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ABSTRACT

Place-Based Policies in Deprived Neighbourhoods: Opportunities for Preexisting Residents and Neighbourhood Revitalisation?*

This paper asks whether Denmark's large-scale intervention in disadvantaged public-housing neighbourhoods on the "Ghetto List" in 2010 altered the trajectories of the neighbourhoods and improved economic outcomes of pre-existing residents through infrastructural improvements and social programmes. We leverage a novel geo-referenced data set linked with administrative registers and defines similar, yet untargeted neighbourhoods and their pre-existing residents as the control group. Our difference-in-difference estimates show that the programme reduced crime, both through a short-run compositional change, and through an 9.5% reduction in the likelihood of a criminal conviction among pre-existing residents, driven by those with a history of criminal activity.

JEL Classification: J6, J24, O15, R23, R28

Keywords: migration, residential segregation, human resources, economic deprivation, local community development, public policy analysis

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I. INTRODUCTION

Place-based policies, like the Opportunity Zones in the US and the Structural Funds in the EU, have been widely adopted by policymakers to enhance opportunities of residents living in socially and economically disadvantaged areas. Such areas often arise from income-based or ethnicity-based residential segregation stemming from differences in the ability to afford housing in a given area.[1] Growing up in a disadvantaged neighbourhood comes at a cost - less disadvantage leads to higher educational attainment and increases earnings (see e.g., Chetty, Hendren and Katz, 2016) and reduces criminal behaviour (Chyn, 2018; Damm and Dustmann, 2014). Thus, neighbourhood effects provide a rationale for place-based policies targeted towards residentially segregated neighbourhoods (Chyn and Katz, 2021).

An example of such a policy is the Danish "Ghetto Programme" from 2010. The "Ghetto Programme" introduced the "Ghetto List", which identified 26 public housing areas as "ghettos" (henceforth we will refrain from using "Ghetto List", "Ghetto Programme" and the term "ghetto", and simply refer to the List, the Programme and the treated areas or areas on the List). The population in the areas on the List constituted 1.1% of the national population. The areas on the List were selected among public housing areas with at least 1,000 residents, based on three statistical criteria - (i) high shares of non-EU/EEA, non-Anglo-Saxon[2] first- and second-generation immigrants[3] (henceforth referred to as non-EU/EEA, non-Anglo-Saxons), (ii) high shares of residents neither employed nor enrolled in education (henceforth referred to as inactive) and (iii) high shares of residents convicted of Penal Code violations (henceforth referred to as criminals). The Programme granted the treated areas eligibility to apply for funding of infrastructural improvements and social initiatives. It also provided subsidies to help tenants relocate to a non-listed

[1] Other explanations of the tendency of low-income and immigrant groups to settle in the same areas include preferences for having neighbours and friends of the same ethnicity (Cutler, Glaeser and Vigdor, 1999; Åslund, 2005; Damm, 2009a), reduced language and cultural barriers (Lazear, 1999), better labour market outcomes (Edin, Fredriksson and Åslund, 2003; Damm, 2009b, 2014; Beaman, 2012) or similar preferences for local private goods, e.g., restaurants or grocery stores (Waldfogel, 2008).

[2] Non-EU/EEA, non-Anglo-Saxon refers to countries that Statistics Denmark define as non-western countries. EU countries, Andorra, Australia, Canada, Iceland, Liechtenstein, Monaco, New Zealand, Norway, San Marino, Switzerland, UK, USA and Vatican City are EU/EEA, Anglo-Saxon countries. All other countries are non-EU/EEA, non-Anglo-Saxon countries.

[3] Statistics Denmark classifies a Danish resident as a descendant/second-generation immigrant if both parents are immigrants and of Danish descent if at least one parent is a Danish citizen and born in Denmark.

neighbourhood, extended opportunities to give priority to housing applicants according to specific criteria, and prohibited municipal assignment of non-EU/EEA, non-Anglo-Saxons and released inmates to the areas. The Programme mandated local governments to formulate a development plan, in cooperation with local public housing associations, with goals and initiatives for the treated areas. The police formulated and implemented a national strategy for fighting crime in the treated areas.

We examine whether the Programme altered the trajectories of the treated areas as well the economic outcomes of pre-existing residents. The Programme may affect the treated areas directly through improving economic outcomes of pre-existing residents. Moreover, it may have affected which types of households chose to live in the treated areas. The Programme tried to change neighbourhood composition through rules on eligibility for a housing vacancy and demolition of blocks of public housing for families, and displacing some of the pre-existing residents. Therefore, we distinguish between individual level effects and compositional (neighbourhood level) effects of the Programme, focusing on whether the Programme had the intended effects of reducing the probability of inactivity and criminal conviction among pre-existing residents and reduced the shares of non-EU/EEA, non-Anglo-Saxons, inactive and criminals in the areas.[4] We fix the treatment group to individuals living in the treated areas in 2009 - prior to the introduction of the Programme, henceforth referred to as the "pre-existing residents". If the individuals, who are affected by the initiatives in the Programme, choose to leave the treated areas, effects at the individual level may not translate into compositional changes at the neighbourhood level. We estimate compositional effects, where we fix the treated neighbourhoods and allow the resident population to change.

We leverage a novel geo-referenced data set that links administrative register data with the data set constructed by Damm, Hassani and Schultz-Nielsen (2021), which identifies individuals' micro neighbourhood[5] of residence. Using georeferenced data on the location of the treated areas, we identify the treated micro neighbourhoods. Clearly, the criteria used

[4] Since 2010, the Danish Ministry of Housing and Social Affairs has published an annual list of public housing areas which are identified as "Ghettos" or since 2021 as "Parallel Societies" using similar statistical criteria. In a pending case before the Court of Justice of the EU, the EU court may rule Denmark's "Parallel Societies" laws set to reduce the share of non-EU/EEA, non-Anglo-Saxons in areas on the List as discriminatory (ECLI:EU:C:2025:98, CURIA - Documents).
[5] A micro neighbourhood has at least 150 households and on average consists of 667 inhabitants in 2010.

to assign treatment introduce a selection bias. We circumvent the selection problem by using propensity score matching to match the neighbourhoods on the List with neighbourhoods that had a similar pre-Programme public housing share and population composition as neighbourhoods on the List. We successfully match neighbourhoods from 25 out of 26 treated areas for two reasons. First, we find control neighbourhoods that meet the population composition criteria but are located in a public housing area with less than 1,000 inhabitants. Second, since public housing areas consist of multiple neighbourhoods, we find control neighbourhoods that meet the criteria but are part of a larger public housing area that does not. Having matched the treated neighbourhoods, we estimate causal effects of the Programme between 2010 and 2019 using a difference-in-difference (DD) approach. Our main identifying assumption is the common trend assumption – that is the neighbourhoods on the List would have followed the same trend in absence of the Programme. We find no evidence of any divergence in pre-Programme trends.

We show that the Programme met the goal of preventing criminal behaviour among pre-existing residents. We find that after the Programme annual conviction probabilities of pre-existing residents drop by 0.15-0.18 percentage points (pp), corresponding to a 7.9-9.5% reduction. The drop is persistent and still statistically significant in years 8-10 after the implementation of the Programme. Importantly, we show that the effect is statistically significant also for individuals that stay in their initial neighbourhood, reducing their crime by 11-12%, ruling out that the reduction in criminal convictions is driven by those leaving the neighbourhoods. Estimating a triple-difference model, we find that a decrease in recidivism of previously convicted residents drives the drop in crime. Individual inactivity probabilities were left unaffected – point estimates are economically insignificant and even lower bound estimates correspond to only modest reductions in inactivity (1.74 pp) – but dynamic effect estimates suggest reduced inactivity probabilities in the long run, by about 1.1 percentage points or 2.4%.

Investigating compositional effects, we find that the reduction in the individual likelihood of conviction translates into a statistically non-significant 9% reduction in the share of criminals living in the targeted neighbourhoods. Estimating dynamic effects, we show that in the short run (year 1-4) the share of criminals drops by 12.5%, but that in the long run this effect disappears, suggesting that Programme has not been as successful in reducing the crime of those moving into the treated neighbourhoods after the Programme was implemented or have attracted more crime-prone residents. DD estimates on the characteristics on in- and out-movers suggest that the latter is not driving the divergence in results at the individual and neighbourhood level. The Programme did not affect the residential composition through other channels than the share of criminals, with the share of non-EU/EEA, non-Anglo-Saxons and the share of inactive in the neighbourhoods remaining unaffected following the introduction of the Programme.

The paper also provides evidence of physical neighbourhood changes; the Programme reduced the share of public housing by around 5%, consistent with public housing demolitions and construction of housing units for the private sector. We cannot rule out that house prices in adjacent neighbourhoods were unaffected; house price estimates are somewhat noisy though.

The paper contributes to the large literature on direct place-based policies[6] targeted towards socially and economically disadvantaged areas. Evidence from the US tends to find only modest or no effects of place-based policies on house prices (Chen, Glaeser and Wessel, 2023) and resident employment (Neumark and Kolko 2010; Freedman 2012; 2013; 2015; Hanson and Rohlin 2013; Freedman et al. 2023),[7] while European studies more often find positive effects on resident employment (Gutierrez Romero and Noble 2008; Gutierrez Romero 2009; Gobillon et al. 2012; Givord et al. 2013; Briant et al. 2015; Charnoz 2018; Criscuolo et al. 2019; Cusimano et al. 2021).[8] The extant studies on place-based policies typically estimates effects at the neighbourhood level. As noted by Taft and Emory (2017) and Chyn and Katz (2021), researchers and policy makers desperately need better data that can track the location and outcomes of former residents in the targeted areas more systematically. Without such data it is unclear if the gains accrue to pre-existing residents or reflect changes in neighbourhood sorting and accrue to in-movers. By estimating both

[6] Neumark and Simpson (2015) distinguish between two types of place-based policies: direct and indirect. Direct place-based policies are policies intended at improving the local labour market of the targeted areas, by increasing economic activities in the areas. Indirect policies instead focus on increasing access of people living in disadvantaged areas to other areas with better employment opportunities.

[7] Notable exceptions include Busso et al. (2013) and Ham et al. (2011), who find positive employment effects of federal Empowerment Zones.

[8] European studies that find zero employment effects include Accetturo and de Blasio (2012) for Italy, Brachert et al. (2019) for Germany, and Einiö and Overman (2020) as well as Gibbons et al. (2021) for the UK.

compositional (neighbourhood-level) as well as effects for the pre-existing residents this paper fills an important gap in the literature. Importantly, we can follow individuals, also after they leave the neighbourhood. Unlike studies at the neighbourhood level, we are therefore able to capture the effects on those residents that leave the area following the implementation of the policy.

The literature on place-based policies has largely focussed on labour market opportunities.[9] By showing that place-based policies can not only reduce the number of crimes committed in an area, but also the criminal behaviour of residents in targeted areas, our paper adds to a growing literature on effects of place-based policies on crime. The existing studies have investigated effects of, among others, local funding to disadvantaged neighbourhoods (e.g., funds targeted towards education, economic activity, public safety and neighbourhood revitalisation), subsidised construction of low-income housing, and community health programmes on area crime rates. Except for Kitchens and Wallace (2022), the studies find that area crime rates are reduced (see e.g., Freedman and Owens 2011; Diamond and McQuade 2019; Alonso et al. 2019; Domínguez and Montolio 2021; Shybalkina 2022).

More generally, the paper adds to the economic literature on crime prevention. A large literature has documented crime deterrent effects of policing (see e.g., Levitt 1997; 1998; Di Tella and Schargrodsky 2004; Evans and Owens 2007; Machin and Marie 2011) and effects of developmental crime prevention programmes (see e.g. the review by Koegl, Farrington, and Welch, 2023). According to routine activity theory, a criminal act may be less likely to occur in the presence of a capable guardian, for example, a teacher (Cohen and Felson, 1979). Consistent with this theory and incapacitation effects, a large number of quasi-experimental studies have found crime preventive effects of compulsory schooling (e.g., Lochner ad Moretti, 2004; Machin et al., 2011; Bell et al., 2014; Hjalmarsson et al., 2015; Beatton et al., 2018), teachers-in-service days (Jacob and Lefgren, 2003), (upper-secondary) school-dropout prevention (e.g., Anderson, 2014; Åslund et al. 2018; Huttunen et al., 2019; Larsen et al., 2022). An emerging literature documents crime preventive

[9] Few existing papers have investigated effects of place-based policies on the ethnic or socio-economic composition. A notable exception is Gonzalez-Pamillon et al. (2020). Their evaluation of an urban renewal policy in Catalonia that improved public spaces and facilities in targeted neighbourhoods finds no effect on ethnic or educational composition, except for historic districts of Barcelona.

effects of summer jobs (e.g., Heller, 2014; Heller et al., 2017), and school-year employment (e.g., Lesner, Damm, Bertelsen and Pedersen, 2022), consistent with incapacitation and role model effects. A growing literature exploits natural experiments to test whether criminal behaviour is contagious and find results consistent with the existence of endogenous peer effects in crime (e.g., Damm and Dustmann, 2014; Dustmann and Landersø, 2021).

The crime preventive effects of the Programme under scrutiny are likely to be due to the holistic approach of the Programme. First, the collaboration between groups with different responsibilities ranging from groups of professionals such as educators, anthropologists and sociologists, social workers and police officers to community-based networks of fathers (mothers). Second, the List includes multiple initiatives, such as the national plan for police responses in the areas, fast handling of cases against young troublemakers, and targeted anti-crime counselling to public authorities in the areas. It also funds programs to improve child and youth educational outcomes, such as community groups of fathers (mothers) working to improve child rearing skills of ethnic minority groups. Additionally, the List funds initiatives to enhance attractiveness of the areas and local schools, general social programmes that help youth find summer jobs, school-year employment and apprenticeships, moving subsidies for tenants, and extends existing flexible letting rules to attract more economically advantaged to the areas. The dosage of each initiative varies across the neighbourhoods on the List, in accordance with the development plan for the neighbourhood, made in cooperation between the local government and the public housing association.

The paper also contributes to a nascent empirical literature on stigma effects of place-based policies inspired by the theoretical arguments put forth by Wacquant (1993; 2007) on territorial stigmatisation. Andersen et al. (2023) and Dominguez et al. (2022) find stigma effects of the Swedish Police List of deprived neighbourhoods in both the short and medium term, while Garrouste and Lafourcade (2023) find that the stigma effects of the French Enterprise Zones are non-persistent. Despite the reduction in crime, we do not find any effect on house prices in adjacent neighbourhoods, possibly because the crime-preventive effects and stigma effects of the Programme outweigh each other.

Finally, the paper adds to the scarce literature on external benefits of place-based policies, by examining effects of the Programme on house prices in adjacent neighbourhoods. Schwartz, Ellen, Voicu and Schill (2006) find that subsidised housing investments in New York City between 1980 and 1999 reduced the price gap between units close to the project and control. Ahlfeldt, Maennig and Richter (2017) find that urban renewal policy in Berlin between 1990-2012 increased building quality and to some extent house prices but find no evidence of multiplier effects. More closely related to our study, Koster and van Ommeren (2019) find that an investment scheme to improve public housing areas in the Netherlands increased surrounding house prices.

The paper proceeds as follows. Section II provides a detailed description of the criteria used to identify areas for the List and the initiatives in and funding provided by the Programme. In Section III, we describe the data used in the study and the selection of the relevant samples, and in Section IV, we explain the estimation strategy used to causally identify the effects of the Programme at both the individual and neighbourhood levels. Section V presents and discusses the results, and Section VI concludes.

II. INSTITUTIONAL SETTING

Public housing constitutes around 20% of the housing stock in Denmark. By Danish law, the municipality can fill every fourth vacancy in the public housing stock to provide affordable housing to socially disadvantaged citizens in case they have an urgent housing need.[10] The remaining vacancies are filled in accordance with the waiting list for a given public housing area. In contrast to public housing in, e.g., the US, public housing in Denmark is not reserved for low-income residents. While the aim of public housing in Denmark is to provide decent housing to all types of households, since the 1970s the Danish public housing sector has predominantly housed lower socioeconomic groups and immigrants. Residential areas with high shares of inactive tend to be areas with high concentration of public housing (Damm, Hassani, Tranæs, and Schultz-Nielsen, 2022), and around 50% of immigrants from non-EU/EEA countries and their descendants live in public housing (Damm, Hassani, and Schultz-Nielsen, 2019).

[10] In Copenhagen, the share is even larger. The municipality of Copenhagen has one third of the public housing at its disposal.

II.1. The 2010 Programme

In October 2010, the Danish government presented its “Ghetto Programme”, with the objective to prevent emergence of parallel societies in Denmark (Danish Government, 2010). The Programme presented a preliminary “Ghetto List”. The List consisted of public housing areas with more than 1,000 inhabitants that met at least two of the following three statistical criteria:

- The share of residents of non-EU/EEA, non-Anglo-Saxon origin exceeded 50 percent; the national share is 6.6 percent.
- The inactive share among 18-64-year-olds exceeded 40 percent (calculated as the average over the previous four years); the national share is 22.6%.
- The share of criminals exceeded 2.7 percent (calculated as the average over the previous four years), corresponding to three times the national share of criminals.

The preliminary list included 29 residential areas from 17 municipalities; yet, the list was not an official one. The Danish Parliament passed the definition by law on December the 22nd, 2010, and the Ministry of Social Affairs published the first official List in January 2011. Three residential areas on the preliminary list no longer met the List’s criteria. Therefore, the first official List consisted of 26 residential areas from 15 (of 98) municipalities. Table A1 in the Appendix states the 26 areas on the first official List.

The Programme featured 32 initiatives across five focus areas. The five focus areas were i) making the treated areas more attractive and less physically and socially isolated, ii) altering the demographic profile of the treated areas, iii) improving children and youth outcomes, iv) reducing dependency on public benefits, and v) preventing social fraud and crime. The first focus area includes initiatives such as infrastructural changes and funding for general social programmes in the areas that help youth find summer jobs and school-year employment and apprenticeships, while the second focus area encompasses flexible letting rules to attract more economically advantaged to the areas, prohibition of assignment of newly recognized refugees, non-EU/EEA citizens and released prisoners to the areas on the List, as well as moving subsidies. The third focus area includes initiatives such as community groups of e.g. fathers (mothers) that work to improve child rearing skills of ethnic minority groups and funding to increase attractiveness of the areas and the local schools, and the fourth focus area contains opening of job centres in areas on the List. Finally, the fifth focus area includes the national plan for police responses in the areas (e.g., more and more visible police presence, more youth crime prevention in cooperation with other local public authorities and stakeholders, see Rigspolitiet 2011 for details), fast handling of cases against young troublemakers and targeted anti-crime counselling to public authorities in the areas. The dosage of each initiative varies across the neighbourhoods on the List, in accordance with the development plan for the neighbourhood, made in cooperation between the local government and the public housing association. Table A2 in Appendix lists all 32 initiatives by focus area.

While the overall aim of the Programme was to reduce problems in the treated areas, some of the initiatives implemented applied not only to the areas on the List, but also to low employment areas. "The Law of Public Housing" of February 11th, 2011, defined a low employment area as a public housing area with at least 1,000 inhabitants and with a share of inactive among 18-64-year-olds of more than 40 percent.[11] (Danish Ministry of Social Affairs, 2010).

Table 1 presents an overview of the initiatives available to the treated areas and low employment areas in 2011. The Programme mandated that the local government in cooperation with the public housing association define objectives for the area and propose initiatives needed to achieve the objectives. In the National Budget for 2011, the government earmarked funds of €12.9 mil. from 2011 to 2014 to support the initiatives in the development plans. An additional €6.5 mil. from the "Special Pool for the Social Area" (in Danish "Satspuljen") was reserved for creation of job centres in the treated areas and for moving subsidies to tenants leaving the treated areas. The police adopted a national plan for handling crime in the treated areas, following the introduction of the List.

[Table 1. Initiatives available to different type of areas]

On December 22nd, 2010, the Danish Parliament passed amendments to the "Law of Public Housing". The amendments reserved €24.1 mil. for infrastructure improvements in

[11] A public housing area with at least 5,000 inhabitants and with more than 30 percent of the 18-64-year olds being inactive was also classified as a low employment area.

the areas on the List each year between 2011 and 2016, and they allowed municipalities to decide that vacancies in treated areas were to be let out according to specific criteria formulated by the municipality. Already before the amendments, the "Law of Public Housing" allowed municipalities to require that public housing associations in low employment areas rejected non-working housing applicants, referred to as "flexible letting rules". The amendments prohibited the municipality to assign non-EU/EEA citizens (excluding students), released prisoners and tenants that had their lease terminated in the previous 6 months due to misconduct to both treated areas and low employment areas.[12] Finally, the amendments set aside €71.1 mil. each year between 2011 and 2014 for social programmes. Both the areas on the List, low employment areas as well as other public housing areas could apply for funding from this pool.

Throughout the 2010's shifting governments have put the social problems in the treated areas on the political agenda. New governments have formulated new List criteria. Appendix A describes in detail the changes of the criteria over time.

III. Data, Sample Selection and Summary Statistics

III.1. Data sources

To evaluate the effect of the Programme, it is necessary to identify the treatment group. The areas on the List are small, delineated public housing areas, which invalidates using typical administrative geographic units such as municipalities or postal code areas as treatment areas, because they are heterogeneous in terms of housing types and resident composition.

Instead, we use the data set constructed by Damm, Hassani and Schultz-Nielsen (2021) that identifies the individuals' neighbourhood of residence using the Danish Housing Population registries. See Damm, Hassani and Schultz-Nielsen (2021) for a detailed description of the algorithm used to cluster adjacent housing units into neighbourhoods. In total there is 8,358 (micro) residential neighbourhoods that have an organic shape.

[12] On May 15th, 2011, the parliament also prohibited assignment of refugees to areas on the List and low employment areas.

Neighbourhoods are bounded by physical barriers and unchanged over time, and relatively homogenous in homeownership and house type.[13]

We link the micro neighbourhood data with administrative register data provided by Statistics Denmark. From the Danish Population Registry, we obtain information on age, gender and origin. We can identify the type of housing that individuals live in from the Danish Housing registries, and we find information on criminal convictions and the date of crime in the Danish Criminal Registries. Finally, from the labour force registry, we obtain information on individuals' labour market status.

III.2. Identification of treatment and control neighbourhoods – propensity score matching

We use georeferenced data on public housing areas in Denmark provided by the Danish Ministry of Housing, and on the micro neighbourhoods constructed by Damm, Hassani and Schultz-Nielsen (2021) to locate the treated micro neighbourhoods. We visualise the areas on the List as polygons on a map and show the micro neighbourhoods as a grid of hectare cells.[14] By laying the grid of hectare cells on top of the layer of areas on the List, we identify the intersections between the areas and the micro neighbourhoods. We characterise a micro neighbourhood as being a treated neighbourhood if all residential buildings in at least one hectare cell of that micro neighbourhood lie within the borders of an area on the List. Figure 1 illustrates the procedure of assigning treatment to neighbourhoods in Tingbjerg/Utterslevshuse - the largest area on the List in Copenhagen municipality. The black polygon in the background is the entire area and each of the twelve colours of the hectare cells laid on top corresponds to a specific micro neighbourhood characterised as treated. The micro neighbourhoods do not perfectly overlap with the treated areas, and so a small share of the housing units in the neighbourhoods we assign to treatment, may in fact not be in the area on the List, i.e. these neighbourhoods are only partly treated. Yet, as shown in Figure 1,

[13]Damm, Hassani and Schultz-Nielsen (2021) use seven criteria to construct the micro neighbourhoods (listed in order of priority). i) The micro neighbourhoods consist of at least 150 households and ii) are unaltered over time. iii) Inhabited residential properties form micro neighbourhoods based on physical proximity and iv) physical barriers bound the neighbourhoods. Within a micro neighbourhood, v) housing units are homogeneous in terms of home ownership and housing type, while vi) the number of inhabitants is homogeneous between neighbourhoods. Finally, vii) the neighbourhoods are - when possible - compact.

[14] The micro neighbourhoods are not actually the shape of grid cells, but for data discretional reasons we visualise them using the National Square Grid - Denmark.

the overlap is quite large, and it is unlikely that the imperfect overlap attenuates our results much.

[Figure 1. Treated micro neighbourhoods in Tingbjerg/Utterlevshuse]

We repeat the procedure illustrated in Figure 1 for all 26 areas on the first official List. We assign 94 micro neighbourhoods to treatment. On average, each area on the List contains 3.6 treated micro neighbourhoods, with the median being three micro neighbourhoods.

Assigning treatment leaves us with a sample of 94 treated micro neighbourhoods and 8,264 untreated neighbourhoods. By definition the treated neighbourhoods are highly selected. Using all untreated neighbourhoods as controls would therefore severely bias the results.

To remove the selection bias, we use propensity score matching on the neighbourhood level to identify control neighbourhoods which resemble the treated neighbourhoods. We estimate propensity scores using a probit model:

$$P(D_n = 1) = \Phi\left(\beta_0 + \beta_1 PH_{n,2009} + \beta_2 NW_{n,2009} + \sum_{t=2006}^{2009} \left(\beta_{3,t} IA_{n,t} + \beta_{4,t} CR_{n,t}\right)\right) \qquad (1)$$

where D_n is an indicator for micro neighbourhood n being on the first official List in 2011. $PH_{n,2009}$ and $NW_{n,2009}$ denotes the share of public housing and the share of non-EU/EEA, non-Anglo-Saxons in micro neighbourhood n in 2009. Finally, $IA_{n,t}$ and $CR_{n,t}$ gives the inactive share and the share of criminals in micro neighbourhood n at time t. We use the share of non-EU/EEA, non-Anglo-Saxons from the previous year, as well as the inactive share and the share of criminals from the previous four years to estimate the propensity scores, since areas on the List were selected based on these criteria. Since all areas on the List are public housing areas, we also use the share of public housing in estimation of the propensity scores.

We use nearest neighbour (NN) propensity score matching with replacement to match the treated micro neighbourhoods with one neighbourhood from the untreated group. The advantages of matching with replacement are that it reduces bias and makes the matching order irrelevant (Caliendo and Kopeinig 2008).

Given the common support assumption, only treated neighbourhoods with propensity scores that overlap with the propensity scores of the untreated group can be matched (Caliendo and Kopeinig, 2008). Running the NN-algorithm, we successfully match 69 treated micro neighbourhoods with 53 untreated micro neighbourhoods leaving us with 122 micro neighbourhoods. The 69 treated neighbourhoods cover 25 out of 26 areas on the List.

Figure 2 shows the distribution of propensity scores in the control and treatment neighbourhoods before and after matching. The left panel shows the distribution of propensity scores before matching in the treated group and the control group. Most untreated neighbourhoods have very low propensity scores, which underlines the importance of matching. The right panel shows the distributions of propensity scores after matching which are almost identical for the treatment and control group.

[Figure 2. Propensity score distributions before and after matching]

Table 2 presents summary statistics across four groups of micro-neighbourhoods in Denmark using the 2009-values: all neighbourhoods (8,358), neighbourhoods that constitute our sample for neighbourhood level analysis (122), treated neighbourhoods (69) and control neighbourhoods (53). The average micro-neighbourhood in the sample has a population of 655 inhabitants. Public housing units constitute 84.0% of the residential units in the neighbourhoods. 57% of the neighbourhoods lies in an area on the 2011-List. The mean share of non-EU/EEA, non-Anglo-Saxons, the mean share of inactive and the mean share of convicted criminals are 44.5%, 45.9% and 2.6%, respectively.

[Table 2. Balancing. Neighbourhood level. 2009 values.]

In the last columns of Table 2 we report results from our tests of whether neighbourhood characteristics balance across the treatment and control groups using 2009 values; the neighbourhood characteristics reported include those used in the matching procedure. The treatment and control group are not statistically distinguishable on any of the variables we test, apart from neighbourhood population, where the treatment neighbourhoods on average have a larger population. It is also evident from Table 2, that both the treatment and control neighbourhoods are much more disadvantaged than the average neighbourhood in Denmark in 2009, with more residents being immigrants, inactive, criminal and low-educated as well as having lower income.

III.3. Sample selection

We delimit our sample period to 2006-2019 to avoid any effects caused by the Covid-19 pandemic and a 2005-change in the "Law of Public Housing". The law allowed the municipality to prohibit letting out public housing to unemployed individuals in low employment areas, previously referred to as the flexible letting rules.

In the individual-level analysis, we investigate effects only for the individuals living in the treated areas prior to treatment to avoid any endogenous selection into treatment. While initiatives in the treated areas only start with the first official list in 2011, the preliminary list from 2010 may give rise to anticipation. Consequently, we define the treatment group as the individuals living in the treatment neighbourhoods at the end of 2009. Similarly, individuals living in the control neighbourhoods at the end of 2009 constitute the control group. This leaves us with 1,096,985 observations.

The areas on the List are public housing areas and so the initiatives target public housing residents. Therefore, to reduce potential measurement error that arise from partly treated neighbourhoods, we limit the sample for the individual-level analysis to individuals living in public housing in 2009, reducing the sample to 859,157 observations. The housing type of those living in the treated areas may change over time due to initiatives in the Programme, but since we focus on the individuals living in the treated neighbourhoods before the implementation of the Programme, the public housing residents constitute the relevant sample.

We consider two different outcomes – being convicted of a crime committed in a given year and being inactive in a given year. For each outcome, we delimit the sample differently. The age of criminal responsibility in Denmark is 15 for most of our sample period, so for the conviction probability sample we include all individuals aged 15 or older. For the inactive probability sample, we delimit the sample to everyone in the working age population (ages 16-64). Table 3 gives an overview of the sample selection criteria used for the two outcomes. Panel A shows the sample selection steps that are the same for both outcomes, whereas Panel B presents the sample selection steps that differ by each outcome.

[Table 3. Sample selection criteria. Individual level.]

In the neighbourhood level analysis, we aggregate the data on individuals living in the treated neighbourhoods to the neighbourhood level for each year in our sample period, which allows us to identify compositional effects of the Programme. We do not restrict the sample to individuals living in public housing in this analysis. The Programme features initiatives designed to change the housing type and tenure form composition of the neighbourhood. To capture the effect of those initiatives, we include all types of housing at the neighbourhood level. We focus on three outcomes: the share of non-EU/EEA, non-Anglo-Saxon immigrants, the share of convicted criminals and the share of inactive residents.

III.4. Descriptive statistics

Sample selection leaves us with three samples - two at the individual level and one at the neighbourhood level. In the following section, we describe the three samples used in the analyses.

Table 4 presents summary statistics of the individual level samples, limited to the relevant age groups in 2009.[15] Columns 1−3 reports summary statistics of the conviction probability sample (ages 15 and above), overall sample and by treatment status. Columns 6 −8 summarises the sample used when considering the inactive probability as outcome (ages 16-64), overall sample and by treatment status. Of individuals in these samples, 61% live in treated neighbourhoods. About half are female, and between 44 and 49% are non-EU/EEA, non-Anglo-Saxon immigrants or descendants, depending on the age restriction. In the conviction probability sample, the mean annual conviction probability is 1.9%, and the inactive probability is 52.9%. For the inactive probability sample, those probabilities are 2.1% and 46.4%.

[Table 4. Balancing. Individual level. 2009 values.]

In columns 4-5 and 9-10, we show balance tests at the individual level (2009 values). Given the large number of individuals even small differences become statistically significant, and we indeed find that the treatment and control group are statistically

[15] Pre-existing residents in the treatment and control neighbourhoods who in the year of observation are in the relevant age group constitute the individual level samples used for impact evaluation, in total 61,131 unique individuals in the conviction probability sample and 54,683 individuals in the inactivity probability sample.

different on some characteristics such as origin, inactivity and conviction rates. However, the differences are small and not economically significant. Furthermore, as we will describe below, we employ a difference-in-difference strategy, that does not rely on the difference between treatment and control to be zero, but rather on the difference being constant during pre-treatment years.

IV. Empirical Strategy – Difference-in-Difference Estimation

For causal identification of the effects of the Programme, we use difference-in-difference (DD) methods. We estimate DD models at both the individual and neighbourhood levels to capture both individual and compositional effects of the Programme. Relocation decisions may be a direct effect of the Programme itself, and so we do not distinguish individuals staying in the treated and control neighbourhoods from those moving away. Instead, our individual level analysis investigates effects for the residents who lived in the treated areas prior to the implementation of the Programme, defined as individuals who lived in a treated neighbourhood in 2009, henceforth referred to as the pre-existing residents in the treated neighbourhoods, comparing their outcomes with those of the pre-existing residents in control neighbourhoods.

IV.1. Identifying assumptions

The first identifying assumption in DD designs is the common trend assumption. The common trend assumption implies that the control and treatment group follow the same trend prior to treatment and would have continued to do so in absence of the treatment. The second part of the assumption is impossible to test, but it is possible to test whether they follow the same trend before treatment. In Figure 3, we plot the trends in outcomes at the neighbourhood level (graphs in top row) and individual level (graphs in bottom row) for the control (blue) and the treatment group (red). An eyeball test suggests that the outcomes follow the same pre-trend. Post-treatment, the share of non-EU/EEA, non-Anglo-Saxons displays a positive trend which on average is similar across treatment and control neighbourhoods. By contrast, post-treatment, the share of inactive and criminals and the probability of inactivity and criminal conviction all display a downward trend across treatment and control neighbourhoods, with a faster decline for treatment neighbourhoods and the pre-existing residents in treatment neighbourhoods. This provides descriptive evidence that by 2019 the place-based policy had not met the goal of reducing the share of non-EU/EEA, non-Anglo-Saxons, but had successfully created new economic opportunities for the pre-existing residents and revitalised the areas on the List.

[Figure 3. Common trend]

To formally test the common trend assumption in the individual-level impact evaluation, we perform a placebo test, where we test whether assignment to the List affects the outcomes prior to reform. We estimate the following model:

$$Y_{int} = a_1 + a_2(2006_t \times D_{in}) + a_3(2007_t \times D_{in}) + a_4(2009_t \times D_{in}) + \rho_1(Post_t \times D_{in}) + Controls_{int} + N_n + \tau_t + \nu_{int}, \quad (2)$$

where Y_{int} is the outcome of interest of individual i, in neighbourhood n at time t. a_1 is a constant and D_{in} is an indicator for living in a treated neighbourhood in 2009. $Post_t$ is an indicator for being in post-treatment period, τ_t are year-fixed effects and 2006_t, 2007_t and 2009_t are dummies for being in the years 2006, 2007 and 2009; 2008 is the base year. N_n are neighbourhood-fixed effects and $Controls_{in}$ is a set of individual controls including gender, origin (native, EU/EEA, Anglo-Saxon and non-EU/EEA, non-Anglo-Saxon) and age-fixed effects. ν_{int} is an individual-specific error term at time t. If the coefficients a_2, a_3 and a_4 are statistically non-significant, the common trend assumption is likely to hold.

Figure 4 shows event study plots for the individual-level outcomes. For both the conviction probability and inactive probability, we find statistically non-significant that estimates of pre-trends.

[Figure 4. Event study plots. Individual level outcomes]

Table B1 in the Appendix presents placebo tests for pre-trends, using alternative control sets. Columns 1 and 3 show the results from estimating equation 2 on conviction probabilities and inactive probabilities. In Columns 2 and 4, we replace the neighbourhood-fixed effects and time-invariant individual characteristics by individual-fixed effects. Across specifications and outcomes, the pre-trends are statistically non-significant at any conventional level.

At the neighbourhood level, we perform similar pre-trends tests, estimating the following equation:

$$Y_{nt} = b_1(2006_t \times D_n) + b_2(2007_t \times D_n) + b_3(2009_t \times D_n) + \rho_2(Post_t \times D_n) + N_n + \tau_t + \varepsilon_{nt}, \tag{3}$$

where Y_{nt} is the outcome of interest of neighbourhood n at time t. D_n is an indicator of being on the first official List, ε_{nt} is a neighbourhood-specific error term at time t. For the common trend assumption to hold, the coefficient estimates b_1, b_2 and b_3 must be statistically non-significant.

Figure 5 plots pre-trend estimates for the neighbourhood-level outcomes as event study graphs and Table B2 shows pre-trend estimates using alternative control sets. The pre-trends tests in Figure 5 and Table B2 are all non-significant, so we conclude that the common trend assumption is satisfied at the neighbourhood level.

[Figure 5. Event study plots. Neighbourhood level outcomes]

The second identifying assumption is the no-anticipation assumption. As mentioned in Section III, we include 2010 in our post-period, since the Programme was announced in 2010. For the same reason we estimate effects for individuals living in the treated areas by the end of 2009.

The third identifying assumption is the Stable Unit Treatment Value Assumption (SUTVA). SUTVA implies that there can be no spillover effects from the treatment to the control group. If individuals move from the treatment to the control neighbourhoods or vice versa, SUTVA is likely to be violated. To investigate this concern, Figure 6 plots the cumulative Kaplan-Meier hazard function of individuals living in the treatment and control areas in 2009. The left plot shows the cumulative hazard function of the treatment group, and the right plot displays the same function for the control group. By 2019, approximately 57% have moved away from the treated neighbourhoods, whereas 63% have moved away from the control neighbourhoods.[16] The figure also shows that only a very small fraction move from a treatment to a control neighbourhood, and vice versa, eliminating this concern of violation of SUTVA.

[16]We do not allow for negative hazards, meaning that once an individual moves out of the treatment or control neighbourhood, we characterise the individual as having left the area even if the individual later moves back.

[Figure 6. Cumulative hazard functions of treatment and control groups]

Another threat to SUTVA is geographical proximity of neighbourhoods in our treatment and control group. If neighbourhoods are geographically close to each other, individuals in those neighbourhoods may share networks and compete in the same labour market. Programme effects on the treatment group may spill over to the control group, through those networks and labour market competition. Since we do not restrict geographic proximity in our matching procedure, we test the robustness of our results to exclusion of control areas in proximity to our control areas in Section V.5.

The final assumption required for causal identification in DD analysis is the absence of simultaneous shocks. Since there were no other policies implemented in the treated neighbourhoods in 2011, we are confident that our analysis meets this assumption.

IV.2. Difference-in-difference models

The pre-trends tests strongly suggest that the common trend assumption holds. Consequently, we proceed to estimate the following DD-model at the individual level:

$$Y_{int} = \alpha + \gamma_1 D_{in} + \delta_1 (Post_t \times D_{in}) + Controls_{int} + \tau_t + \mathrm{e}_{int}, \qquad (4)$$

where Y_{int} is the outcome of interest of individual i in neighbourhood n at time t. α is a constant, D_{in} is an indicator of living in a treated neighbourhood in 2009, $Post_t$ is an indicator for being in post-treatment period, while $Controls_{int}$ is the same set of individual controls as used in Equation 2. τ_t is a vector of year-fixed effects to account for the business cycle. e_{int} is an individual-specific error term at time t. δ_1 captures the treatment effect at the individual level.

In a second specification, we replace the treatment indicator by neighbourhood-fixed effects, where we fix the neighbourhood to an individual's neighbourhood of residence in 2009. This accounts for time-invariant neighbourhood characteristics such as location characteristics, e.g. commuting distance to the centre of the commuting area. Finally, in a third specification, we replace the neighbourhood-fixed effects and time-invariant individual controls by individual-fixed effects to account for time-invariant unobserved individual characteristics such as work attitude and risk preferences. Following Abadie et al. (2023), we cluster standard errors at the treatment area level.[17]

We are also interested in whether the Programme affected the residential composition in the treated neighbourhoods. To capture the compositional effects of the Programme we therefore estimate the following DD-models at the neighbourhood level:

$$Y_{nt} = \beta + \gamma_2 D_n + \delta_2 (Post_t \times D_n) + Controls_{nt} + \tau_t + \epsilon_{nt}, \quad (5)$$

where Y_{nt} is the outcome of interest of neighbourhood n at time t. β is a constant, D_n is an indicator of being on the first official List, $Post_t$ is an indicator for being in post-treatment period, $Controls_{nt}$ are a set of time-varying municipality-level controls that account for local economic shocks and τ_t are year-fixed effects. ϵ_{nt} is a neighbourhood-specific error term at time t. δ_2 captures the effect of assignment to the List in 2011 at the neighbourhood level.

One may be concerned that the Programme affects the municipality characteristics through its effect on the treated areas, although they constitute a small part of municipalities. In a second model, we therefore replace the time-varying municipality characteristics by municipality-fixed effects to account for fixed differences between the municipalities in institutions and local amenities. In our final and preferred specification, we replace the municipality-fixed effects with neighbourhood-fixed effects. As in the individual level analysis, we cluster standard errors at the treatment area level.

V. RESULTS

V.1. Individual-level results for the pre-existing residents in the treated areas

In the following section, we present the results from our DD estimations. We begin by evaluating the individual effects of the Programme on labour market outcomes of the pre-existing residents in the treated areas.

Table 5 presents the estimated effects of the Programme on the probability of being convicted of a crime committed in a given year, in Columns 1-3, and on the probability of being inactive in Columns 4-6. In Columns 1 and 4, we control for age, gender, origin and

[17] We assign control neighbourhoods to the same cluster if they are matched with treated neighbourhoods from the same treatment area.

yearly shocks. In Columns 2 and 5 we add neighbourhood-fixed effects, and in Columns 3 and 6, we replace the individual, time-invariant characteristics and neighbourhood-fixed effects by individual-fixed effects.

[Table 5. Difference-in-difference results. Individual level.]

We find that the Programme was successful in reducing criminal behaviour of pre-existing residents. In Columns 1 and 2, conviction probabilities drop by 0.18 pp, corresponding to a 9.5% reduction compared to the 2009-mean. The effect is statistically significant at the five percent level. When we add individual-fixed effects, the estimated reduction shrinks slightly to 0.15 pp (7.9%), while remaining statistically significant at the ten percent level (p-value of 0.051).

The crime reduction, we find, adds to a growing number of studies showing that place-based policies can be effective in reducing crime in affected areas (Alonso et al. 2019; Domínguez and Montolio 2021; Diamond and McQuade 2019; Freedman and Owens 2011; Shybalkina 2022). However, what we measure is different from existing studies. Whereas the existing literature shows that place-based policies can reduce crimes committed in affected areas, we provide evidence that the Programme reduced criminal behaviour of the *pre-existing residents* in the area. While the two measures are likely positively correlated,[18] they are different in that the area crime rate measures how much crime is committed in an area, and we measure instead whether individuals living in treated neighbourhoods are convicted of crime, irrespective of where they have committed it.

Part of the Programme was increased police presence in the treated neighbourhoods. Assuming that more policing increases the risk of conviction, the classic crime model by Becker (1968) and Ehrlich (1973) predicts that returns to crime should decrease, leading to a reduction in criminal activities. Police presence in the areas on the List reduces incentives to commit crime inside those areas, but not in other areas, where the police do not increase their presence. We would therefore expect that at least part of the crime reduction is driven by fewer crimes committed in the treated areas. Furthermore, while police presence reduces incentives to commit crime, it simultaneously increases the detection rate and thereby the conviction rate. Such increase in detection rates would bias

[18] Damm and Dustmann (2014) reports a correlation between the number of reported crimes per capita and the number of convicted criminals per capita of 0.64 at the municipal level in Denmark between 1986-1998.

our estimates towards zero. The estimated effect of the Programme on the probability of being inactive is both statistically non-significant and very small – the Programme reduced the probability of being inactive by 0.62 pp (0.17 pp, when estimating it with individual-fixed effects). Looking at 95%-confidence bands, we can rule out an inactivity drop of more than 1.74 pp and an inactivity increase of more than 0.5 pp (with individual-fixed effects the 95%-confidence band is -1.23 – 0.89 pp).

The results from our basic DD model that assumes constant treatment effects over time are in line with evidence on similar Italian community policies with local development plans, Patti Territoriali (Accetturo and de Blasio 2012) and Territorial Integrated Planning (Cusimano et al. 2021) and with evidence on the Single Regeneration Budget policy in the UK (Gibbons et al. 2021), which find that these policies left employment unaffected. Gutíerrez Romero (2009) finds positive employment effects of the "New Deal for Community" in the UK only for a subset of individuals on disability benefits or enrolled in education.

Recall from Section IV that geographical proximity between treated and control neighbourhoods could violate the SUTVA, and hence, bias our baseline estimates if the Programme effects on the treatment group has spillover effects on the nearby control group. Since a small number of neighbourhoods in the control group are indeed located close to a treated neighbourhood, we test whether our baseline estimates are robust to removing close-by controls.

In Table 6, Columns 1 and 2, we repeat our baseline estimates of Programme effects on the individual level outcomes. In Columns 3 and 4, we report estimates after excluding control neighbourhoods that are located within 500 metres from a treated neighbourhood. In Columns 5 and 6, we report estimates after excluding control neighbourhoods that are located within 1 km from a treated neighbourhood. Panels A and B report results for conviction and inactivity probabilities, respectively. Comparison of the estimates across columns reveals that our baseline estimates are robust to removal of such neighbourhoods.

[Table 6. Robustness. Remove close-by controls. Individual level.]

While some initiatives in the Programme are fast to implement, such as social initiatives to help youth find school-year employment and internships, national plan for police responses in the areas which included more police presence, fast handling of cases against young troublemakers, expanded access to CCTV in the treated areas, suspended registration of first criminal ruling on young people's criminal record, other initiatives like infrastructural changes take time to implement. To account for differences over time, we estimate DD models with dynamic treatment effects. We split the post period into three periods 2010-2013, 2014-2016 and 2017-2019 and interact an indicator for each of the three periods with the treatment indicator to estimate effects of the Programme in the short, medium, and long run.

Table 7 displays the estimated dynamic effects at the individual level. Column 1 shows the estimates of the dynamic effects of the Programme on the conviction probability, using a specification with neighbourhood-fixed effects. The short, medium and long run effects are a 0.18, 0.2 and 0.17 pp (9.5, 10.5 and 9.0%) reduction in the conviction probability, respectively. All three effects are statistically significant at the ten percent level. We find similar, yet slightly smaller estimates using a specification with individual-fixed effects (Column 2).

[Table 7. Dynamic treatment effects. Individual level.]

While we cannot isolate the effect of each initiative, the immediate reduction in conviction probabilities suggests that initiatives that were fast to implement played an important role in reducing resident's criminal behaviour. Other initiatives such as infrastructural changes, takes longer to implement and so one would expect effects of those initiatives to only emerge in the medium or long run.

Columns 3 and 4 presents the estimation results of the short-, medium- and long-run effects of the Programme on the probability of being inactive. While the estimated reduction grows over time, it is statistically non-significant in the short, medium, and long run. The long-run point estimate suggests the largest effect; the lower bound of the 95%-confidence band imply a 2.54 pp (5.1%) reduction in inactivity probabilities.

Table B3 in the Appendix shows the fully specified dynamic models, where the treatment effect is allowed to differ in each year. The emerging pattern is similar to that found in Table 7, with a negative point estimate of treatment on the conviction probability and the inactive probability in all years, except one. Interestingly, the negative point estimate of treatment on the inactive probability is statistically significant at the ten percent level in 2016 and 2017 with a point estimate of around -1.1 pp, corresponding to about

2.4%; the point estimate remains at that level until the end of our observation period, which provides suggestive evidence of a reduction in the longer run.

Viewed together, the individual-level effects suggest that the Programme reduced crime propensity through initiatives that were fast to implement such as general social programmes for youth, increased police presence, fast handling of cases against young troublemakers and targeted anti-crime counselling to public authorities in the areas, but initiatives that take time to implement such as infrastructural changes and initiatives that take time to produce effects such as better parenting skills and higher quality of local schools may have contributed to reducing crime propensities in the long-run.

V.2. Neighbourhood-level results

We now turn to effects of the Programme on residential composition. We evaluate the effects of the Programme at the neighbourhood level using the share of non-EU/EEA, non-Anglo-Saxons, the share of criminals and the inactive share as outcomes.

Table 8 presents DD estimates of the effect of a neighbourhood being on the List in 2011 on the share of non-EU/EEA, non-Anglo-Saxons in Columns 1-3, the share of convicted criminals in Columns 4-6 and the inactive share in Columns 7-9. Columns 1, 4 and 8 control for yearly shocks and time-varying municipality characteristics. Instead of control for time-varying municipality characteristics, Columns 2, 5 and 8 use municipality-fixed effects, while Columns 3, 6 and 9 use neighbourhood-fixed effects.

[Table 8. Difference-in-difference results. Neighbourhood level.]

The point estimates indicate a 1.1 pp (2.5%) increase in the non-EU/EEA, non-Anglo-Saxon share, but the effect is not statistically significant at any conventional level. Our results suggest that the Programme did not reach its intended target of reducing the share of non-EU/EEA, non-Anglo-Saxon residents. The 95%-confidence bands provide us a lower bound of -1.5 pp (-3.4%), so at the very best, the Programme led to modest reductions in the share of non-EU/EEA, non-Anglo-Saxon residents. The upper bound is a 3.72 pp (8.4%) increase.

Our findings mirror those of Gonzalez-Pampillon et al. (2020), who investigates an urban renewal policy in Barcelona, and find no evidence that the policy changed the ethnic composition of the affected neighbourhoods. Origin is unchangeable, so only changes in the ethnic composition of inflow into and/or outflow from the neighbourhoods can affect the share of non-EU/EEA, non-Anglo-Saxons. Yet, our findings depart from those of Taft and Emory (2017), who find that the US federal redevelopment of public housing via HOPE VI boosted white population shares and reduced poverty rates relative to comparable public housing, however, primarily produced due to displacement of minority and poor residents rather than by the net influx of more advantaged residents.

Since 1986, refugees in Denmark have been subject to spatial dispersal across municipalities upon receipt of asylum (Damm 2009a, 2009b; Azlor, Damm and Schultz-Nielsen, 2020). However, the Programme prohibited municipalities to assign newly recognised refugees and non-EU/EEA immigrants to live in the areas on the List upon receipt of asylum, which should everything else equal reduce the inflow of non-EU/EEA, non-Anglo-Saxons. Andersen (2017) shows that white avoidance[19] is the most important factor explaining residential segregation in Denmark, which could explain why restrictions on municipal assignment of residents to treated neighbourhoods did not lower the non-EU/EEA, non-Anglo-Saxon share. Furthermore, analysing residential location of male refugees who were spatially dispersed across municipalities upon asylum during 1986-1998, Damm (2014) finds that 35 percent of male refugees live in a socially deprived neighbourhood six years after asylum compared to only 17 percent at initial placement, suggesting that the inflow of non-EU/EEA, non-Anglo-Saxons consists primarily of immigrants, who are not subject to municipal placement, but have already lived in the country for some years, reached the top of the regular waiting list for public housing in a treated area and perhaps found a job.

In Table 6, we tested the robustness of our individual level results to leaving out close-by-controls. Table 9 performs the same robustness test for the neighbourhood-level outcomes. Again, the results do not change when we remove close-by control areas.

[Table 9. Robustness. Remove close-by controls. Neighbourhood level.]

According to our baseline DD model that assumes a constant treatment effect, the Programme reduced the share of criminals in the neighbourhood by 0.23 pp, corresponding approximately to a 9% reduction. While the effect is not statistically significant at any

[19] White avoidance refers to natives avoiding neighbourhoods with high concentrations of ethnic minorities.

conventional level (p-values are between 0.110-0.119), the ten percent reduction is similar in size to the drop we find in the individual conviction probabilities.

We find no evidence that the Programme affected inactivity, when estimating the effect on the share of inactive in the neighbourhood. The point estimates are all statistically non-significant and small, and the 95%-confidence bands allow us to rule out reductions larger than 2.71 pp (5.9%) and increases of more than 1.54 pp (3.4%).

Table 10 presents estimates of dynamic effects of the Programme, where we split the post period into a short-run, medium-run and long-run periods as in Table 7. We find that the Programme reduced the share of criminals in the short run by 0.32 pp (12.5%). In the medium run, the estimated effect is similar in size (0.28 pp), although statistically non-significant, but in the long run the effect completely disappears. For the share of non-EU/EEA, non-Anglo-Saxons and the share of inactive, the Programme had no statistically significant effect in the short, medium or long run. In Table B4 we report the full dynamic model, where we allow the effect of the Programme to vary each year. The emerging pattern is similar to that found in Table 10, with a positive point estimate of treatment on the share of non-EU/EEA, non-Anglo-Saxons in all years, and a negative point estimate of treatment on the share of convicted criminals in all years except one, and a negative point estimate of treatment on the share of inactive in all years.

[Table 10. Dynamic treatment effects. Neighbourhood level.]

V.3. Neighbourhood changes (mechanisms)

Sections V.1 and V.2 presented the main results at the individual and neighbourhood levels. In the following section, we zoom in on Programme effects on neighbourhood changes. The Programme allowed local authorities to demolish high-rise buildings of public housing and instead construct lower-density owner-occupied and private rental housing in the treated neighbourhoods. In Table 11, we document this change in the treated neighbourhoods. On average, the Programme reduced the share of public housing by 3.9 pp during our observation period (statistically significant at the ten percent level), thereby potentially changing the residential composition of in- and out-movers.

[Table 11. Mechanisms. Share of public housing.]

In Sections V.1 and V.2, we showed that the Programme reduced individual conviction probabilities. This reduction in individual conviction probability we found to be stable over time, whereas the share of criminals in neighbourhood was only reduced in the short run. There are two possible explanations. Either the Programme changed the composition of in-movers and/or out-movers in terms of crime propensities, or the Programme was only effective in reducing the conviction probability of pre-existing residents. As time goes by, the pre-existing residents in the neighbourhood constitute a smaller share of the neighbourhood population, and their reduction in crime matters less for the share of criminals in the neighbourhood, which could explain the divergence in the results at the individual and neighbourhood level.

To test whether the Programme affected the current characteristics of in- and out movers, we estimate the same DD models as in the neighbourhood analyses, but where we estimate treatment effects on current characteristics of in- and out-movers, respectively. Table 12 presents the DD results from these analyses - Panel A for the in-mover characteristics and Panel B for the out-mover characteristics. We report estimates using OLS and WLS.[20]

[Table 12. Mechanisms. In- and out-mover characteristics.]

We do not find any statistically significant effects at the five percent level. If anything, the Programme reduced the share of convicted criminals among in-movers, consistent with initiative of no assignment of released prisoners. For the share of convicted criminals, our results indicate that Programme-induced changes in criminal propensity of in- and out-movers do not drive the difference in the effect at the individual and neighbourhood level. Taken at face value, the OLS (WLS) point estimate implies that in-movers after the Programme had 0.33 pp (0.55 pp) lower crime propensities compared to 0.16 pp (0.18 pp) among out-movers, albeit imprecisely estimated. For in-movers the 95%-confidence intervals rule out Programme effects of larger than 0.35 pp increase in the conviction probabilities of in-movers, and smaller than 0.69 pp decreases in the criminal propensity of out-movers, so our findings should merely be seen as indicative.

[20] Since neighbourhoods have similar population size, we do not employ WLS in the main neighbourhood analyses. However, neighbourhoods are not necessarily similar in the number of people moving in and out.

For the share of non-EU/EEA, non-Anglo-Saxon the lower and upper bound effects are -2.87 and 6.33 pp among in-movers and -2.24 and 3.96 pp among out-movers (from 95%-confidence intervals). Among in-movers the lower and upper bound for the effect on share of inactive are -4.43 and 1.09 pp, whereas for out-movers it is -3.36 and 2.02 pp.

V.4. Heterogeneity and external benefits

In Section V.3 we estimated Programme effects on the current characteristics of in- and out-movers of the treated neighbourhoods and on the share of public housing. While we did not find any statistically significant evidence that the Programme changed the characteristics of in- and out-movers, the Programme may still affect stayers and out-movers differently. Once residents relocate (whether forced or voluntarily), they no longer reap the benefits associated with the place-based initiatives in the Programme, but they might gain from moving to neighbourhoods with better economic opportunities, or they might simply move because they gained in the first place, i.e., found a job or received a housing subsidy so that they can afford to move to a better neighbourhood.

In Table 13, we therefore test if the effects of the Programme vary by whether individuals leave the treatment area or not. We use two definitions of leavers. Panel A defines individuals as leavers from the time of leave and onwards. Panel B classifies those that leave at some point in the sample period as leavers. Columns 1 and 2 show the estimates for the conviction probability, and Columns 3 and 4 for the inactive probability.

[Table 13. Heterogeneity analysis. Stayers.]

The Programme reduced the conviction probability of stayers by 0.16-0.22 pp (0.21-0.22 pp for those staying throughout the period), and the effect is not statistically distinguishable from the effect on leavers. We can thus rule out that the baseline finding that the Programme reduced the conviction probability is driven by individuals moving away from the neighbourhood. Assuming a constant treatment effect, the inactivity probability is unaffected by the Programme, irrespective of stayer status. However, taking the point estimates at face value, if anything, the Programme has reduced the inactivity probability among leavers only, in which case movers are positively selected. Since relocation may be an endogenous effect of the Programme, the results in Table 11 are only indicative of the effect on stayers, and one cannot interpret them as causal.

In Table 14, Columns 1 and 2, we allow the effect on the conviction probability to vary by criminal history, where individuals previously convicted of a crime committed in the four years prior to the implementation of the Programme are defined as having a criminal history. We test for heterogeneous treatment effects by criminal history, since previously convicted individuals are particularly crime prone. We find that the Programme reduced the conviction probability by 1.91 pp for individuals with a criminal history, in the neighbourhood-fixed effect model. The effect is statistically significant at the five percent level, showing that the Programme successfully reduced recidivism. Importantly, the additional effect on those without a criminal history is also statistically significant and positive. Adding the main and the additional effect, we find that the Programme had no effect on those without a criminal history, showing that the reduction in recidivism drives the crime drop. Once we add individual-fixed effects, the main effect and the additional effect, both become statistically non-significant, but they still follow the same pattern as in the neighbourhood-fixed effect model. Furthermore, the effective variation for identifying the effect of the Programme comes from the treatment area level not the individual level. Adding the individual-fixed effects could soak up some of the within-individual variation and decrease statistical efficiency by reducing the degrees of freedom.

[Table 14. Heterogeneity analysis. Criminal or inactivity history.]

The Programme did not affect inactive probabilities, when using the entire sample and assuming a constant treatment effect over time. Following the approach of Gutiérrez-Romero (2009), we test for heterogeneous treatment effects by whether individuals were inactive in 2009. Table 12, Columns 3 and 4 show the estimated heterogeneous effects of the Programme on the inactive probability by 2009-inactivity status. The Programme neither affected the inactive probability of the individuals that were inactive in 2009 nor of the active group in that year.

We also test for heterogeneous effects across other dimensions. We find no evidence that the individual-level effects of the Programme vary by immigrant status (Table B5), gender (Table B6) or age (Table B7).

We report DD-estimates of the effects on the logarithmic value of house prices in neighbourhoods adjacent to the baseline treatment group (compared to neighbourhoods adjacent to the baseline control group) in Table 15. Since the Programme targeted neighbourhoods with very high concentration of public housing and an ignorable share of owner-occupied housing, we cannot estimate effects of the Programme on house prices in the treated areas. Instead, we estimate spillover effects to nearby neighbourhoods. The house prices may have increased due to the crime-preventive effects of the Programme found earlier. On the other hand, there may be negative stigma effects of the Programme on house prices in adjacent neighbourhoods. Indeed, an emerging literature shows stigma effects of place-based policies. Andersen et al. (2023) and Dominguez et al. (2022) find short- and medium-run stigma effects of the Swedish Police List of deprived neighbourhoods, a list similar to the List we study, while Garrouste and Lafourcade (2023) find that the non-persistent stigma effects of the French Enterprise Zones.

The estimates are statistically non-significant, irrespective of whether we limit adjacent neighbourhoods to neighbourhoods within 1 km or 500 m from a treated or control neighbourhood and whether we restrict the sample according to the share of owner-occupied housing or not. Yet, the standard errors are quite large. For neighbourhoods within 1 km of a treated neighbourhood, the lower bound of house price increases is between -4.3 and -3.7%, while the upper bound varies between 9.2 and 9.8%. In the specifications, using only properties within 500 m of the treatment areas, the lower and upper bounds are -11.6–-8.9% and 5.70-10.3%, respectively.

[Table 15. House price spillovers to nearby neighbourhoods.]

V.5. Benefit and Cost per crime prevented – back-of-the-envelope calculations

Section V.1 showed that the Programme was successful in reducing crime propensities of pre-existing residents in the treated neighbourhoods. What are the benefits to society of the prevented crimes and how large are the cost per crime prevented? We now discuss the benefits to society of the prevented crimes and estimate the cost per conviction prevented by the Programme.

The benefits of the Programme are the foregone costs of crime. These costs are multidimensional, including cost for police and courts, incarceration costs, direct and psychological costs of victims, lost labour market earnings of both offender and victim and broader societal costs (e.g., perceived safety of population), making them hard to quantify. McCollister et al. (2010) estimate the cost of crime (tangible and intangible) in the US context to be approximately €144,000 for assault, €57,000 for robbery, €8,700 for burglary, €6,600 and €4,800 for theft.[21] Jacobsen and Ibsen (2021) estimate the direct cost in the Danish judicial system to be €8,100 for a violent or sexual crime conviction, €3,700 for a property crime conviction and €230 per day of incarceration. The average direct cost for victims of property crimes in Denmark over the period 2010-2019 was €3,700 for burglary, €1,900 for theft and €1,100 for vandalism. (Danish Ministry of Justice 2021). In 2009, violent and sexual crimes constituted 25% of all Penal Code convictions in Denmark, while property crimes constituted 69%. Of the convictions 29% resulted in an unconditional incarceration sentence. Regarding the lost labour market earnings of the offender, the quasi-experimental neighbourhood effects study by Damm and Dustmann (2014) estimates that for male refugee youth aged 15 to 21, a one standard deviation higher youth violent crime conviction rate in the assignment neighbourhood increases the probability to be convicted by 4.5 pp, and reduces the probability to be employed in the age range 23–25 by 1.6 pp, possibly due to long-term labour market effects of criminal behaviour and exposure to delinquent youth in general.

To calculate the cost per conviction prevented, we first need to calculate the total number of convictions prevented. From Table 5 we know that the Programme reduced individual conviction probabilities by 0.15-0.18 pp. Multiplying this with the total number of post-Programme observations in the treatment group, 309,927, we calculate that the Programme prevented between 457 and 567 convictions.[22]

As described in Table 1, the Programme invested €24.1 million annually between 2011-2016 in infrastructural improvements, €12.9 million in total for reaching objectives in mandatory development plans, and €6.5 million in total for creation of job centres and moving subsidies. As any other public housing area, the areas on the List could also apply to the National Building Fund's (NBF) pool for social programmes (€71.1 million annually). Using data on grants provided by NBF between 2011 and 2015, we calculate an average grant per resident in the treatment group of DKK 3,578 compared to DKK 3,230

[21] Nominal USD values are converted to 2024-values using the Bureau of Labor Statistics inflation calculator - http://www.bls.gov/data/inflation calculator.htm and converted to Euro using data from Statistics Denmark on the USD-EUR exchange rate.

[22] Our results in Table 5, uses a dummy for any conviction as outcome. As a result, we underestimate the number of convictions prevented, as we count each individual only once per year, meaning that individuals with multiple convictions for crimes within the same are only counted once.

in the control group, corresponding to an *additional* spending per resident in the treatment group over this period of €58, or a spending difference of €2.8 million in total. Scaling the cost by the share of residents that we match, the total cost of the Programme in the treatment group sums to €121 million. This corresponds to approximately €294,000-365,000 per crime prevented – a very high cost.

The above calculation includes the Programme costs for infrastructural improvements. However, the infrastructural improvements are long-term investments, which takes time to materialise, and can affect the treated neighbourhoods for a very long period beyond our sample period. Additionally, these improvements may well have benefits that extends primarily to other outcomes. The more short-term and fast implementable investments are more likely to have been the ones affecting crime, since we find effects immediately after the Programme introduction. Only considering the costs of these initiatives, by excluding the infrastructural improvements, we estimate the cost per crime prevented to be roughly between €39,000-49,000.[23]

VI. DISCUSSION AND CONCLUSION

Can place-based policies in deprived neighbourhoods improve the trajectories of the treated areas as well the economic outcomes of pre-existing residents? To answer that question, we draw on Denmark's large-scale "Ghetto Programme" in disadvantaged public-housing neighbourhoods on the "Ghetto List" first published in 2010. The areas on the List faced stricter criteria for letting out housing to improve the socio-economic mix of residents and were eligible to apply for reserved funds to improve the areas, both through physical changes, social interventions targeting the residents and subsidies to tenants to move to a non-listed neighbourhood. Municipalities were mandated to formulate development plans for the treated areas in cooperation with local public housing associations, and the police adopted a new strategy for tackling crime in the areas on the List. To answer the question of whether potential gains accrue to pre-existing residents or reflect changes in

[23] The back-of-the envelope calculations do not take into account any additional cost associated with the national police plan.

neighbourhood sorting and accrue to in-movers, we estimate effects on outcomes of the pre-existing residents as well as on neighbourhood composition.

Using a novel georeferenced data set linked with administrative registers between 2006 and 2019, we find that Denmark's large-scale "Ghetto Programme" improved economic outcomes of the pre-existing residents in both the short and long-term and had significant effects on the economic composition of neighbourhoods but no effect on the ethnic composition of neighbourhoods. Our sample includes all, but one area on the List. We identify the control group of similar, yet untargeted areas, using propensity score matching. We show that the treatment and control areas follow the same pre-trends, which allows us to isolate the effects of the Programme from other changes that likely would have occurred in the absence of the Programme, such as positive net-migration from non-EU/EEA, non-Anglo-Saxon countries, increasing labour demand and lower property crime rates. We find that the Programme reduced conviction probabilities of the pre-existing residents by about 9.5%, and that the effect was constant over time. Allowing for dynamic effects of the programme, results suggest that the Programme also reduced inactive probabilities of the same individuals by about 2.5%, but only in the long run. Consistent with the individual-level results, we find that the share of convicted criminals at the neighbourhood level dropped by about 12.5% in the short run, while the share of non-EU/EEA, non-Anglo-Saxons and the inactive share in the neighbourhoods remained unaffected. In the long run, the effect on the share of criminals vanishes.

We further provide evidence of physical neighbourhood changes; the Programme reduced the share of public housing, potentially changing the residential composition. However, in- and out-mover analyses suggest that the absence of a long-run effect in the share of criminals cannot be explained by more crime-prone in-movers or less crime-prone out-movers following the introduction of the Programme. Instead, the divergence in the results at individual level and neighbourhood level is likely to be driven by the Programme being ineffective in reducing criminal behaviour of in-movers.

Importantly, we show that movers do not drive the effect on conviction probabilities. The effect remains statistically significant for those staying in the neighbourhood when we split our sample into stayers and movers. Further heterogeneity analyses show that the

Programme reduced recidivism of those previously convicted of a crime, and that the recidivism decrease is the main driver of the drop in conviction probabilities.

We cannot reject that house prices in adjacent neighbourhoods were unaffected, possibly because the stigma effects of the Programme outweighed the crime-preventive effects of the Programme. But one should be cautious in drawing strong conclusions on the external benefits based on our house price results, due to large confidence intervals.

The findings of the paper highlight the potential of place-based policies to reduce criminal behaviour of residents in areas of economic deprivation. Possibly mechanisms include crime preventive effects of the social programs and the national police strategy that create a social multiplier effect in case of endogenous peer effect effects in crime.

Yet, the paper also documents that the Programme did not reach its aim of reducing the share of criminals and inactive residents in the long run and attracting residents of native or EU/EEA, Anglo-Saxon origin to the areas on the List. A potential explanation for the latter is the name of the List" which is likely to contribute to stigmatization which is probably why the Danish Ministry of Housing and Social Affairs changed the name of the List to "List of Parallel Societies" in 2021. Policy makers could also consider establishing more effective initiatives for reducing dropout from education and promoting employment of residents in the areas and to improve affordable housing possibilities of non-EU/EEA, non-Anglo-Saxon immigrants and descendants in non-listed areas. On a more general note, in the pending case before the Court of Justice of the EU, the EU Court may rule Denmark's "Parallel Societies" laws set to reduce the share of non-EU/EEA, non-Anglo-Saxons as ethnically discriminatory according to the EU Directive on ethnic equality.

Viewed together, the individual-level effects suggest that the Programme reduced crime propensity through initiatives that were fast to implement such as general social programmes for youth, increased police presence, fast handling of cases against young troublemakers and targeted anti-crime counselling to public authorities in the areas, but initiatives that take time to implement such as infrastructural changes and initiatives that take time to produce effects such as better parenting skills and higher quality of local schools may have contributed to reducing crime propensities in the long-run.

The paper provides detailed evidence of the effects of the Programme, yet some important questions remain unanswered. Which of the many initiatives work, and which do not? Does the policy affect other outcomes beyond those targeted by the policy and studied in this paper? And what role does the name of the Programme have for the effectiveness of the policy? Does the name in itself produce a stigma effect, which counteracts effects of the initiatives in the Programme? We leave for future work to identify important mechanisms, to investigate effects on outcomes not directly targeted by the policy and to attempt identifying stigma effects of the policy.

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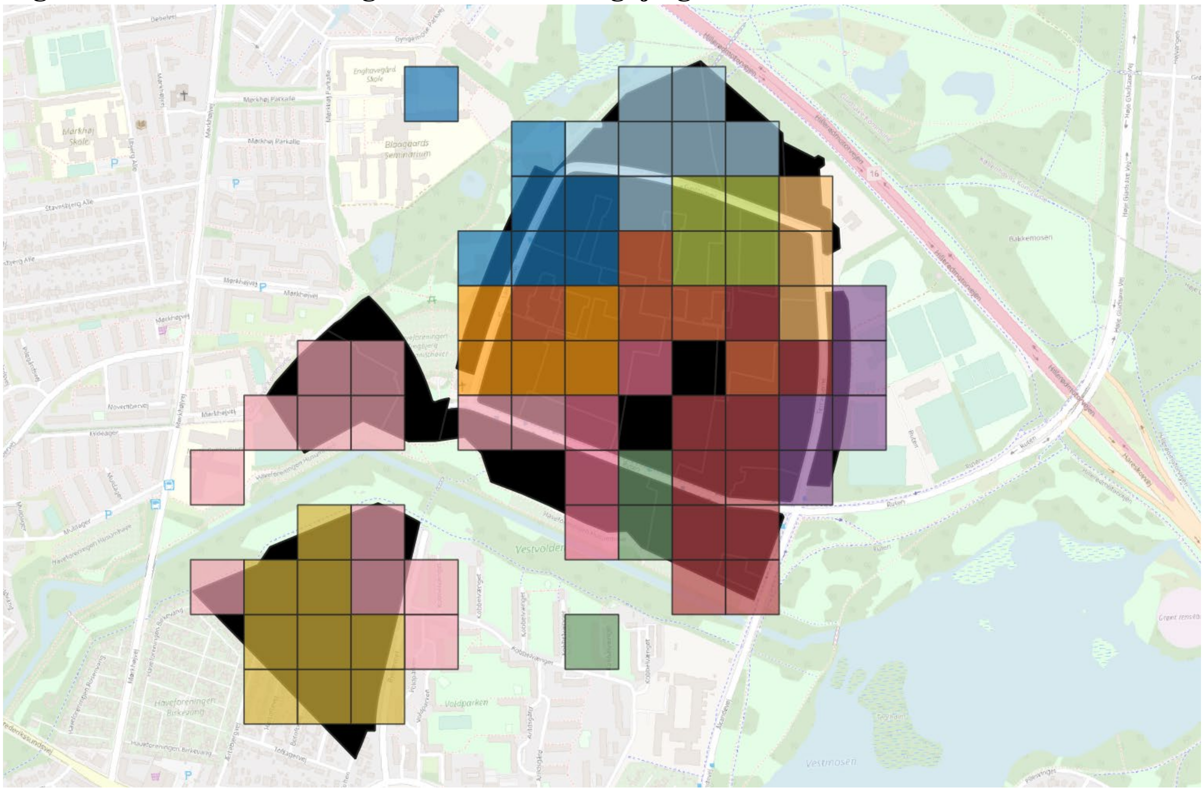
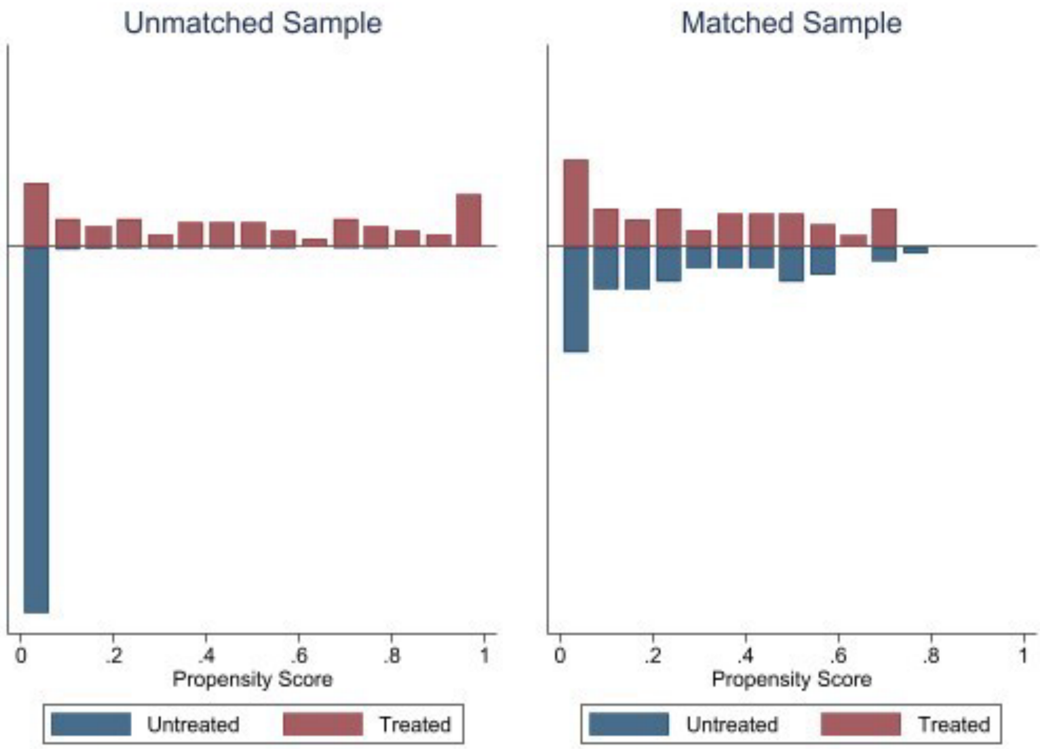
Figure 1. Treated micro neighbourhoods in Tingbjerg/Utterlevshuse



Source: GIS-data on micro neighbourhoods and GIS-data on public housing areas.

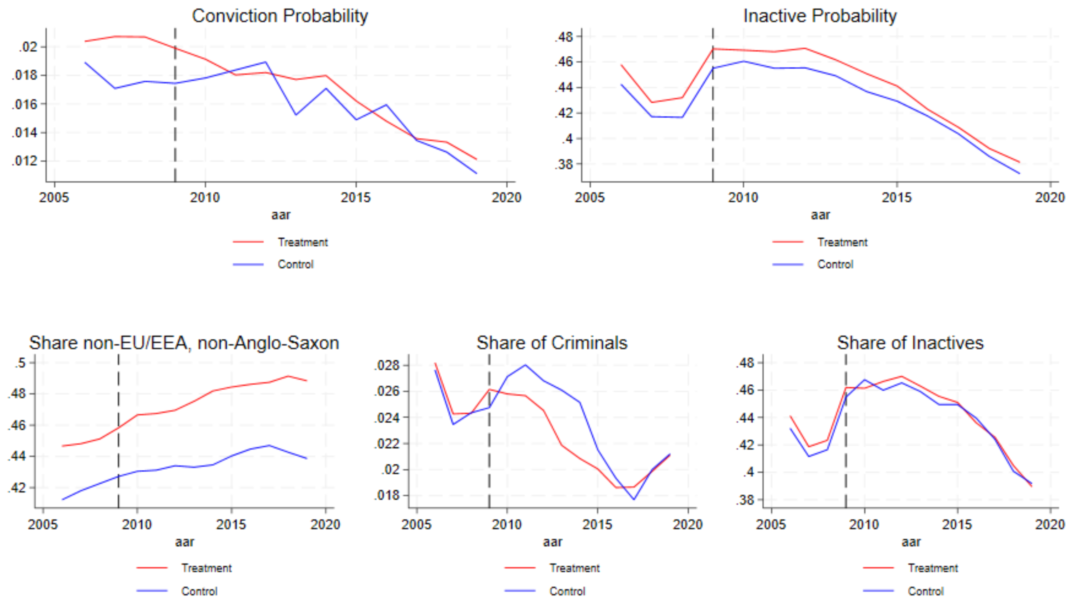
Notes: The black polygon illustrates the public housing of Tingbjerg/Utterslevshuse. The coloured hectare cells represents the treated micro neighbourhoods in the Tingbjerg/Utterslevshuse. Each colour represents one neighbourhood. We show neighbourhoods as hectare cells, rather than their organic shape, for reasons of confidentiality.

Figure 2. Propensity score distributions before and after matching



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

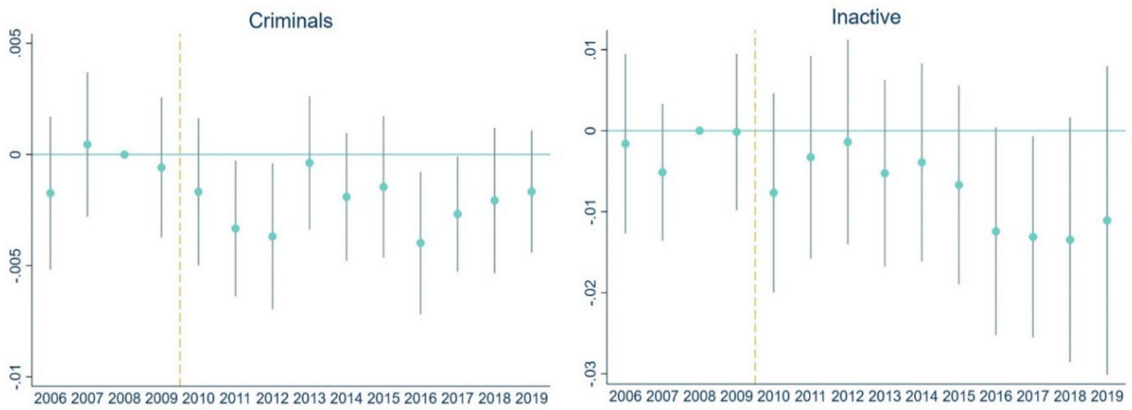
Figure 3. Common trend



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: Individual-level outcomes in the top row and neighbourhood level outcomes in the bottom row. Treatment group indicated by red and control group indicated by blue.

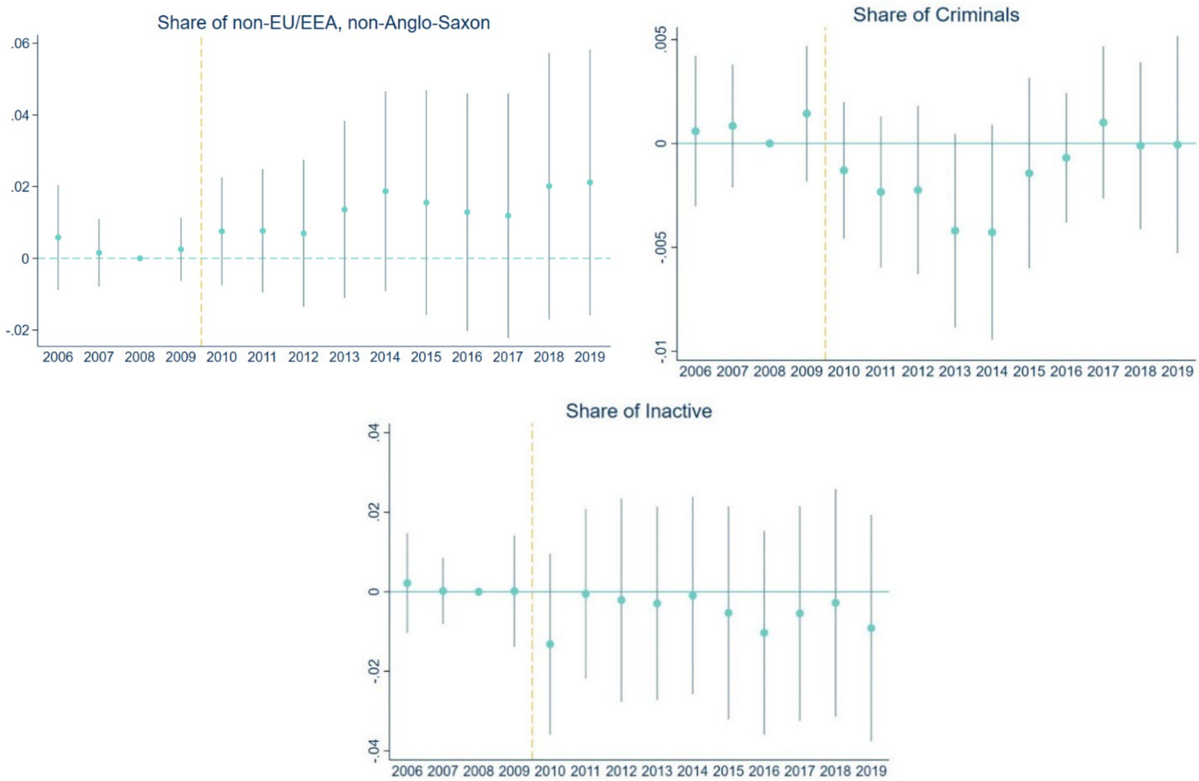
Figure 4. Event study plots. Individual level outcomes



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: Figure 4 plots event study graphs of individual-level outcomes. The graphs show the estimated coefficients of living in a treated neighbourhood in 2009 relative to living in a control neighbourhood in 2009 for each year in the sample period. The left graph use the conviction probability as the outcome, the right graph the inactive probability.

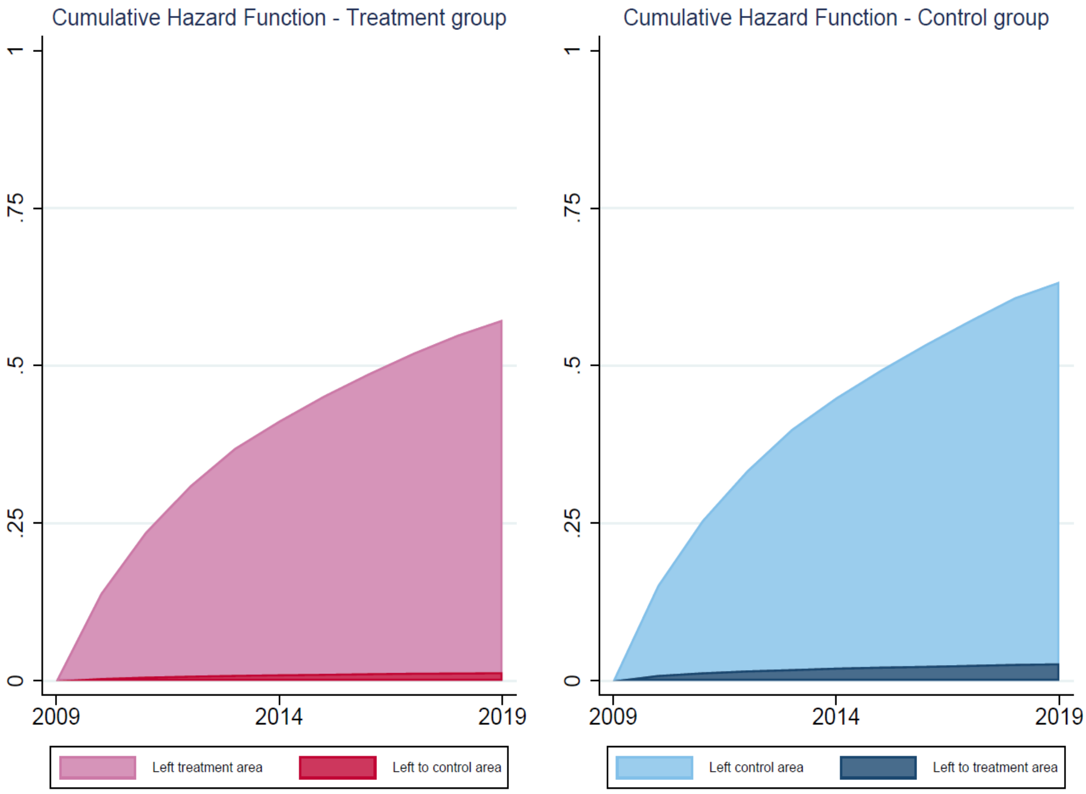
Figure 5. Event study plots. Neighbourhood level outcomes



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: Figure 5 plots event study graphs of neighbourhood-level outcomes. The graphs show the estimated coefficients of a neighbourhood being treated relative to being a control neighbourhood for each year in the sample period. The top left graph use the share of non-EU, non-Anglo-Saxon immigrants as the outcome, the top right graph uses the share of residents convicted of a crime committed in that year and the bottom graph uses the share of inactive residents.

Figure 6. Cumulative hazard functions of treatment and control groups



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: The left and right figure shows the cumulative hazard function of the treatment neighbourhoods and control neighbourhoods, respectively. In the left (right) figure, the cumulative hazard function is indicated by pink (light blue), and the red (navy blue) is the distribution of individuals leaving treatment (control) areas to live in treatment (control) areas.

Table 1. Initiatives available to different type of areas

	Area type	
	Areas on the List	*Low employment areas*
Initiatives:		
Mandatory plan with description of objectives and initiatives needed to improve the area.	x	
Access to funds of €12.9 mil. to reach objectives of mandatory plan.	x	
Access to yearly infrastructure improvement funds of €24.1 mil between 2011-2016	x	
Access to €6.5 mil. from the "Special Pool for the Social Area" (Satspuljen) for creation of job centres and moving subsidies.	x	
Formulation of a national police strategy for prevention and figthing crime in the areas	x	
Possibility for the municipality to decide that vacancies should be let out according to specific criteria.	x	(x)
Access to funds of €71.1 mil. each year between 2011 and 2014 for social programmes	x	x
No municipal assignment of refugees and non-EU/EEA citizens to the area.	x	x
No municipal assignment of released inmates to the area.	x	x
No municipal assignment of tenants who had their lease terminated due to misconduct within the last 6 months.	x	x

Sources: Danish Ministry of Finance (2010), Danish Parliament (2010a, 2010b).
Notes: For low employment areas, the municipality can require rejection of applicants that are outside the labour force. For the areas on the List, the municipality can require rejection of non-working applicants AND formulate other criteria, that applicants has to meet to become a tenant to improve the socioeconomic profile of the area, such as being enrolled in education or other criteria that the municipality deem as socioeconomically advantaged. Prices are in 2024 values. We have used the 2024 yearly average in the DKK-EUR exchange rate to convert the prices from DKK to EUR.

Table 2. Balancing. Neighbourhood level. 2009 values.

Variable		*All neighbourhoods*	*Sample*	*Treatment*	*Control*	*Difference: Treatment - Control*	*P-value*
Population	Mean	660	655	705	590	115	0.0394
	SD	*297*	*308*	*342*	*244*		
Share of non-EU/EEA, non-Anglo-Saxon immigrants and descendants	Mean	0.0696	0.4448	0.4583	0.4272	0.0311	0.2853
	SD	*0.1060*	*0.1586*	*0.1498*	*0.1692*		
Share of inactives	Mean	0.2346	0.4589	0.4619	0.4549	0.0070	0.6665
	SD	*0.0951*	*0.0889*	*0.0813*	*0.0986*		
Share of criminals	Mean	0.0086	0.0255	0.0262	0.0248	0.0014	0.5547
	SD	*0.0081*	*0.0129*	*0.0142*	*0.0111*		
Mean annual personal income (in EUR)	Mean	51282	36162	35607	36885	-1278	0.1033
	SD	*12675*	*4291*	*4929*	*3185*		
Share of residents with only primary education	Mean	0.2464	0.4736	0.4797	0.4657	0.0140	0.3475
	SD	*0.1123*	*0.0813*	*0.0865*	*0.0741*		
Share of public housing	Mean	0.2067	0.8402	0.8521	0.8246	0.0276	0.5832
	SD	*0.3178*	*0.2736*	*0.2467*	*0.3068*		
# neighbourhoods		8358	122	69	53		

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021). Income measured in 2024-values. Use the average 2024 EUR/DKK exchange rate to convert to EUR.

Table 3. Sample selection criteria. Individual level.

Panel A - Sample selection criteria that are the ***same for both*** *outcomes*

Outcome	*#*	*Explanation*	*Sample size*
	1	Gross sample of all individuals living in Denmark between 2006-2019	78,517,569
All outcomes	2	Use propensity score matching to match treated neighbourhoods with an untreated neighbourhood. Match on: - The share of public housing in 2009 - The share of non-EU/EEA, non-Anglo-Saxon immigrants and descendants in 2009 - The share of inactives in 2006, 2007, 2008 and 2009 - The share of convicted criminals in 2006, 2007, 2008 and 2009 Drop individuals living in unmatched treatment neighbourhoods and neighbourhoods that are neither treated nor controls	1,096,985
	3	Drop individuals not living in public housing	859,157

Panel B - Sample selection criteria that are ***different for each*** *outcome*

Outcome	*#*	*Explanation*	*Sample size*
Conviction probability	4	Drop observations where age is below 15	**701,316**
Inactive probability	4	Drop observations where age is below 16 and above 64	587,059
	5	Drop observations where inactive status is missing	**585,969**

Sources: Administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Table 4. Balancing. Individual level. 2009 values.

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variable		*Sample: Age>14*	*Treatment*	*Control*	*Difference : Treatment - Control*	*P-value*	*Sample: Age 16-64*	*Treatment*	*Control*	*Difference : Treatment - Control*	*P-value*
Age	Mean	42.21	41.99	42.55	-0.56	0.0008	37.66	37.50	37.93	-0.43	0.0015
	SD	*18.37*	*18.36*	*18.39*			*13.59*	*13.59*	*13.59*		
Female	Mean	0.5121	0.5106	0.5146	-0.0041	0.3700	0.4976	0.4967	0.4989	-0.0022	0.6520
	SD	*0.4999*	*0.4999*	*0.4998*			*0.5000*	*0.5000*	*0.5000*		
Immigrant	Mean	0.4307	0.4424	0.4124	0.0300	0.0000	0.4710	0.4824	0.4532	0.0292	0.0000
	SD	*0.4952*	*0.4967*	*0.4923*			*0.4992*	*0.4997*	*0.4978*		
Descendant	Mean	0.0664	0.0658	0.0674	-0.0015	0.4961	0.0670	0.0658	0.0687	-0.0029	0.2489
	SD	*0.2491*	*0.2480*	*0.2507*			*0.2500*	*0.2480*	*0.2530*		
EU/EEA origin	Mean	0.0447	0.0460	0.0427	0.0034	0.0733	0.0470	0.0493	0.0435	0.0058	0.0057
	SD	*0.2067*	*0.2096*	*0.2021*			*0.2116*	*0.2164*	*0.2039*		
Non-EU/EEA origin	Mean	0.4524	0.4622	0.4371	0.0251	0.0000	0.4910	0.4990	0.4785	0.0205	0.0000
	SD	*0.4977*	*0.4986*	*0.4960*			*0.4999*	*0.5000*	*0.4996*		
Finished primary school	Mean	0.7696	0.7688	0.7710	-0.0022	0.5821	0.8398	0.8407	0.8386	0.0021	0.5803
(9th grade)	*SD*	*0.4211*	*0.4216*	*0.4202*			*0.3668*	*0.3660*	*0.3679*		
Inactive	Mean	0.5248	0.5290	0.5183	0.0107	0.0189	0.4644	0.4704	0.4551	0.0152	0.0022
	SD	*0.4994*	*0.4992*	*0.4997*			*0.4987*	*0.4991*	*0.4980*		
Criminal convicted	Mean	0.0189	0.0199	0.0174	0.0025	0.0478	0.0213	0.0223	0.0196	0.0027	0.0567
	SD	*0.1363*	*0.1397*	*0.1309*			*0.1442*	*0.1477*	*0.1386*		
Finished upper	Mean	0.4400	0.4349	0.4479	-0.0130	0.0055	0.4625	0.4586	0.4686	-0.0100	0.0502
secondary education	*SD*	*0.4964*	*0.4958*	*0.4973*			*0.4986*	*0.4983*	*0.4990*		
Married	Mean	0.3678	0.3657	0.3711	-0.0053	0.2249	0.3753	0.3731	0.3788	-0.0057	0.2352
	SD	*0.4822*	*0.4816*	*0.4831*			*0.4842*	*0.4836*	*0.4851*		
Unique # individuals		50536	30755	19781			42530	25939	16591		

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Table 5. Difference-in-difference results. Individual level.

			Dependent variable:			
	(1)	(2)	(3)	(4)	(5)	(6)
	Convicted of a crime			*Inactive*		
	Sample: Age 15 and above			Sample: Age 16-64		
Sample mean	0.0189			0.4644		
Std. Dev.	*0.1363*			*0.4987*		
Explanatory variables:						
Living in treated area in 2009 × Post Period	-0.0018**	-0.0018**	-0.0015*	-0.0062	-0.0062	-0.0017
	(0.0009)	(0.0008)	(0.0007)	(0.0057)	(0.0056)	(0.0053)
Living in treated area in 2009	0.0022			0.0164		
	(0.0013)			(0.0181)		
Female	-0.0184***	-0.0182***		0.0716***	0.0725***	
	(0.0008)	(0.0008)		(0.0044)	(0.0044)	
EU/EEA, Anglo-Saxon	-0.0022	-0.0023		-0.0464***	-0.0380***	
	(0.0016)	(0.0015)		(0.0144)	(0.0133)	
Non-EU/EEA, non-Anglo-	0.0013	0.0011		0.0998***	0.1018***	
	(0.0009)	(0.0009)		(0.0124)	(0.0109)	
Adjusted R^2	0.015	0.016	0.003	0.112	0.125	0.025
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood FE	No	Yes	No	No	Yes	No
Individual FE	No	No	Yes	No	No	Yes
Observations	701,316			585,969		
Unique # individuals	61,131			54,683		

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1), (2) and (3) use an indicator for being convicted of a crime as the outcome. Column (4), (5) and (6) use an indicator for being inactive as the outcome. Column (1) and (4) control for gender and origin. In Column (2) and (5) we add neighbourhood FE. In Column (3) and (6), we replace the controls by individual FE. All specifications include year FE and age FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Column (3) and (6) report adjusted within R^2.

Table 6. Robustness. Remove close-by controls. Individual level.

	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Outcome - Convicted of a crime						
Sample mean (main sample)			0.0189			
Std. Dev.			*0.1363*			
Explanatory variables:						
Living in treated area in 2009 × Post Period	-0.0018**	-0.0015*	-0.0019**	-0.0017**	-0.0021**	-0.0017**
	(0.0008)	(0.0007)	(0.0009)	(0.0007)	(0.0009)	(0.0007)
R^2	0.016	0.003	0.016	0.003	0.016	0.003
Observations	701,316		678,353		658,621	
Unique # individuals	61,131		59,124		57,432	
Panel B. Outcome - Inactivity						
Sample mean (main sample)			0.4644			
Std. Dev.			*0.4987*			
Explanatory variables:						
Living in treated area in 2009 × Post Period	-0.0062	-0.0017	-0.0073	-0.0029	-0.0083	-0.0041
	(0.0056)	(0.0053)	(0.0057)	(0.0053)	(0.0059)	(0.0055)
R^2	0.125	0.025	0.126	0.025	0.127	0.025
Observations	585,969		565,962		549,956	
Unique # individuals	54,683		52,831		51,351	
Distance from treated neighbourhood where controls removed	None removed		< 500 m removed		< 1000 m removed	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes	No	Yes

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table 6 checks robustness to leaving out close-by controls for the individual level outcomes. Panel A uses an indicator for being convicted of a crime as the outcome. Panel B uses an indicator for being inactive as the outcome. In Column (1) and (2) we show baseline results, in Column (3) and (4) we remove control areas that has a distance of 500 metres or less to a treated area and in Column (5) and (6) we remove those that has a distance of less than 1000 m. We control for gender, origin, neighbourhood FE, year FE and age FE. In Columns (2), (4) and (6) we replace neighbourhood FE's as well as gender and origin controls by individual FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2), (4) and (6) report adjusted within R^2.

Table 7. Dynamic treatment effects. Individual level.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in	-0.0018*	-0.0015*	-0.0027	-0.0005
2009 ×2010-2013 period	(0.0010)	(0.0009)	(0.0055)	(0.0053)
Living in treated area in	-0.0020*	-0.0015	-0.0060	-0.0011
2009 ×2014-2016 period	(0.0011)	(0.0009)	(0.0058)	(0.0055)
Living in treated area in	-0.0017*	-0.0014*	-0.0109	-0.0043
2009 ×2017-2019 period	(0.0009)	(0.0008)	(0.0072)	(0.0066)
Adjusted R^2	0.016	0.003	0.125	0.025
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1) and (2) uses an indicator for being convicted of a crime as the outcome. Column (3) and (4) uses an indicator for being inactive as the outcome. In Column (1) and (3) we control for gender and origin and neighbourhood FE. In Column (2) and (4) we replace this by individual FE. In all specifications we use year FE and age FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table 8. Difference-in-difference results. Neighbourhood level.

	Dependent variable:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Share of non-EU/EEA, non-Anglo-Saxons			*Share of convicted criminals*			*Share of inactives*		
Sample mean		0.4448			0.0255			0.4589	
Std. Dev.		*0.1586*			*0.0129*			*0.0889*	
Explanatory variables:									
Neighbourhood on the List in Jan 2011 × Post Period	0.0113	0.0111	0.0111	-0.0023	-0.0023	-0.0023	-0.0009	-0.0059	-0.0059
	(0.0134)	(0.0126)	(0.0130)	(0.0014)	(0.0014)	(0.0014)	(0.0075)	(0.0103)	(0.0106)
Neighbourhood on the List in Jan 2011	0.0295	0.0501		0.0005	-0.0008		-0.0030	0.0057	
	(0.0338)	(0.0378)		(0.0021)	(0.0027)		(0.0156)	(0.0192)	
Adjusted R^2	0.038	0.374	0.940	0.062	0.163	0.685	0.411	0.508	0.868
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time-varying municipality controls	Yes	No	No	Yes	No	No	Yes	No	No
Municipality FE	No	Yes	No	No	Yes	No	No	Yes	No
Neighbourhood FE	No	No	Yes	No	No	Yes	No	No	Yes
Observations					1,708				

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1), (2) and (3) use the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (4), (5) and (6) use the share of criminals as outcome. Column (7), (8) and (9) use the share of inactives as outcome. Column (1), (4) and (7) controls for time-varying municipality characteristics. The municipality characteristics included are the share of non-EU/EEA, non-Anglo-Saxon immigrants, the share of criminals, the share of inactives, the share of residents with only primary education and the share of public housing (leaving out the municipality level of the outcome for each specification). The controls also include mean income at the regional level (not available at the municipality level). Column (2), (5) and (8) instead controls for municipality FE. In Column (3), (6) and (9) we use neighbourhood FE. 56.56 percent of the neighbourhoods in our sample were on the List in 2011. Post period is defined as 2010-2019.

Table 9. Robustness. Remove close-by controls. Neighbourhood level.

	Dependent variable:								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Share of non-EU/EEA, non-Anglo-Saxons			*Share of convicted criminals*			*Share of inactives*		
Sample mean (main sample)	0.4448			0.0255			0.4589		
Std. Dev.	*0.1586*			*0.0129*			*0.0889*		
Explanatory variables:									
Neighbourhood on the List in Jan 2011 × Post Period	0.0111	0.0116	0.0153	-0.0023	-0.0023	-0.0025*	-0.0059	-0.0057	-0.0049
	(0.0130)	(0.0134)	(0.0136)	(0.0014)	(0.0014)	(0.0015)	(0.0106)	(0.0106)	(0.0113)
Adjusted R^2	0.940	0.940	0.941	0.658	0.660	0.663	0.857	0.859	0.861
Year FE	Yes								
Neighbourhood FE	Yes								
Distance from treated neighbourhood where controls removed	None removed	< 500 m removed	< 1000 m removed	None removed	< 500 m removed	< 1000 m removed	None removed	< 500 m removed	< 1000 m removed
Observations	1,708	1,652	1,596	1,708	1,652	1,596	1,708	1,652	1,596

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table 9 checks robustness to leaving out close-by controls for the neighbourhood level outcomes. Column (1), (2) and (3) use the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (4), (5) and (6) use the share of criminals as outcome. Column (7), (8) and (9) use the share of inactives as outcome. In Column (1), (4) and (7) we show baseline results, in Column (2), (5) and (8) we remove control areas that has a distance of 500 metres or less to a treated area and in Column (3), (6) and (9) we remove those that has a distance of less than 1000 m. Distance is measured as the average distance between each hectare cell in a neighbourhood to each hectare cell in the other neighbourhood. Post period is defined as 2010-2019.

Table 10. Dynamic treatment effects. Neighbourhood level.

	Dependent variable:		
	(1)	(2)	(3)
	Share of non-EU/EEA, non-Anglo-Saxons	*Share of convicted criminals*	*Share of inactives*
Sample mean	0.4448	0.0255	0.4589
Std. Dev.	*0.1586*	*0.0129*	*0.0889*
Explanatory variables:			
Neighbourhood on the List in Jan 2011 × 2010-2013	0.0065	-0.0032**	-0.0053
	(0.0091)	(0.0014)	(0.0101)
Neighbourhood on the List in Jan 2011 × 2014-2016	0.0132	-0.0028	-0.0061
	(0.0155)	(0.0017)	(0.0116)
Neighbourhood on the List in Jan 2011 × 2017-2019	0.0153	-0.0004	-0.0064
	(0.0179)	(0.0018)	(0.0129)
Adjusted R^2	0.373	0.137	0.492
Year FE		Yes	
Neighbourhood FE		Yes	
Observations		1,708	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1) uses the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (2) uses the share of criminals as outcome. Column (3) use the share of inactives as outcome. We control for year and neighbourhood FE. 56.56 percent of the neighbourhoods in our sample were on the List in 2011. Post period is defined as 2010-2019.

Table 11. Mechanisms. Share of public housing.

	(1)	(2)	(3)
		Dependent variable:	
		Share of public housing	
Sample mean		0.8402	
Std. Dev.		*0.2736*	
Explanatory variables:			
Neighbourhood on the List in Jan 2011 × Post Period	-0.0416*	-0.0394*	-0.0394*
	(0.0224)	(0.0202)	(0.0207)
Neighbourhood on the List in Jan 2011	0.0142	-0.00319	
	(0.0495)	(0.0697)	
Adjusted R^2	0.050	0.192	0.911
Year FE	Yes	Yes	Yes
Time-varying municipality controls	Yes	No	No
Municipality FE	No	Yes	No
Neighbourhood FE	No	No	Yes
Observations	1,708	1,708	1,708

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table 11 uses the share of public housing as outcome. Column (1) controls for time-varying municipality characteristics. The municipality characteristics included are the share of non-EU/EEA, non-Anglo-Saxon immigrants, the share of criminals, the share of inactives and the share of residents with only primary education. The controls also include mean income at the regional level (not available at the municipality level). Column (2) instead controls for municipality FE. In Column (3) we use neighbourhood FE. 56.56% of the neighbourhoods in our sample were on the List in 2011. Post period is defined as 2010-2019.

Table 12. Mechanisms. In- and out-mover characteristics.

	Dependent variable:					
	(1)	(2)	(3)	(4)	(5)	(6)
	Share of non-EU/EEA, non-Anglo-Saxons		*Share of convicted criminals*		*Share of inactives*	
Panel A. Outcome - In-mover characteristics						
Sample mean	0.4092		0.0349		0.4288	
Std. Dev.	*0.1851*		*0.0247*		*0.1148*	
Explanatory variables:						
Neighbourhood on the List in Jan 2011 × Post Period	0.0173	0.0209	-0.0033	-0.0055*	-0.0167	-0.0112
	(0.0230)	(0.0216)	(0.0035)	(0.0031)	(0.0141)	(0.0138)
Adjusted R^2	0.740	0.780	0.392	0.571	0.658	0.797
Panel B. Outcome - Out-mover characteristics						
Sample mean	0.3583		0.0386		0.3985	
Std. Dev.	*0.1586*		*0.0247*		*0.0967*	
Explanatory variables:						
Neighbourhood on the List in Jan 2011 × Post Period	0.0086	-0.0053	-0.0016	-0.0018	-0.0067	-0.0040
	(0.0155)	(0.0188)	(0.0027)	(0.0027)	(0.0137)	(0.0141)
Adjusted R^2	0.731	0.747	0.339	0.458	0.615	0.704
Year FE	Yes					
Neighbourhood FE	Yes					
Estimation method	OLS	WLS	OLS	WLS	OLS	WLS
Observations	1,708					

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Panel A investigates effects on in-mover characteristics, Panel B on out-mover characteristics. Columns (1) and (2) the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (3) and (4) use the share of criminals as outcome. Columns (5) and (6) use the share of inactives as outcome. Columns (1), (3) and (5) estimate effects using OLS and Columns (2), (4) and (6) estimate effects WLS. We control for neighbourhood and year FE. Post period is defined as 2010-2019.

Table 13. Heterogeneity analysis. Stayers.

	(1)	(2)	(3)	(4)
	Dependent variable:			
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Panel A. Stayer = stayer until leaving. Leaver = Leaver from time of leave and onwards				
Explanatory variables:				
Living in treated area in 2009 × Post Period	-0.0022**	-0.0016**	-0.0057	0.0006
	(0.0010)	(0.0007)	(0.0080)	(0.0056)
Living in treated area in 2009 × Post Period × Mover from treatment/control area	0.0013	0.0002	-0.0043	-0.0049
	(0.0015)	(0.0011)	(0.0103)	(0.0046)
Adjusted R^2	0.016	0.003	0.126	0.025
Panel B. Stayer = if staying through entire sample period. Leaver = eventually leaving				
Explanatory variables:				
Living in treated area in 2009 × Post Period	-0.0021**	-0.0022**	-0.0008	0.0012
	(0.0008)	(0.0008)	(0.0080)	(0.0081)
Living in treated area in 2009 × Post Period × Mover from treatment/control area	0.0005	0.0010	-0.0107	-0.0047
	(0.0015)	(0.0014)	(0.0080)	(0.0081)
Adjusted R^2	0.016	0.003	0.127	0.025
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table 13 shows estimates where we allow the effect to vary by whether individuals stay in the treatment/control area. The reference category is stayers. In Panel A, an individual is stayer until they leave their neighbourhood. In Panel B a stayer is an individual that stays in the neighbourhood throughout the sample period. Column (1) and (2) use an indicator for being convicted of a crime as the outcome. Column (3) and (4) use an indicator for being inactive as the outcome. We control for gender, origin, neighbourhood FE, year FE and age FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. In Column (2) and (4) we replace gender and origin controls as well as neigbourhood FE's by individual FE's. The relevant interactions between mover, post period and treatment status are included in the regressions. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table 14. Heterogeneity analysis. Criminal or inactivity history.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in 2009 × Post Period	-0.0191**	-0.0130	0.0007	0.0035
	(0.0091)	(0.0090)	(0.0054)	(0.0070)
Living in treated area in 2009 × Post Period × **NO** Criminal/Inactivity history	0.0196**	0.0133	-0.0055	-0.0077
	(0.0089)	(0.0090)	(0.0082)	(0.0076)
Adjusted R^2	0.129	0.051	0.384	0.033
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table 14 shows estimates where we allow the effect to vary by individuals' criminal or inactivity history, for teh relevant outcome. Columns (1) and (2) use an indicator for being convicted of a crime as the outcome and show results where the effect varies by criminal history. Criminal history is defined as those individuals with at least one conviction for a crime committed in the period 2006-2009. Columns (3) and (4) use an indicator for being inactive as the outcome and show results where the effect varies by inactivity history. Inactivity history is defined as those individuals for those being inactive in 2009. We control for gender, origin, neighbourhood FE, year FE and age FE. In Columns (2) and (4) we replace neighbourhood FE as well as gender and origin controls by individual FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table 15. House price spillovers to nearby neighbourhoods.

	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable:					
	Log of house prices					
Explanatory variables:						
Nearby neighbourhood on the List in Jan 2011 × Post Period	0.0305	0.0135	0.0246	-0.0279	0.0269	-0.0292
	(0.0343)	(0.0524)	(0.0336)	(0.0433)	(0.0358)	(0.0444)
Adjusted R^2	0.738	0.814	0.723	0.779	0.726	0.776
Year FE	Yes					
Neighbourhood FE	Yes					
Housing characteristics controls	Yes					
Distance from treatment/control neighbourhoods	< 1 km	< 0.5 km	< 1 km	< 0.5 km	< 1 km	< 0.5 km
Minimum share of owner-occupied housing in neighbourhood	Un-restricted	Un-restricted	> 0.25	> 0.25	> 0.5	> 0.5
Observations	14,490	2,643	13,574	2,267	11,369	1,818

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1), (3) and (5) restrict the sample to neighbourhoods within 1 km of treated and control neighbourhoods. Column (2), (4) and (6) restrict the sample to neighbourhoods within 500 m of treated and control neighbourhoods. Neighbourhoods within a given distance of both a control and treatment neighbourhood is assigned as treated. Distance is measured as the average distance between each hectare cell in a neighbourhood to each hectare cell in the other neighbourhood. In Column (1) and (2) we do not restrict the share of owner-occupied housing. In Column (3) and (4) we restrict the share of owner-occupied housing to be at least 25%, and in Column (5) and (6) we restrict it to be at least 50%. In all specification we control for year FE, neighbourhood FE and housing characteristics. The housing characteristics included are log of the house size in sq. m, log of the sold area in sq. m, number of rooms, the age of the property and age squared. Post period is defined as 2010-2019.

APPENDIX A – INSTITUTIONAL DETAILS

LATER CRITERIA

In 2013, the social democratic-led government altered the List criteria, which became effective from 2014. The government kept the three statistical criteria from the 2010 definition and added two additional criteria.[24] Consequently, a public housing area with at least 1,000 inhabitants was placed on the List if it fulfilled at least three of five criteria. The additional criteria were:

- The share of residents aged 30-59, who had only finished primary education, exceeded 50 percent.
- The average gross income of residents aged 15-64 (excluding students) was below 55 percent of the regional average of the same age group.

The 2014-definition was effective until 2018, when the liberal-conservative government modified the criteria again. The government made the criterion regarding the share of residents of non-EU/EEA, non-Anglo-Saxon origin a necessary criterion for an area to be on the List. According to the new definition, which is the definition currently used, public housing areas with at least 1,000 inhabitants and a share of non-EU/EEA, non-Anglo-Saxons exceeding 50 percent is on the List if it fulfils two of the following four criteria:

- The inactive share among 18-64-year-olds exceeds 40 percent (calculated as the average over the previous two years).
- The share of criminals exceeds three times the national average (calculated as the average over the previous two years).
- The share of residents aged 30-59, who has only finished basic education, exceeds 60 percent.
- The average gross income of residents aged 15-64 (excluding students) is less than 55 percent of the regional average gross income of the same age group.

[24] The calculation of the existing criteria was changed from being calculated over the previous four years to being calculated over the previous two years.

Table A3 gives an overview of the evolution in the number of areas on the List and the statistical criteria used to make the list, since publication of the first unofficial list in October 2010.

Table A1. Public housing areas on the List

Area	*Municipality*	*Population size in January 2010*
Blågården	Copenhagen	2,440
Lundtoftegade	Copenhagen	1,478
Aldersrogade	Copenhagen	2,356
Mjølnerparken	Copenhagen	1,992
Sjælør Boulevard	Copenhagen	1,265
Akacieparken	Copenhagen	1,160
Gadelandet/Husumgård	Copenhagen	1,037
Tingbjerg/Utterlevshuse	Copenhagen	5,863
Bispeparken	Copenhagen	1,350
Hørgården	Copenhagen	1,583
Charlotteager	Høje-Taastrup	1,701
Askerød	Greve	1,455
Agervang	Holbæk	1,447
Ringparken, Slagelse	Slagelse	1,907
Solbakken mv	Odense	1,294
Vollmose	Odense	9,259
Byparken/Skovparken	Svendborg	1,434
Nørager/Søstjernevej m.fl.	Sønderborg	1,323
Stengårdsvej	Esbjerg	1,847
Korskærparken	Fredericia	1,876
Sundparken	Horsens	1,529
Skovvejen/Skovparken	Kolding	2,361
Bispehaven	Aarhus	2,395
Gellerupparken/Toveshøj	Aarhus	7,191
Havrevej	Thisted	1,086
Houlkærvænget	Viborg	1,008

Sources: Danish Ministry of Interior and Housing.

Table A2. Initiatives in the 2010 Plan by focus areas

Focus area 1: Increased attractiveness and reduction of physical and social isolation
1. Strategic cooperation between the governement and municipalities with areas on the List
2. Strategic demolitions of residential blocks
3. Funding of infrastructural changes to connect the areas more with surrounding areas
4. Renovations
5. Funding of social iniatives in deprived residential areas
Focus area 2: Change the demographic profile of the areas
6. Stop municipal assignment of refugees to areas on the List and deprived residential areas
7. Stop municipal assignment of non-EU/EEA citizens to areas on the List
8. Tighten requirement family reunification rules - consider residence in decision
9. Ease the possibilities to give socioeconomically advantaged first claim to housing in areas on the List (municipality decision)
10. Ease the possibilities to reject possible tenants not in the labour force (municipality decision)
11. Stop municipal assignment of released prisoners to areas on the List
12. Strengthen the power of municipalities in negotiations with housing associations
13. Ease regulations on sale of public housing to finance development plans of deprived residential areas
14. Ease possibilities to evict tenants that severely violate house rules (faster judicial process)
15. Right of municipalities and housing associations to challenge rules that prevent beneficial iniatives, that could work to get an area of the List
16. Funding of moving subsidies to tenants moving away from the areas on the List
Focus area 3: Improve educational outcomes of children and adolescents
17. Mandatory daycare for bilingual children that are not attending daycare (aged 3-5)
18. Municipal possibility to impose parental injuctions
19. Possibility to make school districts that are not geographically contiguous
20. Possibility to establish full-day schools in deprived residential areas
21. More inspection of private schools and increased focus on students with need for language support in public schools
22. Possibility for municipalities to reserve school places for children of non-Danish descent
23. Target internship subsidies to technical colleges with many students from areas on the List
Focus area 4: Reduce dependency on public transfers
24. Open job centres in areas on the List
25. Tighten 450-hour work rule to receive social security for spouses
26. Reduction in housing subsidies to sanction failure to live up to parental responsibilities or the labour market disposal requirement
Focus area 5: Prevent benefit fraud and crime
27. National plan for police responses in areas on the List
28. Fast handling of cases against young troublemakers
29. More inspection of benefit fraud
30. Expand access to CCTV in public housing areas
31. Suspended registration of first criminal ruling on young people’s criminal record
32. Targeted anti-crime counselling of municipalities and public housing associations on design of the physical surroundings

Sources: Danish Government (2010).

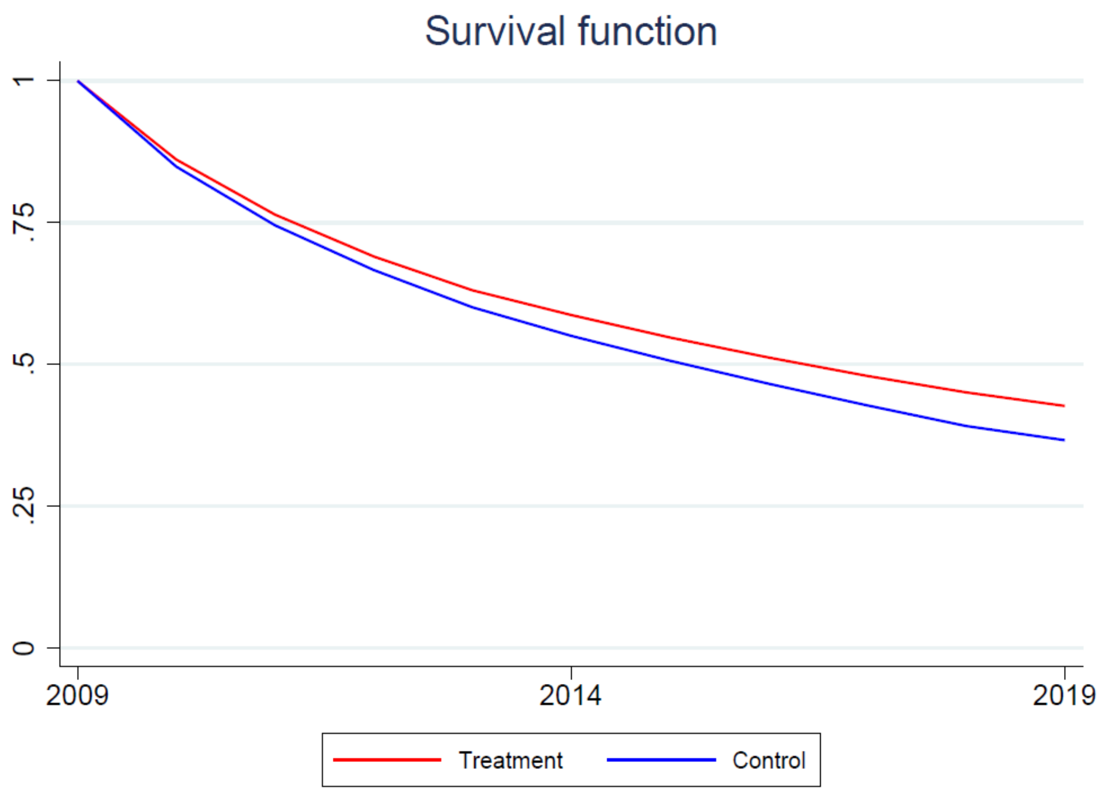
Table A3. Number of areas on the List during 2010 and 2020

Month	*Number of areas on the List*	*Statistical criteria*
Oct. 2010 (unofficial list)	29	Public housing areas that fulfil 2 of the following 3 criteria:
Jan. 2011	26	• The share of non-EU/EEA, non-Anglo-Saxon exceeded 50 percent.
Oct. 2011	28	• The share of inactives among 18-64 year-olds exceeded 40 percent.
Oct. 2012	33	• The share of criminals exceeded 2.7 percent.
Oct. 2013	40	
Feb. 2014	33	Public housing areas that fulfil 3 of the following 5 criteria:
Dec. 2014	31	• The share of non-EU/EEA, non-Anglo-Saxon exceeded 50 percent. • The share of inactives among 18-64 year-olds exceeded 40 percent.
Dec. 2015	25	• The share of criminals exceeded 2.7 percent. • The share of residents age 30-59, who had only finished primary education, exceeded 50 percent.
Dec. 2016	25	• The average gross income of residents age 15-64 (excluding students) was less than 55 percent of the regional average gross income of the same age group.
Dec. 2017	22	
Dec. 2018	29	Public housing areas with a share of non-EU/EEA, non-Anglo-Saxon exceeding 50 percent and that fulfil 2 of the following 4 criteria:
Dec. 2019	28	• The share of inactives among 18-64 year-olds exceeds 40 percent. • The share of criminals exceeds three times the national aver-age. • The share of residents age 30-59, who had only finished pri-mary education, exceeds 50 percent.
Dec. 2020	15	• The average gross income of residents age 15-64 (excluding students) was less than 55 percent of the regional average gross income of the same age group.

Sources: Danish Ministry of the Interior and Housing (2010, 2011a, 2011b, 2012, 2013, 2014a, 2014b, 2015, 2016, 2017, 2018, 2019, 2020).

APPENDIX B – ADDITIONAL DESCRIPTIVE STATISTICS AND RESULTS

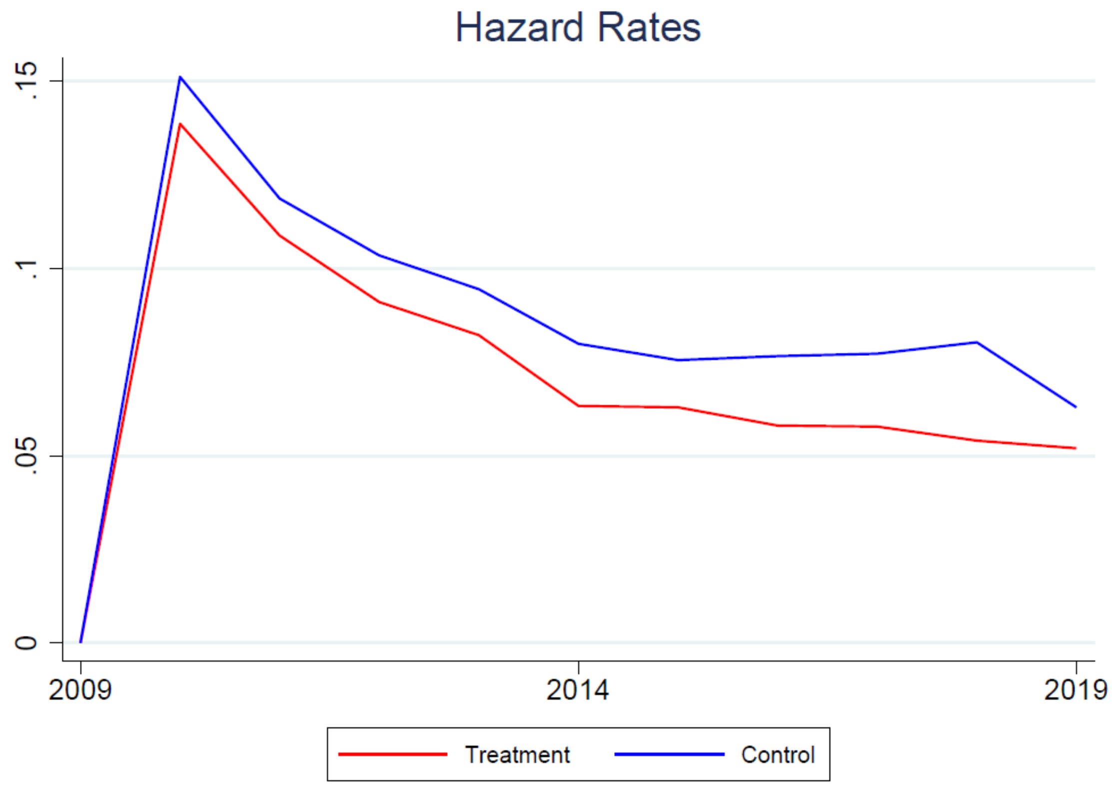
Figure B1. Survival functions



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

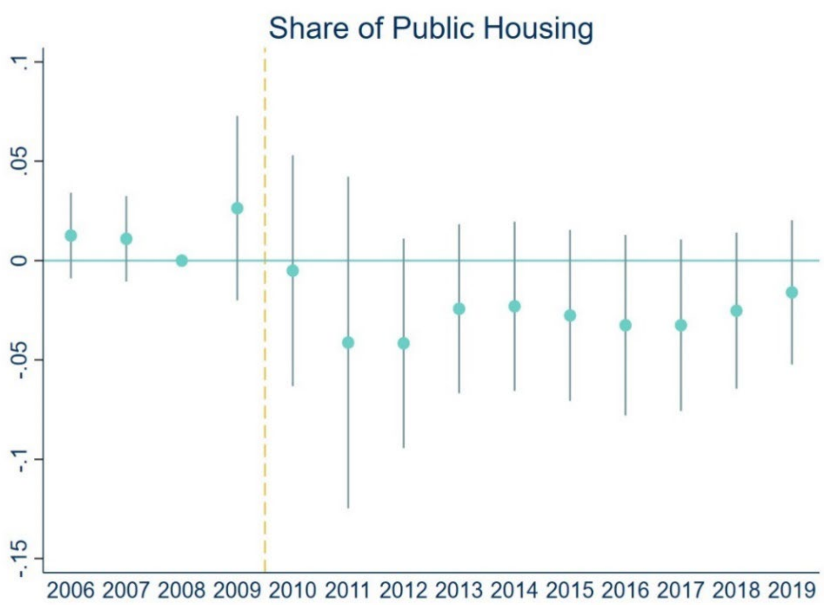
Notes: Treatment group indicated by red and control group indicated by blue.

Figure B2. Hazard rates



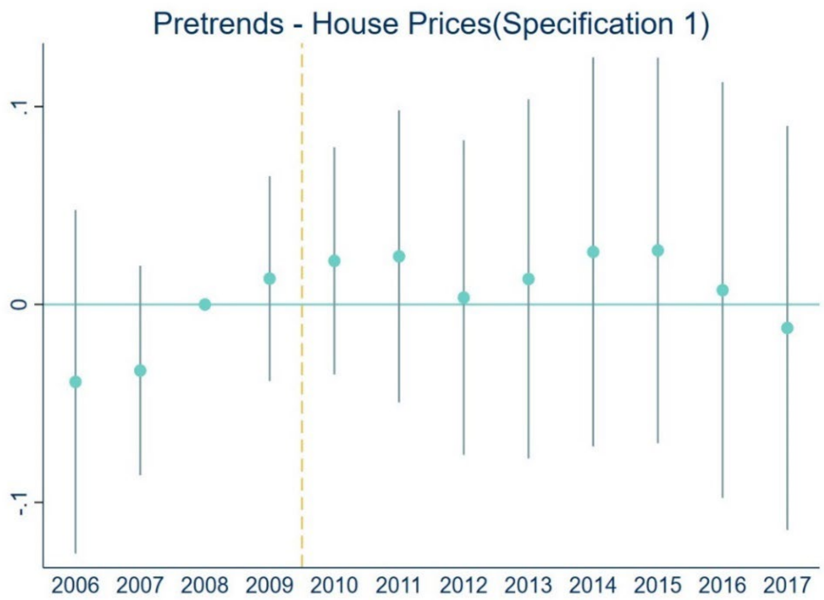
Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).
Notes: Treatment group indicated by red and control group indicated by blue.

Figure B3. Event study plot. Share of public housing



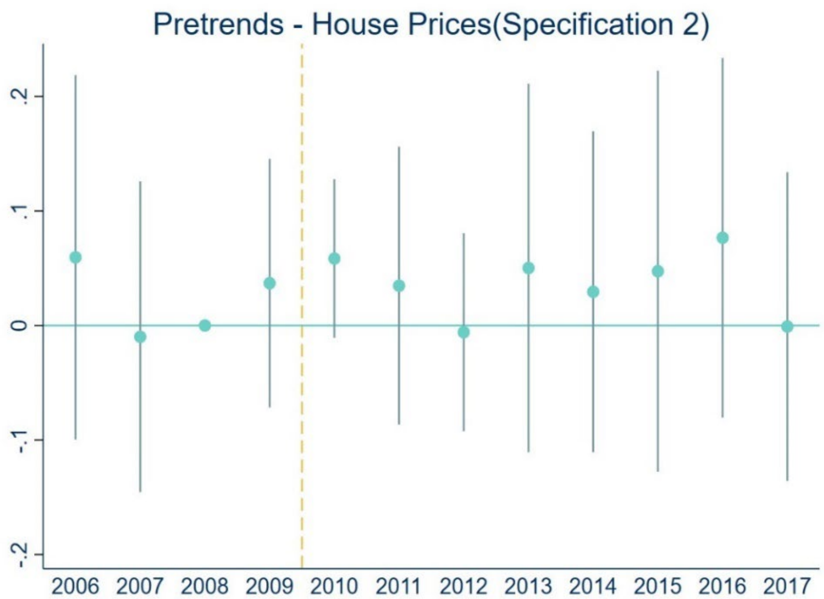
Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Figure B4. Event study plot. House prices in nearby neighbourhoods. Distance < 1000 m



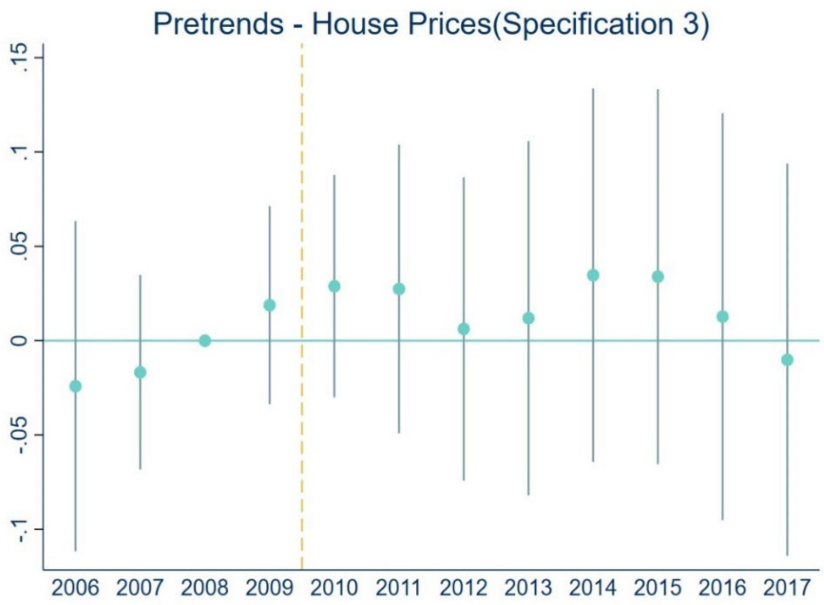
Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Figure B5. Event study plot. House prices in nearby neighbourhoods. Distance < 500 m.



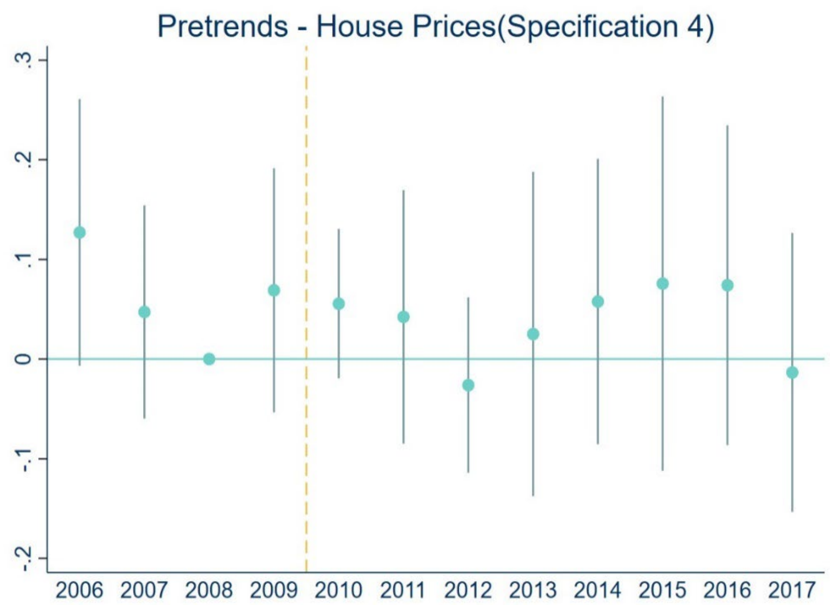
Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Figure B6. Event study plot. House prices in nearby neighbourhoods. Distance < 1000 m & owner-occupied > 25%.



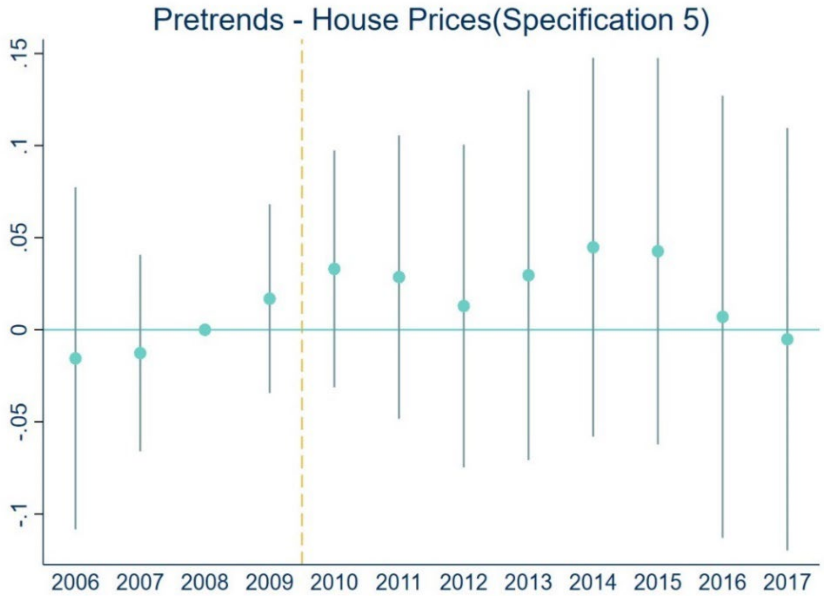
Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Figure B7. Event study plot. House prices in nearby neighbourhoods. Distance < 500 m & owner-occupied > 25%.



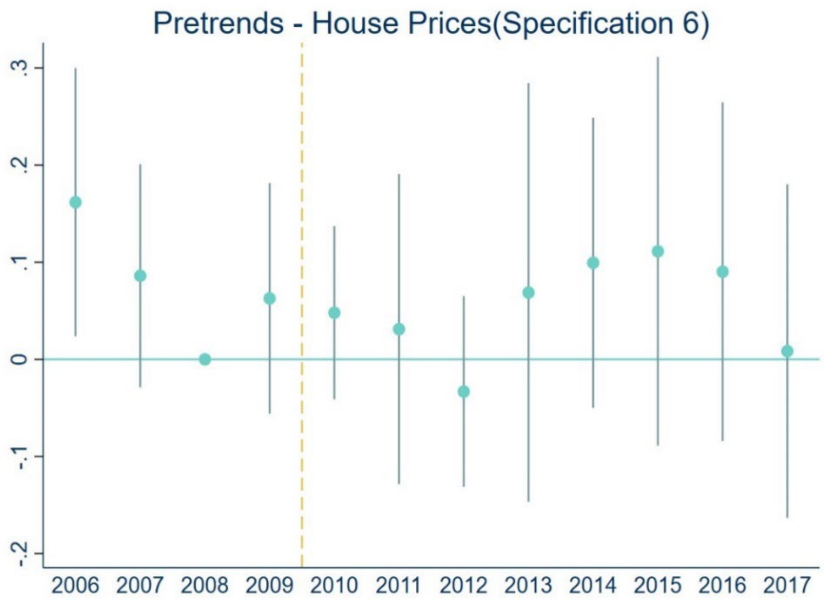
Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Figure B8. Event study plot. House prices in nearby neighbourhoods. Distance < 1000 m & owner-occupied > 50%.



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Figure B9. Event study plot. House prices in nearby neighbourhoods. Distance < 1000 m & owner-occupied > 25%.



Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Table B1. Placebo test for pre-trends. Individual level outcomes.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in 2009 × In 2006	-0.0017	-0.0017	-0.0016	0.0012
	(0.0017)	(0.0016)	(0.0055)	(0.0061)
Living in treated area in 2009 × In 2007	0.0005	0.0007	-0.0051	-0.0035
	(0.0016)	(0.0015)	(0.0042)	(0.0046)
Living in treated area in 2009 × In 2009	-0.0006	-0.0003	-0.0002	-0.0024
	(0.0016)	(0.0015)	(0.0048)	(0.0058)
Living in treated area in 2009 × Post period	-0.0023*	-0.0018	-0.0079	-0.0029
	(0.0012)	(0.0012)	(0.0056)	(0.0054)
Adjusted R^2	0.016	0.003	0.125	0.024
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Controls	Yes	Yes	Yes	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table B1 checks pre-trends for the individual level outcomes. Column (1) and (2) use an indicator for being convicted of a crime as the outcome. Column (3) and (4) use an indicator for being inactive as the outcome. Column (1) and (3) control for gender, origin and neighbourhood FE. In Column (2) and (4), we replace the controls by individual FE. All specifications include year FE and age FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table B2. Placebo test for pre-trends. Neighbourhood level outcomes.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Share of non-EU/EEA, non-Anglo-Saxons	*Share of convicted criminals*	*Share of inactives*	*Share of public housing*
Sample mean	0.4448	0.0255	0.4589	0.8402
Std. Dev.	*0.1589*	*0.0129*	*0.0889*	*0.2736*
Explanatory variables:				
Neighbourhood on the List in Jan 2011 × In 2006	0.0058	0.0006	0.0022	0.0126
	(0.0072)	(0.0018)	(0.0072)	(0.0110)
Neighbourhood on the List in Jan 2011 × In 2007	0.0015	0.0008	0.0002	0.0110
	(0.0047)	(0.0015)	(0.0041)	(0.0110)
Neighbourhood on the List in Jan 2011 × In 2009	0.0025	0.0014	0.0001	0.0264
	(0.0044)	(0.0017)	(0.0069)	(0.0236)
Neighbourhood on the List in Jan 2011 × Post Period	0.0136	-0.0016	-0.0053	-0.0269
	(0.0127)	(0.0017)	(0.0113)	(0.0216)
Adjusted R^2	0.940	0.657	0.856	0.904
Year FE	Yes			
Neighbourhood FE	Yes			
Observations	1,708			

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). In Table B2 we check pre-trends for the neighbourhood level outcomes. Column (1) use the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (2) use the share of criminals as outcome. Column (3) use the share of inactives as outcome. Column (4) use the share of public housing as outcome. We control for year and neighbourhood FE. 56.56% of the neighbourhoods in our sample were on the List in 2011. Post period is defined as 2010-2019.

Table B3. Dynamic treatment effects. Year-by-year. Individual level.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in 2009 × In 2010	-0.0012	-0.0012	-0.0060	-0.0038
	(0.0013)	(0.0013)	(0.0063)	(0.0064)
Living in treated area in 2009 × In 2011	-0.0029**	-0.0025**	-0.0016	0.0009
	(0.0013)	(0.0012)	(0.0062)	(0.0060)
Living in treated area in 2009 × In 2012	-0.0032**	-0.0027*	0.0003	0.0014
	(0.0015)	(0.0014)	(0.0065)	(0.0061)
Living in treated area in 2009 × In 2013	0.0001	0.0004	-0.0036	-0.0005
	(0.0012)	(0.0012)	(0.0057)	(0.0056)
Living in treated area in 2009 × In 2014	-0.0015	-0.0012	-0.0022	0.0019
	(0.0012)	(0.0011)	(0.0060)	(0.0059)
Living in treated area in 2009 × In 2015	-0.0010	-0.0005	-0.0050	-0.0000
	(0.0014)	(0.0012)	(0.0063)	(0.0061)
Living in treated area in 2009 × In 2016	-0.0035**	-0.0029**	-0.0107*	-0.0051
	(0.0015)	(0.0014)	(0.0064)	(0.0060)
Living in treated area in 2009 × In 2017	-0.0022**	-0.0017	-0.0114*	-0.0046
	(0.0010)	(0.0010)	(0.0061)	(0.0053)
Living in treated area in 2009 × In 2018	-0.0016	-0.0014	-0.0118	-0.0059
	(0.0013)	(0.0012)	(0.0073)	(0.0070)
Living in treated area in 2009 × In 2019	-0.0012	-0.0010	-0.0094	-0.0023
	(0.0012)	(0.0011)	(0.0093)	(0.0085)
Adjusted R^2	0.016	0.003	0.125	0.025
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1) and (2) uses an indicator for being convicted of a crime as the outcome. Column (3) and (4) uses an indicator for being inactive as the outcome. In Column (1) and (3) we control for gender and origin and neighbourhood FE. In Column (2) and (4) we replace this by individual FE. In all specifications we use year FE and age FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table B4. Dynamic treatment effects. Year-by-year. Neighbourhood level.

	Dependent variable:		
	(1)	(2)	(3)
	Share of non-EU/EEA, non-Anglo-Saxons	*Share of convicted criminals*	*Share of inactives*
Sample mean	0.4448	0.0255	0.4589
Std. Dev.	*0.1586*	*0.0129*	*0.0889*
Explanatory variables:			
Neighbourhood on the List in Jan 2011 × In 2010	0.0051	-0.0020	-0.0138
	(0.0074)	(0.0016)	(0.0103)
Neighbourhood on the List in Jan 2011 × In 2011	0.0052	-0.0030*	-0.0011
	(0.0088)	(0.0017)	(0.0097)
Neighbourhood on the List in Jan 2011 × In 2012	0.0045	-0.0030	-0.0027
	(0.0104)	(0.0019)	(0.0120)
Neighbourhood on the List in Jan 2011 × In 2013	0.0111	-0.0049**	-0.0036
	(0.0125)	(0.0022)	(0.0114)
Neighbourhood on the List in Jan 2011 × In 2014	0.0163	-0.0050**	-0.0016
	(0.0143)	(0.0025)	(0.0116)
Neighbourhood on the List in Jan 2011 × In 2015	0.0131	-0.0021	-0.0059
	(0.0161)	(0.0021)	(0.0125)
Neighbourhood on the List in Jan 2011 × In 2016	0.0104	-0.0014	-0.0109
	(0.0168)	(0.0014)	(0.0121)
Neighbourhood on the List in Jan 2011 × In 2017	0.0094	0.0003	-0.0061
	(0.0172)	(0.0017)	(0.0128)
Neighbourhood on the List in Jan 2011 × In 2018	0.0177	-0.0008	-0.0034
	(0.0188)	(0.0019)	(0.0137)
Neighbourhood on the List in Jan 2011 × In 2019	0.0187	-0.0008	-0.0098
	(0.0187)	(0.0024)	(0.0135)
Adjusted R^2	0.940	0.659	0.856
Year FE		Yes	
Neighbourhood FE		Yes	
Observations		1,708	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1) uses the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (2) uses the share of criminals as outcome. Column (3) use the share of inactives as outcome. We control for year and neighbourhood FE. 56.56% of the neighbourhoods in our sample were on the List in 2011. Post period is defined as 2010-2019.

Table B5. Heterogeneity analysis. Immigrant status.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in 2009 × Post Period	-0.0016	-0.0016	-0.0063	-0.0040
	(0.0012)	(0.0011)	(0.0058)	(0.0047)
Living in treated area in 2009 × Post Period × Immigrant	-0.0004	0.0003	0.0036	0.0050
	(0.0021)	(0.0019)	(0.0076)	(0.0069)
Adjusted R^2	0.016	0.003	0.125	0.025
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table B5 shows DDD estimates where we allow the effect to vary by individuals' immigrant status. Columns (1) and (2) use an indicator for being convicted of a crime as the outcome. Columns (3) and (4) use an indicator for being inactive as the outcome. We control for gender, origin, neighbourhood FE, year FE, age FE and the relevant interactions between immigrant status, post period and treatment status. In Columns (2) and (4) we replace neighbourhood FE as well as gender and origin controls by individual FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table B6. Heterogeneity analysis. Gender.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in 2009 × Post Period	-0.0019	-0.0013	-0.0046	-0.0015
	(0.0016)	(0.0014)	(0.0069)	(0.0062)
Living in treated area in 2009 × Post Period × Female	0.0002	-0.0003	-0.0031	-0.0004
	(0.0018)	(0.0016)	(0.0062)	(0.0055)
Adjusted R^2	0.016	0.003	0.125	0.025
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table B6 shows DDD estimates where we allow the effect to vary by individuals' gender. Columns (1) and (2) use an indicator for being convicted of a crime as the outcome. Columns (3) and (4) use an indicator for being inactive as the outcome. We control for gender, origin, neighbourhood FE, year FE, age FE and the relevant interactions between immigrant status, post period and treatment status. In Columns (2) and (4) we replace neighbourhood FE as well as gender and origin controls by individual FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table B7. Heterogeneity analysis. Age.

	Dependent variable:			
	(1)	(2)	(3)	(4)
	Convicted of a crime		*Inactive*	
	Sample: Age 15 and above		Sample: Age 16-64	
Sample mean	0.0189		0.4644	
Std. Dev.	*0.1363*		*0.4987*	
Explanatory variables:				
Living in treated area in 2009 × Post Period	-0.0020**	-0.0016**	-0.0038	-0.0020
	(0.0008)	(0.0007)	(0.0059)	(0.0056)
Living in treated area in 2009 × Post Period × Age 25 or younger	0.0006	0.0009	-0.0096	0.0047
	(0.0037)	(0.0022)	(0.0137)	(0.0066)
Adjusted R^2	0.016	0.003	0.125	0.025
Year FE	Yes	Yes	Yes	Yes
Age FE	Yes	Yes	Yes	Yes
Neighbourhood FE	Yes	No	Yes	No
Gender and origin controls	Yes	No	Yes	No
Individual FE	No	Yes	No	Yes
Observations	701,316		585,969	
Unique # individuals	61,131		54,683	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Table B7 shows DDD estimates where we allow the effect to vary by individuals' age. Columns (1) and (2) use an indicator for being convicted of a crime as the outcome. Columns (3) and (4) use an indicator for being inactive as the outcome. We control for gender, origin, neighbourhood FE, year FE, age FE and the relevant interactions between immigrant status, post period and treatment status. In Columns (2) and (4) we replace neighbourhood FE as well as gender and origin controls by individual FE. For ages above 29 age FE are grouped in 30-39, 40-49, 50-59, 60-69, 70-79 and 80 or above. Post period is defined as 2010-2019. Columns (2) and (4) report adjusted within R^2.

Table B8. Pretrends. In- and out-mover characteristics.

	Dependent variable:		
	(1)	(2)	(3)
	Share of non-EU/EEA, non-Anglo-Saxons	*Share of convicted criminals*	*Share of inactives*
Panel A. Outcome - In-mover characteristics			
Sample mean	0.4092	0.0349	0.4288
Std. Dev.	*0.1851*	*0.0247*	*0.1148*
Explanatory variables:			
Neighbourhood on the List in Jan	-0.0367*	-0.0023	0.0106
2011 × In 2006	(0.0184)	(0.0056)	(0.0239)
Neighbourhood on the List in Jan	-0.0152	0.0021	0.0266
2011 × In 2007	(0.0176)	(0.0062)	(0.0166)
Neighbourhood on the List in Jan	0.0082	0.0025	0.0069
2011 × In 2009	(0.0181)	(0.0052)	(0.0175)
Neighbourhood on the List in Jan	0.0064	-0.0027	-0.0057
2011 × Post Period	(0.0254)	(0.0044)	(0.0170)
Adjusted R^2	0.74	0.391	0.628
Panel B. Outcome - Out-mover characteristics			
Sample mean	0.3583	0.0386	0.3985
Std. Dev.	*0.1586*	*0.0247*	*0.0967*
Explanatory variables:			
Neighbourhood on the List in Jan	-0.0402*	0.0019	0.0288
2011 × In 2006	(0.0223)	(0.0066)	(0.0232)
Neighbourhood on the List in Jan	-0.0058	0.0105*	0.0351*
2011 × In 2007	(0.0192)	(0.0055)	(0.0186)
Neighbourhood on the List in Jan	-0.0108	0.0019	0.00002
2011 × In 2009	(0.0162)	(0.0057)	(0.0167)
Neighbourhood on the List in Jan	-0.0056	0.0020	0.0093
2011 × Post Period	(0.0197)	(0.0048)	(0.0128)
Adjusted R^2	0.708	0.283	0.583
Year FE		Yes	
Neighbourhood FE		Yes	
Observations		1,708	

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Panel A investigates effects on in-mover characteristics, Panel B on out-mover characteristics. Columns (1) and (2) the share of non-EU/EEA, non-Anglo-Saxon immigrants as outcome. Column (3) and (4) use the share of criminals as outcome. Columns (5) and (6) use the share of inactives as outcome. Columns (1), (3) and (5) estimate effects using OLS and Columns (2), (4) and (6) estimate effects WLS. We control for neighbourhood and year FE. Post period is defined as 2010-2019.

Table B9. Pre-trends in house prices of nearby neighbourhoods

	Dependent variable: Log of house prices					
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
Nearby to neighbourhood on the List	-0.0391	0.0594	-0.0243	0.127*	-0.0157	0.162*
in Jan 2011 × In 2006	(0.0430)	(0.0776)	(0.0433)	(0.0643)	(0.0458)	(0.0653)
Nearby to neighbourhood on the List	-0.0334	-0.0099	-0.0169	0.0474	-0.0128	0.0868
in Jan 2011 × In 2007	(0.0262)	(0.0664)	(0.0255)	(0.0516)	(0.0263)	(0.0544)
Nearby to neighbourhood on the List	0.0130	0.0365	0.0187	0.0684	0.0167	0.0627
in Jan 2011 × In 2009	(0.0256)	(0.0528)	(0.0260)	(0.0581)	(0.0253)	(0.0556)
Nearby to neighbourhood on the List	0.0128	0.0356	0.0169	0.0353	0.0224	0.0526
in Jan 2011 × Post Period	(0.0406)	(0.0521)	(0.0417)	(0.0532)	(0.0448)	(0.0612)
Adjusted R^2	0.738	0.814	0.723	0.779	0.726	0.777
Year FE	Yes					
Neighbourhood FE	Yes					
Housing characteristics controls	Yes					
Distance from treatment/control neighbourhoods	< 1 km	< 0.5 km	< 1 km	< 0.5 km	< 1 km	< 0.5 km
Minimum share of owner-occupied housing in neighbourhood	Un-restricted	Un-restricted	> 0.25	> 0.25	> 0.5	> 0.5

Source: Own calculations based on administrative register data from Statistics Denmark linked with data set on individuals' micro neighbourhood of residence constructed by Damm, Hassani and Schultz-Nielsen (2021).

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors clustered at the treatment area level in parentheses (for the control group the neighbourhoods matched with the neighbourhoods from the same treatment area, they are defined as one treatment area). Column (1), (3) and (5) restrict the sample to neighbourhoods within 1 km of treated and control neighbourhoods. Column (2), (4) and (6) restrict the sample to neighbourhoods within 500 m of treated and control neighbourhoods. Neighbourhoods within a given distance of both a control and treatment neighbourhood is assigned as treated. Distance is measured as the average distance between each hectare cell in a neighbourhood to each hectare cell in the other neighbourhood. In Column (1) and (2) we do not restrict the share of owner-occupied housing. In Column (3) and (4) we restrict the share of owner-occupied housing to be at least 25%, and in Column (5) and (6) we restrict it to be at least 50%. In all specification we control for year FE, neighbourhood FE and housing characteristics. The housing characteristics included are log of the house size in sq. m, log of the sold area in sq. m, number of rooms, the age of the property and age squared. Post period is defined as 2010-2019.