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Yajna Govind
Copenhagen Business School

Jack Melbourne
Bocconi University

Sara Signorelli
CREST, Ecole Polytechnique and IZA

Edith Zink
University of Copenhagen

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

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

Does the labeling of neighborhoods affect their outcomes? Place-based 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 which designated public housing areas with a large share of immigrant population, high crime, and high unemployment as "ghettos", with minimal additional implications. We exploit Danish administrative data, and adopt a Difference-in-Differences approach at the neighborhood and individual level. We find that the policy led to worsening average characteristics at the neighborhood level, largely due to compositional changes driven by Danish new entrants with lower income and education levels. Following individuals affected by the policy, we find significant negative effects on their income with no discernible effects on criminal behavior. We estimate that Danes are willing to accept a 3% drop in income to move out of labeled neighborhoods. In all, the Danish ghetto policy was largely ineffective, and in some aspects, even detrimental.

JEL Classification: J15, J18, R23

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

Corresponding author:
Sara Signorelli
CREST
5 Avenue Henry Le Chatelier
91120 Palaiseau
France
E-mail: sara.signorelli@polytechnique.edu

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1 Introduction

Socio-economic inequality across residential areas is a common characteristic of most cities, with certain zones concentrating the most disadvantaged population and suffering from high crime rates. Policies aimed at reducing this type of spatial inequality and segregation face the challenge of having to *name* the issue which inevitably leads to labeling and stigmatization. It has been shown that such labels can annul otherwise potentially positive policy effects as they lead to behavioral responses (Besbris et al., 2015; Davezies and Garrouste, 2020). For residential areas, a negative label or stigma has been shown to reduce neighborhood attractiveness, often proxied by housing prices (Tootell, 1996; Aaronson et al., 2021; Hynsjo and Perdoni, 2023; Koster and van Ommeren, 2019; Andersson et al., 2023). Understanding the consequences of such labeling is crucial, but isolating its causal effects on individuals from changes in neighborhood demographics is often challenging.

To examine how labeling might hinder the intended improvements—both in neighborhood composition and individual outcomes—we analyze a place-based policy in Denmark that designated certain neighborhoods as "ghettos", with minimal additional implications. It was introduced in 2010 and intended to increase residential mixing, as a "fight against parallel societies" where "Danish values are not firmly rooted" (The Danish Government, 2010).[1] Public housing areas with at least 1,000 residents and with a high share of inhabitants not in education nor in employment, high crime rate and a high share of non-Western descendants were classified as "ghettos". Besides this classification, the only policy instruments imposed in the initial years of Denmark's "ghetto plan" were some additional police presence to reduce crime and moving restrictions that gave housing associations the autonomy to select new residents from the waiting lists while subsidizing certain individuals to move out. This neighborhood engineering via moving-in restrictions was designed to positively select new movers-in and thus improve the socioeconomic characteristics of the residents. While improving the socioeconomic composition of new residents may generate positive spill-overs on incumbents,[2] if the stigmatizing effect of the label is strong, the effect of the policy might actually go in the opposite direction.

We employ Denmark's rich social register data that provides us with information about individuals' addresses, moving decisions, income, education, country of origin and criminal activity. Using a difference-in-differences strategy coupled with propensity score matching on baseline characteristics, we estimate the effect of being classified as a ghetto by comparing classified neighborhoods to similar but never clas-

[1] 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.

[2] There is a vast literature, mostly focused on the US, describing how neighborhood characteristics impact individual trajectories and chances of success (Chetty et al., 2016; Chetty and Hendren, 2018a,b).

sified areas within the public housing sector. We only keep neighborhoods that are within the common support of the propensity score distribution, which leads to a strong balance of pre-treatment characteristics and common pre-trends. First, we focus on the effect of the policy at the area level, disentangling the effect on incumbents from changes in neighborhood composition. In a second step, we focus on the effect on exposed individuals, defined as the "ghetto" residents at the time of the policy introduction, which we follow wherever they move after.

Overall, we find that the policy has been mostly ineffective, and in some aspects, even detrimental. In fact, we observe a decline in the average education and income levels in targeted neighborhoods, which is the opposite of the policy's objectives, with only a small and non-significant reduction in the number of crimes. The share of residents with low levels of education increases by 5% and average income decreases by 2% at the neighborhood level. The negative effect is much larger among Danes and Western immigrants and descendants and it is entirely driven by changes in neighborhood composition. New Danish or Western entrants in labeled areas are significantly poorer and less educated than in comparable public housing units that were not labeled as "ghettos", thus suggesting that the stigma generated by the policy pushed households with better outside options to refuse to move to these areas. No effect is observed on the composition of non-Western descendants, and the ratio of Western to non-Western descendants, one of the policy's target figures, did not change either. The size of the flows in and out of these neighborhoods also remained constant, suggesting that the compositional changes are driven by changes in the types of households moving in and out, rather than by the size of these flows. We can thus conclude that the labeling of target areas as "ghettos" led to additional concentration of disadvantaged families driven by the avoidance strategy of relatively better-off Danes. The latter is even more striking given that the policy gave additional power to the housing associations to select new residents.

To grasp the full extent of the effect of stigmatizing place-based policies, we then turn our analysis to the individuals who were living in targeted areas at the time of the ghetto listing, following them wherever they moved afterwards. We compare them to the trajectories of similar individuals residing in public housing units that were not classified, again using a Difference-in-differences framework with baseline matching. We find that the policy led to a negative causal effect on the total income of affected individuals (-3%), mainly explained by a drop in wages (-5%), and partly compensated by an increase in social benefits (+2%). The latter is once again more pronounced among Danes and Western descendants, and mostly driven by individuals that moved out of their neighborhood after it was listed. This finding suggests that, in order to escape the stigma associated with these areas, some individuals are willing to accept a drop in income. We explore this finding further by comparing the income of individuals moving out of listed neighborhoods to the one of individuals moving out of comparable non-treated areas. Given that the decision to move is endogenous, we cannot claim causality here, but we still think

that this exercise is informative of the willingness to pay that individuals have to escape listed public housing. Results reveal that Danes moving out of treated neighborhoods lose about 8,000 DK annually (about 1,100 USD) relative to Danes moving out of control areas, which corresponds to 3% of their total income. This effect is mostly driven by lower wages among working individuals and to a much lesser extent by individuals losing their job. We interpret this value as indicative of the disutility associated with continuing to reside in areas that have been labeled as "ghettos".

We contribute to the literature on the effects of neighborhood stigma resulting from neighborhoods' reputation or label. Several studies have estimated the effects of being in a commonly known "bad" neighborhood on residents' economic opportunities. There is evidence from experiments showing lower call-back rates to job applications (Bertrand et al., 2004) or in online market ads (Besbris et al., 2015). Additionally, several papers have identified the long-run negative effects of the practice of red-lining in the 1930s in the US through reduced credit access and subsequent disinvestment (Tootell, 1996; Aaronson et al., 2021; Hynsjo and Perdoni, 2023). This evidence is confirmed by papers exploring more recent practices in labeling neighborhoods as disadvantaged. In particular, they show that neighborhood labeling reduces neighborhood attractiveness, as measured by house prices (see Koster and van Ommeren (2022) in the Netherlands and Andersson et al. (2023) in Sweden). Finally, Davezies and Garrouste (2020) and Garrouste and Lafourcade (2023) study the effect of programs aiming at increasing investments in public schools within deprived neighborhoods in France. They both show that these polices backfired because of the stigma attached to their labeling: wealthier families reacted by putting their children into private schools or into public schools outside the designated areas.

To the best of our knowledge, the causal effects of place-based policies on residential moving patterns have not been studied by the existing literature on neighborhood stigma, most likely due to data availability constraints. While falling housing prices and changes in the school attended indirectly suggest compositional changes in the residents of targeted areas, they are not a definitive proof of it. With our study, we aim to bridge this gap by documenting how the assignment of a stigmatizing label to a residential area affects both the composition of its residents and the trajectory of individuals that are directly exposed to it. We are among the firsts to document that moving responses come at a significant cost in terms of revenues. Additionally, we explore the heterogeneity of the effects across Western and Non-Western descendants, a distinction made particularly salient by the fact that the policy directly targets Non-Western descendants. This allows us to connect the neighborhood stigma literature with the extensive literature describing sorting patterns in which advantaged or majority type households leave neighborhoods as the share of disadvantaged or minority type households increases. This pattern has mostly been described in the US context and has been coined *White Flight* (see for example Boustan (2010); Card et al. (2008); Shertzer and Walsh (2019)). Stonawski et al. (2021) documents similar forms of *Na-*

tive Flight in Denmark, and surveys on neighborhood preferences conducted in Europe (and Denmark) clearly reveal preferences consistent with *Native Flight* or at least *Avoidance* (ESS, 2002) (see Appendix Figure B3).[3] Our results reveal the presence of both native avoidance and native flight and allow us to quantify the willingness to pay of native households to escape stigmatized areas. Given that Denmark's Ghetto Plan combines the labeling with restrictions on who moves in and with moving-out subsidies, our context makes looking at compositional changes all the more interesting, since it allows us to test whether the intended effects are dominated by the unintended ones.

Finally, our paper is related to the literature on the impact of discrimination on minority performance (Coate and Loury, 1993; Hoff and Pandey, 2004). This body of work finds that, when the animosity against a given minority group is made salient, minority group members start fairing worse on a number of outcomes (Glover et al., 2017; Carlana, 2019; Corno et al., 2022). The ghetto policy analyzed in this paper was clearly presented as a measure against immigrant's "parallel societies", which is made particularly salient by the fact that one of the criteria to qualify as a ghetto is to concentrate at least 50% of non-Western population. To the best of our knowledge, this is one of the firsts place-based policies in recent times directly and explicitly targeting minority neighborhoods. Our findings reveal that the policy had a worse impact on natives than on non-Western migrants and descendants, whose conditions remained rather constant. This may suggest that in our context the saliency of the anti-immigrant sentiment did not have a large impact on their outcomes, but we cannot rule out that the negative effect was present but so diffused that it equally affected non-Western individuals in control areas.

The rest of the paper proceeds as follows: we will first give a short overview of the Danish ghetto policy's 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 neighborhoods (Nannestad, 2004), which led to *native-Flight* from these neighborhoods even several decades after the end of the scheme, resulting in the increase in spatial segregation between natives and migrant descendants in Denmark (The Danish Government, 2004; Stonawski et al., 2021). At the beginning of the 2000s, the Danish Social Democratic government started debating about possible policies to

[3] 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).

tackle such segregation, framing it as a combat against the formation of "parallel societies" with a lack of Danish "norms and values".[4] Specifically, it announced the allocation of financial resources to counteract the "ghettoization" trend of certain neighborhoods, followed by four significant strategy plans published between 2004 and 2018, which discussed political measures to achieve a more balanced composition of inhabitants in public housing areas (The Danish Government, 2004, 2010, 2013, 2018). The first concrete policy, known as the "Ghetto Plan", was issued in 2010. It included clear criteria to evaluate public housing areas and to identify "ghettos". The three criteria chosen are the following:[5]

1. the share of residents who are immigrants or immigrant descendants from non-Western countries is higher than 50%.[6]
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.

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.[7] Google trends for the topic "ghetto" and the search term "ghetto list", for example, spike each year exactly at the publication dates, as shown in Figure 1. 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 (see Figure B1). Appendix Figure A1 shows a map of the distribution of targeted areas across the country, located in 17 different municipalities.

Publicly subsidized housing in Denmark is administered by housing associations and is 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

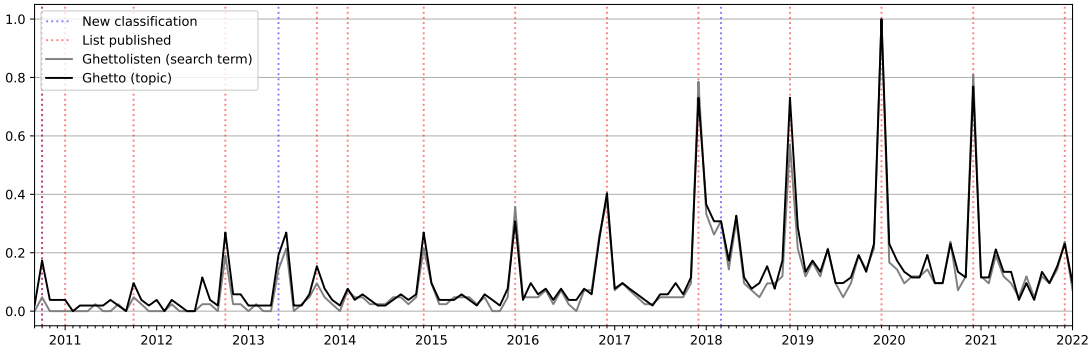
[4] See The Danish Government (2004) p. 11 (authors' translation): "It is the government's goal that the residential areas where immigrants, refugees and their descendants live should be places where they meet Danes. Where networks are established across personal and cultural differences, where you hear 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."

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

[6] Appendix Table B2 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.

[7] Criteria and classification categories changed over time. We focus on estimating the effect of classification treatment in 2010. Table B1 in the Appendix lists the exact definition of criteria and their changes from 2010 until 2018. Appendix B discusses the policy changes in the context of the changing political landscape in Denmark 2010-2018.

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 reaches 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.

can be by-passed, 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).[8] 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:[9]

- 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.

However, when municipalities face the impossibility to find housing for households meeting one or more

[8] Applicants who do not already life 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*).

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

of these criteria, they are allowed to place them in ghetto-designated areas. Housing associations were not bounded by these rules, but they were encouraged to apply *flexible letting*. Moreover, municipal councils in coordination with housing associations could set their own specific criteria to select tenants.[10] 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. Housing associations further offered to jump waiting lists in "non-ghettos" when moving out of a "ghetto".

The only additional provisions of the "Ghetto Plan" are some additional police presence and video-surveillance to reduce crime, as well as additional investments into the physical reconstruction of social housing.[11]

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 Ghetto-plan's evaluation criteria, i.e. country of origin, education levels, criminal activity, income, and labor market participation. We combine the register data with yearly lists of public housing areas, enabling us to precisely delineate the boundaries of neighborhoods targeted by the policy and identify comparable social housing units that narrowly missed the classification criteria.[12] There are in total 198 public housing areas in the country, which are composed of over 190,000 different addresses,[13] with approximately 1,125,000 individuals living in these addresses at any point in time between 2005 and 2019. This is ca. 20% of the Danish population in 2019. 29 neighborhoods were classified as "ghettos" in 2010. Figure 2 plots 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 characteristics and crime rates as well as an indicator for whether and for which years each area was

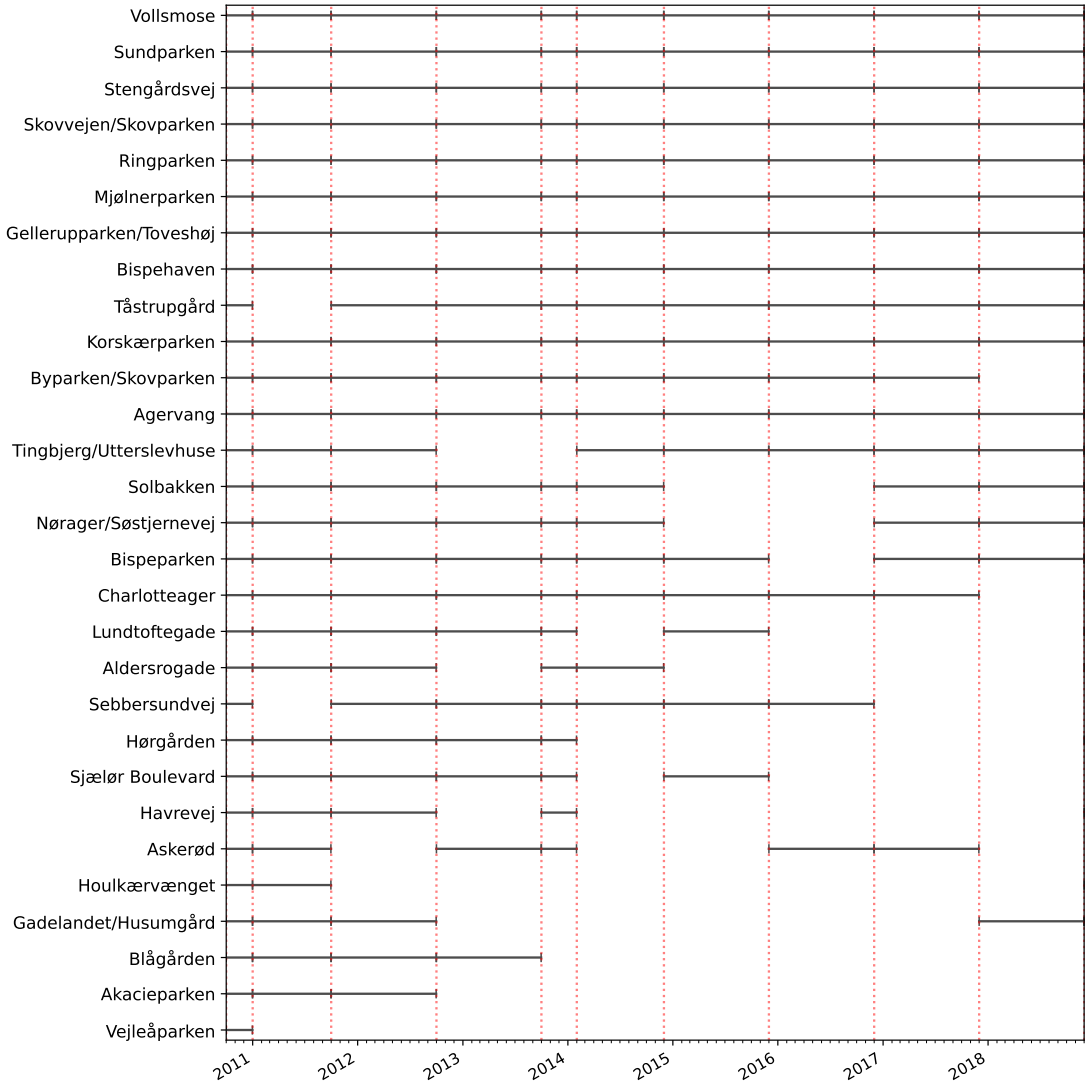
[10] In "ghetto" areas the municipality had full autonomy over these criteria and did not have to negotiate them with the housing associations. See § 60 of Law of Public Housing (*lov om almene boliger*), LBK nr. 103 from February 11, 2011. Criteria have to reviewed at least every four years. It is not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

[11] See § 91-92 of *Lov om almene boliger* (Law on Public Housing), LBK nr. 103 from February 11, 2011. Reconstruction and renovation was, however, minimal over our period of analysis (2010-2018), with mainly two particularly deprived neighborhoods affected by it – Mjølnerparken and Vollsmose –, which are not in our sample because of the lack of comparable controls.

[12] 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.

[13] 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 got published. Areas on the vertical axis are sorted by the date they have been first listed and total number of times listed.

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, whether it is a new entrant, or whether it is about to move out. 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 in a yearly panel of individuals that were residing in the same public housing neighborhood consecutively between 2007 and 2009. These individuals are then followed over the entire period of 2006 to 2018 wherever they 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.

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 which is the last year available in our database. Second, given that 2010 constitutes the first wave of ghetto policies, we can be reassured that there were no 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.[14] In our baseline approach, we consider all social housing units listed in 2010 as treated for the entire duration of the period. This simplification can be seen as an exposure to treatment approach allowing us to take into account the presence of possible persistent effects even after a unit has been taken out of the list. In practice, 11 of the 29 units listed in 2010 remain listed in all following lists up until 2018, and on average our sample is listed in 70% of the years, see 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.

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 A1 show the summary statistics of treated and control units observed between 2006 and 2009, thus prior to the reform. Control 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 neighborhood data taken in 2006 using the probit estimator:

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

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

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.[15] 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 analysis 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.[16]

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 A1 compare the summary statistics among these two groups, showing much higher balance. One might wonder whether treated and control neighborhoods, while similar, may be located in different types of municipalities. Appendix Table A2 compares the summary statistics of municipalities with treated and control units both overall and in the selected sample. We can see that characteristics are very similar on all dimensions.[17]

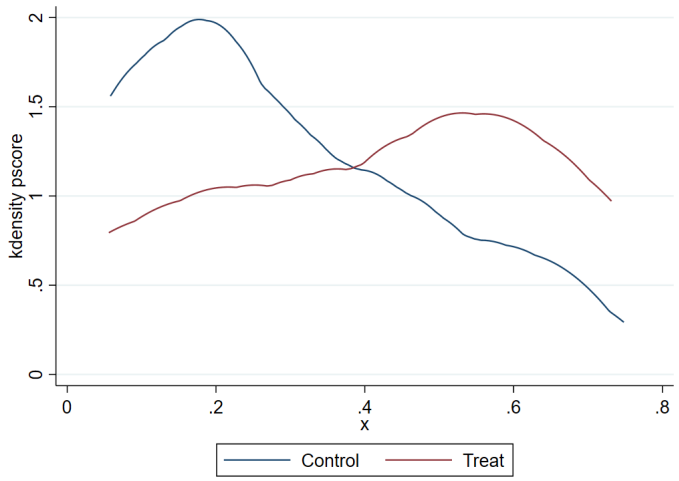
The exclusion of the observations outside common support reduces the number of control neighborhoods from 140 to 33 units and the number of treated neighborhoods from 29 to 14 units. This means that we are only able to estimate the treatment effect for this sub-sample of affected neighborhoods, which arguably are those that did not present the worst conditions to begin with. Our estimated results should

[15] 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.

[16] 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.

[17] Note that 17 municipalities only host control neighborhoods, 8 municipalities only host treated neighborhoods, but 4 municipalities (large cities) host both control and treated areas.

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.

be interpreted as a potential lower bound of the effects on all treated neighborhoods.[18] However, given the absence of suitable controls for the remaining treated areas, we see no viable alternative approach. Additionally, we refrain from directly controlling for the propensity score, as doing so could artificially induce common pre-trends in the outcomes.[19] To verify that characteristics are balanced once we apply this sample selection, 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 selections. Results are presented in Table 1. As already visible from Appendix Table A1, 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 group for the difference-in-differences analysis. Our main estimation model is dynamic, allowing 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 $t \in [0, 9]$

[18] 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.

[19] 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.

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.

show the dynamic effects for the nine 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:

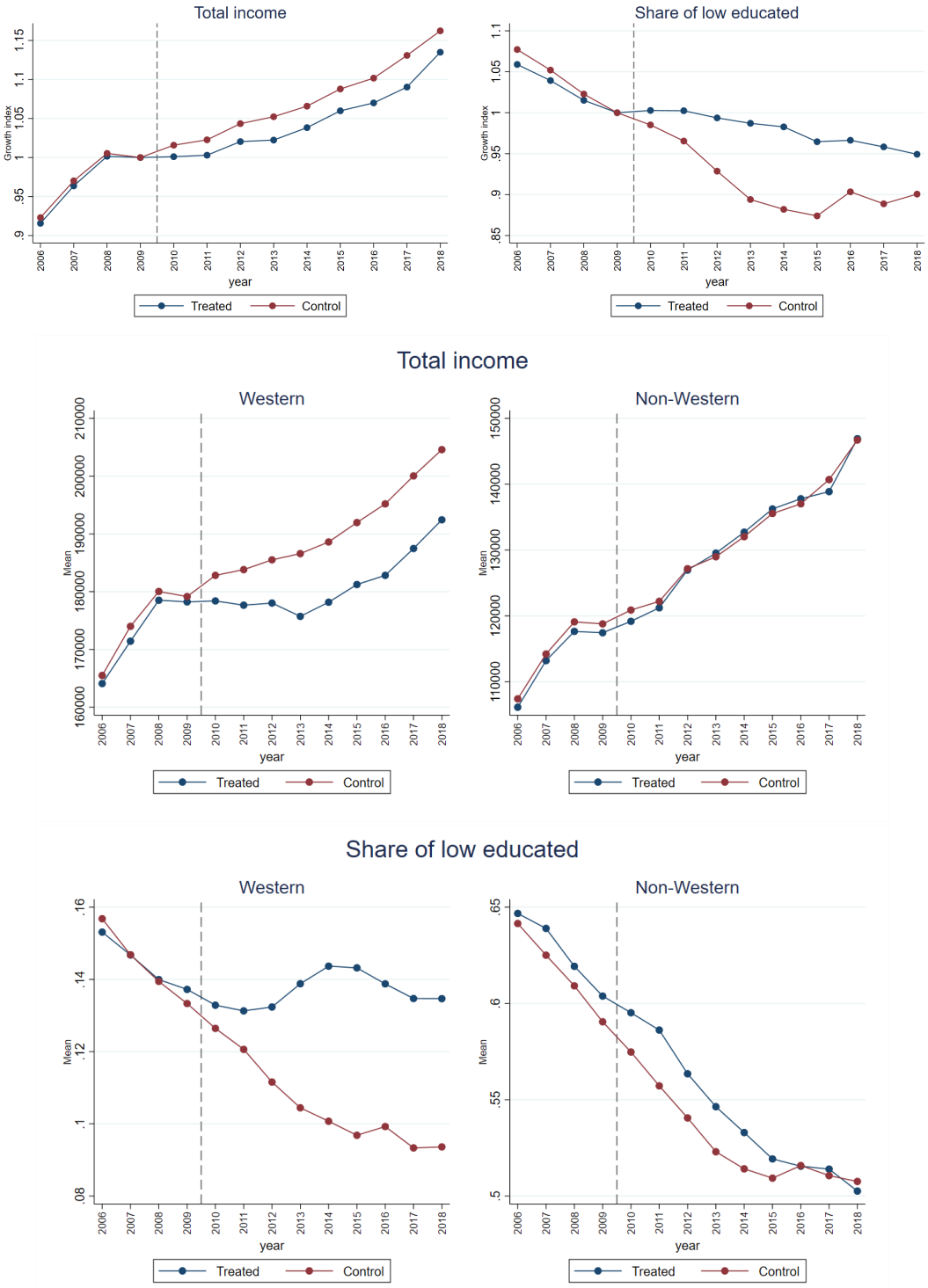
$$Y_{it} = \beta_1 Post_t \times treat_i + \gamma_i + \gamma_t + \epsilon_{it} \tag{3}$$

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 slow-down in income growth and a slow-down in the decline of 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 effect that is visible overall is driven by Danes and Western descendants. Appendix figures A2 and A3 show the same pictures for additional outcomes, overall and by nationality group respectively. Similarly, we observe a slight increase in the

share of 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 more noisy and it is harder to identify a clear trend.

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 migrants and Western, which includes Danes and migrants from Western countries, and have on the Y-axis the level of the outcome.

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).

5 Results

5.1 Overall effect on neighborhoods

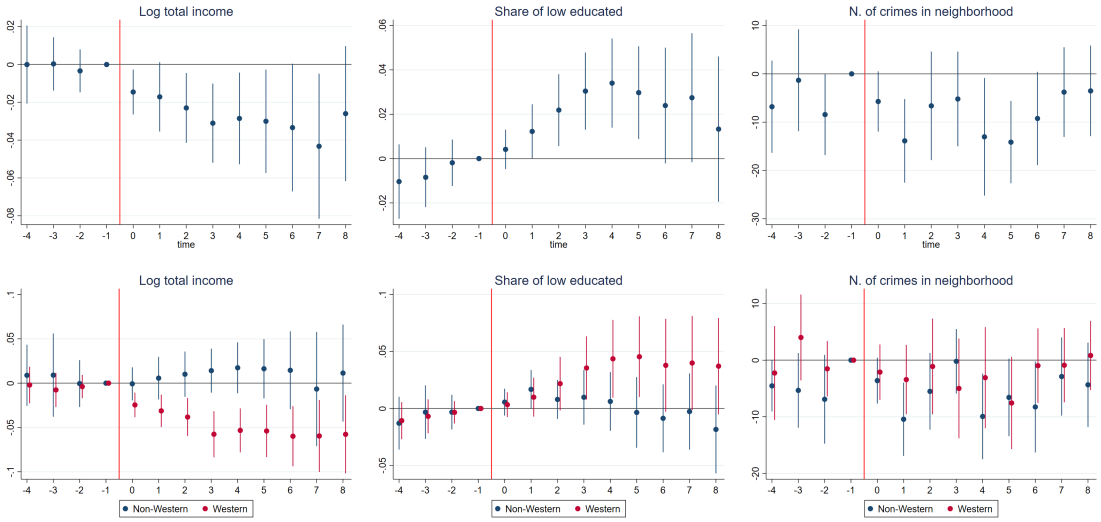
Figure 5 shows the dynamic graphs obtained from estimating equation 2. The vertical lines are the 95% confidence intervals around the estimated coefficients. The top three panels show the outcomes for the overall neighborhood population while the bottom three panels distinguish between Western and non-Western citizens.[20] Overall, these figures confirm what was already visible from the descriptive graphs, pre-trends are parallel across all outcomes, and after the introduction of the Ghetto policy affected neighborhoods see a drop in income and an increase in the share of low educated individuals that are driven by changes among Western citizens. In these graphs it also appears that there was a reduction in the number of crimes committed in the neighborhood. On the contrary, we do not observe any significant effect on the share of non-Western descendants in the neighborhood nor in the share of active population not working (see Appendix figure A4).[21]

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. Appendix table A4 shows the full set of dynamic effects taking place after the policy. The only significant effects visible on the overall sample (Panel A), once we average over the post-reform years, are the drop in total income (-2%) and the increase in the share of low educated individuals (+5%). However, this masks the already described differences in effect among Western and non-Western population that partly

[20] Appendix figures A2 and A3 show the same results for additional outcomes: log wage income, share of non-employed, share of non-Western migrants.

[21] We also see no changes in the average age of residents, which could have explained the drop in income (results available upon request).

Figure 5: Overall effect of the ghetto policy 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.

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 see no significant effect among the Non-Western population, nor do we observe a significant change in the overall composition between Western and non-Western.

To verify the robustness of these results, we perform several tests. First, we consider a social housing 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".[22] Second, we extend our analysis to areas listed as Ghettos after 2010, always using the never treated areas as controls. For this second test, we re-estimate our propensity score model and re-apply the restriction of 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. Third, we go back to our baseline model but we include as a control the propensity score measure interacted with year fixed effects, which ensures even further the comparability between treated and controls. Fourth, we estimate our baseline model on neighborhood characteristics measured in 2009 rather than in 2006. Fifth, we change the variables included in the propensity score model. Here the probability to be listed is only estimated based on average household income expressed in terms of per-adult equivalent and on the share of non-Western migrants and descendants. 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.

[22] We decide to drop the observations in years after 2010 when a given treated social housing unit 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).

Table 2: Overall effect of the ghetto policy on the neighborhoods

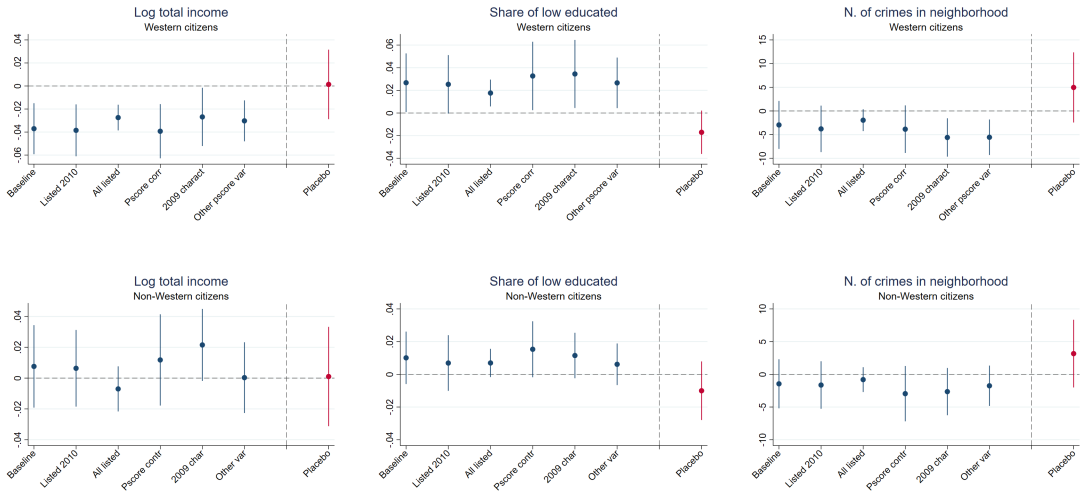
VARIABLES	(1) Share of non-western citizens	(2) Log total income	(3) Log wage income	(4) Share not employed	(5) Share low educ	(6) N. crimes	(7) N. property crimes
Panel A : All citizens							
treat * post	0.0105	-0.0203*	-0.0406	0.00707	0.0221**	-4.395	-2.892
	(0.0124)	(0.0105)	(0.0293)	(0.0110)	(0.00912)	(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 policy, and serves to compute the effect in terms of growth rate relative to baseline.

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 specifications, while the effect is almost always non-significant and close to zero on the placebo test. The only 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

Figure 6: 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 ghettos 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.

positive, we can conclude that if anything our magnitude of effect is a lower bound.

Finally, Appendix Table A5 tests the heterogeneity of the effect across treated neighborhoods with mild and severe pre-policy conditions. In practice, we split the treatment group into two half 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 the overall population, the drop in total income, the increase in the share of low educated and the decrease in number of crimes is larger and more significant among listed areas that had the worse initial conditions. In general, the finding that the magnitude of the effect is larger within areas with more severe initial conditions is true even within the Western and non-Western sub-samples, even if the significance level is not always skewed in this direction. Overall, it seems that crime decreased the most in areas that had higher levels of crime to begin with, a good news, but also that income levels and the share of low educated individuals got worse in areas that were already lagging behind in these dimensions, thus tempering the first positive result. Given that in our analysis we are excluding the listed areas with initial conditions so severe that the lay outside the area of common support of the propensity score, we can hypothesize that our effects are a lower bound relative to what would be observed there, if a comparable control group was available.

In the next section we investigate the mechanisms behind these effects, putting a particular focus on

disentangling changes driven by composition from changes driven by the evolution of conditions among incumbents.

5.2 Mechanisms

The main mechanisms that we test are the presence 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 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 nationality 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 means that moving restrictions aiming at restricting the number of non-Western descendants arriving to these areas have been ineffective, since the share of non-Westerners remained constant.

The second test that we perform to verify the presence of composition effects is to manually replace the values of outcomes within a given household by its mean observed over the entire period. By doing so, we kill all of the variation in outcomes coming from changes within households, thus leaving as only mechanism possible within affected neighborhoods the change in composition of households. Results for the main outcomes are reported in Appendix figure A7 and table A6. 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 are playing 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 in disentangling the average outcome

within a given neighborhood into three components, as following :

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

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 that 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. The 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

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 treated neighborhood prior to the ghetto policy, and serves to compute the effect in terms of growth rate relative to baseline.

and 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 policy and keeps getting worse for average income, while it peaks at t+5 for the share of low educated and then remains constant. In the next section we move away from the neighborhood level perspective to focus on the causal effect of the reform on affected individuals, define as those people living in the soon-to-be listed areas in 2009.

6 Effect on exposed individuals

In this section, we estimate the causal impact of the 2010 Ghetto policy on individuals who were exposed to the policy. To do so, we compare individuals who resided in the above-defined treated neighborhoods between 2007 and 2009 to those living in control neighborhoods during the same period.[23] Although the neighborhoods are similar on average, as seen in Table 1, they are not necessarily similar at the individual level.[24] Columns (1) and (2) in Appendix Table A7 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: their position in the total income distribution. 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 A7 show the balancing test when the estimations are reweighted using the IPW. The only significant differences, despite reweighting, are the level of total income, wages, and benefits, while the other dimensions, including these measures in log, become insignificantly different and very small in magnitude.

We then estimate the dynamic and static models from section 4 (eq. 2 and 3) at the individual level, where *i* are 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.

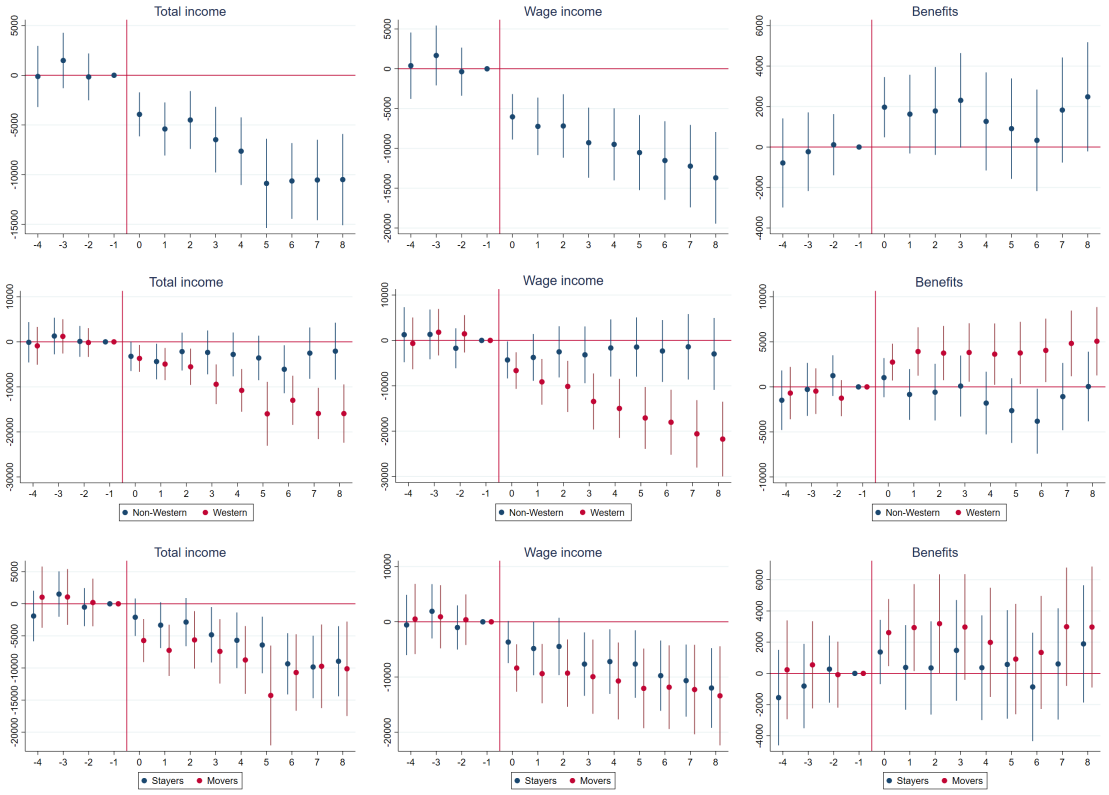
Figure 7 plots the individual-level β_t from equation 2. It shows the difference in outcomes between individuals from the treated and control groups compared to their differences in the pre-treatment period, controlling for time-invariant individual characteristics. The top panel shows the overall effect on total

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

[24] 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 (2007 to 2009) further differentiates the two datasets.

income, wage income, and benefits. We see that these outcomes were evolving on parallel trends up to 2010, while the introduction of the Ghetto policy led to a significant negative effect on total and wage income and an increase in social benefits receipts. Figure A11 in the Appendix, which decomposes total income into its different components, shows that the policy only negatively affected the labor income component. It also suggests that the increase in benefits mitigated the loss in total income. This result suggests that the policy causally harmed the earning capacity of affected individuals.

Figure 7: Overall effect of the ghetto policy 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 reported for the overall sample in the upper panel, decomposed by origin in the middle panel, and decomposed by moving status in the bottom panel. Vertical bars represent the 95% confidence intervals.

Given that the policy directly targeted non-Western immigrants, in the middle panel, we study the differential effect of the Ghetto policy on Western and non-Western individuals. We find that the negative impact on wages and the increase in benefits are much more pronounced for the Western population (Danes and Western migrants and descendants) than for non-Western individuals. This result is consistent with what is observed at the neighborhood level. Finally, we also decompose the overall effect by the moving status in the bottom panel. Individuals who remain in a ghetto neighborhood in the post-2010 period are classified as stayers, and others as movers. Since the decision to move is endogenous, these results are interpreted as descriptive effects on the two groups. They suggest that both movers and stayers were impacted by the Ghetto policy, with a greater effect on movers compared to stayers, especially in

terms of wages and benefits.

In Table 4, we estimate the corresponding static difference-in-differences effect of the Ghetto policy at the individual level for all citizens and decomposed by Western and Non-western groups. In line with the dynamic results, we find that this policy had a significant negative effect on total and wage income (-3% and -5% respectively) partly compensated by a rise in social benefits (+2%). The impact of the policy is larger and more significant for Western citizens. We also find that the Ghetto policy was not accompanied by a reduction in the probability of committing a crime at the individual level. This suggests that the slight decline in the number of crimes at the neighborhood level in Table 2 is entirely driven by composition changes in the residents.

Table 4: Overall effect of the ghetto policy on the individuals

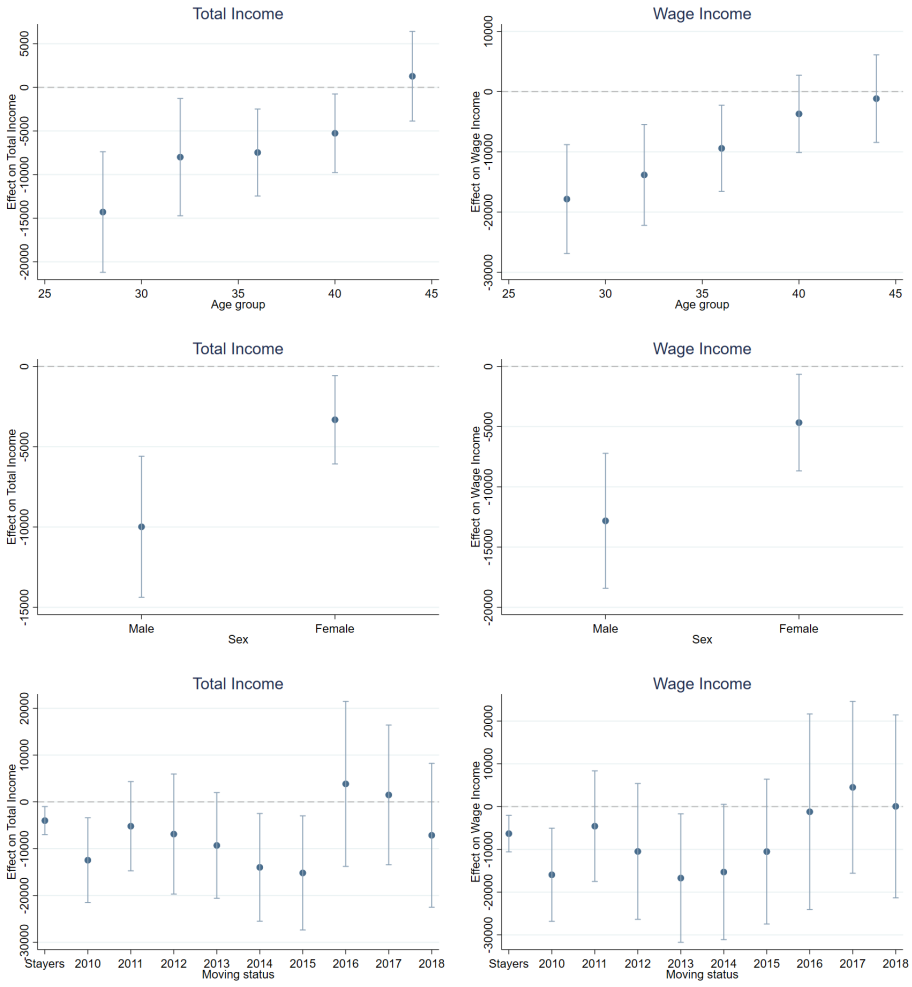
	(1) Total income	(2) Wage income	(3) Pr. not employed	(4) Benefits	(5) Pr. Crime
	OLS	OLS	OLS	OLS	OLS
Panel A : All citizens					
treat * post	-6,772***	-8,715***	0.00597	1,868**	-0.000672
	(1,325)	(1,778)	(0.00524)	(904.5)	(0.00148)
Observations	168,410	168,410	168,410	168,410	168,410
R-squared	0.683	0.782	0.658	0.768	0.312
Baseline mean	240229	160326	0.384	78349	0.0210
Effect (%)	-3%	-5%	2%	2%	-3%
Panel B : Western citizens					
treat * post	-8,434***	-12,562***	0.0115	4,211***	-0.00148
	(1,856)	(2,504)	(0.00722)	(1,248)	(0.00230)
Observations	90,350	90,350	90,350	90,350	90,350
R-squared	0.654	0.795	0.665	0.785	0.314
Baseline mean	256733	185946	0.330	69266	0.0254
Effect (%)	-3,3%	-7%	3%	6%	-6%
Panel C : Non-western citizens					
treat * post	-3,400*	-3,026	0.000921	-661.1	0.000158
	(1,869)	(2,523)	(0.00765)	(1,318)	(0.00192)
Observations	78,060	78,060	78,060	78,060	78,060
R-squared	0.698	0.744	0.639	0.744	0.308
Baseline mean	225852	138008	0.432	86261	0.0172
Effect (%)	-2%	-2%	0%	-1%	1%

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 policy and serves to compute the effect in terms of growth rate relative to baseline.

To shed more light on the groups affected by the Ghetto policy, we present heterogeneity analyses in Figure 8. The upper panel shows the heterogeneity based on age groups. The patterns reveal that the younger the individuals, the more negatively they were affected by the ghetto policy. This could potentially reflect

the more precarious situation of younger groups. The middle panel of Figure 8 shows that men were more affected than women. The bottom panel shows the impact of the policy based on individuals' moving status: Stayers, those who never moved out of the neighborhoods, face a lower penalty than movers, especially those who moved between 2010 and 2015, at the onset of this policy. This final result indicates that moving to escape stigma comes at a cost. In the final exercise of the paper, we aim to quantify this cost.

Figure 8: Heterogeneities based on age, sex, and moving status



Note: The figure reports the estimated β from equation 3 with γ_i being individual fixed effects and total and wage income as dependent variables. Heterogeneous effects on age, sex, and year first moved are reported in the upper, middle, and lower panels respectively. Vertical bars represent the 95% confidence intervals.

6.1 Willingness to pay to move out of Ghettos

Given that the negative effect that the policy had on the wages of affected individuals is larger among movers, we can hypothesize that individuals value leaving the "Ghettos" to the point of accepting a loss in revenues. 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 or to spend some time in unemployment. In fact, we can compare the wage 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 "Ghettos", which is a measure of the disutility associated with remaining in these areas after the policy.

For this analysis, we follow the same approach adopted in the rest of the individual level analysis.[25] In addition, we further select individuals that are moving out of treated and control neighborhoods after 2009, and we consider as the year of move the time of the first move out of the neighborhood after the policy introduction in 2010.[26] Finally, we adopt an event study framework where we follow treated and control individuals before and after moving. It is important to note that we cannot claim causality in this analysis, since moving is an individual choice and thus its timing is endogenous, contrary to the timing of the reform. Nonetheless, we still think that this approach can be informative of the different conditions accepted by movers in treated neighborhoods as compared to controls. Results are presented in Table 5 (static coefficients for the average effect in the three years after moving) and in Appendix Figure A12 for the event study graphs.

We find that, on average, the income of individuals moving out of Ghettos drops by 5,600 DK (about 800 USD) relative to individuals moving out of control neighborhoods. This corresponds to a drop of 2% relative to their income before moving. This is mostly driven by a drop in wage income among working people. There is a slight increase in the share of not employed, but it only lasts one year and thus is not significant over the three years after the move (see Figure A12). Income and employment trends were parallel between treated and control individuals before the move, which comfort us on the validity of this analysis despite the endogeneity of the movement decision. Once again, this effect is only significant among Danes and Western descendants: they are willing to let go 8,000 DK (about 1,100 USD) of income per year, corresponding to 3% of their total income, in order to move out of the listed neighborhoods. This result gives us a monetary value of their disutility of remaining in the neighborhood after it has been publicly designed as a Ghetto.

[25] Namely, 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.

[26] In the Appendix B, we analyze the decision to move out in more detail by regressing indicators for moving out on 2009 characteristics interacted with treatment indicators. We find that Western individuals were more likely to move out of treated neighborhoods. This seems to be driven by Western inactive individuals. Similarly, we look at the probabilities to (a) leave Denmark (no treatment effects), (b) leave the public housing sector (positive treatment effect for Western inactives), and (c) change in municipality (positive treatment effect for inactives). For robustness we repeat the same analysis on the household level using 2009 household IDs. Here, we find that the share of Westerners in a household interacted with the treatment indicator has a positive effect on the probabilities to move and to leave the public housing sector.

Table 5: Willingness to pay analysis

VARIABLES	(1) Total income	(2) Wage income	(3) Wage income if working	(4) Not employed
Panel A : All citizens				
move treatment * post	-5,594*	-5,363*	-9,652**	0.0109
	(3,147)	(2,968)	(3,944)	(0.00836)
Observations	71,448	71,448	48,165	71,015
R-squared	0.759	0.822	0.755	0.683
Baseline mean	251243	159705	239820	0.405
Effect (%)	-2%	-3%	-4%	3%
Panel B : Western citizens				
move treatment * post	-8,003*	-7,310	-10,591**	0.00649
	(4,209)	(4,492)	(5,030)	(0.0104)
Observations	43,802	43,802	32,427	43,617
R-squared	0.762	0.818	0.759	0.682
Baseline mean	264447	179649	250995	0.379
Effect (%)	-3%	-4%	-4%	2%
Panel C : Non-western citizens				
move treatment * post	-1,184	-1,387	-6,334	0.0169
	(2,885)	(2,368)	(3,788)	(0.0111)
Observations	27,645	27,645	15,738	27,397
R-squared	0.738	0.809	0.732	0.674
Baseline mean	234262	134059	222729	0.438
Effect (%)	-1%	-1%	-3%	4%

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.

7 Conclusion

This paper evaluates the impact of a Danish policy aimed at altering the socio-economic composition of certain public housing units labeled as "ghettos," with the objectives of reducing crime and generating positive spillovers for incumbent residents. Our findings reveal that Denmark's Ghetto Plan affected outcomes in the opposite direction to its intended objectives. In targeted areas, average income and educational attainment declined following the reform, while crime rates remained largely unchanged.

These outcomes are driven by two factors: native avoidance and native flight. The socio-economic com-

position of targeted neighborhoods deteriorated as the Danish and Western residents moving in after the policy tended to be poorer and less educated, suggesting that more affluent households avoided these areas. Additionally, Danish and Western migrants who were already residing in the targeted neighborhoods at the time of the policy experienced income declines, primarily among those who relocated elsewhere, highlighting the presence of native flight and a willingness to pay to escape the stigma of living in a labeled neighborhood.

In contrast, we observe minimal changes in the characteristics, outcomes, or behavior of non-Western descendants, the primary population targeted by policymakers. While the stability of their conditions could be viewed as a silver lining, given literature on internalized discrimination, we cannot rule out broader negative effects stemming from heightened anti-immigrant sentiment, which may have impacted residents in both targeted and control areas.

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Online Appendix for

The Making of a Ghetto

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

Yajna Govind
yg.eco@cbs.dk
Copenhagen Business School

Jack Melbourne
j.melbourne@unibocconi.it
Bocconi University

Sara Signorelli
sara.signorelli@polytechnique.edu
CREST, Ecole Polytechnique

Edith Zink
ezi@econ.ku.dk
University of Copenhagen

Outline

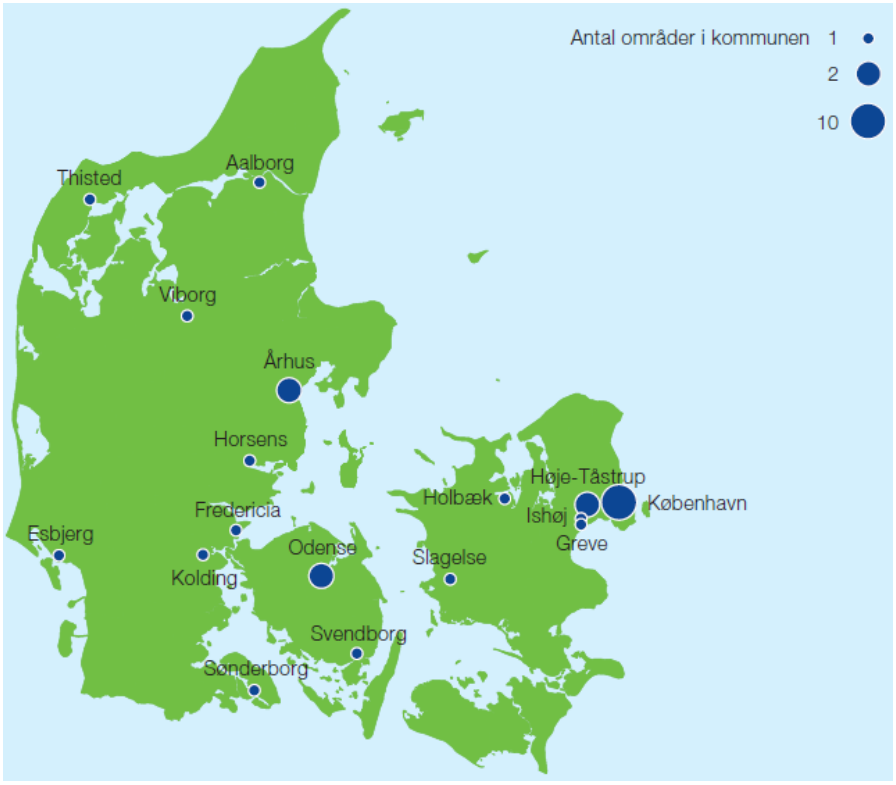
- Appendix A presents additional tables and figures
- Appendix B presents the context and the reform in more details

Appendix

A Additional Tables and Figures

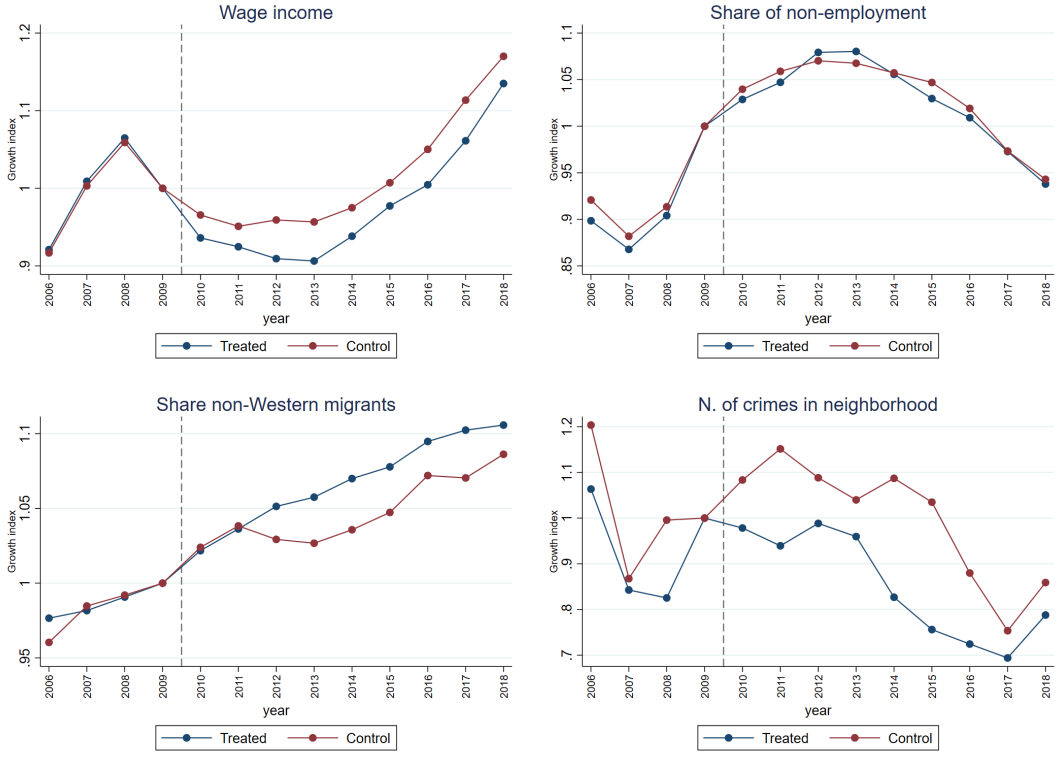
A.1 Figures

Figure A1: Map of listed Ghettos in 2010



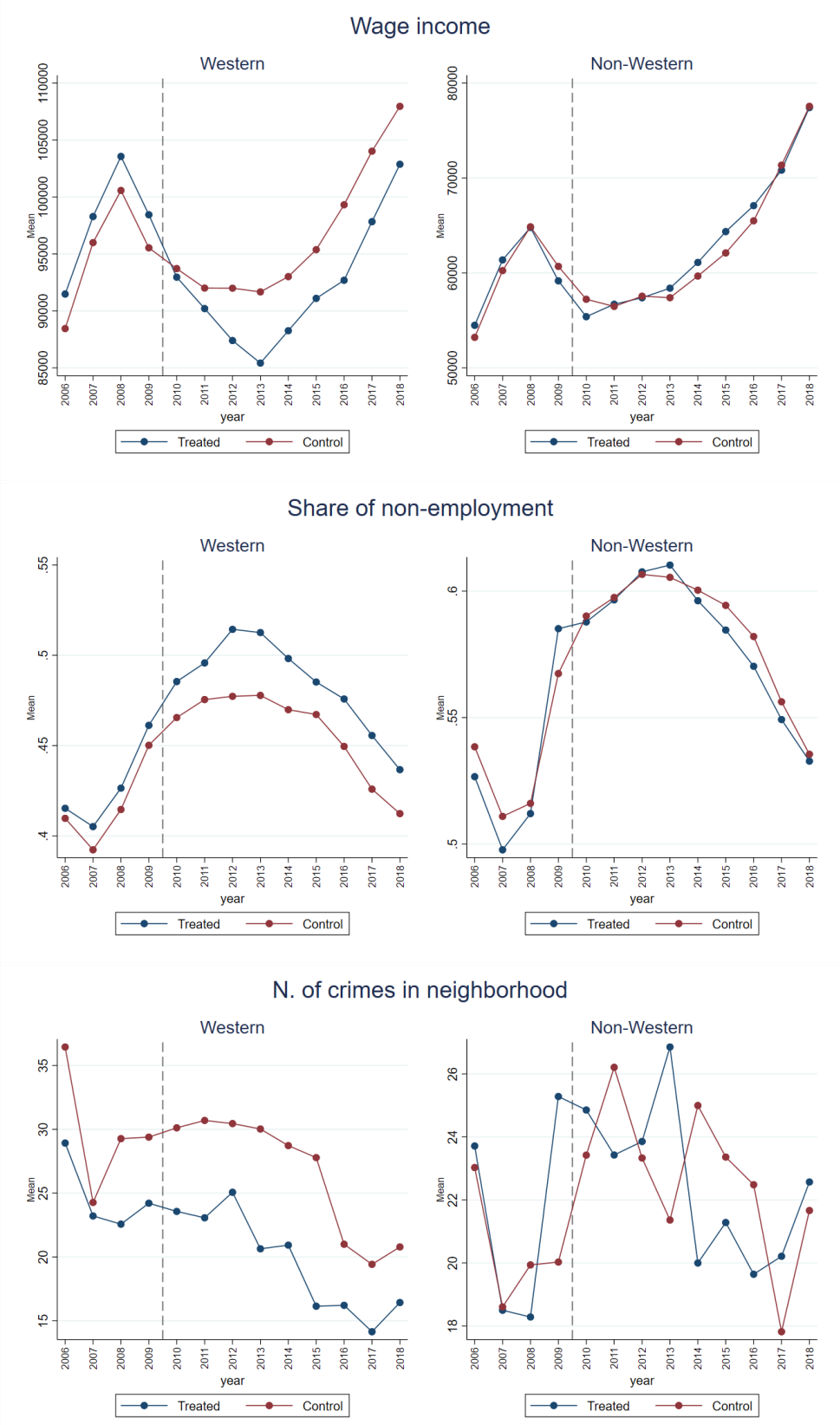
This map shows the location of the neighborhoods listed as “Ghettos” in 2010. Source: Denmark 2010 Ghetto Strategy Plan.

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



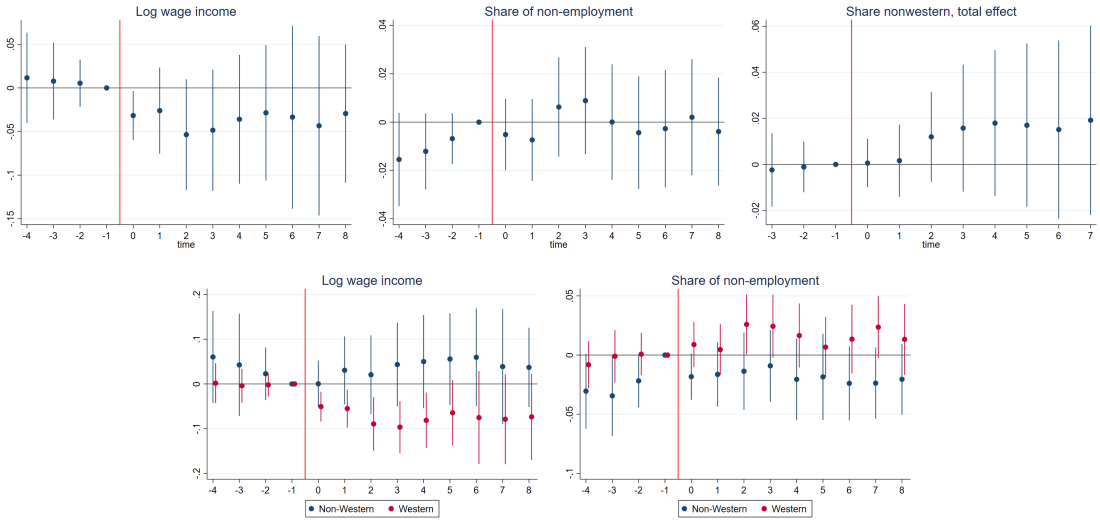
Descriptive evolution of outcomes within treated and control neighborhoods over the period. The Y-axis represent a growth rate index, set to 1 in 2009.

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



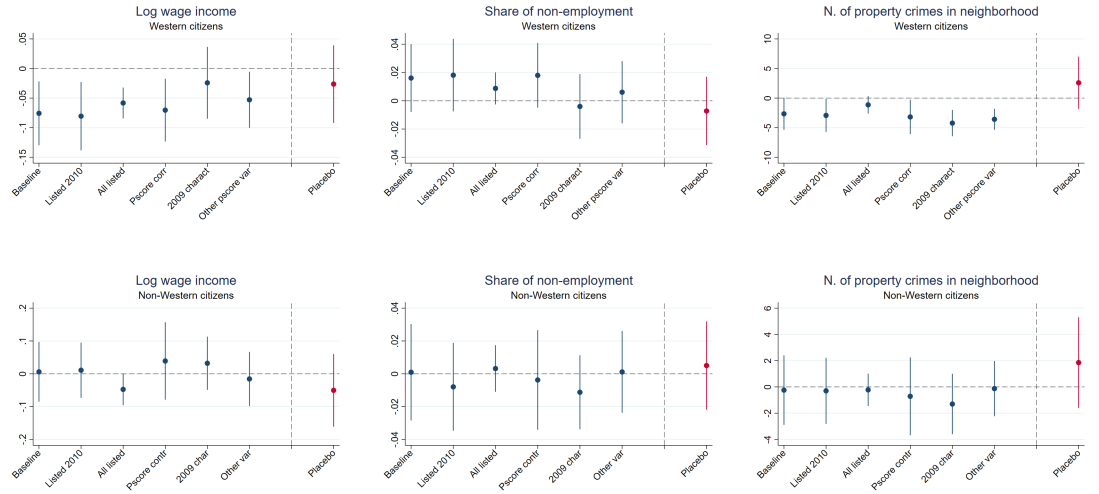
Descriptive evolution of outcomes within treated and control neighborhoods over the period, distinguishing between Non-Western migrants and Western, which includes Danes and migrants from Western countries. The Y-axis captures the level of the outcome.

Figure A4: Overall effect of the ghetto policy on the neighborhoods - additional outcomes



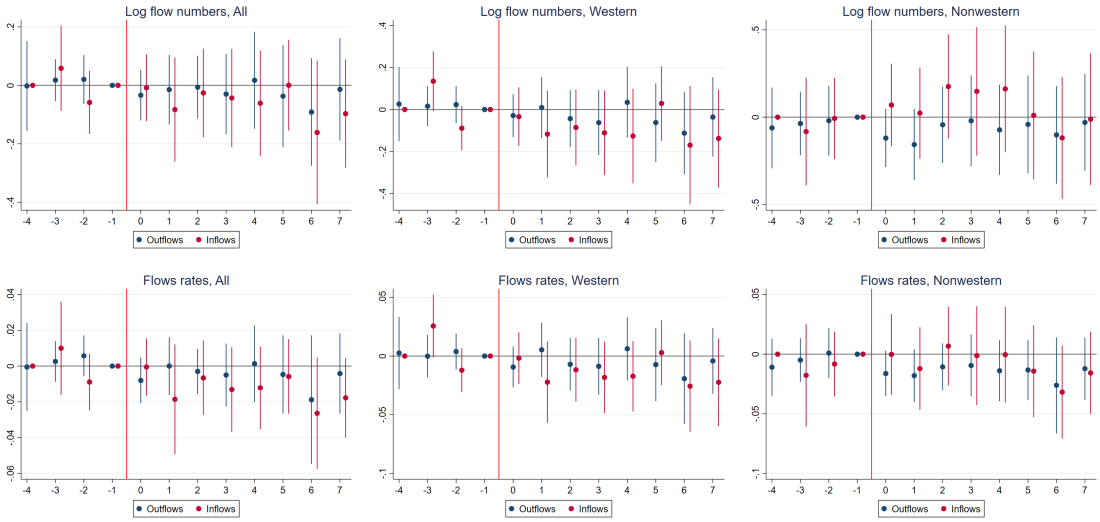
The figure reports the estimated difference in trends between treatment and control occupations with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals.

Figure A5: Robustness tests



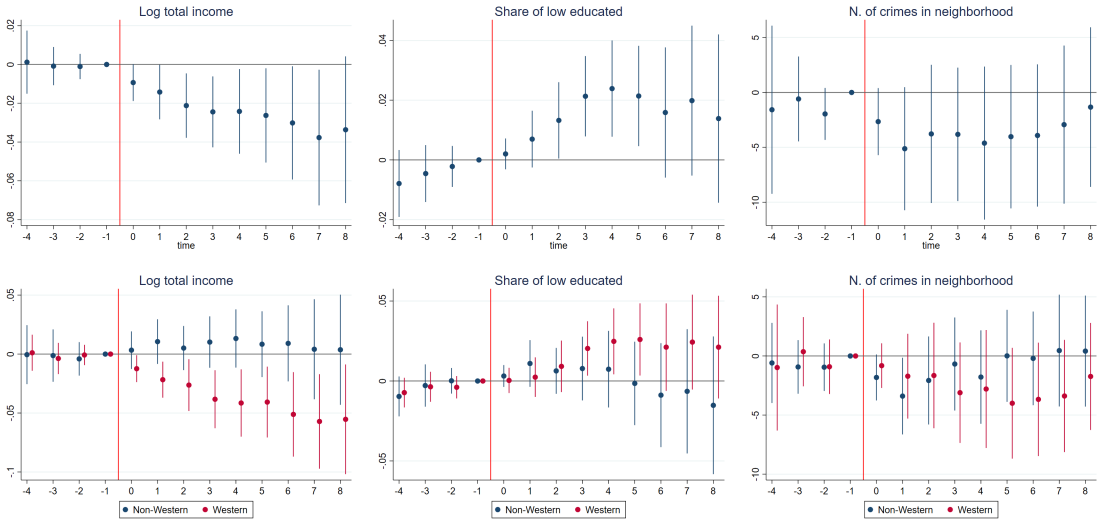
Note: The figure reports the coefficients and the 95% confidence intervals obtained from estimating the static equation 3 in different context. The first coefficient from the left reports the baseline estimates for comparison. The second only considers treatment as long as the neighborhood is listed 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 ghettos 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



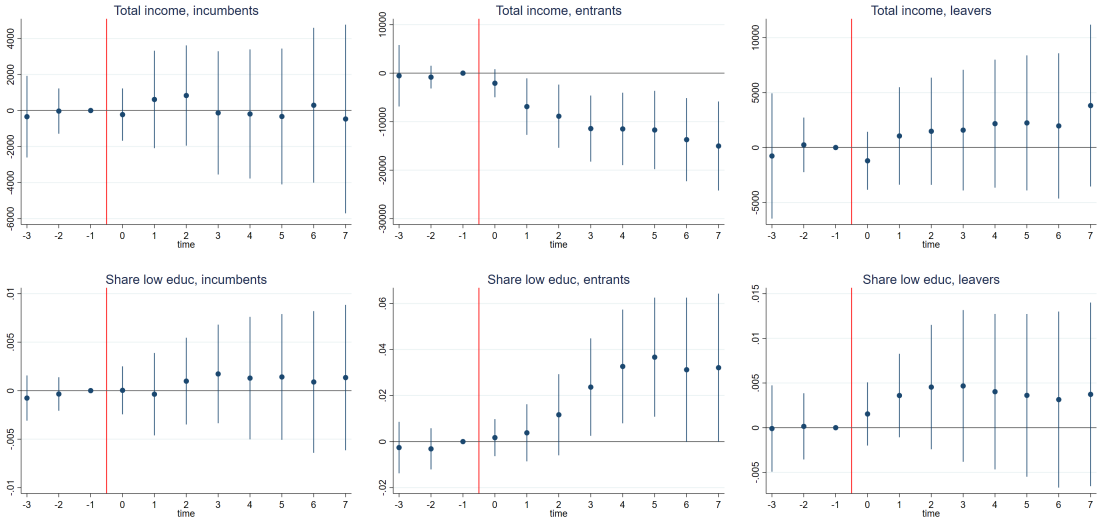
The figure reports the estimated difference in trends between treatment and control neighborhoods over the period with respect to the last pre-reform year obtained from estimating equation 2. Vertical bars represent the 95% confidence intervals. The outcomes compute the size of flows in and out of the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

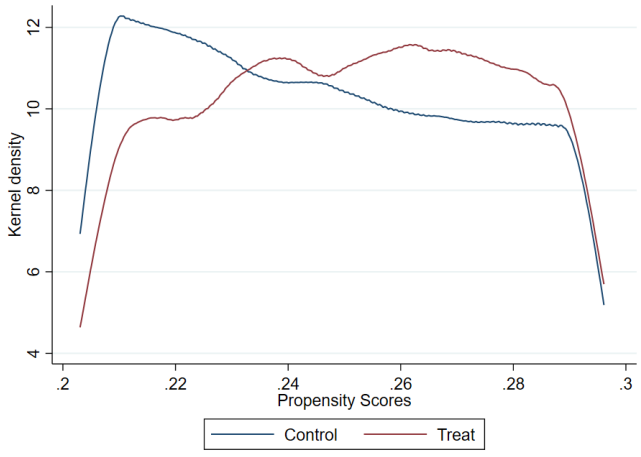
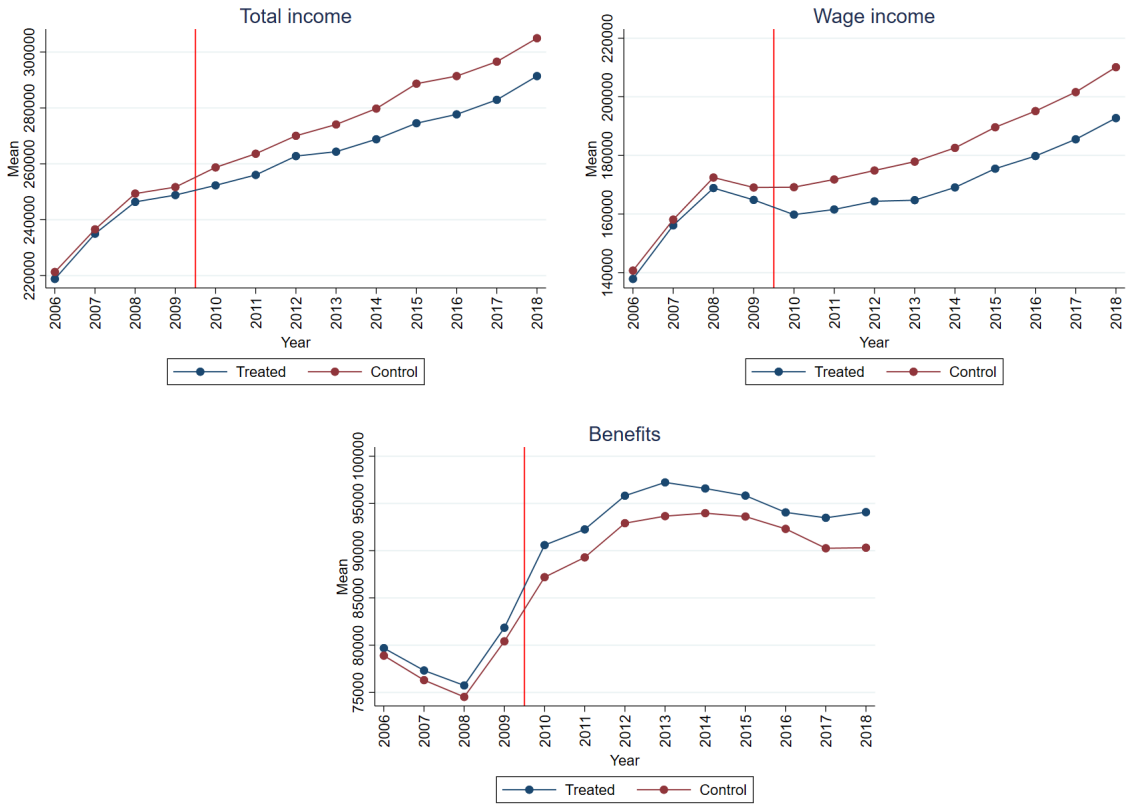
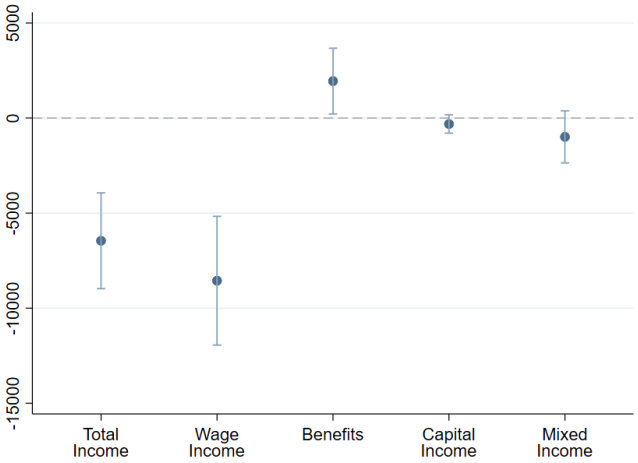


Figure A10: Overall effect of the ghetto policy on exposed individuals



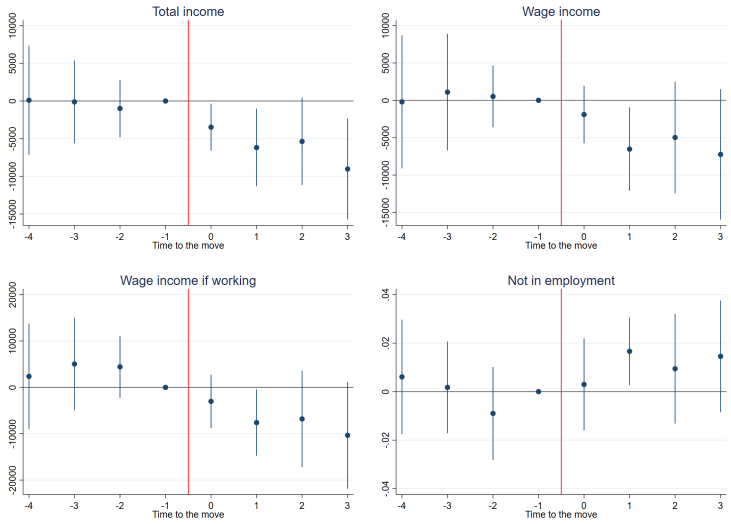
Note: Descriptive evolution of outcomes for treated and control individuals over the period.

Figure A11: Effect decomposed by type of income



Note: This figure reports the point estimates β from equation 3 with different definitions of income. Total income is the sum of wage income, benefits, capital income and mixed income. The vertical bars depict the 95% confidence intervals.

Figure A12: Event studies along the time of move out of the neighborhood



The figure reports the estimated difference in trends between treatment and control neighborhoods 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.

A.2 Tables

Table A1: 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 A2: Summary statistics on municipalities

	(1)	(2)	(3)	(4)
	Treatment dummy			
	Without PSM weights		With PSM weights	
	coef	se	coef	se
Total income	-13919***	(915.3)	-2446**	(939.8)
Log total income	-0.0594***	(0.0065)	-0.0008	(0.0061)
Wage income	-18,288***	(1,286)	-3,139**	(1,328)
Log wage income	-0.112***	(0.0154)	-0.0208	(0.0147)
Pr. not employed	0.0437***	(0.0043)	0.0065	(0.0042)
Log benefits	0.0460***	(0.0113)	-0.0011	(0.0116)
Benefits	4,924***	(672.7)	1,117*	(675.9)
Pr. crime	0.0033**	(0.0013)	0.0011	(0.0013)

The table summarizes the main characteristics of municipalities containing treated neighborhoods and control neighborhoods. 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 policy 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 policy.

Table A5: Heterogeneity of overall effect of the ghetto policy by severity of initial conditions

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 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 policy, separately for mild and severe conditions, and serves to compute the effect in terms of growth rate relative to baseline.

Table A6: 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 A7: Balancing test: individual-level analysis

	(1)	(2)	(3)	(4)
	Treatment dummy			
	Without PSM weights		With PSM weights	
	coef	se	coef	se
Total income	-13919***	(915.3)	-2446**	(939.8)
Log total income	-0.0594***	(0.0065)	-0.0008	(0.0061)
Wage income	-18,288***	(1,286)	-3,139**	(1,328)
Log wage income	-0.112***	(0.0154)	-0.0208	(0.0147)
Pr. not employed	0.0437***	(0.0043)	0.0065	(0.0042)
Log benefits	0.0460***	(0.0113)	-0.0011	(0.0116)
Benefits	4,924***	(672.7)	1,117*	(675.9)
Pr. crime	0.0033**	(0.0013)	0.0011	(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 A8: Overall effect of the ghetto policy on exposed individuals - decomposed by moving status

	(1) Log total income	(2) Log Wage income	(3) Wage income	(4) Share not employed	(5) Benefits	(6) Log Benefits	(7) Pr. Crime
	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Panel A : All citizens							
treat * post	-0.0272***	-0.0747***	-8,715***	0.00597	1,868**	0.0112	-0.00277*
	(0.00781)	(0.0220)	(1,778)	(0.00524)	(904.5)	(0.0217)	(0.00156)
Observations	167,759	114,162	168,410	168,410	168,410	145,554	190,150
R-squared	0.615	0.566	0.782	0.658	0.768	0.728	0.306
Baseline mean	12.28	11.92	160326	0.384	78349	11.06	0.0258
Effect (%)	-3%	-7%	-5%	2%	2%	1%	-11%
Panel B : Movers							
treat * post	-0.0246**	-0.0871***	-10,406***	0.00312	2,261*	0.00739	0.000498
	(0.0116)	(0.0300)	(2,756)	(0.00745)	(1,295)	(0.00945)	(0.00217)
Observations	84,030	61,145	84,360	84,360	84,360	84,360	84,360
R-squared	0.583	0.555	0.770	0.636	0.757	0.520	0.327
Baseline mean	12.35	12.07	185946	0.330	69266	0.727	0.0254
Effect (%)	-2,5%	-9%	-6%	1%	3%	1%	2%
Panel C : Stayers							
treat * post	-0.0275***	-0.0549*	-5,989***	0.00792	1,275	-0.00557	-0.00161
	(0.0105)	(0.0323)	(2,266)	(0.00736)	(1,262)	(0.00823)	(0.00201)
Observations	83,729	53,017	84,050	84,050	84,050	84,050	84,050
R-squared	0.644	0.577	0.791	0.672	0.775	0.504	0.289
Baseline mean	12.21	11.77	138008	0.432	86261	0.819	0.0172
Effect (%)	-3%	-5%	-4%	2%	1%	-1%	-9%

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 movers (individuals who ever move in the post period), and stayers (individuals who stay in ghettos in the post period). Baseline mean reports the mean value of the outcome for treated individuals prior to the ghetto policy and serves to compute the effect in terms of growth rate relative to baseline.

B Probability to move out

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. We run 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.

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

$$move\ ever^{2010-2018} = \alpha + \beta_0\ treated_i^{2010} + \beta_1\ nw_i + \beta_2\ inactive_i^{2009} + \beta_3\ criminal\ record^{2009} + \beta_4\ log(income)^{2009} + \varepsilon_i \tag{6}$$

$$\begin{aligned} move\ ever^{2010-2018} = {} & \alpha + \beta_0\ treated_i^{2010} + \beta_1\ nw_i + \beta_2\ inactive_i^{2009} + \beta_3\ criminal\ record^{2009} + \beta_4\ log(income)^{2009} \\ & + \gamma\ treated_i^{2010} \times \left(nw_i + inactive_i^{2009} + criminal\ record_i^{2009} + log(income)^{2009}\right) + \varepsilon_i \end{aligned} \tag{7}$$

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

$$\begin{aligned} move\ ever^{2010-2018} = {} & \alpha + \beta_0\ treated_i^{2010} + \beta_1\ nw_i + \beta_2\ inactive_i^{2009} + \beta_3\ criminal\ record^{2009} + \beta_4\ log(income)^{2009} \\ & + \gamma\ treated_i^{2010} \times \left(nw_i + inactive_i^{2009} + criminal\ record_i^{2009} + log(income)^{2009}\right) \\ & + \theta\ nw_i \times \left(inactive_i^{2009} + criminal\ record_i^{2009} + log(income)^{2009}\right) \\ & + \zeta\ treated_i^{2010} \times nw_i \left(inactive_i^{2009} + criminal\ record_i^{2009} + log(income)^{2009}\right) + \varepsilon_i \end{aligned} \tag{8}$$

where γ, θ, ζ are vectors of coefficients for all the interaction terms of the treatment indicator and the non-Western indicator with 2009 characteristics.

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

	Leaving 2009 address			Leaving Denmark			Leaving public housing sector			Changing municipality		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	-0.019*	-0.092	0.047	0.003	-0.056	-0.002	-0.016	-0.100	-0.000	0.001	-0.100	-0.005
	(0.008)	(0.142)	(0.186)	(0.004)	(0.127)	(0.170)	(0.008)	(0.142)	(0.185)	(0.007)	(0.135)	(0.172)
Western	0.183***	0.145***	0.159	0.003	0.003	-0.170	0.197***	0.168***	0.085	0.109***	0.097***	-0.034
	(0.008)	(0.009)	(0.160)	(0.004)	(0.004)	(0.146)	(0.008)	(0.008)	(0.161)	(0.007)	(0.007)	(0.161)
Inactive	-0.060***	-0.067***	-0.057***	0.003	0.003	-0.017*	-0.073***	-0.079***	-0.072***	-0.015*	-0.033***	-0.043***
	(0.009)	(0.009)	(0.013)	(0.005)	(0.005)	(0.007)	(0.009)	(0.009)	(0.013)	(0.008)	(0.008)	(0.010)
Convict	0.093**	0.112***	0.158**	0.069***	0.059**	0.018	0.106***	0.107***	0.124*	0.114***	0.087**	0.096*
	(0.029)	(0.030)	(0.048)	(0.020)	(0.021)	(0.028)	(0.029)	(0.031)	(0.049)	(0.029)	(0.029)	(0.043)
log(Income)	-0.013*	-0.014*	-0.014	-0.043***	-0.046***	-0.051***	-0.019**	-0.021**	-0.024**	-0.013*	-0.016*	-0.020*
	(0.006)	(0.006)	(0.009)	(0.005)	(0.006)	(0.008)	(0.006)	(0.006)	(0.009)	(0.006)	(0.006)	(0.008)
Treated × Western		0.075***	-0.296		0.001	-0.164		0.058***	-0.227		0.023	-0.253
		(0.017)	(0.290)		(0.008)	(0.248)		(0.017)	(0.291)		(0.014)	(0.276)
Treated × Inactive		0.011	-0.014		0.000	-0.004		0.010	-0.025		0.034*	0.023
		(0.018)	(0.024)		(0.010)	(0.013)		(0.018)	(0.023)		(0.015)	(0.019)
Treated × Convict		-0.035	-0.091		0.018	0.032		-0.002	-0.026		0.049	0.092
		(0.055)	(0.085)		(0.039)	(0.051)		(0.056)	(0.084)		(0.055)	(0.080)
Treated × log(Income)		0.003	-0.008		0.005	0.000		0.004	-0.003		0.006	-0.001
		(0.011)	(0.015)		(0.010)	(0.014)		(0.011)	(0.015)		(0.011)	(0.014)
Western × Inactive			-0.019			0.040***			-0.013			0.022
			(0.019)			(0.011)			(0.018)			(0.016)
Western × Convict			-0.076			0.066			-0.025			-0.016
			(0.062)			(0.041)			(0.063)			(0.058)
Western × log(Income)			-0.000			0.013			0.007			0.010
			(0.013)			(0.012)			(0.013)			(0.013)
Treated × Western × Inactive			0.058			0.018			0.082*			0.031
			(0.036)			(0.020)			(0.036)			(0.031)
Treated × Western × Convict			0.090			-0.022			0.032			-0.084
			(0.112)			(0.076)			(0.112)			(0.109)
Treated × Western × log(Income)			0.028			0.013			0.020			0.022
			(0.023)			(0.020)			(0.023)			(0.022)
^{2}	0.040	0.041	0.042	0.019	0.019	0.022	0.048	0.049	0.049	0.019	0.019	0.020
Observations	18806	18806	18806	18806	18806	18806	18806	18806	18806	18806	18806	18806

Note: Estimated following equation 6 (columns (1), (4), (7), and (10)), equation 7 (columns (2), (5), (8), and (11)), and equation 8 (columns (3), (6), (9), and (12)). 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)-(3)), leaving Denmark (columns (4)-(6)), leaving the public housing sector (columns (7) - (9)), and for ever moving away from one's 2009 municipality (columns (10)-(12)). Standard errors in parentheses, weights based on propensity score estimated using income percentiles. * p<0.05, ** p<0.01, *** p<0.001.

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

	Leaving 2009 address			Leaving Denmark			Leaving public housing sector			Changing municipality		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treated	-0.018*	-0.073*	-0.076	0.005	0.011	0.001	-0.015	-0.063	-0.074	0.003	-0.010	0.018
	(0.009)	(0.034)	(0.043)	(0.005)	(0.021)	(0.028)	(0.009)	(0.034)	(0.042)	(0.008)	(0.030)	(0.037)
Share Western	0.146***	0.108***	-0.072*	-0.019***	-0.021***	-0.096***	0.168***	0.136***	-0.022	0.080***	0.073***	0.012
	(0.010)	(0.010)	(0.030)	(0.005)	(0.005)	(0.017)	(0.010)	(0.010)	(0.030)	(0.008)	(0.008)	(0.026)
Share inactive	-0.070***	-0.078***	-0.078***	0.019***	0.021***	-0.005	-0.078***	-0.086***	-0.090***	-0.026**	-0.041***	-0.052***
	(0.010)	(0.010)	(0.016)	(0.006)	(0.006)	(0.009)	(0.010)	(0.010)	(0.015)	(0.009)	(0.009)	(0.013)
Share convicted	0.080**	0.096**	0.107*	0.072**	0.062**	0.008	0.091**	0.095**	0.083	0.088**	0.062*	0.057
	(0.031)	(0.032)	(0.053)	(0.023)	(0.023)	(0.033)	(0.031)	(0.033)	(0.053)	(0.030)	(0.031)	(0.047)
log(Income)	-0.005***	-0.005***	-0.011***	-0.004***	-0.004***	-0.006***	-0.005***	-0.005***	-0.009***	-0.006***	-0.006***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Treated × Share Western		0.076***	0.052		0.004	0.015		0.064***	0.066		0.012	-0.064
		(0.019)	(0.060)		(0.011)	(0.036)		(0.019)	(0.061)		(0.017)	(0.054)
Treated × Share inactive		0.014	-0.017		-0.006	-0.012		0.013	-0.021		0.031	0.005
		(0.020)	(0.030)		(0.011)	(0.016)		(0.020)	(0.029)		(0.018)	(0.024)
Treated × Share convicted		-0.029	-0.061		0.018	0.041		-0.009	-0.023		0.046	0.092
		(0.059)	(0.094)		(0.043)	(0.062)		(0.059)	(0.092)		(0.057)	(0.089)
Treated × log(Income)		0.001	0.002		-0.000	0.000		0.001	0.002		-0.000	-0.001
		(0.002)	(0.002)		(0.001)	(0.001)		(0.002)	(0.002)		(0.001)	(0.002)
Share Western × Share inactive			0.006			0.047***			0.012			0.019
			(0.021)			(0.011)			(0.021)			(0.018)
Share Western × Share convicted			-0.021			0.083			0.016			0.006
			(0.067)			(0.045)			(0.068)			(0.062)
Share Western × log(Income)			0.012***			0.003***			0.010***			0.003*
			(0.002)			(0.001)			(0.002)			(0.001)
Treated × Share Western × Share inactive			0.059			0.017			0.066			0.055
			(0.041)			(0.022)			(0.040)			(0.035)
Treated × Share Western × Share convicted			0.051			-0.035			0.021			-0.080
			(0.120)			(0.086)			(0.120)			(0.116)
Treated × Share Western × log(Income)			-0.000			-0.001			-0.002			0.004
			(0.003)			(0.002)			(0.003)			(0.003)
^{2}	0.037	0.039	0.043	0.014	0.014	0.018	0.045	0.046	0.049	0.021	0.022	0.024
Observations	14977	14977	14977	14977	14977	14977	14977	14977	14977	14977	14977	14977

Note: Estimated following equation 6 (columns (1), (4), (7), and (10)), equation 7 (columns (2), (5), (8), and (11)), and equation 8 (columns (3), (6), (9), and (12)). 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)-(3)), leaving Denmark (columns (4)-(6)), leaving the public housing sector (columns (7) - (9)), and for ever moving away from one's 2009 municipality (columns (10)-(12)). Standard errors in parentheses, weights based on propensity score estimated using income percentiles. * p<0.05, ** p<0.01, *** p<0.001.

B Context Appendix

Under prime minister Lars Løkke Rasmussen (liberal party) the first "ghetto" list was introduced in 2010. The underlying strategy paper announced five main areas of intervention (The Danish Government, 2010): (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.[27]

(1) More attractive neighborhoods The 2010 Ghetto Policy 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 (i.e. flexible letting which has already been legal since 2000), and proposed making it easier to evict tenants who violated house rules.

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.[28] 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.[29] 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.[30]

[27] 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".

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

[29] 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.

[30] See interview in DR from December 1, 2020: *Antallet af ghettoer er næsten halveret: 'Hvis jeg ikke*

(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.

B.1 The Ghetto Policy 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 Policy" – 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 punished with a double sentence.[31] 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

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.

[31] 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.

Frederiksen re-branded the "Ghetto List" as "Parallel Society List". However, the term "ghetto" remains in the public perception and discussion of the policy.

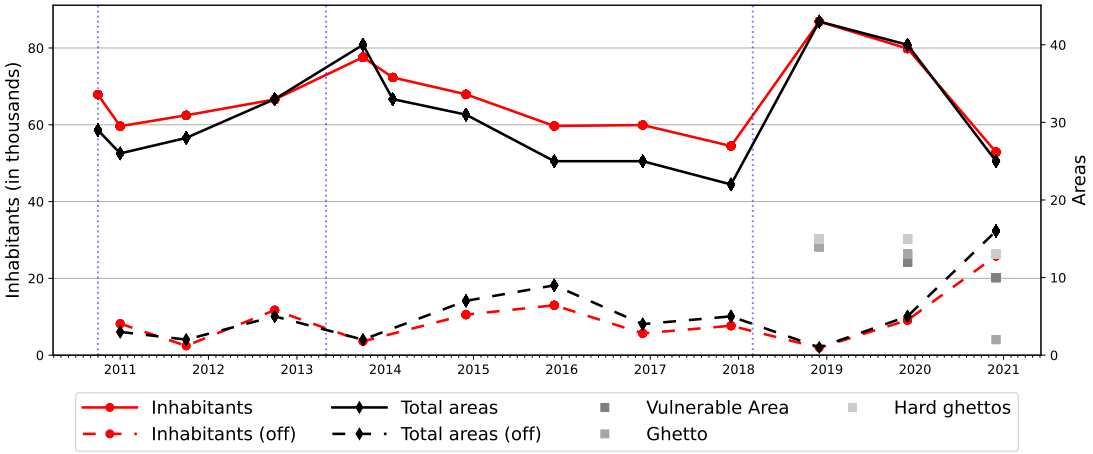
Table B1: Overview over Ghetto Plan: Classification criteria and categories

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 B2: List of countries considered Western

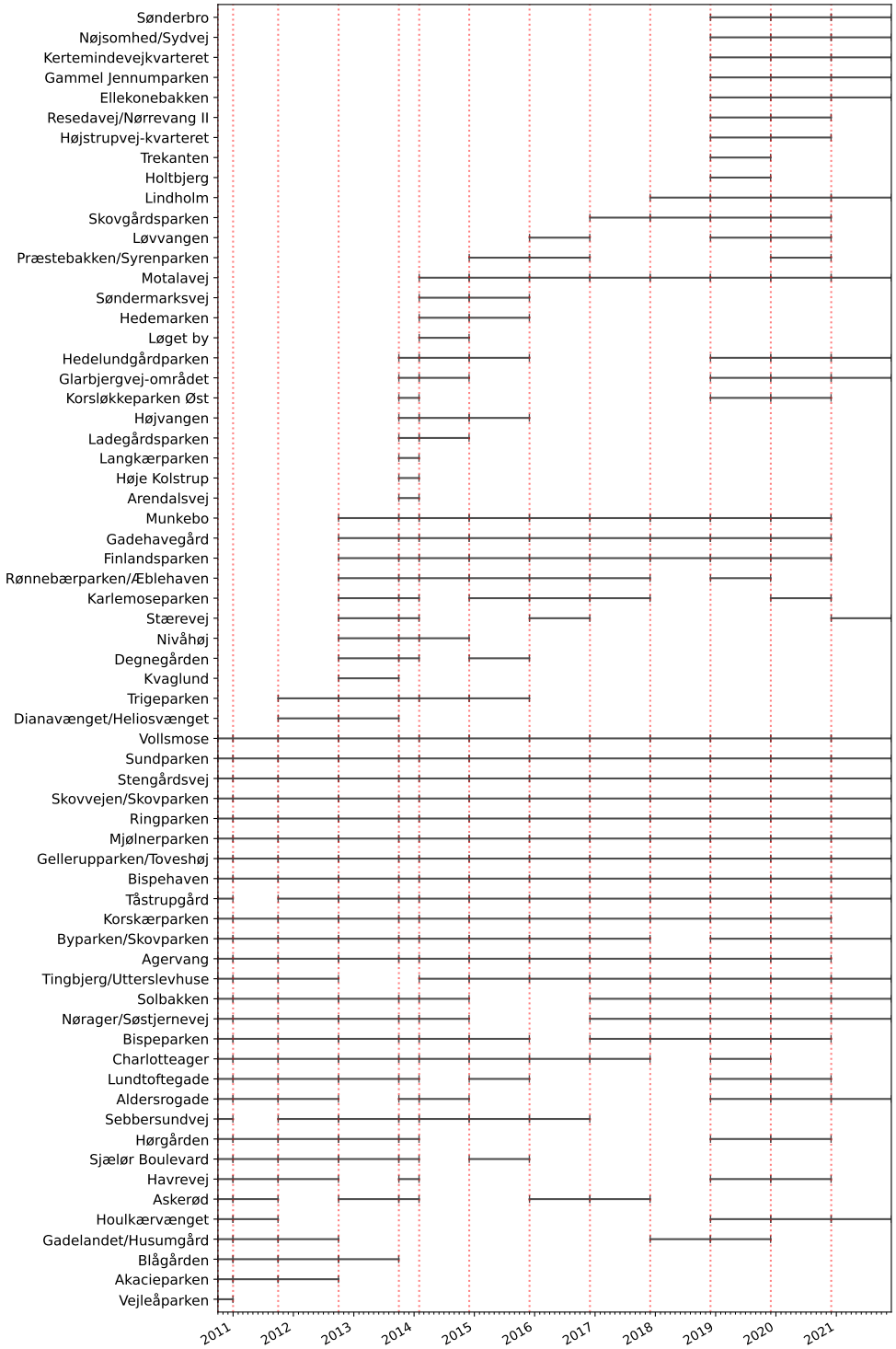
EU countries
Finland, Luxembourg, Sweden, Belgium, Bulgaria, Denmark, France, Greece, Netherlands, Ireland, Italy, Malta, Poland, Portugal, Romania, Spain, Hungary, Germany, Austria, Cyprus, Estonia, Latvia, Lithuania, Croatia (since 2013), Slovenia, Czech Republic, Slovakia, Czechoslovakia
European 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 B1: Number of areas classified and affected inhabitants



Note: Blue dotted lines mark the publication of a new strategy paper, new classification criteria, and area types (The Danish Government, 2010, 2013, 2018). The changes to classification criteria, cut-offs, and requirements are summarized in Table B1. Note that in 2018 different degrees of severity for classification were introduced, see grey squares in the figure. Instead of only one Ghetto List, three lists are published now. The least severe category are so-called vulnerable areas. If a vulnerable areas also has a high share of non-western descendants, it will be a ghetto. Any area that has been a ghetto for four consecutive years, will be a hard ghetto. Different policy implications apply, see Table B1.

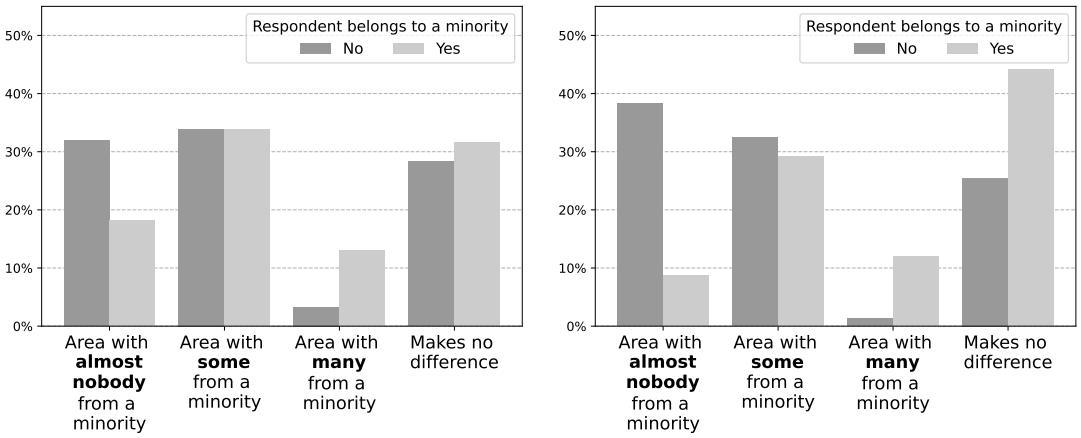
Figure B2: Treatment spells of public housing areas classified in 2010



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 B3: 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.