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Harsh Rhetoric and Cultural Identity:

Backlash Effects of Denmark's Ghetto List*

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Abstract

We study how the cultural identity of non-Western residents responds to the official labeling of their neighborhoods as "Ghettos" and accompanying political rhetoric emphasizing cultural difference. Using a Regression Discontinuity design, we find that non-Western residents in listed neighborhoods become more likely to give their children foreign-sounding names. This shift was accompanied by lower enrollment in early childcare, more traditional gender attitudes, stronger self-identification as an immigrant or member of a religious group, and lower propensity to follow Danish news. Socioeconomic integration was unaffected, and residential composition did not change, suggesting that the policy's stigmatizing nature and public discourse catalyzed the observed cultural backlash.

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1 Introduction

Political rhetoric over immigration has intensified and polarized in Europe and the United States over the past two decades (Holtug, 2021; Bonomi et al., 2021; Card et al., 2022). Perceived cultural consequences of immigration are among the most important determinants of natives' attitudes towards migrants (Hainmueller and Hopkins, 2014, 2015; Bansak et al., 2016) and help explain the rising support for far-right parties. As a reaction to these trends, mainstream political leaders have increasingly adopted immigrant-threat narratives (Abou-Chadi and Krause, 2020; Abou-Chadi and Stoetzer, 2020), often invoking cultural incompatibility to justify stricter immigration and integration policies (e.g. Holtug, 2021).

While there is extensive evidence on how integration policies affect migrants' economic outcomes (see Arendt et al., 2022; Foged et al., 2024; Fouka, 2024, for recent reviews), research on the effects of culturally restrictive policies on migrants' socio-cultural integration has emerged only more recently (Abdelgadir and Fouka, 2020; Fouka, 2020). Even less is known about how anti-immigration rhetoric and stigmatizing labeling practices – which impose no direct pecuniary costs or restrictions – affect the targeted group.[1] Empirically isolating the impacts of rhetoric is challenging because governments typically deploy harsh rhetoric alongside concrete policy interventions, making it difficult to disentangle the two.

This paper provides causal evidence on the consequences of stigmatizing labels and harsh rhetoric for the targeted minority by studying the introduction of Denmark's Ghetto List, a policy that publicly classified neighborhoods as "Ghettos" and portrayed the non-Western residents as a threat to Danish norms, values, and social cohesion (Simonsen, 2016). The policy explicitly framed a high concentration of non-Western residents as a problem, regardless of socioeconomic status: *"Today, more than six out of ten residents in the 29 Ghettos are immigrants or descendants from non-Western countries. This is not acceptable. No areas should have a majority of immigrants and descendants from non-Western countries"* (Regeringen, 2010).[2] While the Ghetto Plan announcing the first list articulated ambitions to transform these areas and reintegrate them into Danish society (Regeringen, 2010), the government lost power in the 2011 general election, and these visions did not re-emerge until 2017. As a result, during the first seven years following the list's introduction, the only operative instruments were moving

[1] A growing body of literature examines how anti-minority rhetoric shapes the attitudes and behavior of the majority group (e.g. Bursztyn et al., 2020; Müller and Schwarz, 2021; Djourelova, 2023), but evidence on its effects on targeted minorities remains scarce.

[2] Throughout the paper, we use the labels Ghetto and Ghetto List as defined and used by the government and propagated by the media.

subsidies, housing associations' autonomy to regulate incoming residents, and restrictions on municipal housing allocations——mechanisms that had been in place since the early-mid 2000s and were applied more broadly to disadvantaged non-profit housing areas.

We achieve causal identification from the institutional setting and data that allow us to perfectly replicate the treatment assignment rule. Non-profit housing areas with at least 1,000 inhabitants were classified as Ghettos if they exceeded two out of three predetermined thresholds for the share of non-Western immigrants and descendants, the welfare dependency ratio, and the crime conviction rate. We exploit the resulting discontinuities in Regression Discontinuity (RD) designs, comparing individuals living in neighborhoods at the margin of treatment assignment.

We tackle the multidimensional nature of the treatment assignment rule in two ways. First, building the spatial RD literature (Dell, 2010; Keele and Titiunik, 2015; Dell et al., 2018; Dell and Olken, 2019; Lowes and Montero, 2021; Méndez and Van Patten, 2022; Lang and Schneider, 2023; Ciccone and Nimczik, 2024), we reduce the three-dimensional setting to a single running variable by calculating the minimum distance to a change in treatment status. This approach increases precision by pooling observations along the entire multidimensional boundary separating treated from untreated neighborhoods. Second, we complement this approach with threshold-specific RD that focus on neighborhoods near individual cutoffs, restricting the sample to areas that exceed exactly one of the other two thresholds.

Our main analysis examines the effects of the Ghetto List on the cultural identity of individuals of non-Western origin. This focus is particularly important because the policy explicitly framed the issue as one of cultural incompatibility, positioning the adoption of Danish norms and values as the central measure of successful integration. Moreover, the Ghetto List provides a salient example of rhetoric from a sovereign authority – the government — whose laws and public statements confer legitimacy and could affect an individual's self-concept derived from group membership and shared attributes (Tajfel, 1970; Tajfel et al., 1971; Mackie, 1986; Turner et al., 1987) and sense of belonging within the host society (Major and O'Brien, 2005). We complement our analysis on identity with economic outcomes, criminal activity, and neighborhood composition, providing a comprehensive assessment of the policy's consequences.

To measure identity, we draw on data from Danish administrative registers and the Danish Citizen Survey.[3] Our primary measure of cultural identity is the Foreign Name Index (following Abramitzky

[3] Owing to the overrepresentation of non-Western individuals in non-profit housing, and by pooling three survey waves,

et al., 2020; Fouka, 2020), which captures all child naming decisions and assigns each child name a value from 0 to 100 based on its historical use by Danish-born versus immigrant parents. Andersen et al. (2025) link Danish administrative data with data from the European Social Survey and show that, in the general population, parents naming choices capture salient aspects of identity – particularly along four dimensions: religiosity, collectivism, nationalism, and traditionalism. We capture immigrants and descendants' sense of belonging by measuring the foreignness of names they give to their children and complement this with indicators for homogamous marriage, childcare enrollment, gender norms, self-reported identity, and behavior. Our baseline analysis focuses on the 2011–2013 period, when the Ghetto criteria remained fixed. We additionally conduct placebo tests in the pre-period (2008–2010) and examine longer-term effects through 2017, when new Ghetto policies began to emerge.

We find that non-Western residents living in neighborhoods listed as Ghettos choose more foreign-sounding names for their children, relative to residents in non-listed neighborhoods at the margin of treatment assignment. There are no effects prior to the policy announcement, and the results are similar when using as an outcome the pre- and post-period differences in child naming patterns within neighborhoods or within families. The impact persists through 2017, remaining at roughly two-thirds of its initial magnitude. The effect of the Ghetto list on identity is similar in magnitude and statistically significant across different types of marginal neighborhoods—both those close to the non-Western share threshold and those near the crime-rate cutoff. In addition, parents in treated neighborhoods become less likely to enroll their young children in public childcare. We find no effects on homogamous marriage or friendships, likely because partners and friends change less in adulthood, and marriages in the post-period might be between partners who met prior to treatment. However, we observe a clear and robust shift towards more traditional gender norms, self-identification as an immigrant or member of a religious group (not always significant), and a lower propensity to follow Danish news. The effect of the list on being religious (yes or no) and on thinking that immigrants get no recognition are less precise and show no clear effect.

Taken together, the evidence points to a cultural backlash among initial non-Western residents. The cultural response does not operate through changes in economic behavior, criminal activity, or residential mobility, consistent with Govind et al. (2025); Damm et al. (2025) who use a difference-in-differences design with a broader group of neighborhoods. Our estimates capture local effects at the treatment margin and therefore likely constitute a lower bound if the rhetoric affected non-Western residents more

we obtain a sufficient sample size in the monitored neighborhoods.

broadly.[4] Contrary to the stated policy goal of reintegration, the Ghetto List appears to have deepened cultural and social divides without improving socioeconomic outcomes, consistent with classic theory on ingroup/outgroup behavior, in which salience makes people orient toward their in-group and adopt more stereotypical behavior (Mackie, 1986; Tajfel et al., 1971; Turner et al., 1987). Our findings align with the theory on oppositional identities (Bisin et al., 2011), whereby majority stigmatization and hostility can induce minorities to strengthen group identity.

Our findings also relate to two main bodies of empirical work. First, we contribute to the emerging literature on the cultural impacts of forced assimilation policies. Abdelgadir and Fouka (2020) study the French 2004 headscarf ban and find evidence of identity backlash: affected Muslim schoolgirls were less likely to complete secondary education, more likely to report discrimination and identify with Islam, and experienced worse labor market outcomes in adulthood. Fouka (2020) examines post-WWI bans on German language instruction in U.S. schools and similarly finds cultural resistance, with German Americans becoming more likely to marry within their ethnic group and choose German names for their children. Bazzi et al. (2024) study Indonesia's 1970s mass schooling effort aimed at secularizing education and find that Islamic schools successfully counteracted these efforts by expanding religious education. However, not all forced assimilation policies generate backlash: Clots-Figueras and Masella (2013) find that bilingual education in Catalonia strengthened regional identity, while Bandiera et al. (2018) show that compulsory schooling in 19th-century America successfully fostered shared civic values among European migrants.

Second, our analysis relates to place-based policies in deprived areas (Ahlfeldt et al., 2017; Busso et al., 2013; González-Pampillón et al., 2020; Koster and Van Ommeren, 2019; Neumark and Kolko, 2010; Reynolds and Rohlin, 2014; Rossi-Hansberg et al., 2010). A few studies have also examined how neighborhood stigmatization affects outsiders' perceptions. Andersson et al. (2023) and Koster and van Ommeren (2023) document lower house prices in labeled disadvantaged neighborhoods in Sweden and the Netherlands, while Garrouste and Lafourcade (2023) and Grönqvist et al. (2022) find changes in school enrollment and student sorting patterns. These studies focus primarily on outcomes reflecting neighborhood attractiveness. In contrast, we examine how residents' identities change when their neighborhood is publicly classified as a "Ghetto" and framed as anti-identity to Danish society (Simonsen, 2016).

[4]The Ghetto Plan received extensive media coverage and intensified the debate on the cultural integration of non-Western immigrants in Denmark. We document the media coverage in Section 2.4.

This paper is organized as follows. Section 2 provides background information including details of the Ghetto list and related legislation, political discourse, and media coverage. Section 3 describes our data. Sections 4 and 5 discuss the empirical approach and main results. Section 6 shows some extensions and Section 7 concludes.

2 Background

2.1 Trends in immigration and residential segregation

Immigration from non-Western countries to Denmark and other European countries began in the 1960s, initially driven by labor migration.[5] Over time, asylum and family reunification became the predominant pathways for non-Western immigration. Today, immigrants and descendants from non-Western countries constitute approximately 11 percent of the population in Denmark.[6]

As in many other countries, the so-called Ghettos in Denmark emerged within large-scale housing developments built in the post-war period, with the aim of providing modern housing for low- and middle-income families. However, the high concentration of affordable rental units soon led to a clustering of economically disadvantaged residents and the emergence of social challenges (Massey and Kanaiaupuni, 1993; Damm et al., 2022; Kolstad and Larsen, 2025). Despite various initiatives, the concentration of low-income residents — many of whom are of non-Western origin — has proven difficult to reverse, partly because many residents in non-profit housing have few realistic alternatives, given the financial barriers to entering the private rental or owner-occupied housing markets, especially in larger cities.

2.2 The non-profit housing sector

The purpose of the Danish non-profit housing sector is to provide suitable housing for everyone, with rents determined by a cost-based principle rather than by supply-and-demand conditions. The Social and Housing Ministry ("Social- og Boligministeriet") establishes the regulatory framework for the non-profit housing sector and is also responsible for updating the Ghetto List annually. The housing stock

[5]The categorization of countries is a political definition used by Statistics Denmark since 2002 that categorizes EU countries, Andorra, Iceland, Liechtenstein, Monaco, Norway, San Marino, Switzerland, United Kingdom, Vatican City, United States, Canada, Australia, and New Zealand as Western and the rest of the world as non-Western. The categorization has since appeared in a wide range of laws, including those regulating the non-profit housing sector and those on integration.

[6]Immigrants from non-Western countries make up 7.4 percent of Denmark's population, while immigrants from Western countries account for 5.1 percent of the population as of January 1, 2025. Including descendants, defined as the children of immigrants, the numbers increase to 10.5 and 5.9 percent for non-Western and Western origins, respectively. All numbers are extracted from Statistics Denmark, StatBank, FOLK2, at `https://www.statbank.dk/FOLK2`.

is owned and managed by non-profit housing associations, collectively represented by the Danish Federation of Non-profit Housing Providers (BL). The National Fund for Non-profit Housing Associations ("Landsbyggefonden" or LBF) acts as a savings and investment account for the entire non-profit housing sector. LBF derives income from rents and reinvests the surplus in large-scale social interventions and redevelopment projects, based on comprehensive project proposals ("helhedsplaner" in Danish) prepared jointly by one or more housing associations and the relevant municipality. After a project has been approved, the typical duration is four years. Hence, while the reform we study does not inject more resources into the non-profit housing sector, it is possible that projects in areas on the Ghetto list somehow receive additional funding. Such investments, however, would appear with a substantial lag relative to the immediate effects we observe.

Non-profit housing is primarily allocated according to seniority on the waiting list, but municipalities have the right to assign a share of vacant units (minimum every fourth) to households with "urgent needs" who are unable to wait the standard period and cannot afford housing in the private market.[7] The non-profit housing sector is a focal point for debates on segregation and integration due to the high share of low-income and immigrant-origin residents, and the sector has, over time, been subject to both policy visions and actual regulatory changes, including the Ghetto Plan and the Ghetto List.

2.3 The introduction of Denmark's Ghetto List

The Danish Ghetto Plan was announced on October 26, 2010, by the government of then-Prime Minister Lars Løkke Rasmussen and received extensive media coverage in the final months of 2010. Part of the plan was enacted before the government left office in 2011. However, more substantial Ghetto policies did not emerge until 2018.

The 2010-2013 Ghetto criteria: The Danish authorities monitor approximately 200 public housing areas nationwide and compile statistics to determine whether a neighborhood should be designated as a Ghetto. The first Ghetto List was published on October 26, 2010. The Ghetto criteria were enacted in law on December 22, 2010, leading to the release of a revised list on January 1, 2011. Subsequent lists have since been published annually between October (until 2014) and December (2014 onwards), each applying to the following calendar year. Ghettos are identified by name, and their precise geographic boundaries and Ghetto criteria values are published publicly, whereas neighborhoods that do not meet the criteria are not publicly disclosed. The Ghetto criteria remained unchanged until 2014. During this

[7] See also Billings et al. (2024) for a description of the non-profit housing market in Copenhagen, and Kolstad and Larsen (2025) for general changes in the non-profit housing sector in Denmark.

period, neighborhoods with at least 1,000 inhabitants were listed as Ghettos if they met at least two of the following three criteria:

- More than 50 percent of residents were non-Western immigrants or descendants.
- More than 40 percent of the 18-64 year olds receive public transfer (full-time equivalents excluding students receiving study grants).[8]
- More than 2.7 percent of residents had been convicted of crimes (violence, weapons, or drugs).

To smooth short-term fluctuations, the latter two variables—welfare dependency and criminal convictions—were calculated as four-year averages.[9] Our analysis employs the exact same neighborhood classification and neighborhood characteristics as those used by the authorities (see Section 3). Although the criteria were fixed between October 2010 and February 2014, the list could still change if neighborhoods had changed. This occurred primarily at the crime threshold, owing to a general decline in crime rates in Denmark and to the volatility of this measure. In contrast, non-Western origin is an innate demographic attribute, and therefore, this neighborhood characteristic does not change unless people move. The welfare dependency ratio and the crime rate were changed to two-year averages in 2014, following complaints about the sluggishness of de-listing to improve neighborhoods. At the same time, two new criteria were introduced: the share with only basic or no schooling, and the average income expressed as a percentage of the regional average. In Sections 3 and 4, we motivate the focus on the 2010 list and the outcomes window. Section 5 tests the sensitivity of our results to alternative lists and time periods.

The legislation before and after: The stated goal of the 2010 Ghetto Plan was to reintegrate the Ghettos into Danish society by altering neighborhood composition through restrictions on municipal housing allocations, expanded autonomy to regulate incoming residents, and subsidies to support relocation for those wishing to move elsewhere - policy tools already available under the existing legislation. The idea was to curb the inflow of socially and economically disadvantaged individuals – particularly those of non-Western origin – while granting preferential access to employed individuals in the Ghettos.

[8]This criterion is sometimes described as neither in education nor in employment (Govind et al., 2025; Damm et al., 2025). However, welfare dependency aligns with the actual criteria, as it is based solely on receipt of specific categories of public transfers. For example, non-employed individuals, such as stay-at-home parents who do not receive any of the included transfers, are not counted.

[9]Juvenile crime is not included. Hence, criminal convictions apply to individuals aged 18 and older. For more details, see Act 1610 of 22/12/2010: (https://www.retsinformation.dk/eli/lta/2010/1610).

Since the early 2000's, municipalities and housing associations have been encouraged to define special letting criteria for non-profit housing areas (a policy known as "fleksibel udlejning" in Danish) and bypass non-employed individuals on the waiting lists (a policy known as "kombineret udlejning" in Danish) to counteract social problems (Kolstad and Larsen, 2025).In addition, since 2006, municipalities have been able to offer moving subsidies covering all or part of tenants' moving costs, with the explicit objective of encouraging out-migration from deprived non-profit housing areas. A 2011 amendment to the Non-profit Housing Act formally expanded the scope of the subsidy to include establishment costs, while leaving it to municipalities to determine a reasonable size for the subsidy. Hence, the practical importance of the amendment is likely minimal.[10]

Amendments to the Integration Act and the Non-Profit Housing Act in 2011 introduced explicit allocation restrictions for specific groups – newly arrived refugees, non-EU citizens (excluding students), and individuals released from correctional or custodial institutions within the past six months. These restrictions applied both to areas on the Ghetto List and to a broader set of disadvantaged areas (45 in total, subject to "kombineret udlejning").[11] As a result, there is no discontinuity in this policy measure at the cutoff. The same logic applies to the explicit provision allowing housing associations to bypass individuals receiving disability pension, sickness benefits, or unemployment benefits for more than six months.[12] This is because existing legislation—(specifically "fleksibel udlejning" and "kombineret udlejning") already permitted housing associations to bypass these groups on waiting lists in order to counteract social concentration..

Consistent with these minor, symbolic amendments to existing legislation, we find no detectable effects on mobility or neighborhood composition (see Sections 6.1 and 6.3). Renovation efforts, infrastructure improvements, and social initiatives continued to be financed through comprehensive project proposals jointly prepared by municipalities and housing associations (see Section 2). Importantly, these grants do not follow the same geographic boundaries as the Ghetto List, their evaluation criteria are not discontinuous at the cutoff, and new investments take time to materialize. As a result, they are unlikely to confound any immediate effects of the Ghetto List, consistent with the slow process of institutional and compositional change in the non-profit sector described in Kolstad and Larsen (2025).

[10]See the Non-Profit Housing Act 1040 of 01/09/2010 (§ 63 c, https://www.retsinformation.dk/eli/lta/2010/1040).

[11]See Act 462 of 18/05/2011 (https://www.retsinformation.dk/eli/ft/201012L00149) and Act 1610 of 22/12/2010 (https://www.retsinformation.dk/eli/lta/2010/1610) on municipal prohibition in placement. Notice that these changes apply to the ghettos as well as a broader category of areas ("kombineret udlejning") defined in the Non-Profit Housing Act 1040 of 01/09/2010 (§ 51 b(3)-(4), https://www.retsinformation.dk/eli/lta/2010/1040).

[12]See Act 1611 of 22/12/2010 https://www.retsinformation.dk/eli/ft/201013L00061.

2.4 Media coverage and media consumption

Figure 2 illustrates the salience of the 2010 Ghetto List by counting all pieces across three types of media outlets — nationwide newspapers, local newspapers, and TV news shows — mentioning policy terms related to the introduction of Denmark's Ghetto List.[13] The 2010 Ghetto List received intensive coverage in the final months of 2010, following the announcement of the Ghetto Plan on October 26. Danish national television covered the topic 146 times in late 2010, while major nationwide newspapers published nearly 400 articles on the subject. Coverage remained elevated relative to pre-2010 levels in subsequent years, partly due to the annual publication of the updated Ghetto List. Local newspapers are divided in the sense that the topic became permanently relevant — and thus more frequently covered — in some areas, whereas it remained marginal elsewhere. Together, this heterogeneity produced a level shift in the average coverage of local newspapers in 2010. A smaller spike in 2004–2005 appears only for national newspapers. The main trigger seems to have been the "Strategy Against Ghettoization" (Regeringen, 2004), which did not receive television coverage and was likely less relevant for local outlets because it entailed no concrete area-level implications.[14] The word Ghetto appears three times more frequently than the specific policy terms related to the introduction of the Ghetto List counted in Figure 2. Summing across all Danish media outlets, it peaked above 12,000 in 2010 (see Appendix Figure A.1). The term "ghetto" entered the Danish media vocabulary well before the 2010 Ghetto Plan and began to increase in 2004, when the Strategy Against Ghettoization identified eight "potential ghetto areas" (see Section 2.3), with the three largest areas seemingly driving pre-2010 usage.

National identity also became a theme in the Prime Minister's New Year's Address of 2011. The address is delivered annually on January 1 at 6 p.m. and broadcast nationwide. Compared with the U.S. State of the Union, it is a shorter, less partisan speech, addressed to the public rather than to parliament. It is intended to foster unity and social cohesion. Interestingly, 2004 and 2011 remain notable for their emphasis on Danishness. The 2011 address, in particular, uses the words "Danish" and "value" roughly twice as often as the average speech in the past 25 years.[15]

[13] We count all pieces mentioning any of the following terms: Ghetto List, Ghetto law, Ghetto Plan, Ghetto Package, and Ghetto proposal (search term used: ghettoliste* OR ghettolov* OR ghettoplan* OR ghettopakke* OR ghettoudspil*). Appendix A provides details on the data source and a figure covering all Denmark-based media outlets, including online sources, over a longer time window and including two additional search terms: (i) a broader one counting all pieces using versions of the word Ghetto (search term: ghetto*), and (ii) a narrower one counting only pieces mentioning the Ghetto List (search term: ghettoliste*).

[14] It gave examples of "potential Ghetto areas" and described policy visions targeted a broader group of "vulnerable housing areas".

[15] Parts of the 2011 New Year's Address rhetorically overlap closely with the 2010 Ghetto Plan. 2011 New Year's Address: *"Our society is built on values that we have fought for through generations: Responsibility for the common good. Freedom in diversity. Equal opportunities for women and men. … These are our shared values. These are Danish values."* 2010 Ghetto Plan: *"In Denmark, we have for generations built a safe, prosperous, and free society. The essential binding force has been,*

Appendix Table A.3 shows that 95 percent of inhabitants in the monitored neighborhoods follow Danish news media; 60 percent report doing so daily. Furthermore, 36 percent of respondents report that immigrants receive little recognition in Danish society. These numbers are suggestive evidence that individuals residing in Ghetto listed neighborhoods are likely to be aware that their neighborhood has been classified as a Ghetto and to be aware of the related public debate.

2.5 Rhetorical shift

The 2010 plan marked a rhetorical shift in mainstream Danish politics. The Ghetto Plan states: *"Today, more than six out of ten residents in the 29 Ghettos are immigrants or descendants from non-Western countries. This is not acceptable. No areas should have a majority of immigrants and descendants from non-Western countries"* (Regeringen, 2010). This rhetoric was echoed by the media and political actors beyond the ruling government and constructed the Ghettos as an antagonistic counter-identity to Danish society, threatening social cohesion and Danish identity (Simonsen, 2016). While the stated objective in the plan was integration, the identification of non-Western origin — an immutable characteristic — as the core problem effectively precluded the possibility of full integration in society. Suggesting that the plan served electoral purposes in the lead-up to the 2011 election.

Prior to the Ghetto Plan, anti-immigrant rhetoric was mainly confined to the Danish People's Party. However, the new discourse aligned with the sentiment expressed by other European leaders at the time. French President Nicolas Sarkozy and British Prime Minister David Cameron described multiculturalism as *"a failure"* in 2011. The former British Prime Minister Tony Blair had emphasized the need for "*integrating to the point of shared, common unifying British values*".[16] Thus, the Danish Ghetto Plan of 2010 may be seen as a crystallization of a broader trend in which the policy discourse increasingly emphasizes national identity and social cohesion, and adopts a harsher rhetoric towards groups perceived as not adapting to the host country's culture.

2.6 Group identity: Theory and measurement

The 2010 Ghetto List (Section 2.3) and its accompanying rhetoric (Section 2.5) brought the Western/non-Western distinction to the forefront of public debate (Section 2.4). The narrative emphasized differences

and still is, our values: Freedom in diversity. Equal opportunities for men and women. Responsibility for the common good. Democracy. Respect for the laws of society. A fundamental trust that we wish one another well. These are strong values that hold Denmark together, and which we must never compromise. But today there are places in Denmark where these values are no longer the prevailing ones."

[16] See Holtug (2021) for a detailed discussion of how social cohesion and national identity became prominent in policy debates in Europe. Breidahl et al. (2018) discuss the Danish case, and Bonomi et al. (2021) tell a similar story for the United States, whereby debates over cultural policies have sharpened and polarized in the past two decades.

in norms and values, creating a sense of threat to Danish identity. We hypothesize that residents in Ghetto-listed areas felt discriminated against and excluded from the rest of society – especially those of non-Western origin, who were portrayed as the root of the problem regardless of their individual socioeconomic integration.

Theoretically, experiences of exclusion and stigmatization can affect identity in opposing ways. On the one hand, they may weaken individuals' identification with the targeted group and incentivize conformity to the majority norms and behaviors as a strategy to avoid stereotyping and discrimination. On the other hand, they may reinforce stereotypes and lead the targeted group to turn inward, thereby strengthening attachment to communities that share the labels of "non-Western origin" and "Ghetto" (Tajfel et al., 1971; Mackie, 1986; Turner et al., 1987). In the first case, the Ghetto List could encourage assimilation; in the second, it may provoke an identity backlash. Hence, the policy may have heightened the salience of cultural divisions and sharpened in-group/out-group boundaries, undermining the policy's stated purpose.

Existing research shows that — even in adulthood — self-reported identity is malleable and may respond to policy changes and life events (Cassan, 2015; Antman and Duncan, 2015; Dahis et al., 2019; Antman and Duncan, 2024) and that people can cross group boundaries through assimilation or exclusion processes (Fouka and Tabellini, 2025). Theoretical models in both Economics and Sociology capture identity decisions by studying marital choices and parental child-rearing decisions (Landis, 1949; Thomas, 1951; Akerlof and Kranton, 2000; Bisin and Verdier, 2000; Bisin et al., 2004; Schøyen, 2021). Empirically, applied researchers have proxied these identity measures by examining homogamy rates, child naming patterns, and schooling choices (Araï et al., 2015; Biavaschi et al., 2017; Bandiera et al., 2018; Abramitzky et al., 2020; Fouka, 2020; Baudin et al., 2025; Adda et al., 2025). We will use some of these measures in our empirical analysis.

3 Data

Data sources: We rely on several data sources to estimate the impacts of Denmark's Ghetto List. First, the monitored neighborhoods are physically connected by cadastral numbers ("matrikelnumre") to public housing units, which do not constitute standard statistical or administrative units, and the neighborhood characteristics used for the Ghetto List also rely on nonstandard variables extracted by Statistics Denmark for the Danish authorities. We obtained access to these data sources through SBST ("Social- and Boligstyrelsen" in Danish), which allows us to replicate the treatment assignment rule. Second, we rely on two special data sources to measure immigrants' cultural identity. We use pseudo-anonymized names to build indices that capture cultural identity by child naming decisions (following Abramitzky et al., 2020; Fouka, 2020), and information on cultural behavior and norms from the Citizen Survey, collected by the Ministry of Immigration and Integration ("Udlændinge- og Integrationsministeriet). We measure other socioeconomic variables using a combination of standard administrative data and the microdata used to construct the Ghetto criteria.

Time periods: The first Ghetto List was announced in October 2010, and the criteria definitions remained fixed until the end of 2013. We estimate the effects of the policy in the three-year period following its first announcement and implementation, 2011 to 2013. There are several reasons for this. First, the announcement of the 2010 list marked a rhetorical shift (Section 2.5) and received greater media coverage (Section 2.4) than subsequent renewals of the list, suggesting it is a "stickier" treatment. Second, from an econometric perspective, it is the cleanest treatment. The Ghetto List criteria are calculated as averages over the past four years, making it difficult for local municipalities to manipulate the initial classification. Third, the criteria definitions for the Ghetto List remained fixed until the end of 2013, and we consider the period from 2011 to 2013 as our main outcome window. We will present our main results when extending the analysis to the 2014-2017 period, during which there were minor changes to the criteria. We do not use the 2018-2023 period, as it coincided with the implementation of several new Ghetto policies (see Section 2.3 for details).

To ensure that we capture the effect of the policy rather than an underlying difference between listed and non-listed neighborhoods, we also examine placebo effects in a period of the same length, from 2008 to 2010, before treatment.

Population of interest: The rhetoric used by the Government and propagated in the media targeted non-Western residents in Ghettos, which may have contributed to perceptions of stigmatization and exclusion from Danish society (see Sections 2.4 to 2.6). To test this hypothesis, we select adult individuals of non-Western origin residing in a monitored neighborhood with more than 1,000 inhabitants as of 31st December 2010 (see Section 2.3 for details on Ghetto criteria). Statistics Denmark defines an individual as having a non-Western background if either the individual or both parents were born in a country classified as non-Western.[17] The age criterion we impose is such that the initial residents are 20 to 50 years old in the first year we observe them, 2008, and 25 to 55 by 2013, which is the last year of our main

[17] Western countries are defined as EU countries, Andorra, Iceland, Liechtenstein, Monaco, Norway, San Marino, Switzerland, United Kingdom, Vatican City, United States, Canada, Australia, and New Zealand, and the rest of the world is categorized as non-Western.

analysis. This is the age range where our primary outcomes — child naming practices and identity questions from the Citizen Surveys — are relevant. Additionally, we require that individuals are observed in Denmark during the period in which we measure our outcomes of interest (2011-13).

These selection criteria yield 175 monitored neighborhoods with at least 1,000 inhabitants in 2010 and 149,751 initial adult residents, of which 53,549 are of non-Western origin. 4.4 percent emigrate or die before we observe outcomes between 2011 and 2013, and do not contribute to our main results (leaving us with 51,171 individuals). To analyze mobility and neighborhood composition, we consider all individuals residing in a monitored area in 2008, 2010, and 2013.

Cultural identity outcomes: To capture cultural identity among adults in the Danish register data, we follow the literature discussed in Section 2.6 and construct a measure of parents' naming decisions for their children. We follow Abramitzky et al. (2020); Fouka (2020) who focus on whether the child's name "sounds" Danish or foreign by calculating the Foreign Name Index (FNI) for the first child born within each time window (2008-2010, 2011-2013, and 2014-2017).[18] A value of 100 indicates that the name has been used exclusively among individuals with a foreign background over the past 20 years, and a value of 0 indicates that it has been used exclusively among individuals of Danish descent.[19] The FNI is our preferred measure of culture, since it has been validated by the literature, is available in the population-wide register data, and is an action/decision that can react fairly quickly to changes in the underlying environment (conditional on having a child).

We construct additional culture measures to support the FNI measure. For the same children, we examine the parental decision to enroll them in daycare between ages 0 and 2 within the analysis window. Childcare is a formal institution regulated by the Danish government, and has been seen by many politicians as a tool to foster the integration of migrant children. Therefore, non-Western parents might prefer to refrain from using public childcare since this institution is formally associated with the discriminatory entity (e.g., the Government that enacted the Ghetto policy). We also calculate homogamy among those who got married between 2011 and 2013. We compute this measure since it has been widely used in the literature. However, given that we examine a relatively narrow time period (2011-13), it is unlikely that the partner choice occurred during this period. One should also note that the overlap with the population who name a child is limited.

Finally, we rely on a set of questions from the 2012-2014 Citizen Survey that relate to our context.[20]

[18] Using the FNI of the last child born in the three-year interval does not change our conclusions.

[19] We use the past 20 years as we can apply this consistently for all time periods, but show robustness checks using smaller and larger windows.

[20] The Citizen Survey is a cross-sectional survey sent to a random sample of adult (18 or older) immigrants and descendants

Namely, we use questions that directly reflect identity (identify as a migrant or a member of a religious group, practices religion, at least half of friends have an immigrant background), connection to Danish Society (thinks that immigrants get recognition in Denmark, follows Danish news regularly), and gender norms (men and women should have equal access to divorce, child custody, inherit, and work). We provide further details on the construction of all the outcome variables and their summary statistics in Appendix B.

Since our outcomes of interest rely on individuals who either have a child, have married, or answered the survey in 2012 or 2014, we compute a set of dummy variables to compare these individuals with those who did not have a child, marry, or answer the survey.

Socioeconomic outcomes: To ensure that any observed effects on cultural identity do not operate through changes in labor market behavior — and because the Ghetto criteria directly incentivize neighborhoods to improve along these dimensions to be delisted — we also compute a set of additional outcomes that capture residential mobility and socioeconomic conditions.

The first Ghetto criterion concerns the share of non-Western immigrants and their descendants in the neighborhood; this share can change only through mobility, as it is an immutable individual characteristic. In contrast, the two remaining criteria — welfare dependency and crime rates — may change either due to mobility or behavioral adjustments among residents.

Our primary individual-level socioeconomic outcomes are drawn directly from the microdata underlying these criteria. We use (i) an indicator for criminal convictions (violence, weapons, or drugs), and (ii) welfare dependency. Welfare dependency is measured in full-time equivalents – the share of the total working year during which the person's primary income consisted of public transfers – and as a dummy variable if an individual has been dependent on welfare for at least one month.

To shed further light on labor market integration as a potential mechanism for cultural integration, we also examine whether an individual has been employed for at least one month, the annual share of months employed during the observation window, and the mean annual gross earnings (in USD 1,000). [21]

from non-Western countries with a minimum of three years of residence in Denmark and a small comparison group of native-born. Most individuals responded only once between 2012 and 2014. In the rare case that an individual is interviewed more than once, we consider their first answer. The survey of year "t" is typically launched in October "t-1" and closed in February t. The three waves we use, therefore, refer to 2011 to early 2014.

[21] We compute the inverse hyperbolic sine (IHS) to account for the zeros.

4 Estimation strategy

The Ghetto List assigns treatment based on three neighborhood characteristics, requiring that any two exceed fixed thresholds. This creates a multidimensional treatment boundary. Our goal is to compare neighborhoods that are just on opposite sides of this boundary, regardless of which specific criterion triggers treatment, to obtain a single estimate for the overall impact of the list and increase power in estimations relying on small samples from the Citizen Survey.

We leverage the precise treatment assignment rule (described in Section 2.3) in a Regression Discontinuity (RD) design, using the exact spatial boundaries of the monitored neighborhoods and the precise neighborhood criteria and cutoffs used by policymakers. This implies that we perfectly predict treatment by the Ghetto List. We normalize each running variable around its cutoff and divide the normalized variable by its standard deviation, such that the running variables, $R_k n(i)$, have cutoffs, c_k, at zero and a comparable scale. The value of the running variable for individual i is determined by the neighborhood, n, of residence in 2010.

Formally, treatment is determined by exceeding the threshold in any two out of the three running variables $\sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} \geq 2$.[22] Figure 1a illustrates the threshold surface plane for the non-Western share. All neighborhoods to the left of the blue surface plane are below the non-Western origin threshold; all on the right are above it. As illustrated in Figure 1b, the boundary separating treated from untreated neighborhoods is a piecewise planar surface resulting from the intersection of the three threshold planes. [23]

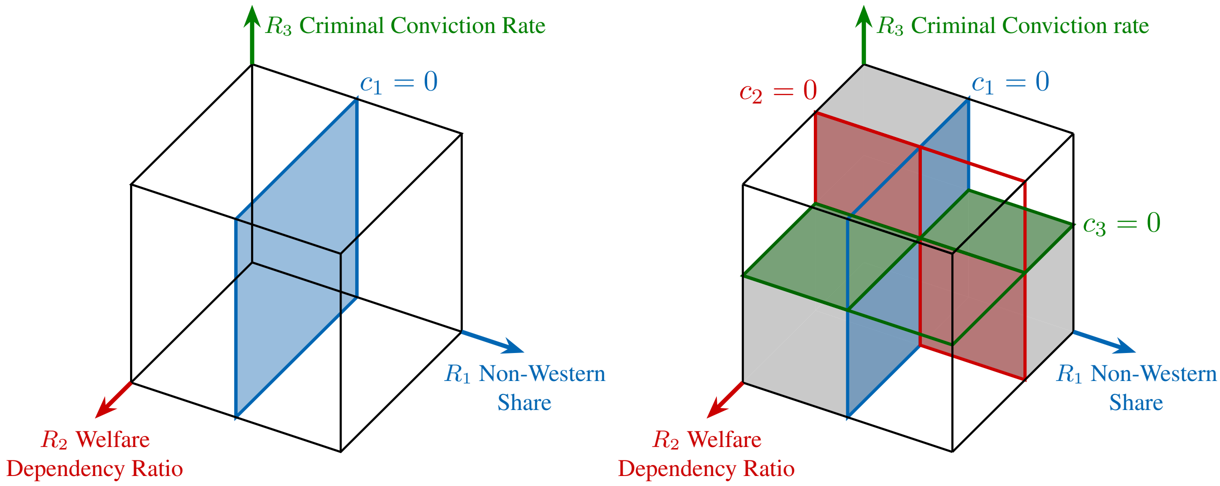
Our main estimator reduces the three-dimensional setting to a single dimension by constructing a new running variable, $M_{n(i)}$, defined as the minimum distance to a change in treatment assignment.[24] The Minimum Distance RD estimator increases precision by averaging along the entire boundary separating treated from untreated neighborhoods. We complement this approach with an RD estimator around a single threshold, c_j, by restricting the sample to neighborhoods that exceed exactly one of the two remaining thresholds, c_k and c_l, where $k, l \neq j$. These subsets allow us to examine whether treatment effects differ across neighborhood types — those with a non-Western share close to 50 percent,

[22]This is true after deleting neighborhoods with less than 1,000 inhabitants in 2010, leaving us with 175 neighborhoods (see Section 3).

[23]Note that only the colored shaded areas of the individual threshold planes separate treated neighborhoods (located in the non-shaded hyper-rectangles) from untreated neighborhoods (located in the gray-shaded hyper-rectangles).

[24]The Minimum Distance Regression Discontinuity estimator (Reardon and Robinson, 2012; Wong et al., 2013) has been applied to spatial discontinuities, combining latitude and longitude into a univariate running variable (see e.g. Dell, 2010; Dell et al., 2018; Dell and Olken, 2019; Keele and Titiunik, 2015; Lowes and Montero, 2021; Méndez and Van Patten, 2022; Lang and Schneider, 2023; Ciccone and Nimczik, 2024) and to study multiple election outcomes (Folke, 2014; James J. Feigenbaum, 2017). We extend this estimator to a three-dimensional setting.

and those with a crime rate close to 2.7.[25]



(a) Non-Western share threshold

(b) Treatment boundary

Figure 1: Three-dimensional setting

Notes: The box represents the space where the 175 monitored neighborhoods can be located. The blue surface plane represents the non-Western share threshold, the red the welfare dependency ratio threshold, and the green the criminal conviction rate threshold. Figure (a) shows only the blue surface plane, where neighborhoods located to the left of the plane are below the non-Western share threshold, and those to the right are above the threshold. Figure (b) shows the intersection of the three surface planes that separates treated neighborhoods (not shaded) from untreated neighborhoods (shaded in gray).

4.1 Minimum distance regression discontinuity

Our baseline estimating equation is an RD specification of the following form:

$$Y_i = \alpha + \tau T_{n(i)} + \beta M_{n(i)} + \alpha_1 T_{n(i)} M_{n(i)} + \sum_s reg^s_{n(i)} + \varepsilon_i, \tag{1}$$

where Y_i is the outcome of individual i, and $T_{n(i)} = \mathbb{1}\{M_{n(i)} > 0\}$ is an indicator equal to one if i is treated and zero otherwise. $M_{n(i)}$ is the minimum distance to a change in treatment status for individual i's neighborhood of residence in 2010 and is defined as:

$$M(R_1, R_2, R_3) = \begin{cases} \min\limits_{k\,:\,R_k>0} \; d_1(R_k)\,, & \text{if } \sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 2 \\ \min\limits_{k\,:\,R_k\leq 0} -d_1(R_k)\,, & \text{if } \sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 1 \\ \min\limits_{k,\,j} \; d_2(R_k, R_j)\,, & \text{if } \sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 3 \\ \min\limits_{k,\,j} -d_2(R_k, R_j)\,, & \text{if } \sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 0 \end{cases}$$

[25] Additionally, we use the threshold-specific RD approach as a placebo test by restricting the sample to neighborhoods that do not exceed two thresholds, such that passing a specific threshold does not trigger treatment. As we discuss in the next section, we cannot estimate a local effect around the welfare dependency threshold (40 percent) due to missing mass above this threshold after subsetting the data.

When using the Euclidean distance (baseline), $d_1(R_k) = \sqrt{R_k^2}$ and $d_2(R_k, R_j) = \sqrt{R_k^2 + R_j^2}$, when using the Manhattan distance (robustness check), $d_1(R_k) = |R_k|$ and $d_2(R_k, R_j) = |R_k| + |R_j|$, and when using the Mahanobis distance (robustness check), $d_1(R_k) = \sigma_k^{-1}\sqrt{R_k^2}$ and $d_2(R_k, R_j) = \sqrt{s_{kk}R_k^2 + 2s_{kj}R_kR_j + s_{jj}R_j^2}$, where is the standard deviation and s_{kk}, s_{kj} and s_{jj} are elements of the inverse covariance matrix. Intuitively, in the first two parts of M, neighborhoods above two thresholds or above only one threshold must change in exactly one variable to flip treatment status.[26] $M_{n(i)}$ will thus equal the value of the running variable that is closest to the threshold. In the third and forth parts of M, neighborhoods above or below all three thresholds must change in at least two variables to alter treatment status.[27] In this case $M_{n(i)}$ will equal to the sum of the two running variables that are closest to the threshold.[28]

β is the causal parameter of interest capturing the average effect of the Ghetto List along the entire boundary separating treated from untreated neighborhoods in the space spanned by the three neighborhood characteristics (R_1, R_2, R_3). Finally, equation (1) includes a set of indicators, $reg_{n(i)}$, equal to one for the nearest of seven regions in the assignment space. In six regions, treatment status changes across a single threshold $c_k = 0$, conditional on $R_j > 0$ and $R_l \leq 0$, such that we are comparing neighborhoods that have similar values of R_j and R_l, but some are just below the threshold $\left(\sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 1\right)$ while others are just above the threshold $\left(\sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 2\right)$, where $k, l, j \in \{1, 2, 3\}$ are distinct. Visually, we are comparing neighborhoods around each of the six colored-shared areas of the treatment boundary in Figure 1b. The seventh is a residual region which includes the neighborhoods that are very close to $(c_1 = 0,\ c_2 = 0,\ c_3 = 0)$ but either just above $\left(\sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 3\right)$ or just below $(\sum_{k=1}^{3} \mathbb{1}\{R_k > 0\} = 0)$. This corresponds to the neighborhoods that are close to the intersection point of the three threshold surface planes in Figure 1b. Note that, by construction, in this seventh region, neighborhoods are restricted to be closer to the cutoff than in the other six regions. This happens because we require that $d_2(R_k, R_j) \leq 1$, since this implies that $\sqrt{R_k^2 + R_l^2} \leq 1$ it must be that $\sqrt{R_k^2} < 1$ and $\sqrt{R_l^2} < 1$. These region-fixed effects follow the recent literature on spatial RD and ensure that we

[26]Once we impose the restrictions on the right-hand-side, the shortest distance of a point with coordinates $\mathbf{r} = (r_1, r_2, r_3)$ to a threshold is either the vertical or the horizontal distance. For example, consider the second case of a non-treated neighborhood $\mathbb{1}\{R_k > 0\} = 1$ with $R_3 = r_3 > 0$. Then we want to find the distance of $\mathbf{r}$ to the boundary points $(0, r_2, r_3)$ and $(r_1, 0, r_3)$. $M_{n(i)}$ will equal the shortest of the two distances, $d\big(\mathbf{r}, (0, r_2, r_3)\big) = \sqrt{(r_1 - 0)^2 + (r_2 - r_2)^2 + (r_3 - r_3)^2} = \sqrt{(r_1)^2} = r_1$ and $d\big(\mathbf{r}, (r_1, 0, r_3)\big) = \sqrt{(r_2)^2}) = r_2$.

[27]If we consider a point with coordinates $\mathbf{r} = (r_1, r_2, r_3)$, we want calculate the distance between this point and three boundary points: $(0, r_2, r_3)$, $(r_1, 0, r_3)$, and $(0, r_2, r_3)$.

[28]Note that in the second part of M, while neighborhoods need to cross the threshold in two running variables, by construction, we are being more restrictive in terms of the distance to the cutoff in each running variable. Namely, we require that $d_2(R_k, R_j) \leq 1$, since this implies that $\sqrt{R_k^2 + R_l^2} \leq 1$ or $|R_k| + |R_l| \leq 1$ it must be that $R_k < 1$ and $R_l < 1$. In the first two parts of M we only require that $d_1(R_k) \leq 1$, such that $R_k \leq 1$.

are comparing proximate observations within each region of the multidimensional threshold structure.[29]

We use a one-standard-deviation bandwidth ($M_{n(i)} \in [-1,1]$) and a triangular kernel, assigning greater weight to observations near the cutoff, as our baseline.[30] Standard errors are clustered at the neighborhood level. In the robustness checks (Section 5), we consider uniform weights, alternative bandwidths, and higher order polynomials of $M_{n(i)}$ which are included linearly in equation (1) as well as a specification including the predetermined covariates (listed in the balancing tests).

4.2 Threshold-specific regression discontinuity

To estimate the average treatment effect at threshold c_j, we restrict the sample to individuals in neighborhoods that are above only one of the other two thresholds, c_k or c_l, so that crossing c_j triggers treatment. Formally, we estimate the following equation with neighborhoods fulfilling ($R_k \leq 0,\ R_l > 0$) or ($R_l \leq 0,\ R_k > 0$) where $k, l \neq j$:

$$Y_i = \alpha + \tau D_{jn(i)} + \beta_j R_{jn(i)} + \alpha_1 D_{jn(i)} R_{jn(i)} + \left[\rho_1 R_{kn(i)} + \rho_2 R_{ln(i)}\right] + \varepsilon_i. \tag{2}$$

Y_i is the outcomes of individual i, and $D_{jn(i)} = \mathbf{1}(R_{jn(i)} > 0)$ is an indicator that equals one if i is treated and zero otherwise. Passing $c_j = 0$ is sufficient to trigger treatment under our sample restrictions. We control for the other two neighborhood criteria, R_k and R_l for $k, l \neq j$, to account for correlations across neighborhood criteria. β_j is the RD estimate of the average causal effect of the Ghetto List for individuals in neighborhoods near the threshold c_j.

Equation 2 is estimated with bandwidth of one standard deviation ($R_j n(i) \in [-1,1]$) and a triangular kernel. Standard errors are clustered at the neighborhood level. We provide robustness checks similar to those for the Minimum Distance RD estimator.

4.3 Preferred estimator

The threshold-specific RD produces three average treatment effects — one for individuals residing in neighborhoods where non-Western immigrants and descendants account for about 50 percent of residents, one for neighborhoods with roughly two in five residents dependent on public transfers, and one for neighborhoods with around 27 out of 1,000 residents convicted of crime. The main limitation of

[29]In spatial RD designs, the boundary is a geographic border that is typically divided into several same-length segments represented with fixed effects similar to ours (Dell, 2010; Keele and Titiunik, 2015; Dell et al., 2018; Dell and Olken, 2019; Lowes and Montero, 2021; Méndez and Van Patten, 2022; Lang and Schneider, 2023; Ciccone and Nimczik, 2024).

[30]The baseline specification follows the recent consensus in the literature and uses a local linear specification and triangular kernel, allowing for different linear slopes at each side of the cutoff and giving more weight to the observations closer to the cutoff (Gelman and Imbens, 2019; Cattaneo et al., 2020a).

this design is that the sample restrictions reduce the effective data mass near some thresholds.[31] Appendix Figure A.2 shows that there is insufficient mass above the welfare-dependency cutoff to support a credible RD estimate around that threshold after sectioning the data. Therefore, we only show the threshold-specific RD estimates at the welfare dependency and non-Western share thresholds.

Substantial heterogeneity across threshold-specific estimates would support this approach. However, as shown in Section 5, we find no evidence supporting such heterogeneity. Hence, our preferred specification is the minimum-distance estimator, which uses all neighborhoods and increases precision by estimating a single causal effect along the entire treatment boundary.[32] This is particularly important when using the Citizen Survey, where we have a reduced number of observations and the threshold-specific RD is not feasible.

The RD estimates from either model should be interpreted as local causal effects of the 2010 Ghetto List, not confounded by differences further from the cutoffs that a Difference-in-Differences (DiD) approach using a wider set of neighborhoods might capture. Govind et al. (2025) and Damm et al. (2025) study changes in neighborhood composition (labor market outcomes and crime) by comparing ghetto-listed and non-listed neighborhoods in a DiD setting over an extended time period. They do not observe the actual criteria used by the authorities and therefore cannot compare neighborhoods around the boundary separating treated from non-treated in the three-dimensional space spanned by the three criteria.

4.4 Validity checks

Discussion of potential manipulation: Identifying the causal effects of the 2010 Ghetto List on initial residents requires that the treatment assignment was not manipulated. The first list was published in October 2010, the legislation was passed in December, and an updated list followed in January 2011. Three criteria enter the treatment assignment rule (Section 2.3). The only difference between the October and January lists is that Statistics Denmark incorporated more recent crime data, so that both the crime rate and the welfare dependency ratio were calculated for 2006-2009, while the 2010 list used 2005-2008 for the crime criterion. The population on January 1, 2010, was used to compute the non-Western share in both cases. Because all criteria rely on data predating the announcement, actors outside the government could not have manipulated the 2010 (or 2011) list. By contrast, later lists could reflect behavioral responses by individuals, housing associations, or municipalities, which is why we focus on the first list.

[31] Particularly given that our running variables are defined at the neighborhood level and we have 175 neighborhoods.

[32] Notice also that neighborhoods can contribute to more than one of the threshold-specific RD estimates, while the Minimum Distance estimator uses each neighborhood only once.

A remaining concern is that the government chose the criteria to include certain neighborhoods. Policymakers certainly knew that some large ghettos —– also named in the 2004 Strategy Against Ghettoization (Regeringen, 2004) — would qualify under the criteria they created. At the margin, however, such targeting was less controllable and depended on data updates from Statistics Denmark, including revisions between 2010 (when the criteria were set) and January 2011 (when the first binding list was produced). Three neighborhoods were removed from the legally binding list relative to the list announced in late 2010—all due to reductions in crime. By October 2011, two additional neighborhoods were de-listed: one due to declining crime and the other because its population fell below 1,000.

Unlike the large, well-known ghettos, marginal neighborhoods had no prior "Ghetto" label. For these areas, the designation and the intense media coverage likely came as a surprise. Even the government, despite defining the assignment rule, had imperfect control over which neighborhoods ended up treated at the margin. Finally, the threshold values themselves suggest limited scope for fine-tuned targeting. The cutoff for the non-Western share (50 percent) is simple, easy to communicate, and consistent with the policy's framing: *"No areas should have a majority of immigrants and descendants from non-Western countries"* (Regeringen, 2010). The welfare-dependency cutoff (40 percent) is also a round number. The crime-rate threshold (2.7 percent) was mechanically set to three times the national average.[33]

Statistical tests: Figure 3 plots estimated density functions using local polynomial methods (Cattaneo et al., 2020b). Panels a–c show densities for each criterion, standardized and centered at their thresholds; Panel d shows the minimum Euclidean distance to the multidimensional boundary. All densities appear smooth, with no indication of manipulation. Estimated discontinuities are statistically indistinguishable from zero. As noted in Section 4.3, data are thin above the welfare-dependency cutoff, leaving insufficient mass to estimate a credible threshold-specific RD at that margin.

Figure 4 assesses covariate balance using predetermined individual characteristics as the dependent variable in equation (1) for the entire population of adults residing in a monitored neighborhood in 2010. Variables are dichotomized for comparability across outcomes. Residents of listed and non-listed neighborhoods near the boundary look similar across all characteristics. Ghettos have slightly fewer refugees, but the difference is small, and a single statistically significant difference is expected when testing 19 predetermined characteristics. In Section 5.1, we also show placebo effects for naming

[33] However, it was fixed at 2.7 until 2017 and then varied between 1.98 and 2.35.

decisions between 2008 and 2010, which provide an additional check on baseline comparability.

5 The effects on non-Western initial residents

Our primary objective is to assess whether the Ghetto List encouraged cultural assimilation among non-Western initial residents in listed neighborhoods, who were targeted by the policy, or whether it prompted the opposite response —– a reinforcement of minority identity and further distancing from Danish norms. We begin with child naming outcomes, which are observed for the full population in register data and allow placebo tests, threshold-specific RDs, and heterogeneity analyses (Section 5.1). We then turn to additional measures of cultural identity, mainly from the Citizen Survey (Section 5.2). Although the survey provides rich information on norms and behaviors, its small sample and post-treatment timing limit the strategies we can use to our baseline specification with homogeneous effects.[34] Appendix Table A.4 shows no discontinuities in marriage, fertility, or Citizen Survey response rates at the cutoff, suggesting no selection into our cultural outcomes. As secondary outcomes, we analyze socioeconomic integration (Section 5.3) to shed light on potential channels and provide a broader picture of the responses (see also Damm et al., 2025; Govind et al., 2025). We conclude this section by adding the residents of Western origin and extending the time horizon (Section 5.4).

5.1 Effects on child naming patterns

Main results: Table 1 shows our main results on the impact of the 2010 Ghetto List on child naming patterns in listed neighborhoods. Column 1 reports the causal effect in the post-treatment period (2011-2013) using our minimum-distance RD design (Panel A) and threshold-specific RD designs based on the non-Western share (Panel B) or the crime rate (Panel C) as running variables.

Using our preferred estimator in Panel A, we find a 6.3 index point increase in the foreignness of names given to newborns, corresponding to a 9 percent increase relative to the untreated mean at the cutoff. The threshold-specific RD estimates yield effects of comparable magnitude to the baseline estimates, despite being based on substantially fewer observations.

The density and balancing plots (Figures 3-4) suggest that the treatment-control difference in outcomes at the cutoff can be interpreted as a causal effect of the policy. To further bolster our confidence that the RD estimate reflects a causal response to the Ghetto List rather than an underlying difference between the treated and untreated, we estimate the same designs in a placebo period (2008–2010) and

[34] The limited number of observations also necessitates pooling across three survey waves, which determines the choice of three-year outcome windows.

compute First-Difference RD estimates. Column 2 reports the placebo estimate using all individuals who had a child in the pre-period, while Column 4 collapses the data to the neighborhood level and re-weights observations using those who had a child in the post-period. This allows us to compute a First-Difference RD estimate within families who have a child in both periods (Column 3) and within neighborhoods using all individuals who had a child before and/or after treatment.

The First-Difference RD estimates are similar to those in Column 1. Together, the estimates in Table 1 reveal that the treatment-control differences in outcomes we identify after treatment did not exist before treatment; instead, they emerged in response to the treatment, and the response is surprisingly similar looking within families who have a child in both periods and looking within neighborhoods, though smaller within families. This pattern holds not only for our preferred minimum-distance RD design but also for the threshold-specific designs evaluated at neighborhoods with approximately 50 percent non-Western origin and a crime rate around 2.7 – the two thresholds with sufficient mass to support estimation (see Section 4.3).

As an additional placebo test, Figure 5 shows the estimates from Panels B and C, Column 1 of Table 1, alongside a similar estimate based on the residual neighborhoods in which crossing the threshold does not trigger treatment.[35] The effects of these placebo treatments are zero and insignificant.

Figure 6 visualizes the RD estimates in the post-treatment period (Panel a), the pre-treatment period (Panel b), and the first-difference RD specification (Panel c). Each panel plots the neighborhood-level mean foreign name indices against the minimum distance to the cutoff, along with the fitted regression lines. The mean outcome for untreated neighborhoods at the cutoff is higher in the pre-period, reflecting a general downward trend in foreignness of names given to newborns over time since immigration of the parent and across parents by age at first birth (Appendix Figure A.3).

Robustness: Figure 7 summarizes a broad set of robustness checks. The estimated effect is stable across bandwidths (Figure 7a), decreasing slightly for wide bandwidths and increasing modestly for narrow ones. This pattern is reassuring, as it indicates that the estimated effect is consistently present across a wide range of bandwidths and varies little in magnitude. Figure 7b considers alternative specifications. First, we show the estimate from a donut specification, excluding the neighborhood closest to the boundary on each side (two in total). Second, we include all predetermined covariates listed in

[35]For the true treatment, we condition on exactly one of the other two criteria being satisfied, so that meeting the second criterion triggers treatment. For the placebo treatment, we condition on satisfying none of the other two criteria.

Figure 4 and add year-of-birth fixed effects for the newborns (we pool three post-treatment years). Next, we show estimates based on uniform weights and control for distance to the boundary more flexibly using second-order polynomials. Finally, we measure distance using the Manhattan and Mahalanobis distances rather than the Euclidean distance used in our benchmark. In all cases, the estimates remain similar to the baseline, with reduced precision only for higher-order polynomials (consistent with overfitting near the cutoff, Gelman and Imbens, 2019).

Figures 7c and 7d evaluate alternative treatment and outcome definitions. Our baseline is to assign treatment status based on the 2010 list as this is the most exogenous and the strongest treatment measured by political rhetoric and media coverage (Sections 2.4 and 2.5). We obtain slightly larger effect sizes by excluding the three neighborhoods that dropped off the list in January 2011 and restricting treatment to areas consistently labeled through 2013. Cleaning the control group of a few areas that later became listed produces effect sizes similar to the baseline. Likewise, Figure 7d shows that our main results are robust to alternative constructions of the Foreign Name Index, using a longer or shorter history of naming decisions.

Heterogeneity: One question that emerges from our main results is whether individuals who contributed economically to Danish society perceived the government rhetoric and the Ghetto label as more unjust than individuals who were detached from the labor market and formal public institutions. Similarly, government statements that frame individuals of non-Western descent as "them" and those of Danish and Western descent as "us" may have differential effects on immigrants versus descendants, who may have little or no lived experience in their parents' country of origin.

Figure 8 examines heterogeneity by pre-treatment (2010) socioeconomic status and settlement history. Individuals with a criminal conviction history – 5.3 percent of residents– respond strongly to the Ghetto and non-Western labels. The designation as a Ghetto – *"where Danish values are no longer the guiding norm, and where the rules that apply in the rest of society therefore do not have the same effect."* (Regeringen, 2010) – appears to have further deepened the existing divide for this group. The non-employed show no response, whereas the employed majority (approximately 60 percent) accounts for the main effect. This heterogeneity suggests that individuals who contribute economically to Denmark and remain attached to formal public institutions respond more strongly to government rhetoric claiming that ghetto residents *"do not contribute to society"* (Regeringen, 2010). We find no evidence of heterogeneous effects by immigrant generation or time since immigration.

Summary Individuals portrayed as non-Danish by the policy (Sections 2.5-2.6) responded by giving their children more foreign-sounding names, thereby increasing the cultural distance between their naming practices and those of the majority population. The strongest responses come from employed residents and those with criminal records; the non-employed do not respond. This pattern suggests disappointment among those who invested in labor market integration and further distancing among those with weaker economic ties to the Danish society.

5.2 Effects on additional measures of cultural identity

Table 2 provides additional evidence that the 2010 Ghetto List triggered a cultural backlash among the targeted population, drawing on two additional register-based outcomes—childcare enrollment and homogamous marriage—and several outcomes from the Citizen Survey. The detailed definitions of the survey variables can be found in Appendix Table A.2.

Childcare enrollment: For the same parents who named a child in the three-year outcome window, we observe whether the child entered Danish childcare between ages 0 and 2. Childcare is widely regarded as an instrument for integrating minority and disadvantaged groups and fostering social cohesion. If more foreign-sounding names reflect a deeper intention to strengthen in-group identity and belonging, parents may also delay or avoid placing the child in Danish childcare. Consistent with this, we find an 8.2 percentage point decline from a baseline enrollment rate of 87 percent (Table 2, Column 1).

Homogamy and friendships: We find no effect on homogamous marriage (Table 2, Column 2) or on whether at least half of a respondent's friends have an immigrant background (Table 2, Column 5). This likely reflects the short time horizon (three years) and the fact that partnership and friendship networks among adults tend to be stable, with many post-treatment marriages based on relationships formed before the policy.

Self-identification, perceptions and religiosity: We find a large increase in self-identification as an immigrant or member of a religious group: 17 percentage points in our baseline sample and 22 percentage point when including all survey respondents in monitored neighborhoods, including individuals older than 55 in 2013. We also find a 9 percentage point (insignificant) decrease in the belief that immigrants receive recognition in Danish society — a significant 12 percentage point decrease across all ages — consistent with heightened perceptions of exclusion. Table 2, Column 4 shows that individuals in Ghetto-labeled neighborhoods are not more likely to become religious. However, we cannot infer whether religiosity increased among those who already believe in God, because the survey question on religious practice allows only yes, no, or do not know responses.

Consumption of Danish news: The vast majority of residents in monitored neighborhoods are well integrated into Danish information channels. Among individuals just below the treatment threshold, 93 percent report following the Danish news daily or weekly. The Ghetto designation leads to an 11 percentage point decline in this measure (Table 2, Column 7). This suggests a withdrawal from mainstream information channels that were also central to the public discourse surrounding the Ghetto List.

Gender norms: Gender equality featured prominently in the announcement and justification of the Ghetto Plan. The Citizen Survey includes four questions on gender norms during the post-treatment years. We find systematic shifts toward more traditional gender attitudes, which are statistically significant for two or three of four items (Table 2, Columns 8-11). Agreement on equal rights to divorce declines by 22 percentage points, and agreement on equal opportunity to work and to inherit (insignificant in baseline sample) decline by 8 percentage points. We find no statistically significant change in views on child custody, where the underlying norm may be less clear.

Summary: These results indicate a broader cultural backlash among individuals living in neighborhoods publicly listed as Ghettos and portrayed as having non-Danish values (Sections 2.5 and 2.6). Treated individuals distance themselves from Danish naming patterns, adopt more traditional gender norms, and sharpen their in-group identification, while reducing engagement with Danish media. The patterns we observe suggest increased perceived boundaries between the majority and non-Western population and more stereotypical norms. We interpret this as a cultural backlash. This pattern is robust to the same alternative bandwidths, specifications, and treatment definitions implemented in Section 5.1 for the FNI (Appendix Figures A.4, A.5 and A.6).

5.3 Socioeconomic integration

Cultural and socioeconomic integration could be intertwined. For instance, individuals with an immigrant background may learn about host-country norms and culture by interacting with native-born individuals at the workplace. Table 3 examines whether changes in labor market integration——measured by employment and earnings——could represent a mechanism behind the cultural effects we document, and whether the Ghetto List altered the behavior of initial residents along the dimensions that determine listing—crime and welfare dependency.[36]

Employment between 2011-2013, measured in full-time equivalents, is 38.9 percent among the untreated (non-Western) residents near the cutoff, which is close to the overall mean across all residents in monitored neighborhoods of 41.8 percent (Appendix Table A.1). The difference in employment is small and insignificant at the cutoff. We find that employment, measured in full-time equivalents, among the treated is 2.1 percentage points higher than among the control group and 0.8 percentage points higher at the extensive margin. These differences are small, statistically insignificant, and similar to the pre-treatment difference (the balance test for the extensive margin of employment is 0.7 percentage points). Similarly, we find small and insignificant effects of the Ghetto List on having a criminal conviction, being dependent on welfare (measured in full-time equivalents), and mean earnings (inverse hyperbolic sine) during the 2011-2013 period.[37] We can therefore conclude that the Ghetto List did not change labor market integration or criminal behavior of the targeted minority, non-Western residents in the Ghettos. These results hold for the subsample who named a child between 2011 and 2013, and in the threshold-specific RD used to study the impact of the list on the Foreign Name Index (Appendix Tables A.7 and A.5).

The findings in this section suggest that neither changes in criminal behavior nor shifts in labor market integration catalyzed the cultural responses we observe. There is no evidence that new peers in workplaces or criminal networks constitute plausible mechanisms for the cultural backlash. Residential peers are discussed in Section 6 when we analyze compositional changes in the neighborhood.

5.4 Extended time period and Western origin

Figure 9 extends the analysis to a longer outcome window and considers all residents, split into non-Western and Western origin groups with the former comprising the residents studied in Sections 5.1–5.3. Two main findings emerge from this analysis. First, the impact estimated in Section 5.1 persists through 2014–2017, with an effect size roughly two-thirds of that observed in 2011-2013, and remains highly statistically significant. Second, the estimated impact on Western-origin individuals is substantially smaller and statistically insignificant in all periods. About 92 percent of Western-origin parents in monitored neighborhoods have a child with another Western partner, and 88 percent of non-Western-origin parents

[36] The third criterion is the share of residents who are immigrants or descendants from non-Western countries (defined in Section 2.1). We return to this criterion when examining compositional changes at the neighborhood level in Section 6.3.

[37] In Appendix Table A.6, we show these conclusions hold when using the dichotomized versions of the variables (e.g., being employed at any point between 2011-2013).

have a child with another non-Western partner. Mixed couples constitute a small share of the population, resulting in less precise estimates for this group, with somewhat larger point estimates at the margin of significance in 2011-2013 (Appendix Figure A.7).

While no additional funding was formally earmarked for Ghetto-listed areas, we cannot fully rule out the possibility that more resources were ultimately channeled toward these neighborhoods. However, the design and approval of new projects are lengthy processes, and the average duration of such projects after approval is approximately four years (Section 2.2). Therefore, the immediate effects of the Ghetto List on cultural outcomes are clean estimates of the Ghetto label and the intense public discourse surrounding the Ghetto List in late 2010. Appendix Figure A.8 provides year-by-year estimates of the impact of the 2010-Ghetto List on the naming decisions and shows that the impact emerged immediately in 2011 and remained highly similar within the first three post-treatment years used in our main analysis. Pooling outcomes over the three-year window is necessary for the survey, which relies on a small number of respondents and improves overall precision for the FNI.

6 Mobility and neighborhood composition

It is possible that the neighborhoods listed as Ghettos in 2010 changed significantly in the post-period and over time. These compositional changes could contribute to the identity backlash we observe. In Section 6.1, we study inflows and outflows, and in Section 6.2, we analyze the characteristics of newcomers and leavers. This allows us to determine whether selective entry or exit occurred, and whether a change in neighborhood composition contributed to the cultural responses observed among the initial non-Western residents.

6.1 Mobility

In Table 4, we examine inflows of new adult residents (Columns 1–3) and outflows of initial adult residents (Columns 4–6) during our main outcome window, 2011–2013. Mobility is defined as a change in an individual's neighborhood of residence between the end of 2010 and the end of 2013 (Columns 1 and 4). We further distinguish between mobility within Denmark —– moves between the focal area and the rest of the country (Columns 2 and 5) – and international mobility (Columns 3 and 6).[38]

[38]Inflows and outflows are not perfectly balanced, reflecting changes in household size and possibly short gaps between tenants. These discrepancies are small and would be expected to approach zero if mobility were defined at the household level, as non-profit housing units rarely remain vacant for long.

We estimate the impact on mobility rates at the area level, normalizing all flows to the 2010 adult population. Panel A considers mobility among individuals of non-Western origin, Panel B among those of Western origin, and Panel C total mobility. All 18 estimates are small, and only one is statistically significant. Inflow rates are slightly more positive than outflow rates, although all estimates are statistically indistinguishable from zero, which runs counter to the pattern one would expect if residents were leaving Ghetto-listed areas. Overall, we find no evidence that the Ghetto List affected mobility or generated differential mobility between treated and untreated neighborhoods.

6.2 Selective entry and exit

While we find no evidence of differences in mobility rates between listed and non-listed areas, individuals moving into or out of listed areas may differ in terms of underlying characteristics. In Figure 10, we examine whether predetermined characteristics of individuals who moved out of a monitored neighborhood (Panel a) and individuals who moved into a monitored neighborhood by the end of 2013 (Panel b) differ in systematic ways near the boundary separating the ghetto from non-ghetto listed neighborhoods. The predetermined characteristics are gender, age, non-Western origin, refugee status, foreignness of one's own name, marital status, fertility, education, welfare dependency, employment, and earnings, measured in 2010. These graphs show that newcomers and leavers are not differentially selected on predetermined socioeconomic characteristics across treatment status. There are only minor differences: those who move out of a ghetto neighborhood are 7 percentage points less likely to be married, and those who move into a ghetto neighborhood are 4 percentage points less likely to be refugees.[39]

6.3 Neighborhood composition over time

Finally, Figure 11 summarizes neighborhood composition based on the incentivized neighborhood outcomes. To leave the Ghetto List, a neighborhood must change on one of the three criteria used to determine listing: the non-Western share, the crime rate, and the welfare dependency rate. Figure 11 shows that neighborhoods close to the cutoff have similar non-Western share, conviction rates, and welfare dependency rates in the period before the announcement of the Ghetto List (2008-2010). Importantly, we find that these characteristics did not change significantly in the first period after the Ghetto List (2011-2013) or in the longer run (2014-2017).

[39]We do not find this too worrying given that statistically significant differences are expected when testing 16 predetermined characteristics.

7 Conclusion

In this paper, we study whether Denmark's 2010 Ghetto List encouraged cultural assimilation or instead backfired by deepening social, economic, and cultural divides. Using a regression discontinuity design, we compare individuals residing in neighborhoods just above and just below the thresholds determining inclusion on the 2010 Ghetto List prior to its announcement. This approach allows us to isolate the causal impact of being publicly designated as a "Ghetto" neighborhood.

Our findings point to a clear and persistent cultural response among non-Western residents living in listed neighborhoods. Rather than converging toward majority norms, individuals respond by reinforcing minority identity and distancing themselves from Danish cultural practices. We document shifts in child naming patterns, childcare participation, gender norms, media consumption, and self-identification that are consistent with a cultural backlash. In contrast, we find little evidence of changes in labor market outcomes, criminal behavior, or residential mobility, and we observe no meaningful compositional change in the neighborhoods themselves. These patterns suggest that the policy primarily affected cultural identity and perceptions of belonging rather than economic integration or neighborhood structure.

The estimated effects persist for several years after the introduction of the list and remain broadly stable through 2017, before the introduction of additional, more far-reaching ghetto policies. Importantly, our estimates are local to neighborhoods at the margin of being listed and do not speak to potential effects in areas far from the thresholds. Moreover, while our design identifies the causal impact of the Ghetto List, the harsh public rhetoric surrounding the announcement could have broader effects. If individuals of non-Western origin living outside the listed neighborhoods also felt stigmatized or targeted by the discourse, the effects we document should be interpreted as a lower bound on the overall effect of the Danish Ghetto List and the rhetoric used to motivate its implementation.

Taken together, our results highlight a tension between the stated goals and the actual consequences of policies that rely on public labeling and assimilationist narratives. Rhetoric about promoting shared values and national identity may instead strengthen in-group identity among targeted minorities and sharpen social boundaries, without improving economic integration. These findings underscore the importance of accounting for the rhetorical and symbolic dimensions of integration policy.

Figures and tables

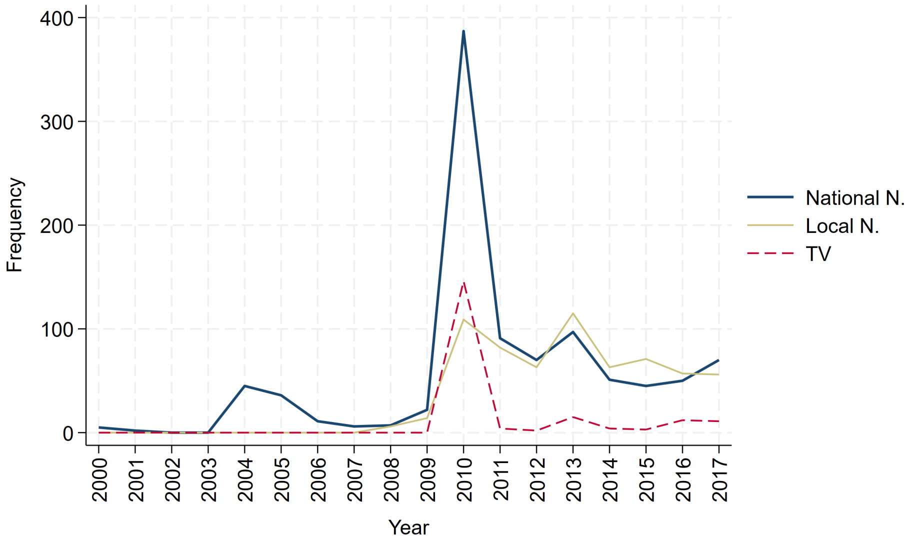


Figure 2: Trends in media coverage of Ghetto policies

Notes: The plotted time series counts all media hits containing any of the words Ghetto law, Ghetto list, Ghetto plan, Ghetto package or Ghetto proposal in national newspapers, local newspapers and news segments of TV. This excludes among other things many digital outlets.
Source: Own calculations based on Infomedia (details in Appendix Section A).

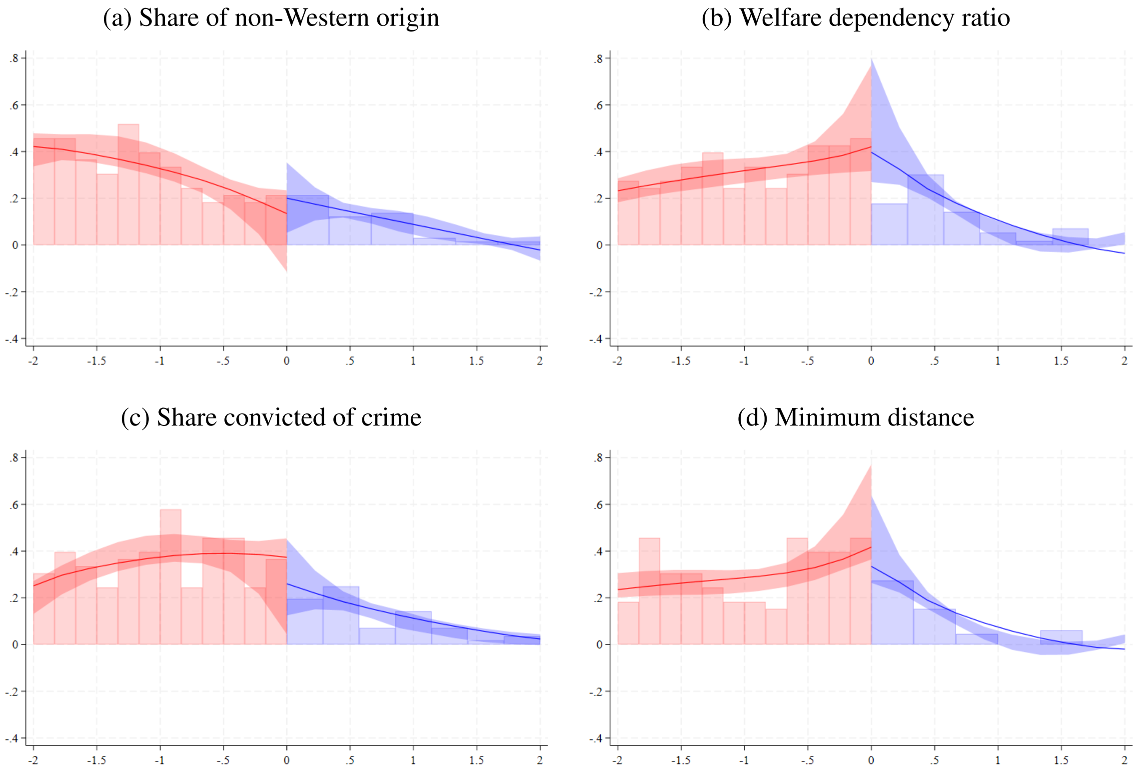


Figure 3: Density tests

Notes: Each graph shows estimated densities following Cattaneo et al. (2020b) of the normalized and standardized neighborhood criteria (Panels a to c) or the minimum distance (Panel d) described in Section 4. The t-statistic and p-value are 1.194 and 0.232 in Panel a, -0.001 and 0.999 in Panel b, 0.296 and 0.767 in Panel c, and -0.635 and 0.525 in Panel d, respectively.

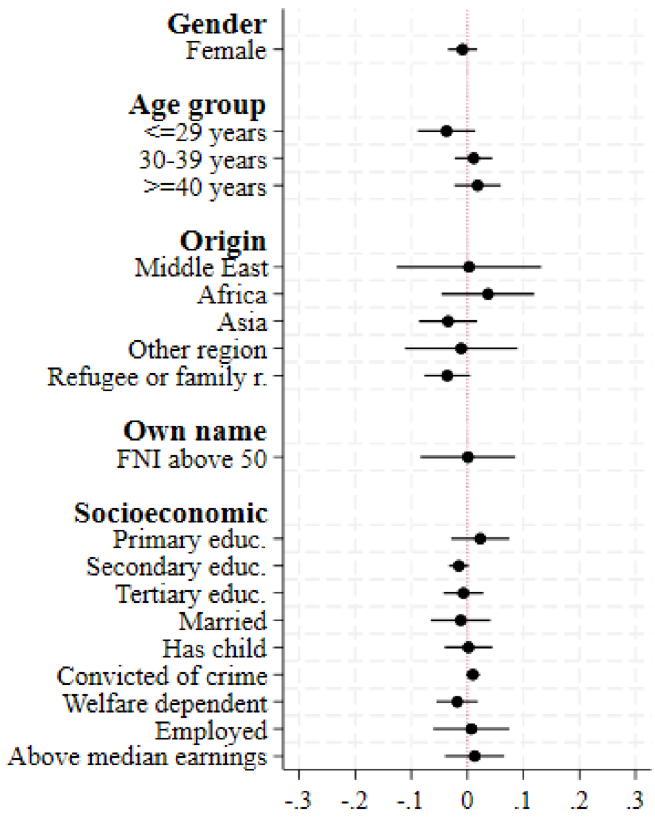


Figure 4: Covariate balance test

Notes: The graph shows RD estimates and 95 percent confidence intervals based on equation (1), using as the dependent variable the 2010 individual characteristic listed on the left.

Table 1: Main results on Foreign Name Index

		Within Individuals		Within Neighborhoods RW	
	2011-13 Effect	2008-10 Placebo	First Diff. Effect	2008-10 Placebo	First Diff. Effect
	(1)	(2)	(3)	(4)	(5)
Panel A: Minimum distance					
Ghetto Listed	6.318***	-0.114	5.354*	-0.295	6.614***
	(2.039)	(1.728)	(2.754)	(1.785)	(1.891)
Mean Untreated	70.832	75.170	-4.395	75.110	-4.278
Effective Observations	4843 (83)	6056	1748	83	83
N Neighborhoods Untreated	58	58	58	58	58
N Neighborhoods Treated	25	25	25	25	25
Panel B: Threshold-specific - Share of non-Western origin					
Ghetto Listed	7.429***	-1.340	5.650**	-1.903	9.332***
	(1.917)	(2.095)	(2.319)	(2.408)	(2.178)
Mean Untreated	67.674	75.030	-8.189	75.108	-7.435
Effective Observations	1926 (27)	2413	692	27	27
N Neighborhoods Untreated	15	15	15	15	15
N Neighborhoods Treated	12	12	12	12	12
Panel C: Threshold-specific - Share convicted of crime					
Ghetto Listed	5.900*	-2.507	7.026	-2.457	8.357**
	(3.477)	(1.767)	(5.108)	(1.769)	(4.095)
Mean Untreated	70.011	77.361	-1.101	77.510	-7.500
Effective Observations	1852 (28)	2312	645	28	28
N Neighborhoods Untreated	16	16	16	16	16
N Neighborhoods Treated	12	12	12	12	12

Notes: *p<0.10; **p<0.05; ***p<0.01. Each table cell is an RD estimate and the corresponding standard error in parentheses using equation (1) in Panel A and equation (2) in Panels B and C.

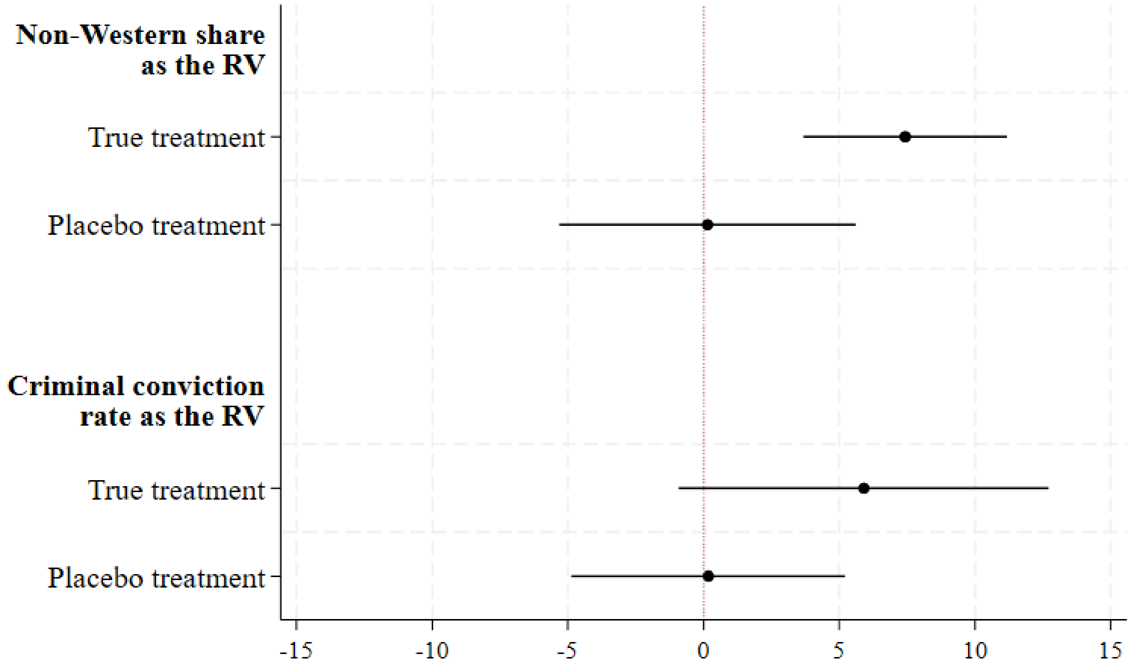


Figure 5: Threshold-specific placebo tests

Notes: The graph shows RD estimates and 95 percent confidence intervals based on equation (2).

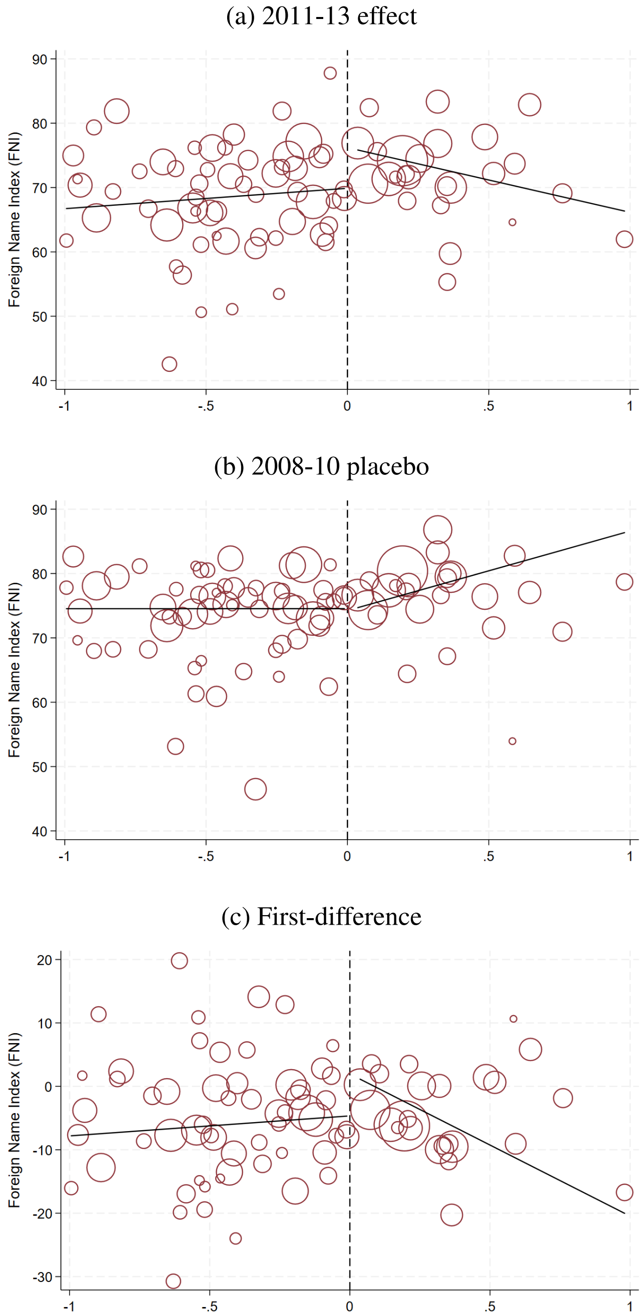


Figure 6: Mean Foreign Name Index in neighborhoods

Notes: The graph plots the mean value of FNI against the Minimum Distance to the boundary. Each circle represents a neighborhood, and the size of the circle is proportional to the number of individuals who named a child between 2011 and 2013.

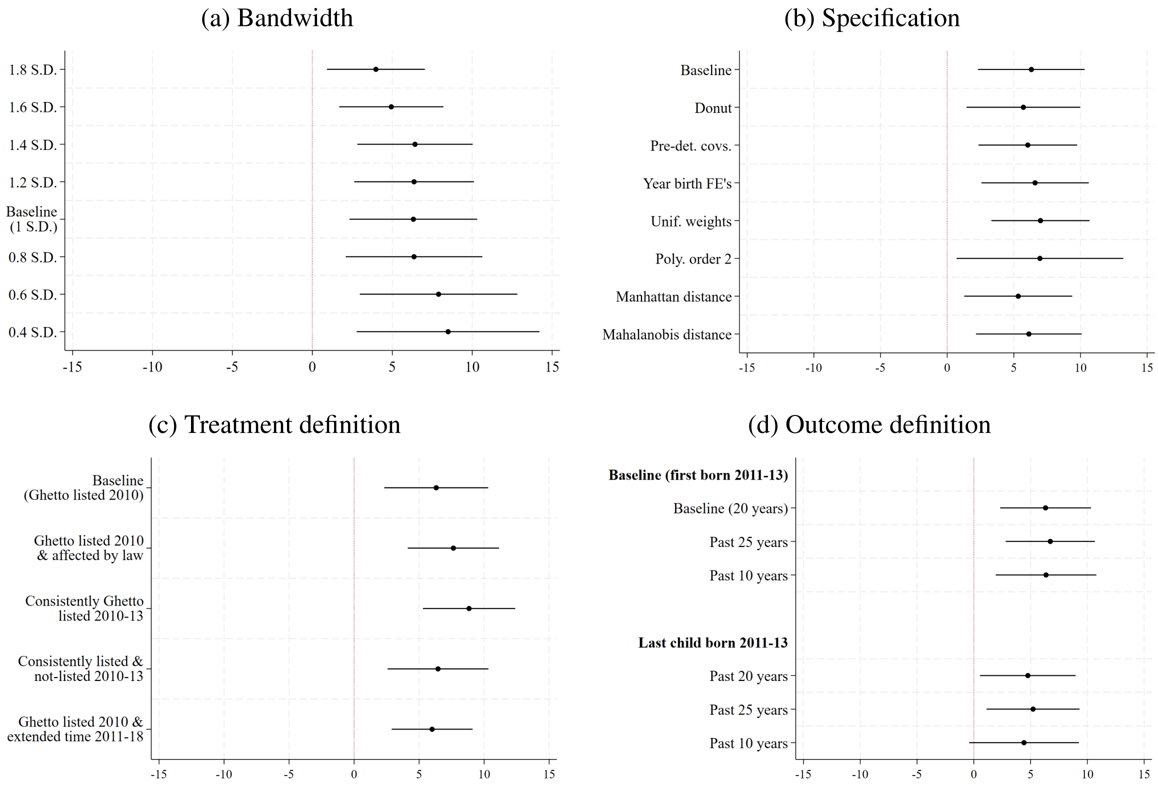


Figure 7: Robustness checks of effect on Foreign Name Index

Notes: Each panel reports the baseline estimate — and 95 percent confidence interval — from Table 1, Panel A, Column 1, along with robustness checks using alternative bandwidths (Panel a), alternative specifications (Panel b), alternative treatment definitions (Panel c) and alternative outcome definitions (Panel d).

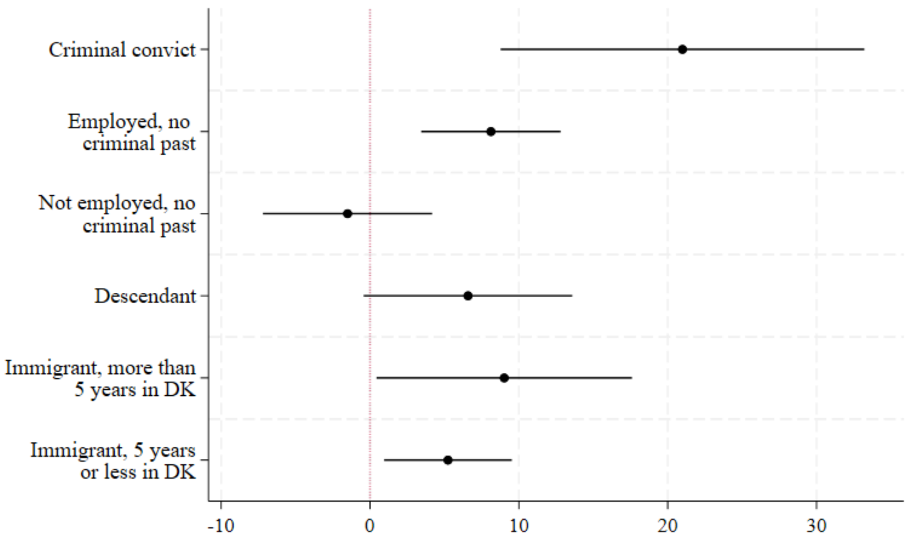


Figure 8: Heterogeneous effects on Foreign Name Index

Notes: The graph shows RD estimates and 95 percent confidence intervals based on equation (1) for different subpopulations listed on the left.

Table 2: Estimated effects on additional cultural outcomes

	Child aged 0-2 in childcare (1)	Homogamous marriage (2)	Identify as migrant or member of religious group (3)	Practices religion (4)	At least half of friends have immigrant background (5)	Immigrants get recognition in DK (6)	Follows Danish news regularly (7)	Gender equal access to divorce (8)	Gender equal access to get child custody (9)	Gender equal access to inherit (10)	Gender equal access to work (11)
Panel A: Baseline											
Ghetto Listed	-0.082***	-0.044	0.173*	0.038	0.014	-0.088	-0.108***	-0.221***	-0.078	-0.120*	-0.080***
	(0.026)	(0.051)	(0.103)	(0.114)	(0.068)	(0.066)	(0.038)	(0.053)	(0.055)	(0.069)	(0.031)
Mean Untreated	0.868	0.657	0.217	0.809	0.785	0.762	0.933	0.859	0.895	0.886	0.944
Effective Observations	4721	930	261	490	511	512	515	508	507	506	514
N Neighborhoods Untreated	58	58	58	58	58	58	58	58	58	58	58
N Neighborhoods Treated	25	25	25	25	25	25	25	25	25	25	25
Panel B: All respondents											
Ghetto Listed	-0.082***	-0.066	0.224**	0.018	0.069	-0.120**	-0.060*	-0.095**	-0.066*	-0.023	-0.050
	(0.026)	(0.054)	(0.093)	(0.092)	(0.054)	(0.048)	(0.032)	(0.040)	(0.040)	(0.055)	(0.031)
Mean Untreated	0.868	0.703	0.151	0.732	0.743	0.800	0.939	0.847	0.899	0.849	0.931
Effective Observations	4721	1375	421	746	777	779	781	774	774	773	783
N Neighborhoods Untreated	58	58	58	58	58	58	58	58	58	58	58
N Neighborhoods Treated	25	25	25	25	25	25	25	25	25	25	25

Notes: *p<0.10; **p<0.05; ***p<0.01. Each table cell is an RD estimate and the corresponding standard error in parentheses using equation (1). Panel A shows estimated effects for the baseline sample described in Section 3. Panel B shows estimated effects for all respondents of non-Western origin aged 18 or older.

Table 3: Estimated effects on socioeconomic outcomes

	Convicted of crime (1)	Welfare dependency (FTE) (2)	Employment (FTE) (3)	Mean gross earnings (IHS) (4)
Ghetto Listed	-0.006	-0.017	0.021	0.085
	(0.006)	(0.024)	(0.028)	(0.132)
Mean Untreated	0.046	0.494	0.389	1.992
Effective Observations	30501	30501	30501	30501
N Neighborhoods Untreated	58	58	58	58
N Neighborhoods Treated	25	25	25	25

Notes: *p<0.10; **p<0.05; ***p<0.01. Each table cell is an RD estimate and the corresponding standard error in parentheses using equation (1).

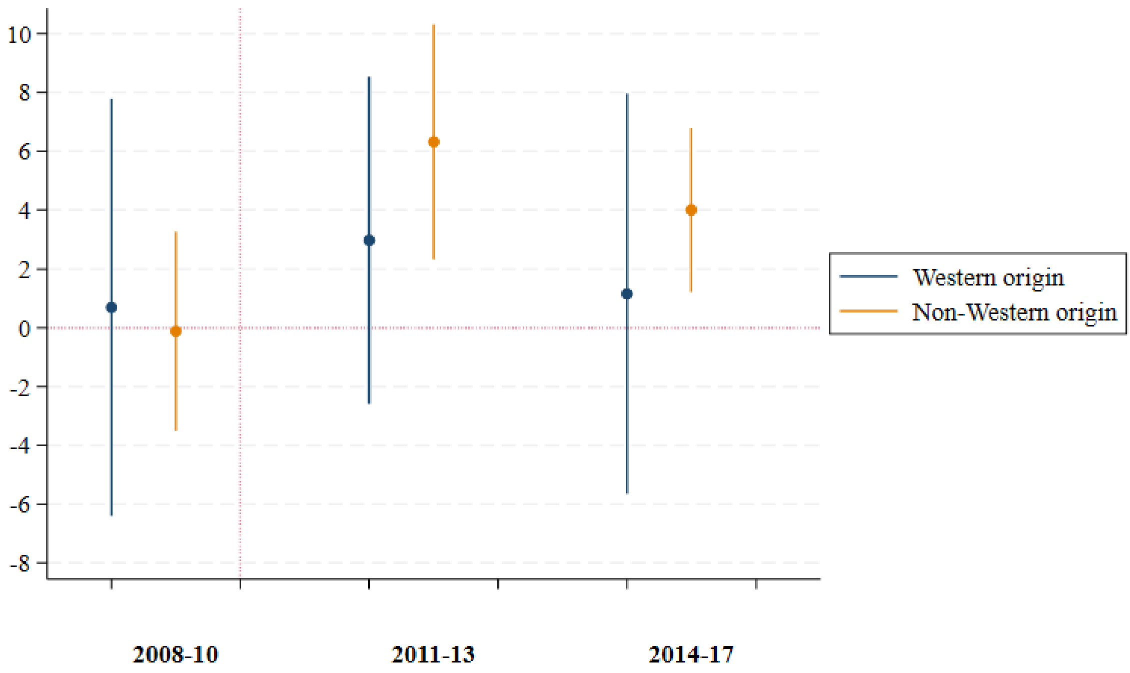


Figure 9: Foreign Name Index over time and by non-Western and Western origin

Notes: The graph shows the impact of the 2010 Ghetto List by Western and non-Western origin residents in the neighborhoods in 2010 in three-year intervals shown on the x-axis.

Table 4: Neighborhood exit and entry

	Share of newcomers			Share of leavers		
	Total	From DK	Outside DK	Total	Within DK	Outside DK
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Non-Western and Western origin						
Ghetto Listed	0.044	0.000	0.043	-0.001	-0.006	0.005
	(0.042)	(0.022)	(0.032)	(0.028)	(0.025)	(0.006)
Mean Untreated	0.338	0.291	0.047	0.376	0.348	0.028
Panel B: Only Western origin						
Ghetto Listed	0.009	-0.014	0.023	0.004	-0.002	0.007***
	(0.022)	(0.017)	(0.016)	(0.017)	(0.017)	(0.003)
Mean Untreated	0.195	0.175	0.020	0.226	0.214	0.012
Panel C: Only Non-Western origin						
Ghetto Listed	0.034	0.014	0.020	-0.005	-0.003	-0.002
	(0.039)	(0.018)	(0.024)	(0.026)	(0.022)	(0.006)
Mean Untreated	0.143	0.116	0.027	0.151	0.135	0.016
Effective Observations	83	83	83	83	83	83
N Neighborhoods Untreated	58	58	58	58	58	58
N Neighborhoods Treated	25	25	25	25	25	25

Notes: *p<0.10; **p<0.05; ***p<0.01. Each table cell is an RD estimate and the corresponding standard error in parentheses using equation (1).

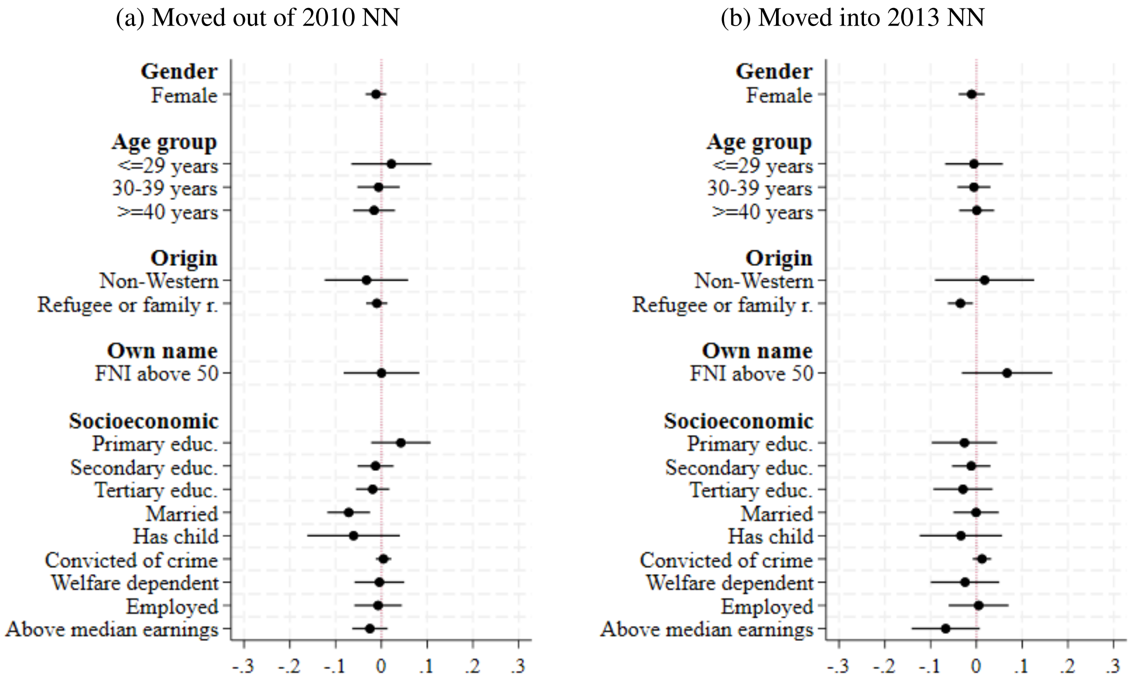


Figure 10: Test of selective exit and entry from neighborhoods

Notes: Each line in the graph represents a separate estimate based on equation (1) using as dependent variable the 2010 individual characteristic listed on the left.

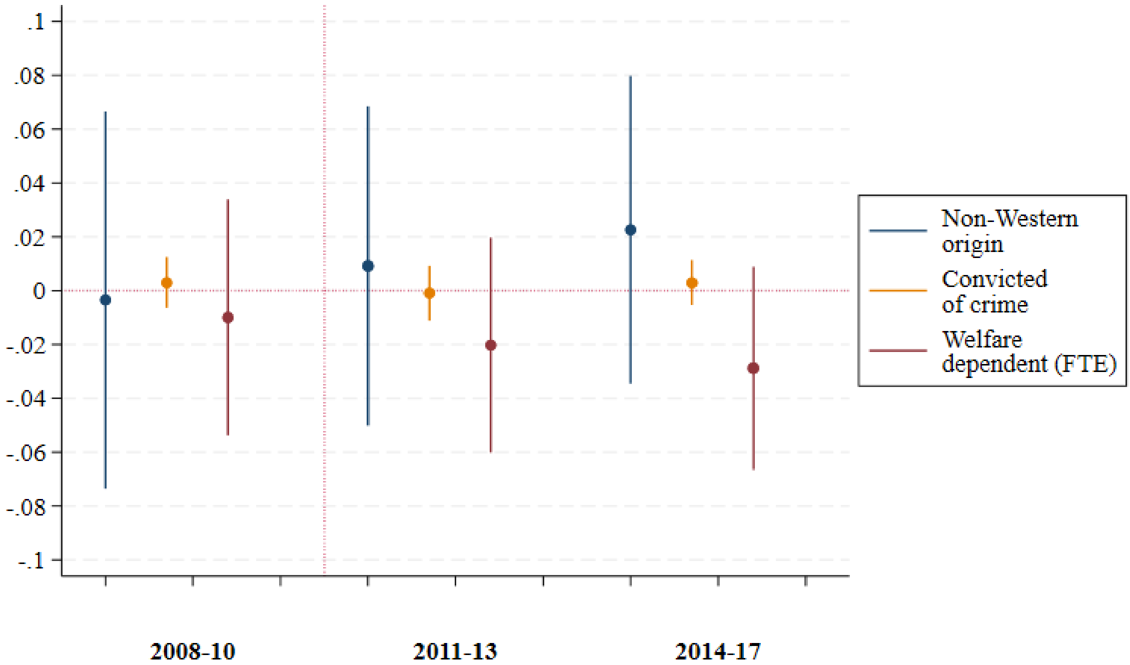


Figure 11: Neighborhoods over time and incentivized outcomes

Notes: Each panel shows the difference in a Ghetto criteria, averaged over the three-year interval on the x-axis, between individuals residing in treated and control neighborhoods by the end of 2007, 2010, and 2013.

Appendix

A Media coverage

We rely on Infomedia to analyze how Danish media covered Ghettos and Ghetto policies over time (2000–2024).[40] Using this database, we track the frequency of media hits mentioning specific words and word combinations in Danish news, spanning online media, print, TV, and radio.

More specifically, we track yearly media mentions of Ghetto-related words through three search terms:

1. Ghetto: mentions of "Ghetto" or any word containing "Ghetto", e.g., "Ghetto List" or "Ghetto law" (search term: Ghetto*)

2. Ghetto Policy: mentions of "Ghetto List", "Ghetto law", "Ghetto plan", "Ghetto package" or "Ghetto proposal" (search term: Ghettoliste* OR Ghettolov* OR Ghettoplan* OR Ghettopakke* OR Ghettoudspil*).

3. Ghetto List: mentions of the word "Ghetto List" (search term: Ghettoliste*).

Appendix Figure A.1 shows total media hits for each of the search terms 1 to 3 in the list above.

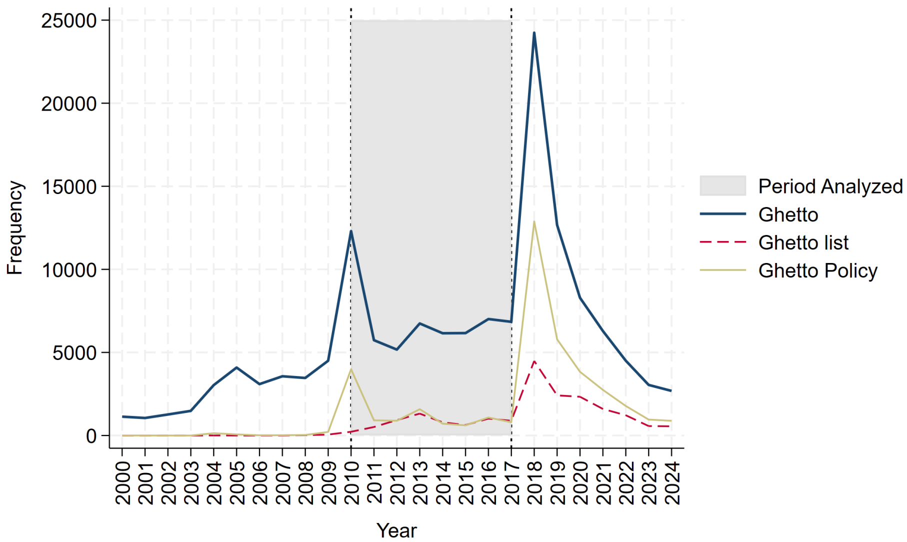


Figure A.1: Media coverage of words related to Ghetto

Notes: The time-series labeled "Ghetto" counts all media hits including variants of the word "Ghetto". The time-series labeled "Ghetto policy" counts all media hits containing any of the words Ghetto law, Ghetto List, Ghetto plan, Ghetto package, or Ghetto proposal, while "Ghetto List" counts mentions of the specific term Ghetto List. *Source:* Infomedia.

[40] Infomedia is by far the largest media intelligence provider in Denmark, combining all media outlets into one platform. Customers can access media coverage statistics and analysis, as well as search the database, through their subscriptions. They can also access news according to the access level agreed upon in their subscription. In 2025 the company changed name to Retriever.

B Outcome variable definitions and descriptive statistics

Table A.1: Summary statistics of register data outcomes, 2011-2013

	Not listed	Ghetto listed	Total
Married	0.0369	0.0321	0.0353
	(33787)	(17383)	(51170)
Homogamy	0.650	0.673	0.657
	(1131)	(499)	(1630)
Had child	0.155	0.165	0.158
	(33787)	(17383)	(51170)
FNI: first child born in 2011-13	68.28	74.09	70.34
	(5166)	(2828)	(7994)
Child aged 0-2 in childcare	0.849	0.812	0.836
	(5120)	(2768)	(7888)
Convicted of crime	0.0498	0.0581	0.0526
	(33787)	(17383)	(51170)
In welfare dependency at some point	0.646	0.715	0.670
	(33787)	(17383)	(51170)
Welfare dependency (FTE)	0.441	0.529	0.471
	(33787)	(17383)	(51170)
Employed at some point	0.635	0.545	0.605
	(33787)	(17383)	(51170)
Employment (FTE)	0.446	0.364	0.418
	(33787)	(17383)	(51170)
Log mean gross earnings	2.841	2.650	2.783
	(21464)	(9480)	(30944)

Notes: The table entries denote means of the variables and count in parentheses.

Table A.2: Survey questions and response options, 2012-2014

Question text	Answer options	Indicator= 1 if
To what extent do you think that others in Denmark recognize the contributions that people with an immigrant background make to society?	(1) To a great extent; (2) To some extent; (3) To a lesser extent; (4) Not at all; (5) Don't know; (6) Prefer not to answer	(1) or (2)
Which of the following do you identify with the most;	(1) Dane; (2) Dane with an immigrant background; (3) Immigrant; (4) Member of a specific religious group; (5) Other; (6) Don't know; (7) Prefer not to answer	(3) or (4)
How many of your friends have an immigrant background?	(1) None; (2) Almost none; (3) Less than half; (4) About half; (5) More than half; (6) Almost all; (7) All; (8) Don't know; (9) Prefer not to answer	(4), (5), (6) or (7)
How often do you follow news about Danish society, e.g. through radio, TV, the internet, or newspapers?	(1) Every day; (2) 3–4 days a week; (3) Less than once a week; (4) Never; (5) Don't know; (6) Prefer not to answer	(1), (2) or (3)
Do you practice your religion?	(1) Yes; (2) No; (3) Don't know; (4) Prefer not to answer	(1)
How much do you agree or disagree that men and women should have the same opportunity to get divorced?	(1) Strongly agree; (2) Somewhat agree; (3) Neither agree nor disagree; (4) Somewhat disagree; (5) Strongly disagree; (6) Don't know; (7) Prefer not to answer	(1) or (2)
How much do you agree or disagree that men and women should have equal right to obtain custody of shared children after a divorce?	(1) Strongly agree; (2) Somewhat agree; (3) Neither agree nor disagree; (4) Somewhat disagree; (5) Strongly disagree; (6) Don't know; (7) Prefer not to answer	(1) or (2)
How much do you agree or disagree that men and women should have equal right to inherit after a death in the immediate family?	(1) Strongly agree; (2) Somewhat agree; (3) Neither agree nor disagree; (4) Somewhat disagree; (5) Strongly disagree; (6) Don't know; (7) Prefer not to answer	(1) or (2)

How much do you agree or disagree that men and women should have equal opportunity to work?	(1) Strongly agree; (2) Somewhat agree; (3) Neither agree nor disagree; (4) Somewhat disagree; (5) Strongly disagree; (6) Don't know; (7) Prefer not to answer	(1) or (2)

Table A.3: Summary statistics of survey data outcomes, 2012-2014

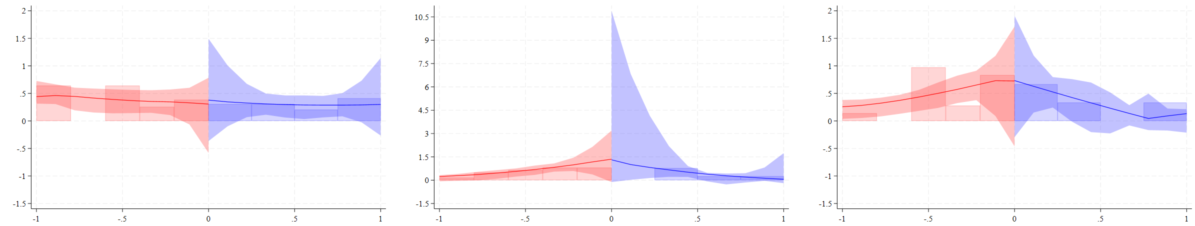
	Not listed	Ghetto listed	Total
Selected for survey	0.0161	0.0143	0.0155
	(33787)	(17383)	(51170)
Replied to all survey questions	0.915	0.936	0.922
	(544)	(249)	(793)
Identify as migrant or member of religious group	0.288	0.339	0.304
	(281)	(127)	(408)
Practices religion	0.674	0.732	0.692
	(512)	(239)	(751)
At least half of friends have immig. background	0.711	0.837	0.750
	(539)	(246)	(785)
Immigrants get recognition in DK	0.765	0.768	0.766
	(540)	(246)	(786)
Follows Danish news regularly	0.950	0.964	0.954
	(541)	(248)	(789)
Gender equal access to divorce	0.877	0.837	0.864
	(535)	(246)	(781)
Gender equal access to get child custody	0.888	0.833	0.871
	(535)	(245)	(780)
Gender equal access to inherit	0.875	0.833	0.861
	(534)	(245)	(779)
Gender equal access to work	0.963	0.964	0.963
	(543)	(248)	(791)

Notes: The table entries denote means of the variables and count in parentheses.

C Additional results

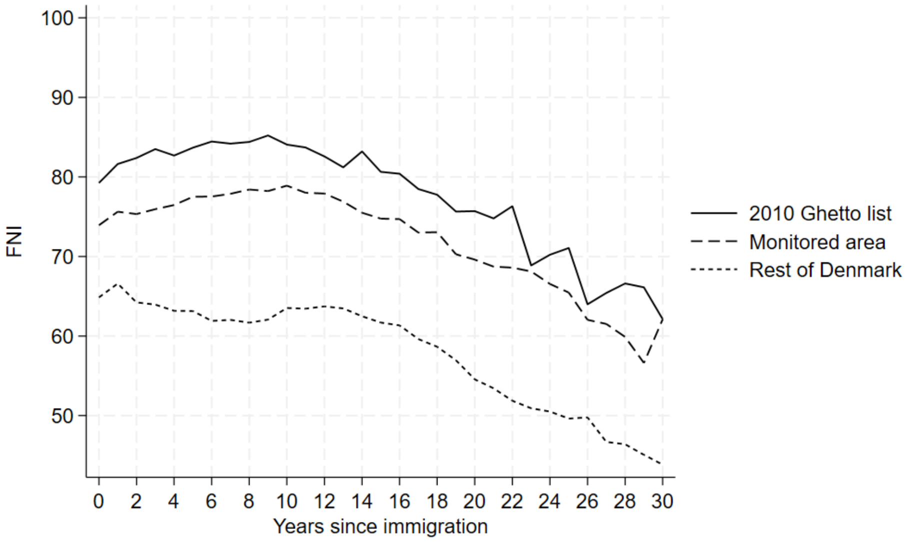
Figure A.2: Data mass analysis

(a) Non-Western share (b) Welfare dependency ratio (c) Criminal conviction rate



Notes: The graphs show the estimated densities as proposed by Cattaneo et al. (2020b) for each Ghetto list criterion after conditioning on exceeding the threshold in exactly one of the other two criteria, such that observations above the plotted thresholds are treated.

(a) Naming decisions of non-Western immigrants by years since immigration



(b) Naming decisions of non-Western immigrants and descendants by age at first child

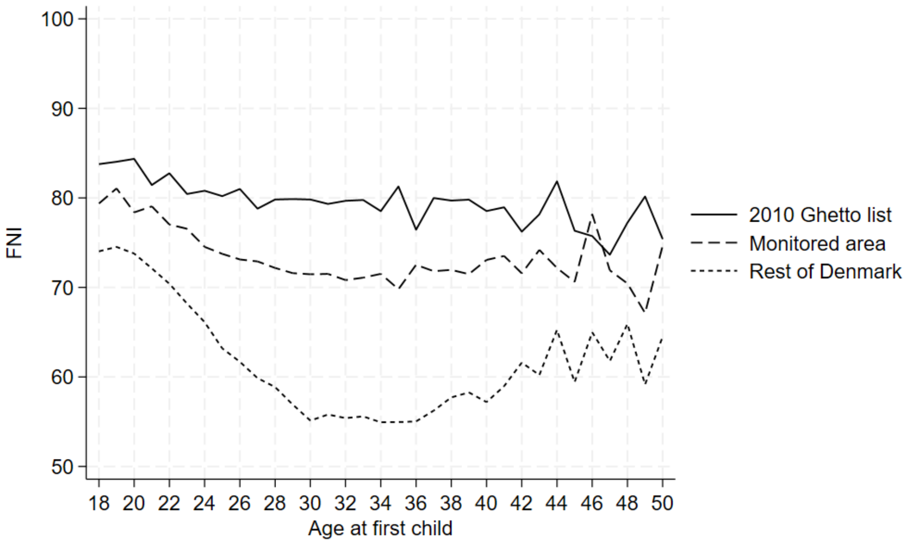


Figure A.3: Foreign Name Index over time

Notes: Panels a and b show the mean Foreign Name Index (FNI) by parents' years since immigration (for immigrants only), and by parents' age at first child split into three time-series based on the address of the parent. Irrespectively of the year of observation, we map each address into: areas on the 2010 Ghetto list, other monitored areas, and rest of Denmark.

Table A.4: Estimated effects on selection outcomes

	Married	Had at least 1 child	Selected for survey	Replied to all questions
	(1)	(2)	(3)	(4)
Ghetto Listed	0.000	-0.004	-0.001	-0.023
	(0.003)	(0.015)	(0.003)	(0.045)
Mean Untreated	0.032	0.155	0.017	0.901
Effective Observations	30501	30501	30501	516
N Neighborhoods Untreated	58	58	58	58
N Neighborhoods Treated	25	25	25	25

Notes: *p<0.10; **p<0.05; ***p<0.01. Each table cell is an RD estimate and the corresponding standard error in parentheses using equation (1).

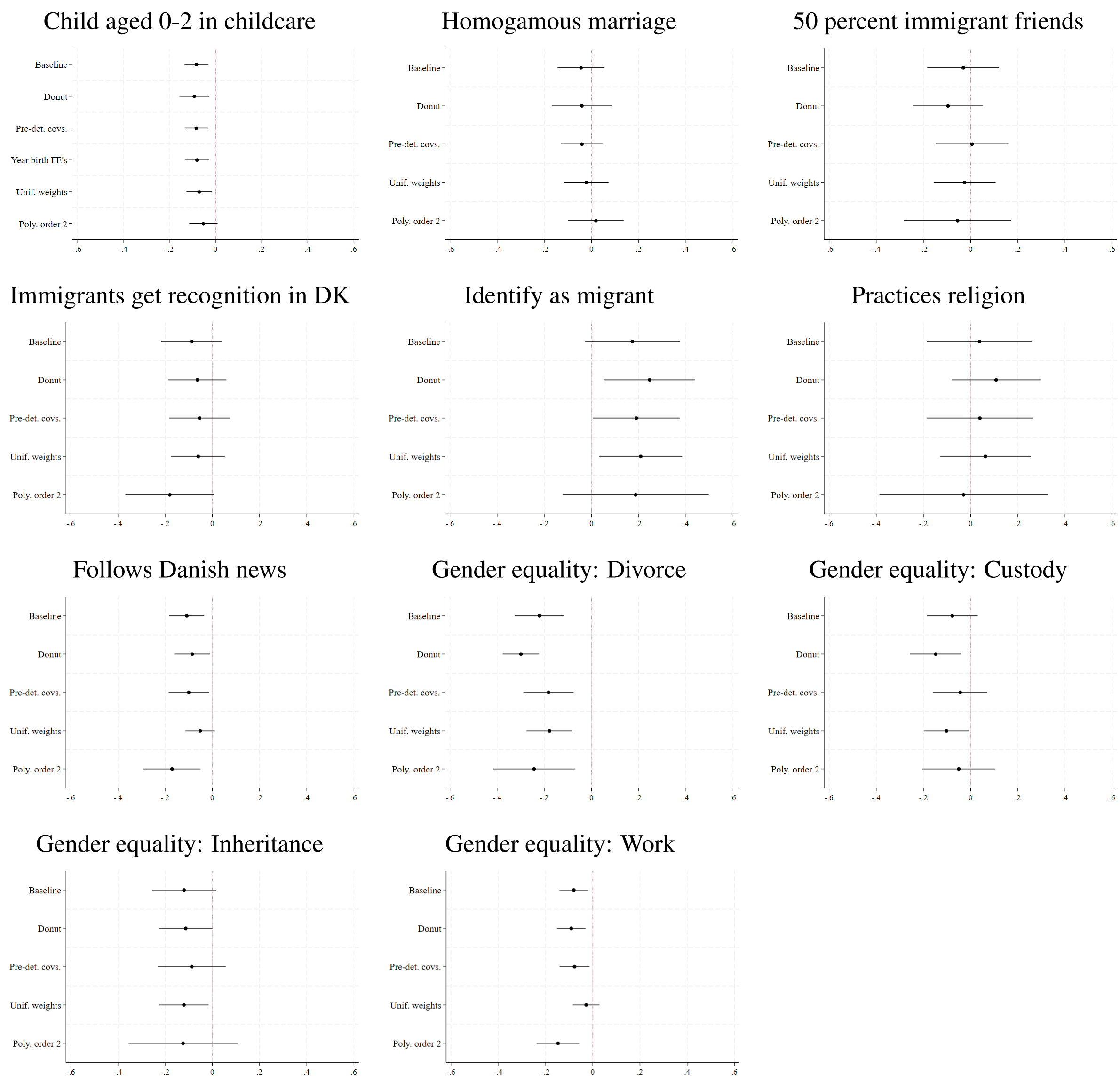


Figure A.4: Robustness checks of functional form for effects on additional cultural outcomes

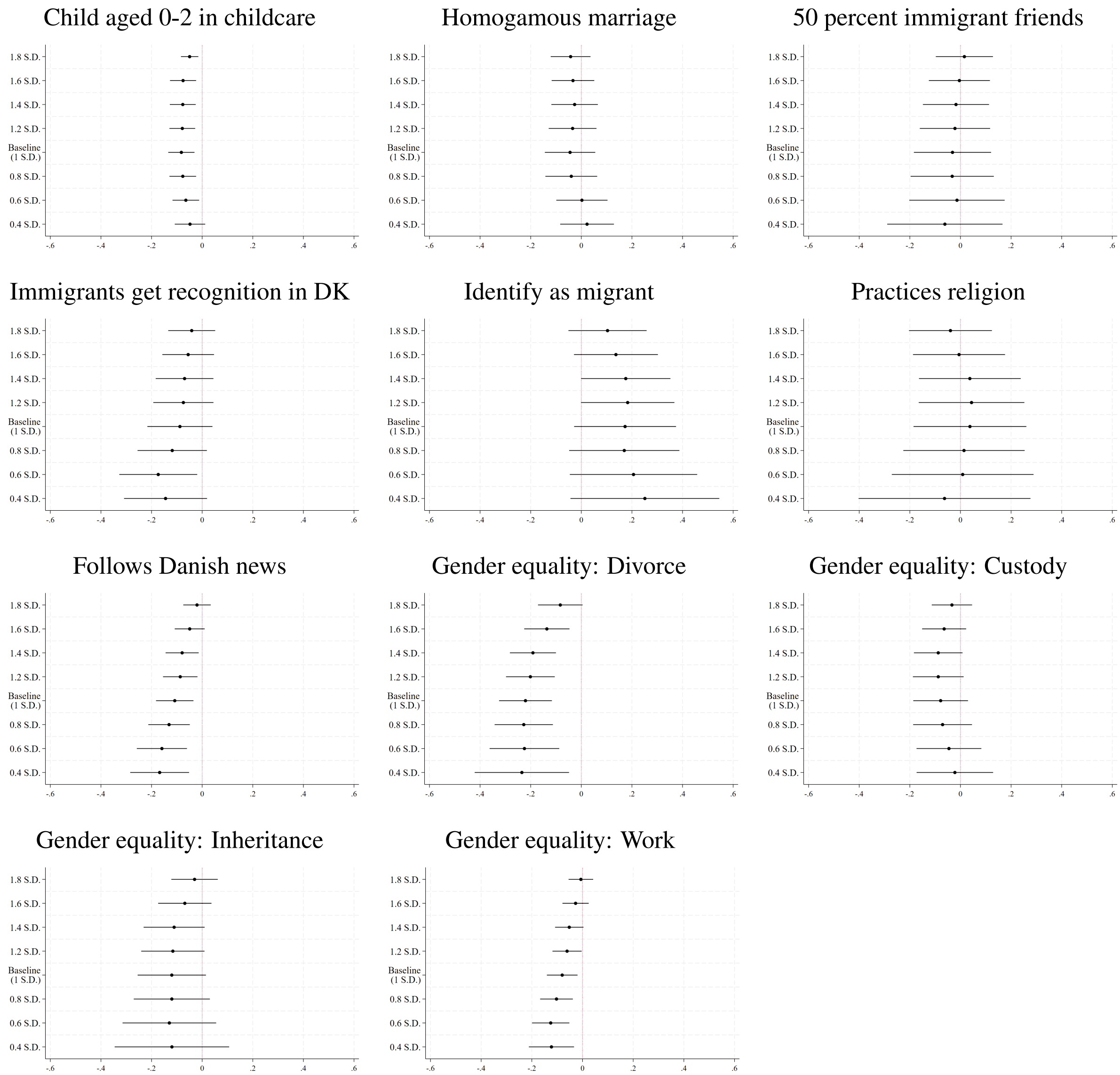


Figure A.5: Robustness checks of bandwidth selection for effects on additional cultural outcomes

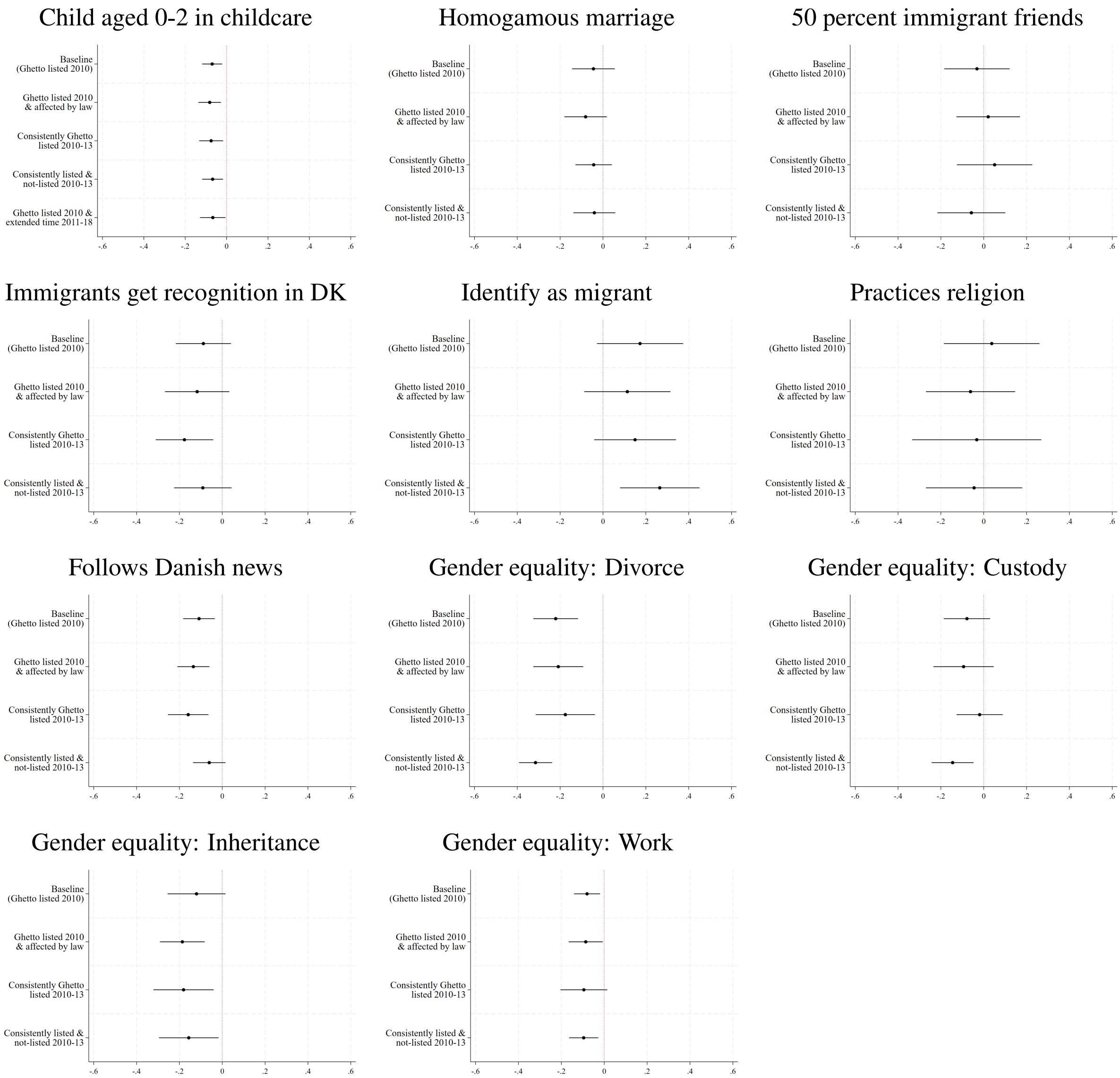


Figure A.6: Robustness checks of sample restrictions for effects on additional cultural outcomes s

Table A.5: Estimated effects on socioeconomic outcomes: Threshold specific RD

	Convicted of crime (1)	Welfare dependency (FTE) (2)	Employment (FTE) (3)	Mean gross earnings (IHS) (4)
Panel A: Threshold-specific- Share of non-Western origin				
Ghetto Listed	-0.012	0.003	-0.001	-0.079
	(0.009)	(0.034)	(0.032)	(0.139)
Mean Untreated	0.059	0.475	0.430	2.210
Effective Observations	11985	11985	11985	11985
N Neighborhoods Untreated	15	15	15	15
N Neighborhoods Treated	12	12	12	12
Panel B: Threshold-specific - Share convicted of crime				
Ghetto Listed	0.007	-0.034	0.037	0.207
	(0.009)	(0.034)	(0.030)	(0.159)
Mean Untreated	0.043	0.495	0.385	1.964
Effective Observations	11565	11565	11565	11565
N Neighborhoods Untreated	16	16	16	16
N Neighborhoods Treated	12	12	12	12

Notes: *p<0.10; **p<0.05; ***p<0.01. Each column is an RD estimate and the corresponding standard error in parentheses using equation (2).

Table A.6: Estimated effects on dichotomous socioeconomic outcomes: Minimum distance

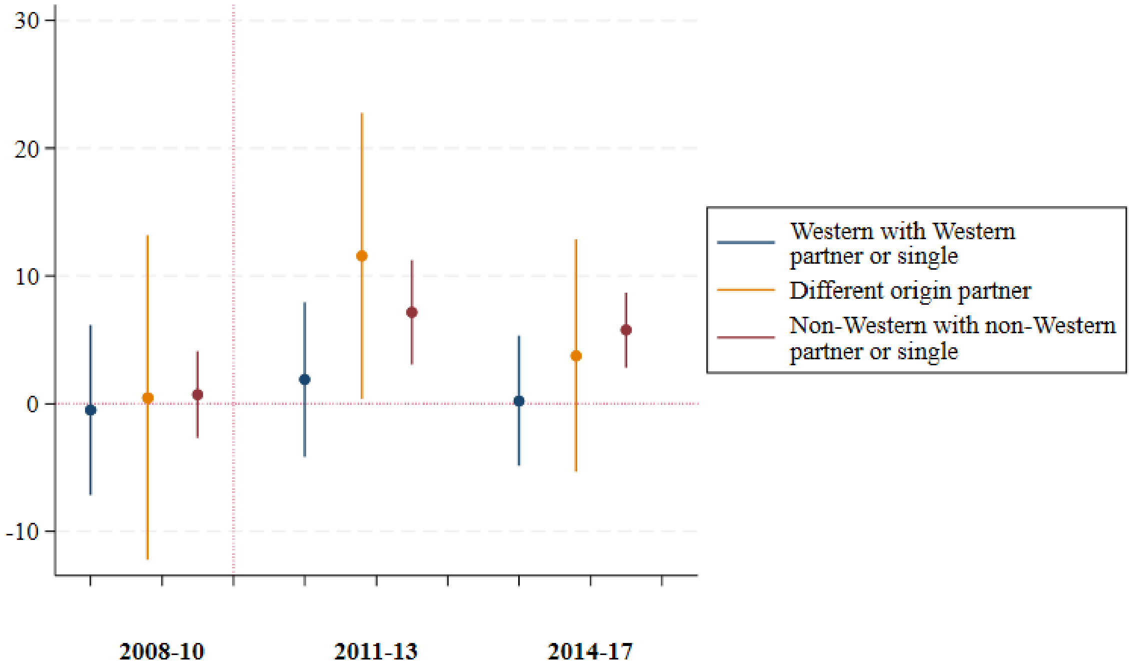
	Criminal conviction (1)	Welfare dependency (2)	Employment (3)
Ghetto Listed	-0.006	-0.022	0.008
	(0.006)	(0.020)	(0.029)
Mean Untreated	0.046	0.695	0.578
Effective Observations	30501	30501	30501
N Neighborhoods Untreated	58	58	58
N Neighborhoods Treated	25	25	25

Notes: *p<0.10; **p<0.05; ***p<0.01. Each column is an RD estimate and the corresponding standard error in parentheses using equation (1).

Table A.7: Estimated effects on socioeconomic outcomes for individuals having a child in 2011-13

	Convicted of crime (1)	Welfare dependency (FTE) (2)	Employment (FTE) (3)	Mean gross earnings (IHS) (4)
Panel A: Minimum distance				
Ghetto Listed	0.003	0.039	-0.002	-0.087
	(0.010)	(0.025)	(0.037)	(0.161)
Mean Untreated	0.046	0.360	0.456	2.433
Effective Observations	4843	4843	4843	4843
N Neighborhoods Untreated	58	58	58	58
N Neighborhoods Treated	25	25	25	25
Panel B: Threshold-specific - Share of non-Western origin				
Ghetto Listed	0.018	0.080*	0.002	-0.141
	(0.014)	(0.047)	(0.049)	(0.200)
Mean Untreated	0.034	0.356	0.493	2.636
Effective Observations	1661	1661	1661	1661
N Neighborhoods Untreated	15	15	15	15
N Neighborhoods Treated	12	12	12	12
Panel C: Threshold-specific - Share convicted of crime				
Ghetto Listed	-0.025	0.060	-0.028	-0.142
	(0.023)	(0.055)	(0.048)	(0.258)
Mean Untreated	0.064	0.335	0.476	2.448
Effective Observations	1568	1568	1568	1568
N Neighborhoods Untreated	16	16	16	16
N Neighborhoods Treated	12	12	12	12

Notes: *p<0.10; **p<0.05; ***p<0.01. Each table cell is an RD estimate and the corresponding standard error in parentheses using equation (1) in Panel A and equation (2) in Panels B and C.



Notes: The graph shows the estimated impact of the 2010 Ghetto List and 95 percent confidence interval by Western, non-Western, and mixed couples in the neighborhoods in 2010 in three-year intervals shown on the x-axis.

Figure A.7: Estimated effect on Foreign Name Index over time and by origin

Notes: The graph shows the estimated impact of the 2010 Ghetto List and 95 percent confidence interval on the Foreign Name Index of the first child born between 2011-2013 disaggregated by year.

Figure A.8: Estimated effects on Foreign Name Index by year

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