Stigma Without Sorting: Denmark's Parallel Society Designation and Neighborhood Composition

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Abstract

Does government labeling of neighborhoods cause demographic displacement? Denmark's 2018 Ghetto Package designated 29 public housing estates across 15 municipalities as "parallel societies," triggering mandatory demolition timelines, doubled criminal penalties for residents, and compulsory integration contracts. Using quarterly population register data from Statistics Denmark (2008–2026), I estimate a difference-in-differences comparing designated to non-designated municipalities before and after the reform. The non-Western immigrant share shows a precise null: -0.07 percentage points (SE=0.37), with clean pre-trends (joint F-test $p = 0.130$) and a randomization inference p-value of 0.87. I can rule out effects larger than 0.73 percentage points—roughly 9% of the treated-municipality mean. One of Europe's most aggressive neighborhood-labeling policies produced no detectable compositional change over seven years.

JEL Codes: R23, J15, H75
Keywords: neighborhood effects, place-based policy, residential sorting, immigration, Denmark, parallel societies

1. Introduction

In December 2018, Denmark's government published a list of 29 public housing estates officially designated as "ghettos"—neighborhoods it declared to be "parallel societies" incompatible with Danish values. The designation triggered an extraordinary package of interventions: estates on the list for five consecutive years would face mandatory demolition to reduce public housing below 40%; criminal penalties for offenses committed within designated boundaries would double; children would face compulsory daycare enrollment; and residents would sign integration contracts or risk eviction. The policy was, by the government's own framing, a deliberate act of neighborhood stigmatization designed to break up concentrations of non-Western immigrants.

The expectation—shared by proponents, critics, and the academic literature on territorial stigma—was that designation would drive out residents. Wacquant (2007) argues that official labeling of neighborhoods creates a "blemish of place" that compounds the disadvantage of residents, while Slater (2021) documents how stigma discourages investment and accelerates decline. If designation works as intended, non-Western immigrant-origin residents should leave designated municipalities, either pushed by the stigma itself or pulled by the threat of future demolition.

This paper tests that prediction with seven years of post-treatment data and finds it false. Using quarterly population registers from Statistics Denmark covering all 98 municipalities from 2008 to 2026, I estimate a difference-in-differences comparing the 15 municipalities containing designated estates to 90 control municipalities. The non-Western population share—the variable the policy explicitly targets—shows a precisely estimated null effect of -0.07 percentage points (SE=0.37 pp). Pre-treatment trends are clean: a joint F-test on eight pre-treatment leads yields $p = 0.130$, and a Callaway–Sant'Anna estimator robust to heterogeneous treatment effects confirms the null (ATT=0.03 pp, $p > 0.50$). Randomization inference with 500 permutations gives $p = 0.87$. I can rule out effects exceeding 0.73 percentage points, roughly 9% of the treated-municipality mean non-Western share of 7.9%.

An important caveat: the analysis is at the municipality level, while the policy targets specific estates within municipalities. Because designated estates typically house 3–5% of a municipality's total population, even complete displacement from an estate would reduce the municipality-level non-Western share by only 0.15–0.40 percentage points—within my confidence interval. The null therefore rules out municipality-wide compositional transformation but not estate-level displacement. Three features nonetheless make this finding informative. First, the treatment is maximally salient: the government literally published a named list, widely covered in national media, with criteria explicitly referencing the non-Western

immigrant share (Seemann, 2021). If place-based stigma causes sorting, this is the setting where it should be most visible. Second, the data are comprehensive—population registers with no sampling error, covering every resident in every municipality quarterly. Third, the post-treatment window is long (seven years), allowing ample time for residential adjustment.

The paper contributes to three literatures. Within the study of neighborhood effects (Kling et al., 2007; Chetty et al., 2016), it provides the first causal estimate of designation-driven sorting in a European register-data context, complementing the experimental evidence from Moving to Opportunity. For the literature on territorial stigma and neighborhood branding (Wacquant, 2007; Jensen and Christensen, 2012; Galster, 2017), it demonstrates that even maximal stigma—with mandatory demolition timelines as enforcement—does not generate detectable compositional change at the municipality level, establishing a boundary condition for theories of place-based sorting. For the growing literature on Danish immigration policy (Damm and Dustmann, 2014; Schultz, 2019; Foged et al., 2022), it shows that the 2018 Ghetto Package did not produce the municipality-wide demographic transformation its proponents envisioned, motivating future estate-level analysis using DST Research Service microdata to determine whether displacement occurs within municipalities rather than across them.

2. Institutional Background

The Danish designation system. Denmark has published an annual list of "vulnerable residential areas" (udsatte boligområder) since 2010. The system was overhauled in 2018 with the Ghetto Package (Act No. 1000 of June 30, 2018), which introduced three tiers of designation based on objective criteria. The strictest tier—originally called "ghetto," renamed "parallel society" (parallelsamfund) in 2021—requires that an estate have at least 1,000 residents in social housing, a non-Western immigrant or descendant share exceeding 50%, and meet at least two of four socioeconomic thresholds: unemployment above 40%, crime rates triple the national average, less than 60% with upper secondary education, or average income below 55% of the regional median.

Consequences of designation. The 2018 reform attached mandatory consequences to designation. Estates designated as parallel societies for five consecutive years—classified as "redevelopment areas" (omdannelsesområder)—must reduce their share of public family housing below 40% through demolition, sale, or conversion. Within designated areas, criminal penalties for certain offenses double; landlords can terminate leases on public-order grounds; children must attend 25 hours of Danish-language daycare from age one; and residents sign integration contracts committing to employment and language requirements. These provisions

represented a dramatic escalation from the pre-2018 regime, which attached few binding consequences to designation (Seemann, 2021).

The 2018 list. The December 2018 list designated 29 estates as ghettos (later, parallel societies) across 15 municipalities. Copenhagen contained seven designated estates (including Mjølnerparken, Tingbjerg, and Hørgården), Aarhus three (Gellerupparken, Bispehaven, Skovgårdsparken), Odense three (Vollsmose, Solbakken, Korsløkkeparken Øst), and Høje-Taastrup three (Tåstrupgård, Charlotteager, Gadehavegård). The remaining municipalities each contained one or two designated estates. Treatment intensity—measured as the number of designated estates per municipality—varies from one to seven, providing additional identifying variation.

Subsequent evolution. The number of designated parallel societies declined from 29 in 2018 to 10 in 2022, rose to 12 in 2023, and fell to 8 in 2024 and 5 in 2025. Exits reflect a combination of physical demolition, conversion of social housing to private tenure, and demographic shifts. Several estates entered the five-year redevelopment trigger, most prominently Gellerupparken and Vollsmose, where large-scale demolition proceeded from 2020 onward (Abdelaziz, 2022).

3. Data

I construct a balanced panel of 105 geographic units (98 municipalities plus 7 region-level aggregates) observed annually from 2008 to 2026, using two tables from Statistics Denmark's StatBank API.

Population composition. Table FOLK1E provides the population at the first day of each quarter, disaggregated by municipality, sex, age, and ancestry. Ancestry categories distinguish persons of Danish origin, immigrants from Western countries, immigrants from non-Western countries, descendants of Western immigrants, and descendants of non-Western immigrants. I use Q1 snapshots (January 1) for annual observations and compute the non-Western share as the sum of non-Western immigrants and descendants divided by total population.

Employment. Table RAS200 provides the employment rate by municipality, ancestry, age group, and year. I use the employment rate for non-Western immigrants aged 16–64 as a secondary outcome capturing labor market integration.

Designation data. The treatment variable is constructed from the December 2018 designation list, compiled from official government publications, the Danish National Building Fund (LBF) records, and journalistic coverage in the Copenhagen Post and The Local Denmark.

I code a binary municipality-level indicator equal to one if the municipality contains at least one estate designated as a ghetto/parallel society in 2018, and a continuous treatment intensity measure equal to the count of designated estates.

Table 1: Summary Statistics: Pre-Treatment Municipality Characteristics (2008–2018)

	Designated		Non-Designated	
	Mean	SD	Mean	SD
Total Population	136776.048	(131528.080)	164446.149	(636622.182)
Non-Western Pop.	13328.018	(20228.737)	11264.739	(48024.971)
Non-Western Share	0.079	(0.042)	0.057	(0.049)
NW Imm. Employment Rate	48.841	(4.699)	52.202	(8.784)
Total Employment Rate	72.041	(3.041)	73.675	(3.861)
Danish-Origin Pop.	116863.448	(101313.380)	147129.240	(564997.038)
Municipalities	15		90	
Municipality-years	165		990	

Notes: Pre-treatment period: 2008–2018. "Designated" municipalities contain at least one public housing estate on the 2018 ghetto/parallel society list. Non-Western share is the fraction of residents who are immigrants or descendants from non-Western countries. Employment rate is from DST StatBank RAS200 for ages 16–64.

Table 1 reports pre-treatment summary statistics. Designated municipalities have a higher non-Western share (7.9% vs. 5.7%) but are somewhat smaller on average (137,000 vs. 164,000 residents). These level differences are absorbed by municipality fixed effects in the estimation.

4. Empirical Strategy

I estimate a two-way fixed effects difference-in-differences model:

$$Y_{mt} = \alpha_m + \gamma_t + \beta \cdot \text{Designated}_m \times \text{Post}_t + \varepsilon_{mt} \tag{1}$$

where Y_{mt} is the outcome in municipality m in year t, α_m are municipality fixed effects, γ_t are year fixed effects, Designated_m is a binary indicator for containing at least one estate on the 2018 list, and Post_t equals one for $t \geq 2019$. Standard errors are clustered at the municipality level. The parameter β captures the average change in outcomes in designated relative to non-designated municipalities after the 2018 reform, conditional on municipality and year effects.

The identifying assumption is that outcomes in designated and non-designated municipalities would have followed parallel trends absent the Ghetto Package. I validate this with an

event-study specification:

$$Y_{mt} = \alpha_m + \gamma_t + \sum_{k \neq -1} \delta_k \cdot \text{Designated}_m \times \mathbf{1}[t - 2019 = k] + \varepsilon_{mt} \qquad (2)$$

where $k = -1$ (2018) is the omitted reference period.

Because all treated municipalities share the same treatment date (2019), the standard concern about heterogeneous treatment effects in staggered designs does not arise with full force. Nevertheless, I estimate a Callaway–Sant'Anna (Callaway and Sant'Anna, 2021) model as a robustness check, using never-treated municipalities as the control group and a universal base period.

Threats to validity. The main concern is that designation is endogenous to local demographic trends: municipalities are designated precisely because they have high non-Western shares. The DiD design addresses this by comparing within-municipality changes over time, so the level difference is absorbed. The event study tests whether designated municipalities were on differential trajectories before 2018.

A second concern is treatment intensity heterogeneity: Copenhagen (seven estates) faces a very different treatment dose than Viborg (one estate). I address this with a continuous treatment specification using the estate count.

A third limitation is geographic aggregation. Designated estates are neighborhoods within municipalities, and municipality-level outcomes dilute estate-level effects. If designated estates contain 5% of a municipality's population and that subpopulation is displaced, the municipality-level effect would be roughly 5% of the estate-level effect. I discuss the implications of this dilution in Section 6.

5. Results

5.1 Main Results

Table 2 reports the main estimates. The effect on non-Western population share (column 1) is -0.0007—essentially zero—with a standard error of 0.0037, yielding a 95% confidence interval of $[-0.0081, 0.0067]$. This rules out effects larger than 0.81 percentage points in absolute value, or roughly 10% of the treated-municipality mean.

The log non-Western population (column 2) shows a significant negative coefficient $(-0.093, p = 0.001)$. However, the event-study specification reveals that this result is driven by pre-existing convergence in non-Western population growth rates, with large and significant pre-treatment coefficients at horizons $t - 9$ through $t - 5$. I therefore do not interpret this as

Table 2: Effect of Parallel Society Designation on Municipality Outcomes

	(1) NW Share	(2) Log NW Pop.	(3) NW Emp. Rate	(4) Log Total Pop.
Designated × Post	-0.0007	-0.0928***	-0.2725	0.0355**
	(0.0037)	(0.0276)	(0.7146)	(0.0146)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,995	1,995	1,785	1,995
Municipalities	105	105	105	105

Notes: Each column reports a separate two-way fixed effects regression of the outcome on the interaction of designation status with a post-2018 indicator. Standard errors clustered at the municipality level in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. "Designated" equals one for municipalities containing at least one estate on the 2018 parallel society list. "Post" equals one for years 2019 and later. NW Share is the non-Western immigrant and descendant share of total population. NW Emp. Rate is the employment rate of non-Western immigrants aged 16–64.

a causal effect of designation.

The non-Western immigrant employment rate (column 3) shows no significant effect (-0.27 pp, $p = 0.704$), and total population (column 4) increases slightly in designated municipalities (3.5%, $p = 0.017$), inconsistent with a displacement narrative.

5.2 Event Study

Table 3 reports the event-study coefficients for non-Western population share. The pre-treatment coefficients ($t - 9$ through $t - 2$) are uniformly small, precisely estimated, and statistically insignificant. A joint F-test on all eight pre-treatment leads yields $p = 0.130$, failing to reject the parallel trends assumption. The post-treatment coefficients ($t + 0$ through $t + 7$) are similarly small and insignificant, showing no evidence of divergence even seven years after the reform. This pattern is consistent across the Callaway–Sant'Anna estimator, which yields an overall ATT of 0.0003 (SE=0.0025) with a pre-test p-value of 0.116.

5.3 Robustness

Table 4 reports robustness checks. Treatment intensity—using the continuous estate count instead of the binary indicator—shows no dose-response relationship for the non-Western share (column 1). The placebo outcome (Danish-origin population share, column 3) is appropriately null ($p = 0.918$). Restricting to urban municipalities with population above 20,000 (column 4) produces identical results.

Randomization inference with 500 permutations of the treatment assignment gives $p =$

Table 3: Event Study: Non-Western Population Share

Event Time	Coefficient	Std. Error
$t-9$	0.0011	(0.0025)
$t-8$	0.0014	(0.0023)
$t-7$	0.0013	(0.0019)
$t-6$	0.0015	(0.0016)
$t-5$	0.0014	(0.0015)
$t-4$	0.0012	(0.0012)
$t-3$	0.0004	(0.0007)
$t-2$	-0.0001	(0.0004)
$t+0$	0.0005	(0.0004)
$t+1$	0.0007	(0.0008)
$t+2$	0.0005	(0.0012)
$t+3$	0.0001	(0.0017)
$t+4$	-0.0000	(0.0022)
$t+5$	-0.0000	(0.0031)
$t+6$	0.0003	(0.0041)
$t+7$	0.0002	(0.0051)
Observations	1,785	
Reference period	$t-1$ (2018)	

Notes: Event study coefficients from a regression of non-Western population share on leads and lags of the designation indicator interacted with treated status. Municipality and year fixed effects included. Standard errors clustered at the municipality level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Reference period is $t-1$ (2018). Sample restricted to 2010–2026.

Table 4: Treatment Intensity and Robustness

	(1) NW Share Intensity	(2) Log NW Pop. Intensity	(3) Danish Share Placebo	(4) NW Share Urban
Estates × Post	-0.00004	-0.03671***		
	(0.00176)	(0.00796)		
Designated × Post			0.0006	-0.0008
			(0.0056)	(0.0037)
Municipality FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,995	1,995	1,995	1,843
RI p-value				

Notes: Columns (1)–(2) use treatment intensity (number of designated estates per municipality) interacted with the post-2018 indicator. Column (3) is a placebo test using Danish-origin population share as the outcome. Column (4) restricts to urban municipalities (population > 20,000 in 2018). Standard errors clustered at the municipality level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. Randomization inference p-value for the main NW share specification: 0.868 (500 permutations).

0.868, confirming the null is not an artifact of the cluster structure. Leave-one-out analysis shows the coefficient is stable when dropping any single treated municipality (range: $[-0.0038, 0.0001]$). Using 2018 instead of 2019 as the treatment year yields -0.0008 ($p = 0.824$).

6. Discussion

The central finding is a precisely estimated null: Denmark's 2018 Ghetto Package produced no detectable change in the non-Western population share of designated municipalities over seven years. This result admits three interpretations.

Interpretation 1: Labels don't move people. The most direct reading is that neighborhood stigma—even when officially imposed and backed by mandatory demolition timelines—is insufficient to alter residential sorting at detectable levels. This is consistent with the broader literature finding that residential decisions are driven by housing costs, employment access, and social networks rather than neighborhood reputation (Krysan and Crowder, 2017). The Danish welfare state's generous social housing protections may further insulate residents from stigma-driven displacement.

Interpretation 2: Aggregation dilution. The municipality is a coarse unit for measuring estate-level displacement. A back-of-the-envelope calculation quantifies the dilution. The typical designated estate houses roughly 2,000–5,000 residents in a municipality of 50,000–600,000. If a designated estate represents 4% of the municipal population and the estate's non-Western share (approximately 55%) falls by 10 percentage points due to displacement, the municipality-level non-Western share would decline by only $0.04 \times 0.10 = 0.004$ (0.4 percentage points). My 95% confidence interval extends to ± 0.74 percentage points, so I can only rule out estate-level NW-share declines exceeding roughly 19 percentage points—a dramatic threshold. More modest displacement of 5–10 percentage points at the estate level would be undetectable. This dilution interpretation cannot be ruled out with public API data and would require estate-level microdata from the DST Research Service.

Interpretation 3: Offsetting flows. Displacement from designated estates may be offset by in-migration of non-Western immigrants to other parts of the same municipality, leaving the aggregate share unchanged. This compositional churning would be invisible at the municipality level but consequential for individual residents.

The minimum detectable effect of 0.73 percentage points (approximately one-tenth of the treated mean) is informative. It rules out municipality-wide demographic transformation of the kind implied by the policy's rhetoric. Whether estate-level displacement occurs beneath

this detection threshold is an open question that requires granular register data.

The finding connects to the broader question of whether place-based policies operate through reputation or through physical intervention. The Ghetto Package bundles both channels: the designation list stigmatizes, while the five-year demolition trigger physically restructures. My null suggests that the stigma channel is weak at the aggregate level. The declining number of designated areas from 29 (2018) to 5 (2025) likely reflects the demolition channel—physical removal of public housing units—rather than reputation-driven out-migration (OECD, 2020).

7. Conclusion

Denmark's government labeled 29 neighborhoods as parallel societies, doubled criminal penalties within their boundaries, mandated demolition timelines, and imposed integration contracts on residents. Seven years later, the demographic composition of designated municipalities is indistinguishable from what it would have been without the policy. At the municipal aggregate, the label did not produce the demographic transformation its architects intended. Whether designation drives displacement at the estate level—beneath the detection threshold of municipality-level data—remains an open question that estate-level register data can answer. For the growing number of countries considering designation-based integration policies—from France's quartiers prioritaires to Sweden's utsatta områden—the finding offers a boundary condition: even maximal stigma does not propagate into municipality-wide compositional change, and policymakers should not expect designation alone to achieve the neighborhood-level sorting that physical demolition may or may not deliver.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
Non-Western Share	-0.0007	0.0037	0.0485	-0.0140	0.0766	Small negative
Log NW Population	-0.0928	0.0276	1.6700	-0.0556	0.0165	Moderate negative
NW Employment Rate	-0.2725	0.7146	8.4051	-0.0324	0.0850	Small negative
Panel B: Heterogeneous (Treatment Intensity Split)						
NW Share (High Intensity)	0.0047	0.0076	0.0485	0.0971	0.1572	Moderate positive
NW Share (Low Intensity)	-0.0043	0.0023	0.0485	-0.0880	0.0478	Moderate negative

Notes: **Country:** Denmark. **Research question:** Does official designation of public housing estates as "parallel societies" under the 2018 Ghetto Package cause displacement of non-Western immigrant-origin residents from designated municipalities? **Policy mechanism:** The 2018 Ghetto Package (Act No. 1000) mandated that estates designated as parallel societies for five consecutive years must reduce public family housing below 40% through demolition or conversion, imposed doubled criminal penalties for residents, and required mandatory integration contracts—creating both stigma and direct displacement pressure. **Outcome definition:** Non-Western share is the fraction of municipal population who are immigrants or descendants from non-Western countries (DST FOLK1E, ancestry codes 25 and 35); NW Employment Rate is the employment rate of non-Western immigrants aged 16–64 (DST RAS200). **Treatment:** Binary indicator equal to one if the municipality contains at least one estate on the 2018 parallel society designation list, interacted with a post-2018 indicator. **Data:** Statistics Denmark StatBank API (FOLK1E, RAS200), 98 municipalities, 2008–2026, annual (Q1 snapshots for population). **Method:** Two-way fixed effects (municipality + year FE), standard errors clustered at municipality level; Callaway–Sant'Anna for robustness. **Sample:** All 98 Danish municipalities; 15 treated (containing ≥ 1 designated estate), 83 control. SDE = $\hat{\beta}/$SD(Y) where SD(Y) is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).