	stayers	movers 2010-12	movers 2013-15	stayers	movers 2010-12	movers 2013-15	stayers	movers 2010-12	movers 2013-15
Treat * post	-9,805***	-12,764***	-19,618***	-8,198**	-16,290***	-20,678***	0.0307***	0.00348	0.0124
	(3,414)	(4,575)	(6,167)	(3,542)	(4,732)	(6,711)	(0.0108)	(0.0112)	(0.0176)
Observations	38,520	32,620	14,120	25,875	24,375	10,393	38,520	32,620	14,120
R-squared	0.824	0.770	0.800	0.746	0.717	0.724	0.743	0.662	0.711
Baseline mean	173743	194665	177765	245101	249068	256196	0.291	0.218	0.306
Effect (%)	-6%	-7%	-11%	-3%	-7%	-8%	11%	2%	4%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the individual level. All regressions control for individual and year fixed effects, as specified in the static regression equation 3. Period of analysis: 2006 - 2015. The table reports the effect of the reform on exposed Western individuals by moving status: stayers, leavers between 2010 and 2012, and leavers between 2013 and 2015.

Table A13: Summary statistics by moving status

	Leavers 2010-12		Leavers 2013-15		Leavers 2016-18	
	Control	Treat	Control	Treat	Control	Treat
Panel A: Western citizens						
Total income	292473	266171	310009	285941	316951	277209
Wage income	222800	188359	235968	200578	239230	178538
Non employment rate	0.22	0.25	0.23	0.28	0.24	0.37
Wage income if working	287739	253162	305628	276690	314525	284650
N. hours worked in the year	1500	1430	1534	1515	1610	1465
Hourly wage	133	123	145	125	148	142
Share with low education	0.02	0.02	0.02	0.03	0.02	0.04
N. of children in 2009	0.83	0.66	0.80	0.81	0.90	0.90
Crime rate	0.02	0.03	0.03	0.02	0.02	0.04
Panel B: non-Western citizens						
Total income	237737	242783	247510	235110	258551	226524
Wage income	150431	158395	153173	129147	149623	117404
Non employment rate	0.35	0.30	0.36	0.42	0.38	0.46
Wage income if working	232965	228995	239742	228368	243383	220355
N. hours worked in the year	1231	1337	1306	1144	1279	1247
Hourly wage	106	99	107	113	118	101
Share with low education	0.56	0.55	0.55	0.67	0.56	0.62
N. of children in 2009	1.51	1.66	1.74	1.87	1.73	1.72
Crime rate	0.03	0.04	0.04	0.02	0.03	0.01

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.
The table presents the summary statistics on outcomes measured the year before moving for treated and controls leaving at different points in time.

Table A14: Willingness to pay analysis on neighborhood characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Building characteristics		Parish characteristics		Municipality characteristics	
	Ave. household income	Share of Non-Western	Ave. household income	Share of Non-Western	Ave. household income	Share of Non-Western
Panel A: All individuals						
Treat *post	-3,389*	-0.000466	-2,226***	0.0203***	-1,808***	0.0114***
	(2,007)	(0.00638)	(516.8)	(0.00265)	(365.2)	(0.00200)
Observations	67,550	67,550	70,530	70,530	71,461	71,461
R-squared	0.526	0.775	0.671	0.693	0.689	0.765
Mean in controls	120182	0.455	142517	0.181	149131	0.133
Effect (% relative to controls)	-3%	0%	-2%	11%	-1%	9%
Panel B: Western citizens						
Treat *post	-3,976	0.00300	-2,908***	0.0125***	-2,198***	0.00564**
	(2,581)	(0.00650)	(656.2)	(0.00276)	(486.7)	(0.00239)
Observations	41,490	41,490	43,283	43,283	43,782	43,782
R-squared	0.523	0.787	0.684	0.750	0.700	0.790
Mean in controls	123506	0.356	141833	0.172	148512	0.124
Effect (% relative to controls)	-3%	1%	-2%	7%	-1%	5%
Panel C: Non-Western citizens						
Treat *post	-1,559	0.00636	-559.8	0.0266***	-1,126**	0.0149***
	(3,201)	(0.0121)	(837.3)	(0.00495)	(569.4)	(0.00333)
Observations	26,060	26,060	27,247	27,247	27,679	27,679
R-squared	0.522	0.711	0.658	0.646	0.675	0.749
Mean in controls	115766	0.586	143430	0.193	149957	0.144
Effect (% relative to controls)	-1%	1%	0%	14%	-1%	10%

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Standard errors clustered at the neighborhood level. All regressions control for individual and year fixed effects, as specified in the static regression equation 3. Panel A) shows the effect for the whole population while Panel B) and C) distinguish between Westerners (including Danes and Western migrants), and Non-Western Migrants. Here the event is the move out of the neighborhood and not the introduction of the policy. Individuals are followed from 4 years prior to the move to 3 years after. Baseline mean reports the mean value of the outcome for treated neighborhood prior to moving and serves to compute the effect in terms of growth rate relative to baseline. In addition to other controls, we also add deciles of initial neighborhood characteristics interacted with year fixed effects, to compare treated and control individuals leaving from comparable origin characteristics.

Online Appendix for

The Making of a Ghetto

Place-Based Policies, Labeling, and Impacts on Neighborhoods and Individuals

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Outline

- Appendix B presents additional results on moving decisions.
- Appendix C presents the context and the reform in more details.

B Mobility responses

In Figure A6 we have shown that on the neighborhood level flow number and rates are not affected by treatment status. Point estimates for number and rates of outflows and inflows are very similar across our period of analysis. This is almost mechanical since the supply of public housing and limited and waiting lists are long. We also looked at the in- and outflow of Western and non-Western individuals separately. Treatment effects are also insignificant and confidence intervals for in- and outflows also overlap. At the neighborhood level these numbers and rates would only change if neighborhood sizes changed significantly (which did not happen) or the composition of in- and outflows with respect to origin changed fundamentally.

In this section, we use the individual level data to understand mobility responses as a potential mechanism behind our main findings. We define treated and control individuals based on their residence in 2007-2009, conditioning on having lived in the same neighborhood over this period, and we retain only neighborhoods that are inside the base of common support obtained at the area level (equation 1). Further, we construct regression weights based on the propensity score equation 5.

B.1 Static effects

Figure B1 gives a descriptive overview. It plots the shares of all individuals in our sample who do not move, move within public housing, leave public housing or leave Denmark by origin (Western vs. non-Western) and treatment status. The general pattern is that non-Westerners are less mobile and most moves out are out of public housing but within Denmark. These overall patterns are similar for the treated and never treated populations. From treated neighborhoods, slightly more than 60% of Western inhabitants and around 40% of non-Westerners leave their 2009 address.

Figure B1: Moves in public housing, out of public housing, out of Denmark

Note: Moving out is defined as having a different address than the 2009 address at any point between 2010-2018.

Even though the overall patterns in Figure B1 appear very similar, we run a series of regressions to directly test whether individuals move more frequently under treatment and whether some sub-populations react more strongly than others. We are mostly interested whether people who fulfill the classification criteria on an individual level move more frequently.

$$
\begin{aligned}
move^{2010-2018} = \alpha + \beta_0\ treated_i^{2010} + \beta_1\ western_i + \beta_2\ inactive_i^{2009} + \beta_3\ convict^{2009} \\
+ \beta_4\ log(income)^{2009} + \beta_5\ child(ren)^{2009} + \varepsilon_i
\end{aligned} \tag{6}
$$

where we will use different dependent variables indicating moving decisions between 2010-2018 to also test how characteristics and treatment are associated with the decision to leave Denmark, the public housing sector, or the municipality.

$$
\begin{aligned}
move^{2010-2018} = {} & \alpha + \beta_0\ treated_i^{2010} + \beta_1\ western_i + \beta_2\ inactive_i^{2009} + \beta_3\ convict^{2009} \\
& + \beta_4\ log(income)^{2009} + \beta_5\ child(ren)^{2009} \\
& + \gamma\ treated_i^{2010} \times \left(western_i + inactive_i^{2009} + convict_i^{2009} + log(income)^{2009} + child(ren)\right) + \varepsilon_i
\end{aligned} \tag{7}
$$

where γ is a vector of coefficient for all the interaction terms of the treatment indicator with 2009 characteristics.

Overall, the regressions confirm the pattern in Figure B1. Westerners are more likely to move than non-Westerners. Overall moving frequencies are not significantly different the treated and the not treated population. We estimate a small negative treatment effect on the probability to move (-2 pp, see column (1) of Table B1). This is significant at the 5% level only and entirely driven by Westerners being significantly more likely to move if treated (+ 7 pp, see column (2) of Table B1. It follows that non-Westerners are 7 pp less likely to move under treatment. The overall treatment effects for leaving Denmark, leaving the public housing sector and leaving the municipality are insignificant. We do, however, estimate that Westerners are significantly more likely to leave the public housing sectors under treatment (+6 pp, see column (6) of Table B1) and inactive people are slightly more likely to leave the municipality under treatment (+ 3 pp, see column (8) of Table B1). Note also that Westerners are less likely to leave Denmark, but more likely to leave the public housing sector and their municipality. This could indicate that non-Westerners are somewhat constrained in their mobility by factors such as discrimination in the private housing market.

We run these regressions both on individual- and 2009 households-level since moving decisions are often made at the household level. For the regressions on the household level we re-construct regression weights based on total household income following equation 5. The coefficients are in Table B2 and the conclusions are the same as for the individual level results: there is a small negative treatment effect on leaving the 2009 address, Western households react more strongly to treatment in their moving decisions and decide to leave the public housing sector.

The shares of people moving are significant shares of our sample. Moreover, we observe that Westerners react differently in their mobility than non-Westerners on average. It is, therefore, relevant to analyze in more detail what happens around the time of moving, whether these dynamics are different for treated vs. not-treated individuals, and discuss mobility as an important mechanisms behind the "Ghetto Plan"

Table B1: Probability of moving (2010-2018) on pre-treatment characteristics

	Anywhere		Leave Denmark		Leave public housing		Leave municipality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.019*	-0.027	0.003	0.017	-0.015	-0.027	0.001	-0.020
	(0.009)	(0.150)	(0.004)	(0.134)	(0.009)	(0.150)	(0.007)	(0.145)
Western	0.159***	0.122***	-0.015**	-0.012*	0.171***	0.141***	0.082***	0.068***
	(0.009)	(0.009)	(0.005)	(0.005)	(0.009)	(0.009)	(0.008)	(0.008)
Inactive	-0.062***	-0.066***	0.004	0.005	-0.074***	-0.078***	-0.014	-0.030***
	(0.009)	(0.009)	(0.005)	(0.006)	(0.009)	(0.009)	(0.008)	(0.008)
Convict	0.088**	0.105***	0.070**	0.062**	0.095**	0.093**	0.101***	0.079**
	(0.030)	(0.031)	(0.022)	(0.023)	(0.030)	(0.032)	(0.030)	(0.030)
log(Income)	-0.013*	-0.011	-0.046***	-0.046***	-0.018**	-0.017*	-0.014*	-0.013*
	(0.006)	(0.007)	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)	(0.006)
Child(ren)	-0.056***	-0.057***	-0.040***	-0.030***	-0.064***	-0.066***	-0.071***	-0.073***
	(0.009)	(0.009)	(0.005)	(0.005)	(0.009)	(0.009)	(0.008)	(0.008)
Treated × Western		0.074***		-0.007		0.059**		0.026
		(0.019)		(0.010)		(0.019)		(0.017)
Treated × Inactive		0.005		-0.001		0.006		0.031*
		(0.018)		(0.011)		(0.018)		(0.016)
Treated × Convict		-0.032		0.014		0.002		0.041
		(0.057)		(0.042)		(0.057)		(0.057)
Treated × log(Income)		-0.003		-0.000		-0.002		-0.001
		(0.012)		(0.011)		(0.012)		(0.012)
Treated × Child(ren)		0.004		-0.020*		0.004		0.004
		(0.019)		(0.010)		(0.019)		(0.016)
R^2	0.042	0.044	0.027	0.028	0.050	0.051	0.025	0.025
Observations	17909	17909	17909	17909	17909	17909	17909	17909

Note: Estimated following equation 6 (odd column numbers), and equation 7 (even column numbers). Dependent variables are indicators for different moving decisions at any point between 2010 and 2018 relative to people's 2009 location: ever moving (columns (1)-(2)), leaving Denmark (columns (3)-(4)), leaving the public housing sector (columns (5) - (6)), and for ever moving away from one's 2009 municipality (columns (7)-(8)). Standard errors in parentheses, weights based on propensity score estimated using income percentiles. * p<0.05, ** p<0.01, *** p<0.001.

overall ineffectiveness.

Table B2: Probability of moving (2010-2018) on pre-treatment characteristics – household level

	Anywhere		Leave Denmark		Leave public housing		Leave municipality	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treated	-0.018*	-0.076*	0.005	0.012	-0.013	-0.056	0.003	-0.022
	(0.009)	(0.034)	(0.005)	(0.021)	(0.009)	(0.034)	(0.008)	(0.030)
Share Western	0.135***	0.097***	-0.032***	-0.032***	0.152***	0.120***	0.066***	0.056***
	(0.010)	(0.010)	(0.006)	(0.006)	(0.010)	(0.010)	(0.009)	(0.009)
Share inactive	-0.065***	-0.074***	0.025***	0.028***	-0.073***	-0.081***	-0.019*	-0.035***
	(0.010)	(0.010)	(0.006)	(0.006)	(0.010)	(0.010)	(0.009)	(0.009)
Share convicted	0.083**	0.104**	0.081***	0.073**	0.087**	0.094**	0.083**	0.064*
	(0.032)	(0.033)	(0.024)	(0.026)	(0.032)	(0.034)	(0.031)	(0.032)
log(Income)	-0.003***	-0.003***	-0.003***	-0.003***	-0.002*	-0.002*	-0.004***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
Child(ren)	-0.052***	-0.049***	-0.039***	-0.033***	-0.064***	-0.065***	-0.065***	-0.068***
	(0.011)	(0.011)	(0.006)	(0.005)	(0.011)	(0.011)	(0.009)	(0.009)
Treated × Share Western		0.074***		0.000		0.062**		0.020
		(0.021)		(0.012)		(0.021)		(0.018)
Treated × Share inactive		0.016		-0.006		0.014		0.032
		(0.020)		(0.011)		(0.020)		(0.018)
Treated × Share convicted		-0.040		0.015		-0.016		0.035
		(0.060)		(0.047)		(0.061)		(0.059)
Treated × log(Income)		0.001		-0.000		0.000		-0.000
		(0.002)		(0.001)		(0.002)		(0.001)
Treated × Child(ren)		-0.006		-0.012		0.002		0.006
		(0.022)		(0.011)		(0.022)		(0.019)
R^2	0.038	0.040	0.020	0.020	0.047	0.048	0.025	0.025
Observations	14470	14470	14470	14470	14470	14470	14470	14470

Note: Estimated following equation 6 (odd column numbers), and equation 7 (even column numbers). Data aggregated to the household level using 2009 household identifiers. Dependent variables are indicators for different moving decisions at any point between 2010 and 2018 relative to people's 2009 location: ever moving (columns (1)-(2)), leaving Denmark (columns (3)-(4)), leaving the public housing sector (columns (5) - (6)), and for ever moving away from one's 2009 municipality (columns (7)-(8)). Standard errors in parentheses, weights based on propensity score estimated using income percentiles. * p<0.05, ** p<0.01, *** p<0.001.

B.2 Dynamic Effects

Figure B2: Shares of movers over time by treatment status

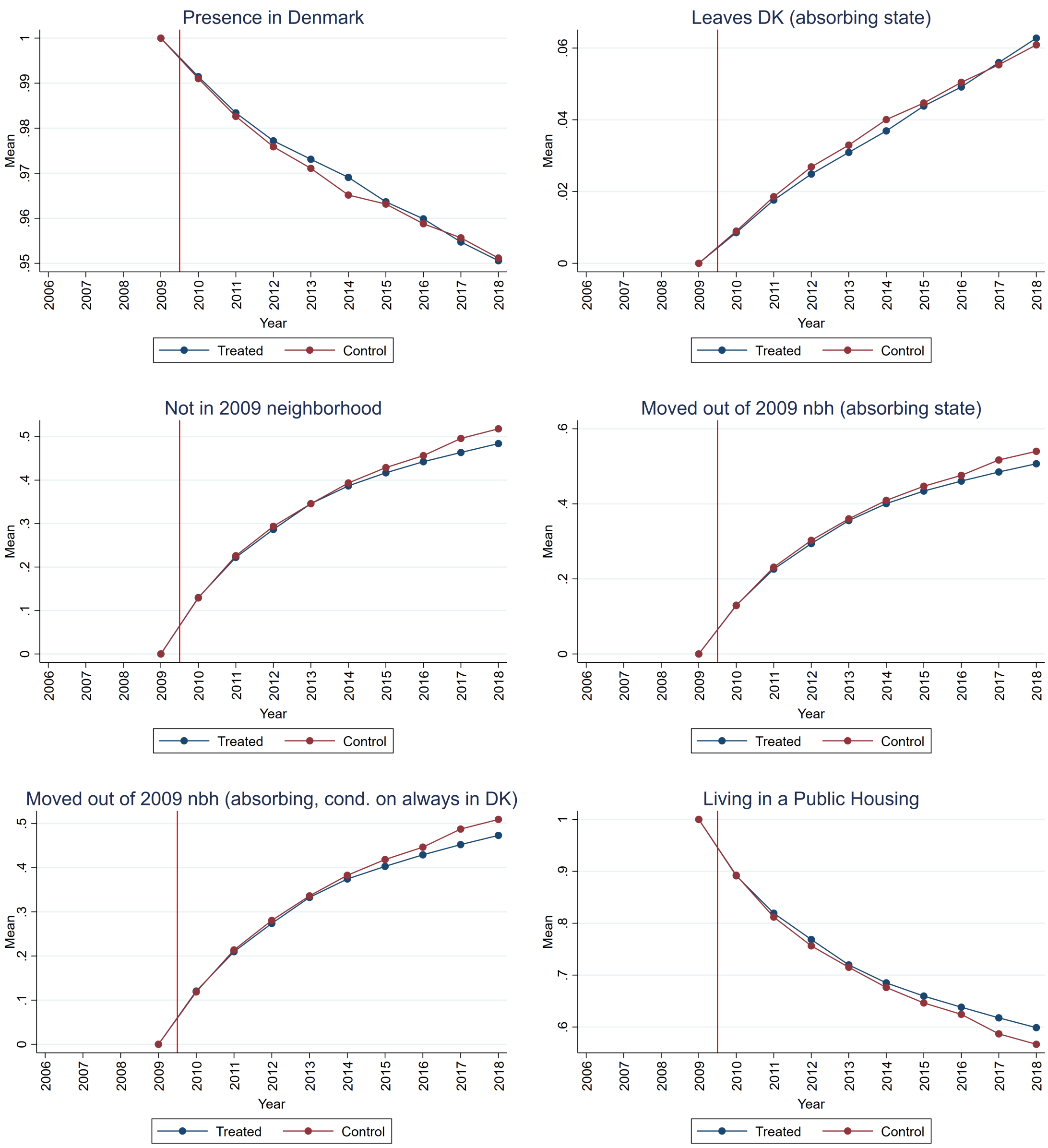


Figure B3: Treatment effects on moving decisions

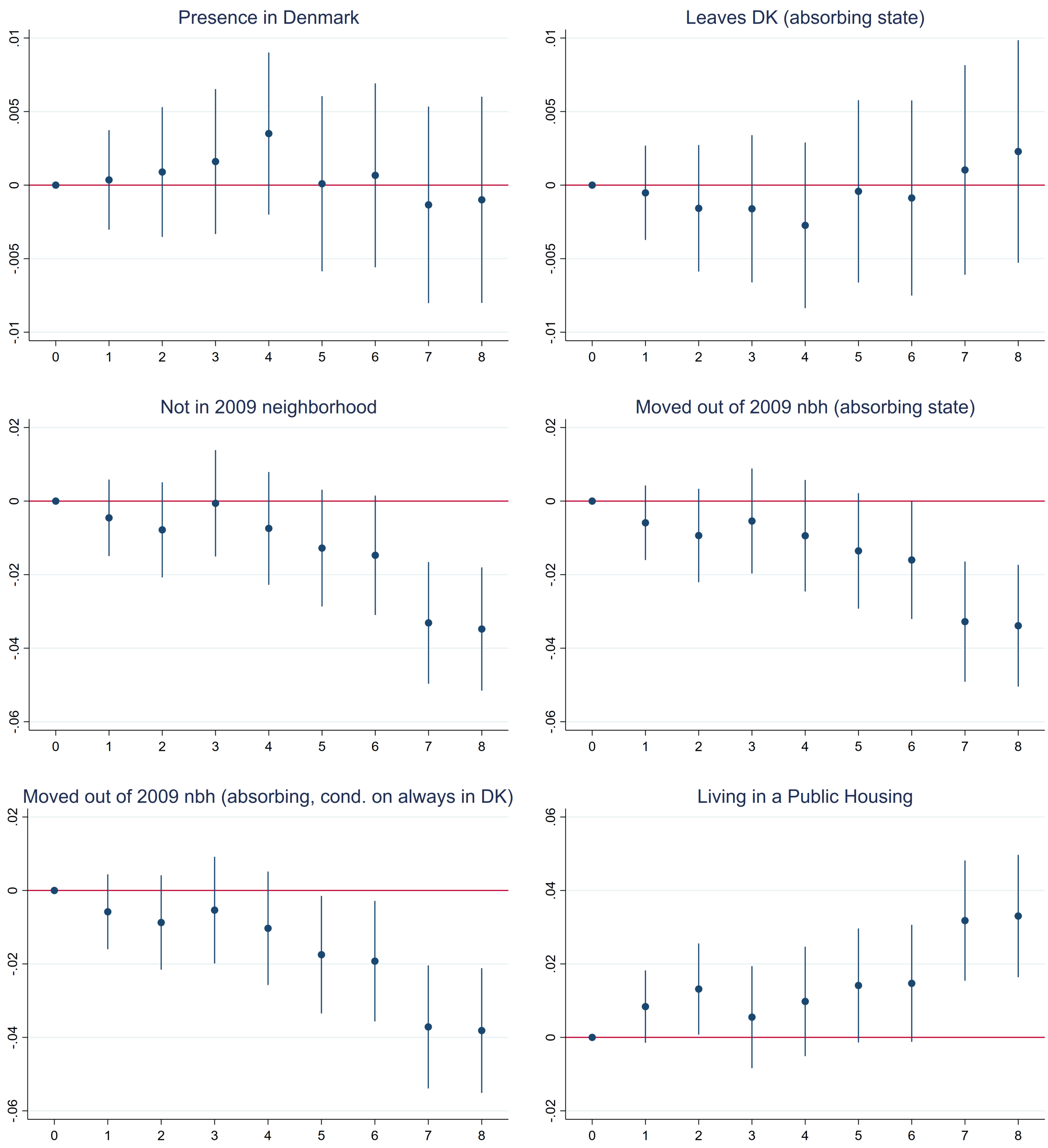


Figure B4: Treatment effects on moving decisions by nationality

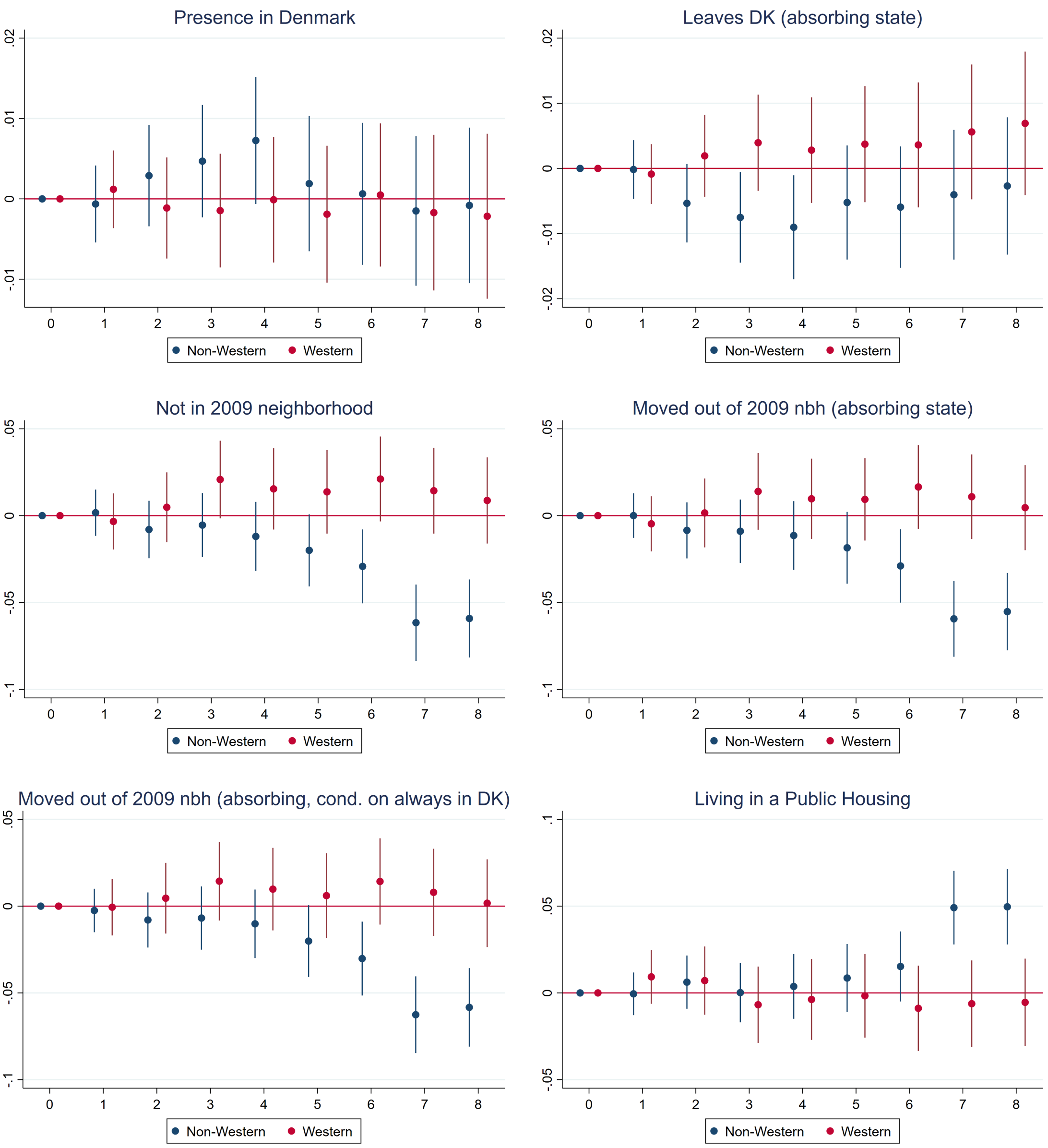


Figure B5: Treatment effects on moving decisions by child in 2009

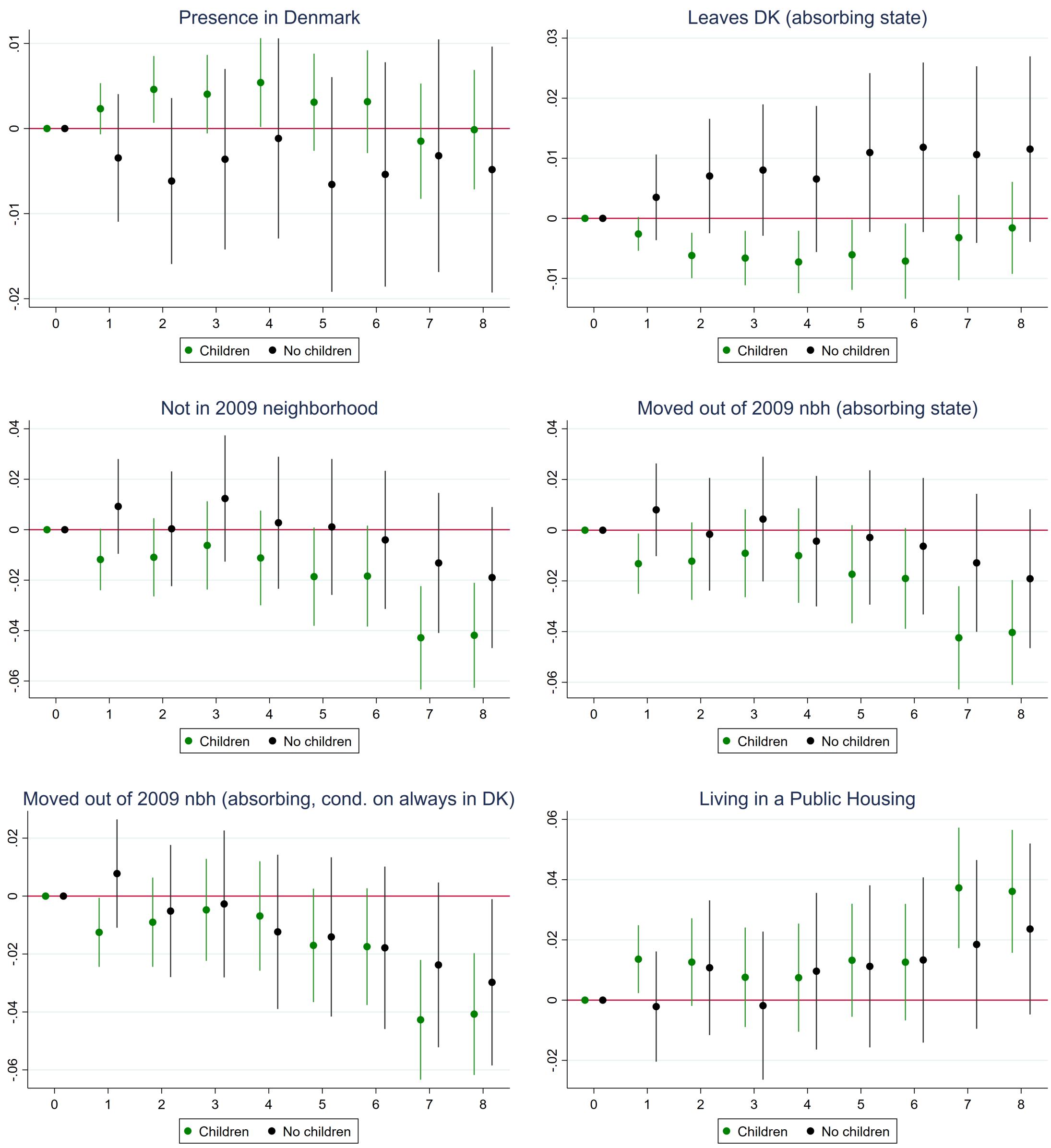
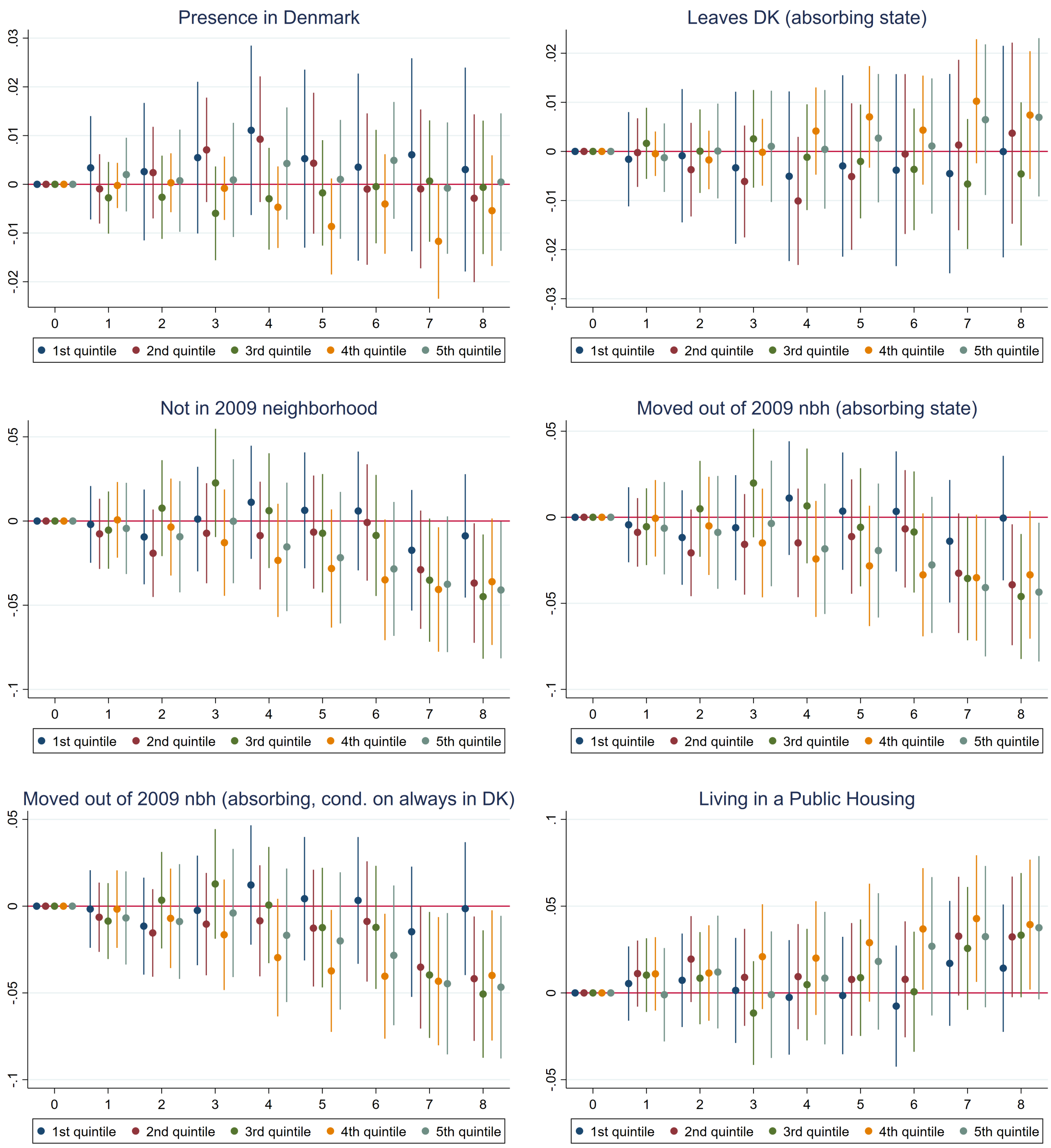


Figure B6: Treatment effects on moving decisions by income quintile



C Context Appendix

Under prime minister Lars Løkke Rasmussen (liberal party) the first “ghetto” list was introduced in 2010 (The Danish Government, 2010). The underlying strategy paper announced five main areas of intervention: (1) more attractive neighborhoods, (2) balance the resident composition, (3) efforts for children and young people, (4) lower dependency on public benefits, and (5) fight crime.[37]

(1) More attractive neighborhoods The 2010 "Ghetto Plan" proposed making neighborhoods more attractive by strategically demolishing certain housing blocks to create space for new housing types, traffic connections, or commercial areas, and improving infrastructure to better connect “ghettos” with the rest of the city. It also included plans for significant investments in renovating outdated housing to make them more appealing and continuing social housing initiatives to improve living conditions and community services. These plans were followed by a specific budget for such initiatives under control of the Landsbyggefonden (National Building Fond, for public and social housing). However, to get that money and to implement reconstructions or renovations, project proposals had to planned by housing associations and municipalities, including other stake-holders as well. Then, these project proposals had to improved so that it took several years before any of these projects were actually started.

(2) Balance in the resident composition To achieve a more “balanced” resident composition, the policy suggested allowing municipalities to prioritize resourceful residents for housing in "ghetto" areas and preventing new refugees and non-EU residents from being allocated housing in these areas. This was actually put into law, see Law on Social Housing § 59. It also encouraged the sale of public housing to create mixed ownership and attract more resourceful residents and proposed making it easier to evict tenants who violated house rules. Already since 2000, municipalities and housing associations have been allowed to select and reject candidates from waiting lists to control neighborhood composition in areas with high unemployment. The idea was that neighborhoods “can be improved by attracting more resourceful” inhabitants (The Danish Government (2004), p. 22).[38] This practice of *flexible letting* was tightened in 2010 with the introduction of the first official “ghetto list”. Specifically, as of January 2011, vacant public housing apartments that are under municipal control and located in a "ghetto" area are not allowed to be offered to households if at least one member:[39]

– has been convicted of a criminal offense and was released from prison or probation services within the last 6 months,

– has had its tenancy terminated as a result of gross violations of good manners or order within the past 6 months,

– is not a citizen of a country that is a member of the EU, with the exception of students who are enrolled in a publicly recognized educational establishment,

– receives disability pension, or have for 6 consecutive calendar months received unemployment benefit, or sickness benefit.

[37]The 2010 strategy paper was pre-dated by a 2004 strategy paper The Danish Government (2004), which aimed to improve integration and reduce “ghettoization”. It mentions a variety of initiatives and tools: flexible letting (bypassing waiting lists for public housing, a practice in place since 2000), promoting private investments, supporting entrepreneurship, enhancing education and youth programs, crime prevention, volunteer work, public-private partnerships, and targeted urban renewal. However, it mainly outlined plans without actual legislative changes and lacked clear criteria to identify “ghettos”.

[38]Applicants who do not already live in the respective public housing area and who have been dependent on benefits for six consecutive months could be rejected. See §51 b in the Law of Public Housing (*lov om almene boliger*).

[39]See § 59, stk 6 of the Law of Public Housing (*lov om almene boliger*), LBK nr. 103 from February 11, 2011.

In order to be able to selectively fill vacancies, moving out subsidies were implemented and movers out of disadvantaged areas were offered to jump waiting lists in other public housing. Anecdotal evidence supports that the moving-out subsidy as well as the offer to jump waiting lists did not find the expected demand. The housing association Bo Vita, for example, administers public housing areas in Copenhagen.[40] One is Mjølnerparken in Nørrebro which has been listed on every list since the "Ghetto Plan" got introduced. Another one is in Christianhavn which is much closer to the city center and has never been listed. The contact person for flats in Christianshavn said in an interview that they thought there would be strong demand for their flats when offered to people in Mjølnerparken. This turned out not to be the case. He said many people are actually happy to live in Mjølnerparken, mainly because of their social networks.[41] In another interview with someone who made the decision to leave Mjølnerparken, the primary reason for moving was highlighted as the uncertainty surrounding the conditions for their continued residence and livelihood under the "Ghetto Plan". It was crucial for them to find alternative housing in close proximity since that was the person's childhood neighborhood and where their family resides.[42]

(3) Efforts for children and young people The policy aimed to support children and young people by requiring children with language difficulties to attend daycare, strengthening measures to ensure parents fulfill their responsibilities, and allowing the creation of non-geographical school districts to balance student composition. It also proposed establishing full-day schools in or near "ghetto" areas to provide extended learning opportunities. Most measures were not concretized or formally implemented until later years.

(4) Lower dependency on public benefits To reduce dependency on public benefits, the policy included establishing job centers in "ghetto" areas to help residents find employment, implementing stricter consequences for those who do not comply with job search or education plans, and strengthening the rules requiring couples on welfare to work a minimum number of hours to continue receiving benefits. Additionally, it provided targeted support to help young people transition from welfare to education or employment. However, the enforcement of these initiatives varied and was not legally enacted.

(5) Fight crime The policy paper proposed increasing police visibility and presence in "ghetto" areas, implementing fast-track procedures for handling cases involving young offenders, and intensifying efforts to combat social benefit fraud and illegal work. It also aimed to strengthen preventive measures, including better lighting, CCTV, and community policing. However, this was not systematically put in place in "ghetto" areas.

The exact classification criteria were put into law with a change to § 61 of the law on public housing (lov om almene boliger) effective on Dec. 17, 2010 (see lovforslag nr L60, Folketinget 2010-11). An area had to fulfill 2 out of 3 criteria to be listed as a "ghetto":

1. the share of residents who are immigrants or immigrant descendants from non-Western countries is higher than 50%.[43]

[40] See online: `https://bo-vita.dk/` - last accessed January 2023.

[41] See Arte documentary *Denmark's Immigration Hardline - Re: Ghetto Laws* from November 11, 2021, online at `https://www.arte.tv/en/videos/100300-027-A/denmark-s-immigration-hardline/` - last accessed January 2023.

[42] See interview in DR from December 1, 2020: *Antallet af ghettoer er næsten halveret: 'Hvis jeg ikke var nødt til at flytte, så havde jeg ikke gjort det'* (English: The number of "ghettos" nearly halved: 'If I did not have to move, I would not have done it'), online at `https://www.dr.dk/nyheder/indland/antallet-af-ghettoer-er-naesten-halveret-hvis-jeg-ikke-var-noedt-til-flytte-saa` - last accessed January 2023.

[43] Table C2 lists the countries that are considered Western. Non-Western immigrants and their descendants are defined as individuals whose neither parent is both a Danish citizen and born in Denmark.

2. the share of residents aged 18-64 without employment and not in education is higher than 40%, calculated as an average over the previous 4 years.

3. The number of residents convicted of violating the Criminal Code, Weapons Act, or the law on euphoriant substances per 10,000 residents aged 18 years and older exceeds 270 people, calculated as an average over the previous 4 years.

Prior assessments of Denmark's "Ghetto Plan" highlight negative impacts on mental health (Gulis et al., 2020)[44], social citizenship (Seemann, 2020), and territorial stigma (Stender and Mechlenborg, 2022). The latter links media portrayals to residents' experiences, with one describing it as "almost like having a virus" when revealing they live in Mjølnerparken. Overall, Stender and Mechlenborg (2022) find that while some residents actively resist the stigma, others distance themselves from it.

Christensen et al. (2021) evaluate the effectiveness of social housing initiatives funded by the National Building Fund from 2011–2014, aimed at improving conditions for youth, employment, and community engagement. One example is the *leisure job initiative* (fritidsjobindsatsen), where children earned pocket money from the housing association by helping with tasks like childcare during parent programs, cleaning, and distributing materials. While overall effects were limited, the initiatives showed promise in increasing youth employment. However, there were no significant impacts on school absenteeism, crime, or inactivity, though some signs suggest a narrowing education gap with respect to the overall population. Consistent with our findings, Christensen et al. (2021) also note a larger employment gaps between Western residents inside versus outside "ghetto" areas, compared to non-Western residents. Christensen et al. (2019) follow up and evaluate the 2015–2018 social housing initiatives, noting generally high–but below average—-levels of safety and well-being in disadvantaged areas, largely due to greater social vulnerability. Comparisons between "ghetto" areas and other disadvantaged areas show no consistent differences. Individual factors, more than area characteristics, seem to explain variations in safety and well-being, highlighting the need to prioritize resident-focused interventions.

In addition, the Ministry of Interior and Housing publishes yearly reports describing the year-to-year developments of classification, policy implications, initiatives, and socio-economic characterization of inhabitants.[45] The evaluation of the 2018 status showed, for example, that over the eight years that the "Ghetto Plan" has been in place there was a decrease of 2 pp in the share of children not enrolled in day-care, a decrease of 2 pp in the share of young (15-29) inactive and a decrease of 1 pp in the share of young convicted criminals for the same age group. These developments are in line with the policy intentions, while the slight increase in neighborhoods with over 25% of people of non-Western origin was not (Indenrigsministeriet, 2019).[46] This analysis, however, purely describes the evolution of the characteristics within targeted areas and lacks a counterfactual. As such, these trends cannot be interpreted as the causal effect of the policy.

Iversen et al. (2019) identify vulnerable neighborhoods based on socio-economic characteristics (i.e. income, labor force participation, education, crime, deliberately excluding origin) for the period 1986-2017. Their definition overlaps with the government's classification since 2010. They find that individuals with lower than neighborhood average socio-economic status move into vulnerable areas, while individuals who move out tend to do better subsequently. Using quasi-random allocation of refugees in Denmark,

[44] See also Chen et al. (2023).

[45] The reports can be accessed on the Indenrigs- og Boligministeriet's (*Ministry for Interior and Housing*) website `www.im.dk`, they are called 'Redegørelse om Parallelsamfund' (*Report on Parallel Society*).

[46] See Table 1.2 [p. 26] in the report, in Danish.

they show that being located in a vulnerable neighborhood worsens income prospects but does not have an effect on crime. Their analysis is a general assessment of the development of vulnerable areas in some parts of Denmark. It does not speak to nor evaluate the policies that the Danish government adopted since 2010.

C.1 The "Ghetto Plan" after 2010

In 2011, Helle Thorning-Schmidt from the social democratic party became prime minister. Under her government, two additional classification criteria were introduced: share without occupational education and share with income smaller than the regional average. To be considered a “ghetto”, an area now had to meet three of the five. The intention was to give less weight to the non-Western criterion.

In 2015, Lars Løkke Rasmussen got re-elected and implemented the major revisions to his “Ghetto Plan” – effective in 2018. Two new area types were added: vulnerable area and hard "ghetto". Neighborhoods that did not have a high share of non-Westerners but met two of the other four criteria were vulnerable areas, vulnerable areas with a high share of non-Westerners were “ghettos”. Neighborhoods that have been a “ghetto” for four consecutive periods become “hard ghettos”. Together with these additional area types, came additional policy tools. Since 2018, there is a mandatory day-care requirement for children in classified neighborhoods. Additionally, children have to take language tests, a budget of 10 billion DKK for demolitions and transformations was introduced, and the police could declare treated neighborhoods as “enhanced penalty zones”. Crimes committed in “enhanced penalty zones” could be punished with a double sentence.[47] Non-compliance with the requirements to enroll children in day-care or take regular language tests can lead to the loss of social benefits.

In 2019 government power shifted again to the social democratic party and the government under Mette Frederiksen re-branded the “Ghetto List” as “Parallel Society List”. However, the term “ghetto” remains in the public perception and discussion of the policy.

Christensen et al. (2022) conduct a thorough evaluation of the post-2018 measures when the physical reconstructions and building renovations became more relevant and salient. But they also provide a more general discussion of how territorial stigma has developed and been reflected in Danish media since the policy’s inception in 2010.

[47] These “enhanced penalty zones” are not new. Previously, they have been used around e.g. stadiums in case of games with high potential for conflict and violence. It is new that they could be applied in any neighborhood that fulfills the classification criteria at any time.

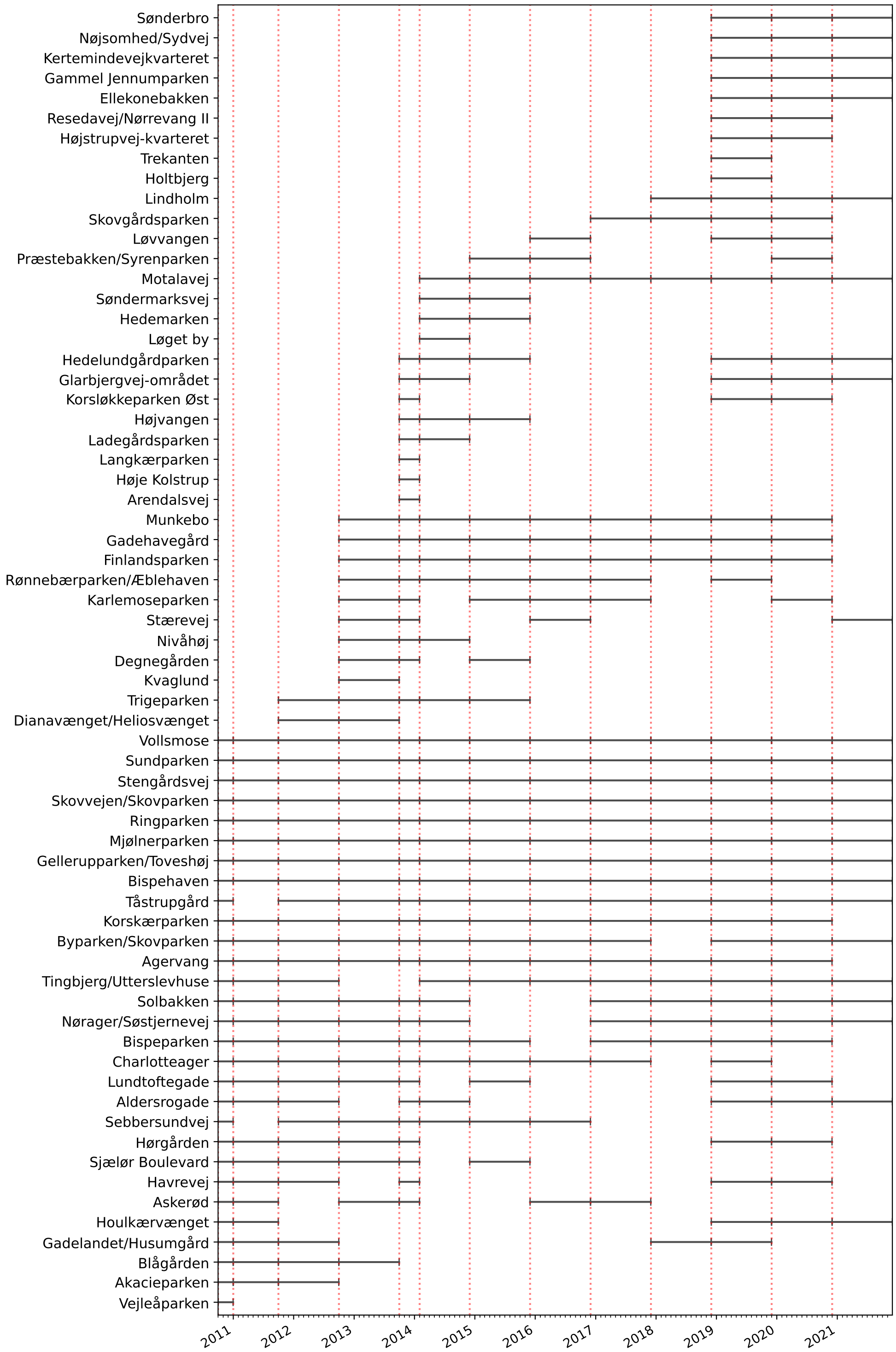
Table C1: Overview over "Ghetto" Plan: Classification criteria and categories

Criterion	**2010** *Introduce classification criteria*	**2013** *Add 2 new criteria*	**2018** *Change criteria, add two more area types*
(1) Non-western origin	> 50%	> 50%	> 50%
(2) Unemployment	> 40%	> 40%	> 40%
(3) Convicts	> 2.7% (4-year avg.)	> 2.7% (4-year avg.)	> 3× country avg. over past 2 yrs
(4) Education		> 60% w/o occupational education	> 60% no more than primary
(5) Income		< 60% of reg. avg.	< 55% of reg. avg.
Classification Rules Applied to all public housing residential areas with at least 1,000 residents.			
Vulnerable area			2 of (2)-(5)
"Ghetto"	2 of (1)-(3)	3 of (1)-(5)	2 of (2)-(5) and (1)
Hard "ghetto"			"Ghetto" for 4 consecutive yrs.

Table C2: List of countries considered Western

EU countries
Finland, Luxembourg, Sweden, Belgium, Bulgaria, Denmark, France, Greece, Netherlands, Ireland, Italy, Malta, Poland, Portugal, Romania, Spain, Hungary, Germany, Austria, Cyprus, Estonia, Latvia, Lithuania, Croatia (since 2013), Slovenia, Czech Republic, Slovakia, Czechoslovakia
European countries that are not members of the EU
Iceland, Liechtenstein, Monaco, Norway, Andorra, San Marino, Switzerland, Vatican, UK, Ukraine (since 2022)
Non-European countries
Canada, USA, Australia, New Zealand

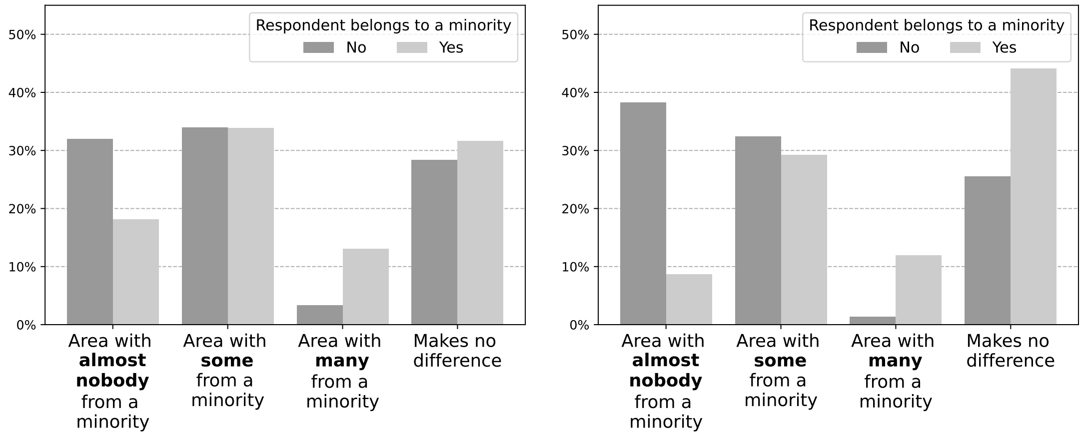
Figure C1: Treatment spells of public housing areas classified in 2010



Note: Plotted are classifications for the years 2010-2020. Vertical lines in red mark the dates when a new list got published. Areas on the vertical axis are sorted by the date they have been first listed and total number of times listed. The last classification considered in this analysis is the one published in 2018.

Figure C2: Which type of area would you ideally wish to live in? (% of respondents)

(a) European countries **(b)** Denmark



Note: Data from the first wave of the European Social Survey (ESS (2002), variable was called "idetalv". The exact wording of the question was: *Suppose you were choosing where to live. Which of the three types of area would you ideally wish to live in?* The answer options where *1 - An area where almost nobody was of a different race or ethnic group from most people*, *2 - Some people were of a different race or ethnic group from most people*, *3 - Many people were of a different race or ethnic group*, and *4 - It would make no difference*. The shares are computed among the people who identify as belonging to a minority or not (variable called "blgetmg"). The sum of shares among each sub-population is different from 100% due to missing replies. The left panel uses data for all countries in the sample, and the right panel is for Denmark only, which is the context of this study. The data are weighted.