Direct and Indirect Effects of a Housing Policy on Neighborhoods, Residential Mobility, and Inequality*

Christian Dustmann, Rasmus Landersø, and Mikkel Mertz

March 2025

Abstract: We analyze a reform in Copenhagen that converted approximately 20,000 public rental units into privately owned properties. We show that the policy attracted more affluent residents not only to the converted housing units but also to other units within the same neighborhoods. However, when expanding the analysis to encompass all neighborhoods in Copenhagen, we find an increase in the disparity between disadvantaged and affluent areas. While the neighborhoods directly impacted by the policy experienced positive development, other low-income neighborhoods saw a growing concentration of disadvantaged residents. These changes to residential patterns translated into changes in local crime rates and affected children's outcomes such as GPA and high-school completion underscoring both the large potential effects of residential segregation and place-based policies but also the importance of assessing policy effects beyond the initially targeted communities.

Keywords: Neighborhood Policies, Spillover Effects, Inequality.

JEL codes: I38, J18, R23, R28

---

* Dustmann: Department of Economics, Centre for Research and Analysis of Migration (CReAM) and ROCKWOOL Foundation Berlin (RFBerlin), University College London, Gower Street, London WC1E 6BT, England (e-mail: c.dustmann@ucl.ac.uk); Landersø: The ROCKWOOL Foundation, Ny Kongensgade 6, 1472, Copenhagen, Denmark (e-mail: rl@rff.dk); Mertz: The ROCKWOOL Foundation, Ny Kongensgade 6, 1472, Copenhagen, Denmark (e-mail: mme@rff.dk). We gratefully acknowledge support from The ROCKWOOL Foundation.

1 Introduction

The role of neighborhoods in shaping economic opportunity and mobility is critical, as demonstrated in numerous studies (see e.g. Chetty et al., 2016; Chetty and Hendren, 2018). Growing up in disadvantaged or segregated neighborhoods can significantly limit a person's chances for upward mobility, while access to higher-opportunity areas can improve long-term outcomes for residents. A crucial question arising from this research is whether initiatives targeting specific neighborhoods through, for example, redevelopment or revitalization projects can lead to improved outcomes for the people who live there (Asquith, 2023; Blanco and Neri, 2023; Boustan et al., 2023; Chetty et al., 2024; Diamond and McQuade, 2019; Neri, 2024; Staiger et al., 2024; Tach and Emory, 2017).[1] However, while the direct effects of such initiatives are of key interest, changed in- and outflows of residents in some neighborhoods implies that the composition of other neighborhoods change in response to such policies as well. Measuring the total policy effects, therefore, requires understanding such broader repercussions of interventions as they ripple through the city and potentially affect neighborhoods that were not directly targeted by the policy through residential mobility. Yet, there has so far been limited work examining these wider impacts, leaving a gap in the understanding of how targeted neighborhood improvements may affect communities throughout an entire city.

This paper presents new evidence on the effects of a housing policy that converted public rental units into private ownership, on the residential composition of targeted units, the spillover effects to surrounding neighborhoods, and how the policy reshapes the composition of neighborhoods across the entire city.

[1] Other related studies examine the consequences of subsidized homeownership (Disney et al., 2023; Hausman et al., 2022; Sodini et al., 2023) and rent control regulations (Autor et al., 2014; Diamond et al., 2019) for residential composition in the targeted areas.

We leverage a large natural experiment in Copenhagen, Denmark, where approximately 20,000 public rental units—representing 6% of the city's housing stock and primarily located in disadvantaged neighborhoods—were transformed into co-op-owned housing.[2] This policy shift offers insights into the broader urban dynamics and socioeconomic changes resulting from such local housing interventions. The integration of comprehensive population register data, containing individual information such as home address, education, income, and labor market attachment, with housing unit register data, including address, neighborhood, ownership status, and sales prices, enables us to assess the policy's impact from the level of individual housing units to a city-wide perspective.

The policy, known as TOR, was announced in November 1994 and implemented on January 1, 1995, leaving minimal time for anticipation effects. Using a difference-in-differences design with control groups drawn from comparable housing units and neighborhoods in Denmark's three largest cities outside of Copenhagen (Aarhus, Odense, and Aalborg), we estimate the policy's effects on three levels. First, directly treated units (TOR units), where we compare residents in these units to those in similar public rental units in the three comparison cities. Second, the neighborhood impact (TOR neighborhoods), where we compare residents in TOR neighborhoods (but not TOR units) to those in similar neighborhoods in the comparison cities. And third, city-wide effects, where we evaluate the policy's impact on each neighborhood in Greater Copenhagen by constructing a synthetic control group for each neighborhood using data from Aarhus, Odense, and Aalborg.

Our findings reveal that six years after implementation, the average earnings rank of residents in TOR units increased by seven percentiles, and the share of adult residents without a job or not enrolled in

[2] A housing co-operative – in short *co-op* – is a group of individual residents who jointly own a building (e.g., an apartment complex) with owner shares reflecting the relative size of their units in the co-op. Thereby, the individuals can share risks and form loans at more lenient terms than if they were to buy their housing unit outright. Section 2 and Appendix B describe this in detail.

education dropped by ten percentage points compared to both the control group and other housing units in Copenhagen. These effects were driven entirely by changes in resident composition, as the new co-ops prioritized younger, more affluent individuals with stronger labor market attachments.[3] In addition, residents' crime rates had declined by almost three percentage points driven by fewer property and drug-related crimes.

Next, we assess the policy's impact on the residential composition of other housing units in the same neighborhoods as the TOR units. We find that the policy led to a delayed but notable influx of affluent individuals into these areas, with the effects being most pronounced in neighborhoods with a higher concentration of TOR units. Six years post-policy, the average earnings rank of residents in high-concentration TOR neighborhoods increased by around three percentiles compared to their control counterparts, while low-concentration neighborhoods saw an increase of around one percentile. Crime rates also dropped by roughly one percentage point in TOR neighborhoods, while housing prices rose by approximately 10%, indicating heightened demand due to the policy. Thus, the TOR policy appears to have improved the quality of directly affected neighborhoods.

The shifting movement of low- and high-resource individuals in and out of certain neighborhoods likely generates ripple effects influencing other areas across the city. To explore this, we estimate the policy's impact on all neighborhoods in Greater Copenhagen by comparing each with synthetic control counterparts. Our findings reveal that the policy significantly altered the residential composition within Copenhagen, with significant heterogeneity in the policy's effects that extend beyond just the TOR neighborhoods. While many neighborhoods, particularly those with TOR units,

[3] We find that converting public rental housing units into co-ops had no significant effect on the financial or labor market outcomes of pre-policy tenants who initially purchased the co-ops. The sales occurred at market prices without public subsidies, unlike a similar policy in Stockholm (Sodini et al., 2023), and did not result in direct capital gains for the buyers. A post-policy survey reveals that residents chose to form co-ops primarily due to uncertainty about potential sales of their housing units to third parties (COWI, 2000).

benefited from an increased concentration of affluent residents, others were disadvantaged due to a rise in low-resource individuals.[4] Not only did the residential composition of many neighborhoods change due to the policy, we also evidence that exposure to policy effects during childhood is associated with children's later outcomes. Both 9th grade GPA and the probability of completing high school increase, and the probability of being charged for a crime as a teenager declines for children growing up in neighborhoods with higher policy induced inflows of affluent residents. Thus, the policy did successfully bring positive changes and increased opportunities to the target neighborhoods. Yet, it also led to a higher concentration of disadvantaged residents in other areas, and consequently the average income gap between low- and high-resource neighborhoods increased.

By investigating the impact of a housing policy that converts public housing into co-ops on neighborhoods that are not directly affected by the policy and the residential composition of an entire city, our paper contributes to the literature that investigates programs that develop disadvantaged neighborhoods (e.g., Chetty et al., 2024; Staiger et al. 2024). While our findings show that the TOR policy significantly and permanently improved the residential composition in directly affected neighborhoods, we also show that low-resource households became more concentrated in other disadvantaged areas, thereby increasing inequality between neighborhoods. These results contribute to the literature on the broader impact of improving neighborhood quality on residents' lives (e.g., Chyn and Katz, 2021; Bergman et al., forthcoming) including children. We show that children living in areas with positive policy effects (i.e., the directly targeted areas) were better off, while children in areas with negative policy effects were worse off. Enhancing disadvantaged neighborhoods is crucial for promoting equality of opportunity, as neighborhood environments significantly influence

[4] We use earnings, employment, inactivity (neither studying nor employed), and crime as indicators of individuals' and neighborhoods' socio-economic status. For simplicity, we refer to differences across these dimensions as representing low-resource and high-resource individuals and neighborhoods.

individual outcomes, including earnings, education, marriage rates, fertility, and criminal behavior (Chetty et al., 2016; Chetty and Hendren, 2018a,b; Chyn, 2018; Damm and Dustmann, 2014; Deutscher, 2020; Dustmann et al., 2023). However, understanding how residential policies affect moving patterns, particularly among disadvantaged groups, is essential for effectively promoting equality of opportunity on a larger scale.

Our study also contributes to the literature on the broader consequences of housing policies.[5] Several studies have shown that homeownership can benefit individual homeowners through capital gains (Sodini et al., 2023) and positively impact the local neighborhood, for example, by increasing labor market attachment (Hausman et al., 2022) and reducing crime rates (Disney et al., 2023). Similarly, regeneration or redevelopment of housing has been found to alter the composition of residents and increase housing prices in targeted neighborhoods (e.g., Asquith, 2023; Blanco and Neri, 2023; Boustan et al., 2023; Diamond and McQuade, 2019; Neri, 2024; Tach and Emory, 2017).[6] While we confirm neighborhood-level spillovers from policies targeting specific housing types, we provide new evidence on how local housing policies can more broadly reshape neighborhoods across an entire city.[7] Specifically, our finding that improving some low-resource neighborhoods can adversely affect other low-resource areas addresses a major concern associated with gentrification.[8] Although gentrification may not immediately cause a large outflow of disadvantaged households (e.g., Vigdor, 2002; Freeman and Bracconi, 2004; McKinnish et al., 2010), our results show that changing moving

[5]A related body of literature examines the impact of changing administrative borders, such as school districts, on segregation (e.g., Baum-Snow and Lutz, 2011; Billings et al., 2014; Bjerre-Nielsen and Gandil, 2024). In contrast, the policy analyzed in this paper maintains constant administrative boundaries, with all variation arising solely from the change in ownership types.

[6] Similar effects have been found in studies of targeted school grants (Billings et al., 2018; Friday and Smith, 2023).

[7] These city-wide spillovers also highlight the risks of using "untreated" yet geographically close neighborhoods as control groups, as is common in many studies (see also Alves et al., 2023).

[8] Our paper is also related to studies on the spillovers from place-based housing policies to surrounding neighborhoods (e.g., Schwartz et al., 2006; Eriksen and Rosenthal, 2010; Rosenthal, 2014), the impact of aggregate demand shocks on local prices (Guerrieri et al., 2012, 2013), and the effects of changes in rent control (e.g., Autor et al., 2014; Diamond et al., 2019).

patterns and inflows into neighborhoods can accumulate over time, leading to increased disparities between neighborhoods in the longer run.

The remainder of the paper is organized as follows: Section 2 presents the TOR policy, and Section 3 introduces the data. Section 4 describes the econometric framework. Section 5 presents the results for TOR units and TOR neighborhoods. Section 6 presents the estimated effects for all neighborhoods in Copenhagen, investigates the consequences for children, and analyzes the characteristics of neighborhoods that experienced shifts in residential composition. Finally, Section 7 concludes.

2 Background and the TOR Policy

Copenhagen is consistently ranked among the most livable cities in the world.[9] In the early 1990s, however, Copenhagen had a higher concentration of low socio-economic status (SES) residents than the rest of the country, leading to increased public expenditures and reduced tax revenues. As a result, the Municipality of Copenhagen's finances declined, with budget deficits rising each year.[10]

The TOR policy was introduced to help address these financial challenges. The Municipality of Copenhagen owned nearly 20,000 rental apartments (234 residential properties/housing blocks), representing about 6% of the city's total housing stock. These were small, inexpensive rental units scattered across the city (see Appendix Figure A.1), primarily located in low-resource neighborhoods. Many of the apartments were in poor condition, with estimated renovation costs of approximately $1.2 billion in 2020 prices. In June 1994, the State and the Municipality of Copenhagen agreed to sell these apartments.

---

[9] See for instance the 2023 ranking of the best cities to live in done by the Economists Intelligence Unit, where Copenhagen is ranked second (https://www.eiu.com/n/campaigns/global-liveability-index-2023/?utm_source=media&utm_medium=press_release&utm_campaign=liveability23).

[10] For instance, the 1995 budget proposal projected a deficit of $450 million, nearly 30% of the city's revenue (Statistics Denmark, 2024). By the end of 1994, the Municipality of Copenhagen's total debt had reached $2.3 billion (Municipality of Copenhagen, 2001).

As a first step, on January 1, 1995, the apartments were sold at an inflated price to a company named TOR I/S (hereafter TOR). The company was jointly owned by the State (15%) and the Municipality of Copenhagen (85%), with the State guaranteeing the company's deficits.[11] In the second step, TOR sold the apartments as co-ops, granting preemption rights to existing tenants.[12] A co-op (co-operative) is a form of collective property ownership where a group of individuals collectively purchases a set of residential units, such as a building or apartment complex, with each member owning a share of the building that reflects their proportional contribution to the property's purchase.[13] Co-op owners thereby share risks involved with loan-taking and real estate purchase.

The TOR policy was announced in the fourth quarter of 1994, giving tenants little time to respond before its implementation on January 1, 1995. Leaflets were sent to all tenants, informing them that they could purchase their current apartment by forming a co-op with other tenants in the building and providing guidance on how to do so. The apartments were sold to third parties if tenants did not express interest. When a third party bid on an apartment, the tenants were again offered the opportunity to buy it as a co-op under the same terms as the third-party bidder. Appendix B provides further details about the TOR policy and the co-op formation process.

---

[11] The sales prices from the municipality to TOR were inflated, while the sales prices from TOR to individual buyers were set at market rates by realtors, reflecting the value of similar apartments in Copenhagen at that time. This arrangement effectively served as a substantial financial transfer from the State to the Municipality of Copenhagen. Under the agreement, the State covered any deficits up to $500 million.

[12] TOR purchased the housing units for $500 million at the start of 1995, but the value of the properties was reassessed to $400 million later that year. Additionally, TOR was unable to recover the initial cost through apartment sales and ended up with a deficit of around $150 million upon closing, which the State covered.

[13] When a co-op is formed, the future co-op owners jointly take up a mortgage to fund the purchase. The price of a share in the co-op is subsequently given by the co-op's net value (joint valuation of the residential units minus the mortgage) adjusted for the relative size of the share in the co-op (i.e., apartment size). During the period in question, the valuation of the residential units in co-ops were set at market value when the co-op was formed, but subsequently valuations were close to constant implying that co-op prices did not increase with the general prices in the housing market. Thus, there is no direct transfer of wealth to co-op owners from the TOR units. To fund the co-op's interest payments and running costs (e.g., maintenance and future renovations), co-op owners pay a monthly rent.

Figure 1A illustrates the impact of the TOR policy on the housing composition in Copenhagen during the implementation period. In 1995, there was a noticeable decrease in the number of public rental units and a corresponding increase in private rental units, reflecting the sale of public rental properties owned by the Municipality of Copenhagen to TOR. From 1995 onward, there was a gradual decline in the number of private rental units and a corresponding increase in the number of co-op units, reflecting the ongoing sales from TOR to residents. These changes were entirely driven by converting public rental units to co-op apartments under the TOR policy, as shown in Figure 1B, which focuses on the ownership status of housing units identified as potential policy candidates. By January 1, 1995, around 95% of these units had transitioned from public to private ownership, representing the sale from the Municipality of Copenhagen to TOR. By 1998, nearly all of these apartments had been sold again, primarily to residents, and converted into co-ops (95%), with the remaining units turned into private rentals by third-party buyers.

2.1 Previous Tenants, New Owners, and the Co-ops

A key aspect of the TOR policy was that sales from TOR to private owners were conducted at market prices, considering the buildings' conditions and renovation needs. There were no clear financial gains for tenants in forming co-ops. However, nearly all units were converted to co-ops within a few years due to the threat of sales to third parties. In a 1999 survey, original tenants cited uncertainty about future rents as the primary reason for forming co-ops (COWI, 2000). Moreover, forming a co-op required only a majority of residents in a building, not unanimous consent.

To enable participation even for tenants with low income and assets, the Municipality of Copenhagen allowed existing tenants to use their rental deposits as down payments when forming a co-op. Appendix B.3 describes the consequences of forming a co-op for the original tenants and their wealth in greater detail, and shows that the formation and (potential) subsequent sale of co-op shares have

not impacted the wealth of the original tenants (which is in contrast to Sodini et al. (2023), who studied a conversion of public housing units to co-ops in Stockholm).

Despite the measures aimed at preventing displacement and facilitating the conversion from rental to co-ops for all previous tenants, the resident composition changed significantly in the following years. As original residents were replaced through the ongoing churn (which did not change across the policy as illustrated in Figure A.2), many co-ops introduced selection criteria favoring young individuals, families with stable employment or students (COWI, 2000; VIVE, 2002). In Section 5, we demonstrate how this significantly altered the composition of residents in TOR units in the years after the policy's implementation.

3 Data

Unique anonymized address identifiers enable us to link housing units to neighborhoods and residents, while unique individual identifiers allow us to connect data from various administrative sources to these residents. Appendix C provides detailed descriptions of the data sources, analysis sample, and all outcomes.

3.1 The Analysis Sample

We first identify all housing units in Denmark, including unit-specific details such as ownership status (e.g., public, private, co-op), type (e.g., house or apartment), and year of construction, and link units to neighborhoods, each containing approximately 150 households.[14] When a unit is sold, we capture the sale date and price (for owner apartments) from the housing register, which allows us to construct neighborhood-level housing price measures.

[14] These neighborhoods are based on the classification documented in Damm and Schultz-Nielsen (2008).

Next, we link all housing units in Denmark (apartments and houses identified through unique address information) to individual-level data on residents using a combination of unique addresses and individual identifiers. We gather annual information on residents' age from demographic registers, earnings, disposable income, and wealth from tax registers, and labor market status from employment registers.

We focus on 1991 to 2001, allowing us to analyze both pre- and post-reform trajectories of TOR and non-TOR housing units.[15] Our sample includes housing units and residents from Copenhagen, as well as the three largest cities in Denmark after Copenhagen (Aarhus, Odense, and Aalborg), which serve as control groups for estimating the effect of the TOR policy (Section 4 details the construction of these control groups). We also include the Greater Copenhagen area to examine the spillover effects of the TOR policy across the larger metropolitan region.[16]

We classify housing units as treated by the TOR policy if they were owned by and located in the Municipality of Copenhagen in 1994, and refer to them as TOR units. Out of approximately 300,000 housing units in Copenhagen in 1994, around 19,000 were classified as TOR units.

3.2 Key Variables and Descriptives

We examine several individual outcomes, including earnings (from both labor and self-employment), labor market status, age, and whether individuals have been charged with a crime. Earnings are measured as ranks, because it directly indicates individuals' positions within Denmark's overall

[15] We conclude our analysis in 2001 because, from that point onwards, Copenhagen begins to diverge from the rest of Denmark in terms of housing prices. Additionally, several new policies specifically targeting Copenhagen were introduced around this time. As shown in Appendix Figure A.3, between 1992 and 2001, the average real estate selling price per square meter for apartments in Greater Copenhagen, Aarhus, Odense, and Aalborg followed a similar trend. However, from 2002 onwards, prices in Copenhagen increased relative to Odense and Aalborg.

[16] We define Copenhagen as comprising the Municipalities of Copenhagen and Frederiksberg, with the latter situated within the former. The Greater Copenhagen area includes Copenhagen, Frederiksberg, Gentofte, Gladsaxe, Herlev, Glostrup, Rødovre, Hvidovre, and Tårnby.

earnings distribution in a year. We focus on earnings as the primary outcome because it provides a clean measure of neighborhood quality and resident composition, whereas policy effects on alternative income measures, such as disposable income after taxes and transfers, could be confounded by Denmark's generous tax-transfer redistribution system. Labor market status is categorized as employed, studying (enrolled in education), and the residual category inactive (i.e., neither employed nor studying). These classifications capture significant changes in neighborhood composition during the study period. Age is categorized into three groups—18-34, 35-49, and 50 or older—to differentiate between the in- and outflow of specific demographic groups. We construct an indicator for whether an individual has been charged with a non-traffic crime committed in a year to identify criminal offenders. We also use information on the type of crime to distinguish between violent, property, and drug-related offences. Additionally, we consider the logarithm of sales prices of privately owned housing units in neighborhoods. Finally, when we study the effects of the policy on children's outcomes, we also consider 9th grade GPA and high school completion.

Table 1 presents the mean characteristics of adult residents in TOR units (columns 1 and 4), adult residents in other housing units in Copenhagen (columns 2 and 5), and adult residents in public housing units in the other three cities with similar construction years as the TOR units (columns 3 and 6). Characteristics are measured in 1994 before the TOR policy was implemented, and in 2001, six years after its initiation. Comparing columns 1 and 2 shows that in 1994, residents in TOR units were older, less likely to be employed or studying, had lower earnings and disposable income, and were more likely to have been charged with a crime than residents in other housing units in Copenhagen. Comparing columns 1 and 3 reveals that residents in TOR units were more similar to those in public housing units in the other three cities. However, they still exhibited lower labor market attachment and income levels. By 2001, as shown in columns 4 to 6, the differences in average

characteristics between residents of TOR units and those in other parts of Copenhagen and public housing units in the other three cities had either disappeared or diminished significantly.

As an initial visual illustration of the changes in resident composition following the TOR policy, Figure 2 depicts the average earnings rank and employment rate over time for residents in TOR units, other housing units in Copenhagen, and public rental units in the three comparison cities. Figures 2A and 2B show the levels of these outcomes, while Figures 2C and 2D index them relative to the year 1994. The figures indicate large and stable differences in earnings ranks and employment rates before 1995, followed by a marked convergence in the years immediately after the TOR policy, driven entirely by changes in the average characteristics of residents in TOR units. The estimation results in Section 5 will confirm this pattern and demonstrate how these changes have broadly influenced the composition of residents across Copenhagen neighborhoods. Finally, while Figure 2 shows differences in levels and trends between the city of Copenhagen and the three other cities, the introduction of synthetic control neighborhoods in combination with a broader treatment area including suburbs in the Greater Copenhagen area will eliminate this concern.

4 Empirical Strategy

Let y_{it} denote the average outcome of residents in housing unit i, in year t and let y_{it}^{d} denote the potential outcome of housing unit i in year t with ($d = 1$) and without ($d = 0$) the TOR policy. Our objective is to identify the effect of the TOR policy on the average characteristics of residents in different groups of housing units ($G_i = g$) affected by the policy ($tor_i = 1$). For affected housing units in group g in year t, the average effect of the policy is given by: $\delta_t^g = E(y_{it}^1 - y_{it}^0 | G_i = g, tor_i = 1)$. The main challenge to identify δ_t^g is that the counterfactual outcome y_{it}^0 is unobserved for affected housing units after the TOR policy is implemented. Therefore, we use a difference-in-differences design to estimate the effect of the policy as the difference in mean outcomes between

affected and control units in year t, relative to the difference in mean outcomes between these two groups before the policy.[17] These estimates have a causal interpretation if the mean outcome of residents in housing units affected by the TOR policy would have followed the same trajectory as residents in the control housing units in the absence of the TOR policy (the parallel trends assumption).[18] While this assumption is not directly testable, we provide supporting evidence in Sections 5 and 6 by showing that the treatment and control groups follow the same trend in the pre-policy period.

We estimate regressions of the following form:

$$y_{it} = \alpha^g + \sum_{\substack{k=1991 \\ k\neq 1992}}^{2001} (\delta_k^g \, tor_{\mathrm{i}} + \beta_k^{\mathrm{g}}) \cdot 1[t = k] + \gamma^g tor_i + u_{it}^g \qquad (1)$$

where α^g is a constant, tor_i is an indicator of housing unit i being in Copenhagen and therefore potentially affected by the TOR policy, and $1[t = k]$ is an indicator equal to one if the year of observation (t) is equal to k. We omit year 1992 such that α^g captures the mean outcome of the control group in 1992. The $\beta_k^{g\prime}s$ capture the mean outcome of housing units in the control group in year k, compared to 1992, and γ^g captures the difference in mean outcomes between affected housing units and the control group in 1992. Finally, estimates of δ_k^g are the key parameters of interest. They capture the difference in mean outcomes between treated and control units in year k, relative to the difference in mean outcomes between treated and control units in 1992, and causally identify $E[y_{it}^1 - y_{it}^0 | G_i = g, tor_i = 1]$ if the common trends assumption is satisfied.

---

[17] This strategy is widely employed in papers studying effects of residential policies on residential characteristics (see e.g. Diamond et al., 2019; Hausman et al., 2022; Sodini et al., 2023).

[18] Specifically the common trends assumption for estimating the impact of the TOR policy on housing units in group g in year t, using year 1992 as the baseline period becomes: $E(y_{it}^0 | G_i = g, tor_i = 1, t) = E(y_{it} | G_i = g, tor_i = 1, t = 1992) + E(y_{it} | G_i = g, tor_i = 0, t) - E(y_{it} | G_i = g, tor_i = 0, t = 1992)$.

We focus on estimates of δ_k^g for three groups: the average effect of the TOR policy on TOR housing units, the average impact of the TOR policy on non-TOR housing units located in neighborhoods with TOR units, and the effect of the TOR policy on housing units located in each neighborhood in Greater Copenhagen.

4.1 Choice of Control Group

The choice of control group depends on which of the parameters of interest we seek to estimate. The next subsections describe these in detail.

4.1.1 Estimating the Direct Effect of the TOR Policy

To estimate the impact of the policy on TOR units, we use a control group comprising public rental housing units in Aarhus, Odense, and Aalborg that have similar construction years as the TOR units.[19] The advantage of this control group is that these cities are located far from Copenhagen and are therefore not influenced by the TOR policy through spillover effects, unlike housing units in or near Copenhagen (see Sections 4.1.3 and 6, and Alves et al., 2023, for a detailed discussion). We show in Section 5.1 that the TOR units and their control units followed similar pre-policy trends for all outcomes we consider.

A potential concern with using these control cities is that other local shocks to the Copenhagen housing market could mistakenly be attributed to the policy's estimated effect on TOR units. To address this, we conduct robustness checks using non-TOR housing units in Copenhagen as a control group, finding no evidence that Copenhagen-specific shocks drive our estimates. Additionally, housing prices in Copenhagen followed a similar trend to those in other urban areas of Denmark from

[19] We consider public rental housing in Aarhus, Odense and Aalborg built between 1900 and 1967.

1992 to 2001, suggesting that local market shocks are unlikely to bias our results significantly (see Appendix Figure A.3).

4.1.2 Estimating the Effect of the TOR Policy on Neighborhoods with TOR Units

To estimate the policy's impact on non-TOR units in neighborhoods with TOR units, we use a control group of housing units in neighborhoods with public rental units in Aarhus, Odense, and Aalborg. We differentiate between neighborhoods with a low concentration of TOR units (20% or fewer) and those with a high concentration (more than 20%). To ensure comparability, we similarly distinguish between control units in neighborhoods with corresponding low and high concentrations of public rental housing units, respectively. We show in Section 5.2 that the TOR neighborhoods and their controls followed similar pre-policy trends for all outcomes we consider.

4.1.3 Estimating the Impact of the TOR Policy on Each Neighborhood in Greater Copenhagen

To estimate the impact of the TOR policy on housing units within each neighborhood in Greater Copenhagen, we construct a synthetic control neighborhood for each area using a donor pool of housing units from neighborhoods in Aarhus, Odense, and Aalborg (see Abadie and Gardeazabal, 2003; Abadie et al., 2010, and Appendix D.1 for more details). We then estimate separate regressions, similar to Eq. 1, for housing units in each neighborhood in Greater Copenhagen using their corresponding synthetic control group.

Figure A.4 documents that the synthetic control neighborhoods followed their treatment counterparts closely both during the matching period (year 1991-1994) and in the pre-matching years. In addition, the neighborhood-specific analysis in Section 6 provides two key findings supporting the validity of our design. First, almost all estimates of δ_k^g are close to zero in the pre-policy years, confirming common trends between treatment and control neighborhoods leading up to the policy. Second, the average effects $E(\delta_k^g)$ across all neighborhoods in Copenhagen in post-policy years are close to zero,

indicating that, on average, the control group follows the same trajectory as Copenhagen neighborhoods. Thus, all neighborhood-specific treatment effects result from changes in residential composition within Copenhagen in the years following the implementation of the TOR policy.

5 Results

5.1 Direct Effects

Figure 3 shows the estimated effects of the TOR policy (δ_k^g in Eq. 1) on the earnings rank of residents in TOR units from 1991 to 2001 when the control group is public rental housing units in Aarhus, Odense, and Aalborg. In the period leading up to the TOR policy, the TOR units and their controls followed a similar trend, supporting the common trends assumption. After the implementation of the TOR policy, there is a marked increase in the earnings rank of residents in TOR units compared to their controls. By 2001, the average earnings rank of residents in TOR units had risen by six percentile points relative to their counterfactual.[20]

Several studies have shown that homeownership can lead to increased earnings and labor market attachment among original residents (e.g., Hausman et al., 2022; Disney et al., 2023). To explore this, we decompose the estimates from Figure 3 into changes in the earnings of original residents in TOR units who remain in those units and changes in the composition of residents in TOR units (i.e., inflows and outflows). Additionally, we examine the impact of the policy on all original residents even those who moved to different housing, as they may have benefited from the policy in other ways, such as climbing the housing ladder, as suggested by Sodini et al. (2023).

[20] One potential concern with the control group used in Figure 3 is that other local shocks to the Copenhagen housing market could lead to biased estimates (see Section 4.1.1). To address this, Figure A.5 presents estimates using a control group of other housing units within Copenhagen. The results are similar to those obtained using public rental housing units in other cities as controls, suggesting that such shocks are unlikely to be driving our estimates.

In a year t, the estimates shown in Figure 3 can be written as: $(\bar{Y}_t^{TOR} - \bar{Y}_{92}^{TOR}) - (\bar{Y}_t^{OC} - \bar{Y}_{92}^{OC})$, where $\bar{Y}_t^{TOR}$ is the mean outcome of residents in TOR units in year t and $\bar{Y}_t^{OC}$ is the mean outcome of residents in the control housing units in year t. Let $\bar{Y}_t^{TOR,stayer}$ denote the mean outcome in year t of individuals who lived in TOR units in 1992 and in year t (stayers), let $\bar{Y}_t^{TOR,inflow}$ denote the mean outcome in year t of individuals who lived in a TOR unit in year t but not in 1992 (inflows), and let $\bar{Y}_t^{TOR,outflow}$ denote the mean outcome in year t of individuals who lived in TOR units in 1992 but not in t (outflow). Then we can rewrite the first term as:

$$(\bar{Y}_t^{TOR} - \bar{Y}_{92}^{TOR}) = (1-w) \cdot \underbrace{\left(\bar{Y}_t^{TOR,stayers} - \bar{Y}_{92}^{TOR,stayers}\right)}_{\text{Effects through stayers}} + w \cdot \underbrace{\left(\bar{Y}_t^{TOR,inflow} - \bar{Y}_{92}^{TOR,outflow}\right)}_{\text{Effects through composition}}$$

where w is the fraction of new residents from 1992 to year *t*. The last part of the decomposition is referred to as the composition effect, while the first part represents the impact driven by stayers.

Table 2 shows the baseline estimates in column 1, corresponding to Figure 3. Column 2 shows the decomposition with effects due to original TOR residents who remain in TOR units, $\left(\bar{Y}_t^{TOR,stayers} - \bar{Y}_{92}^{TOR,stayers}\right)$, and column 3 shows the effects due to changes in the composition of residents, $\left(\bar{Y}_t^{TOR,inflow} - \bar{Y}_{92}^{TOR,outflow}\right)$. Column 4 shows the relative weight of the compositional change, w, in the overall policy effect from column 1. Finally, column 5 of Table 2 shows the estimated impact of the TOR policy on the original residents, irrespective of whether they remain in a TOR unit.[21]

[21] Note that coefficients for stayers and original residents in columns 1 and 5, respectively, differ because the stayers are a subset of the original residents. We estimate the impact of the TOR policy on the original residents by comparing the outcome trajectories of those living in TOR units in the beginning of 1994 to the outcome trajectories of residents of public housing units in Aarhus, Odense and Aalborg in the beginning of 1994 based on Eq. (1).

The substantial increase in the earnings rank of residents in TOR units is entirely attributable to a change in the composition of residents (see columns 2 and 3). Meanwhile, there is no evidence of a positive effect of the policy on the original residents of the TOR housing units (see column 5).

Figure 4 presents estimates of the impact of the TOR policy on the labor market status (employed, studying, or neither) and age composition of residents in TOR units (18–34, 35–49, 50+). The policy resulted in an increased share of residents who were either employed or studying and a decrease in those who were neither employed nor studying (Figure 4A). By 2001, the share of employed residents in TOR units had risen by approximately 10 percentage points, and the share of residents studying had increased by about five percentage points. Conversely, the proportion of residents neither employed nor studying dropped by 15 percentage points. Figure 4B shows a significant shift in the age composition of TOR residents, with a rise in younger residents at the expense of older populations. These changes indicate that lower-resource residents were replaced, as many co-ops introduced selection criteria favoring younger individuals who were either employed or studying.[22] The changing age profile in TOR units raises the question of whether this affects earnings outcomes. To explore this, Figure A.6A shows the policy's impact on residents' earnings rank, broken down by age groups: 18-34, 35-49, and 50+ years old. The figure demonstrates that earnings ranks increased across all age groups. By 2001, the average earnings rank had risen by approximately 9, 6, and 2 rank points for individuals aged 18-34, 35-49, and 50+, respectively, within the overall population distribution. While the TOR policy did not lead to direct effects on the original residents' asset accumulation as described in Section 2.1 and Appendix B.3, Figure A.6B shows that the increased earnings ranks among residents are mirrored in the residents' assets as well. Six years after the policy

[22] Figure A.5 shows that these estimates remain similar when using a control group consisting of housing units within Copenhagen, rather than public rental units in other large cities. While the Copenhagen-based control group is influenced by city-wide spillovers, which we will discuss in Section 6, Figure A.5 confirms that the effects observed for TOR units are not driven by broader differences in trends between Copenhagen and other parts of Denmark.

introduction, the average assets rank of residents in TOR units were around 6, 2, and 1 rank points for individuals aged 18-34, 35-49, and 50+, respectively, due to the introduction of the policy.

A substantial body of literature has documented a strong link between household resources and crime. Therefore, we next examine the policy's effect on the share of individuals charged with a crime, as shown in Figure 5. Given that crime rates vary significantly by age, Figure 5 presents the estimated effects on the overall share of offenders with a crime charge and broken by age groups (18-24, 25-34, 35-49, and 50+). The figure reveals that, on average, the share of individuals in TOR units charged with a crime committed in 2001 dropped by approximately 3-4 percentage points, representing more than a 50% reduction compared to pre-reform levels. The largest crime reductions occurred among 18-24-year-olds in TOR units, where the share of individuals charged with a crime committed in 2001 fell by 8 percentage points, or 60% relative to pre-policy levels. These reductions are primarily driven by a decline in economically motivated crimes, such as property and drug-related offenses (see Figure 5B), consistent with existing literature that highlights a strong relationship between economic conditions and income-generating crime (e.g., Yang, 2017; Dustmann, Landersø, and Andersen, 2024).

Appendix Table A.1 breaks down the crime effects into those arising through original residents of the TOR housing units and those arising through changes in the composition of residents, specifically for individuals aged 18-34. Unlike the earnings effects (see Table 2), a substantial portion of the crime reductions was driven by changes in the criminal behavior of those who remained in the TOR units ("stayers") mirroring the findings from Disney et al. (2023), who documents a link between home ownership and criminal behavior. However, while crime reductions were primarily driven by behavioral responses of stayers in the years immediately following the policy's implementation, the effects of changes in resident composition emerged more gradually. By 2001, approximately 20% of

the overall crime reduction among residents in TOR units could be attributed to stayers, whereas changes in the composition of residents accounted for the remaining 80%.

5.2 Indirect Effects on Neighborhoods

We next examine the impact of the TOR policy on non-TOR housing units located in the same neighborhoods as TOR units. Changes in the composition of residents in TOR units could influence the attractiveness of these neighborhoods to other residents.

Figure 6 presents estimates of the TOR policy's effect on the earnings rank of residents in non-TOR units within neighborhoods containing TOR units. We differentiate between neighborhoods with a high concentration of TOR units (more than 20%) and those with a low concentration (20% or fewer). The control group comprises housing units in Aarhus, Odense, and Aalborg located in neighborhoods with public rental housing units.[23] Figure 6 shows no evidence of differential trends between the earnings rank of residents in non-TOR units within TOR neighborhoods and their controls in the period leading up to the TOR policy. After the policy's implementation, the average earnings rank of residents in non-TOR units in TOR neighborhoods increased relative to their counterfactual. However, these increases occurred later and were smaller in magnitude compared to the direct effects on TOR units (see Figure 3). As expected, the spillover effects were more pronounced in neighborhoods with a higher concentration of TOR units. By 2001, the average earnings rank of residents in non-TOR units had risen by 2.5 percentiles in neighborhoods with a high concentration of TOR units and by about one percentile in neighborhoods with a low concentration of TOR units. In addition, Figure A.8 shows that earnings ranks increased in high-concentration TOR

[23] Figure A.7 shows the distribution of TOR units across neighborhoods in Copenhagen that have at least one TOR housing unit. On average, TOR units make up 25% of these neighborhoods, with the median neighborhood having 11% TOR units. For the control group, we also differentiate between neighborhoods where public housing constitutes no more than 20% of the housing and those where public housing exceeds 20% of the total housing stock.

neighborhoods for families both with and without children (we return to the effects on children in Section 6.3).

Figure 7A shows that the TOR policy also led to an increase in the share of employed residents and a decrease in the share of inactive residents (neither employed nor studying) in non-TOR units located in neighborhoods with a high concentration of TOR units. Figure 7B indicates that these housing units also experienced an increase in the share of residents aged 18–34. Finally, Figure 7C documents a reduction in crime among residents in TOR neighborhoods for crime, which is driven by fewer property and drug-related crime charges. Thus, the impact of the TOR policy extended beyond TOR units to other housing units in the same areas.

These results suggest that the TOR policy made TOR neighborhoods more attractive, particularly to younger and more affluent individuals. This increased demand is also reflected in housing prices. Using sales price data, Appendix Figure A.9 shows that housing prices in TOR neighborhoods gradually increased following the policy—by 2001, they had risen by more than 10% relative to the counterfactual. This is in line with findings by Hausman et al. (2022).

6 City-wide effects

The TOR policy resulted in an influx of relatively affluent individuals into TOR housing units and other housing units in TOR neighborhoods. However, the policy's impact likely extended beyond these areas. Some individuals who would have resided in TOR neighborhoods in the absence of the policy now live elsewhere, while others who moved into TOR neighborhoods would have chosen different locations without the influence of the TOR policy.[24] These city-wide spillovers are central

[24] There is no evidence of any changes in overall mobility following the introduction of TOR. Figures A.2A and B show the number and fraction of housing units in Copenhagen with new residents from one year to the next, from 1991 to 2005. Figures A.2C and D test for differences in new residents for TOR and non-TOR neighborhoods against their synthetic control counterparts.

to concerns about gentrification (e.g., Diamond and McQuade, 2019; Boustan et al., 2023), yet they have not been explicitly identified in previous studies. To address this, we investigate the TOR policy's impact on neighborhood inequality by estimating the average effect of the policy on housing units in each neighborhood across Greater Copenhagen, using synthetic control neighborhoods as described in Section 4.1.3.[25]

6.1 Effects of the TOR Policy on All Copenhagen Neighborhoods

We use synthetic control neighborhoods to estimate the effect of the TOR policy on the average characteristics of residents in housing units within each of the approximately 2,000 neighborhoods in Greater Copenhagen. These neighborhood estimates $\hat{\delta}_{j,t}$, are unbiased but noisy measures of the true underlying neighborhood specific effect of the TOR policy $\delta_{j,t}$, which are the actual parameters of interest. We, therefore, employ an empirical Bayes shrinkage strategy through the following hierarchical model:

$$\hat{\delta}_{j,t}|\delta_{j,t} \sim N\left(\delta_{j,t}, s_{j,t}^2\right) \tag{2}$$

$$\delta_{j,t} \sim N\left(\mu_{\delta,t}, \sigma_{\delta,t}^2\right),$$

where $s_{j,t}^2$ is the sampling variance in period t of the estimator $\hat{\delta}_{j,t}$ and where the hyperparameters $\mu_{\delta,t}$ and $\sigma_{\delta,t}^2$ characterize the distribution of the distribution of $\delta_{j,t}$ after accounting for the estimation noise in $\hat{\delta}_{j,t}$ (Walters, 2024, provide a description of the application of empirical Bayes methods). We estimate the hyperparameters (that describe the prior distribution of $\delta_{j,t}$) by applying maximum likelihood to the model specified in (2) separately for neighborhoods with a low concentration of

[25] Appendix Figure A.10 replicates the results from Sections 4.1 and 4.2 using synthetic control groups. Appendix Figure A.10A shows the average effect of the TOR policy on TOR units, while Appendix Figure A.10B shows the effect on non-TOR units in TOR neighborhoods. Estimates are similar to those obtained using a control group consisting of public rental units in Aarhus, Odense and Aalborg.

TOR units, neighborhoods with a high concentration of TOR units and neighborhoods with no TOR units.[26] Furthermore, these hierarchical models can be used to construct empirical Bayes posterior means of underlying neighborhood effects $\delta_{j,t}$ as:

$$\delta_{j,t}^{*} = \left(\frac{\hat{\sigma}_{\delta,t}^{2}}{\hat{\sigma}_{\delta,t}^{2} + \hat{s}_{j,t}^{2}}\right)\hat{\delta}_{j,t} + \left(\frac{\hat{s}_{\delta,t}^{2}}{\hat{\sigma}_{\delta,t}^{2} + \hat{s}_{j,t}^{2}}\right)\hat{\mu}_{\delta,t}, \tag{3}$$

where $\hat{\sigma}_{\delta,t}^{2}$, $\hat{s}_{\delta,t}^{2}$ and $\hat{\mu}_{\delta,t}$ are estimates of $\sigma_{\delta,t}^{2}$, $s_{\delta,t}^{2}$ and $\mu_{\delta,t}$, respectively.

Figure 8 displays the estimated prior distributions of the neighborhood estimates ($\delta_{j,t}$) for 1993 (Figure 8A), 1998 (Figure 8B), and 2001 (Figure 8C), using residents' earnings rank as the dependent variable. In Figure 8, an estimate of 0.02 indicates that the TOR policy let to a two-rank point increase in average earnings rank of residents in a given treatment neighborhood.[27]

As discussed in Section 4.1.3, the common trend assumption now applies to each neighborhood individually, not just to treatment and control neighborhoods on average. The estimates for the pre-policy years serve as a test of this assumption for each neighborhood and its synthetic control and estimates significantly different from zero indicate potential violations of the common trend assumption. Figure 8A shows that estimates for 1993 (i.e., pre-policy) are closely centered around zero, supporting the validity of the assumption.

---

[26] The hierarchical model in (2) leads to the following integrated likelihood function that conditions only on hyperparameters: $\mathcal{L}^{I}\left(\hat{\delta}_{j,t}\middle|\mu_{\delta,t},\sigma_{\delta,t}^{2},s_{j,t}^{2}\right) = \frac{1}{\sqrt{2\pi\left(\sigma_{\delta,t}^{2}+s_{j,t}^{2}\right)}}\exp\left(-\frac{1}{2\left(\sigma_{\delta,t}^{2}+s_{j,t}^{2}\right)}\left(\hat{\delta}_{j,t}-\mu_{\delta,t}\right)^{2}\right)$. Empirical Bayes estimates of the hyperparameters are then given by: $\left(\hat{\mu}_{\delta,t},\hat{\sigma}_{\delta,t}^{2}\right) = \arg\max_{\mu_{\delta,t},\sigma_{\delta,t}^{2}} \sum_{j}\log\mathcal{L}^{I}\left(\hat{\delta}_{j,t}\middle|\mu_{\delta,t},\sigma_{\delta,t}^{2},\hat{s}_{j,t}^{2}\right)$, where $\hat{s}_{j,t}^{2}$ estimates $s_{j,t}^{2}$.

[27] Figure A.11 presents the distribution of all 2,000 neighborhood-specific estimates obtained from the synthetic control strategy (black markers), the posterior estimates obtained from Equation 3 for neighborhoods with no TOR units (gray markers), the posterior estimates for neighborhoods with a low concentration of TOR units (blue markers), and the posterior estimates for neighborhoods with a high concentration of TOR units (red markers) for 1993, 1998, and 2001.

Figures 8B and 8C show the distribution of the impact of the TOR policy on Copenhagen neighborhoods in 1998 and 2001, respectively. There are two key takeaways:

First, the policy had heterogeneous effects across neighborhoods. Some neighborhoods, particularly those with a high concentration of TOR units (red lines), experienced an increase in the average earnings rank of residents in the years following the reform, while other neighborhoods saw a decrease. Specifically, while the average effect of the policy is 2.3 rank points for high concentration TOR neighborhoods in 2001, it is just 0.5 rank points for low concentrations TOR neighborhoods and very close to zero for non-TOR neighborhoods. Section 6.2 explores the characteristics of neighborhoods with positive and negative outcomes.

Second, comparing Figures 8B and 8C shows that the variability in estimates has increased by time since the reform. For example, the standard deviation of the estimated effects of the TOR policy on the average earnings rank of residents in high concentration TOR neighborhoods was 2.2 points in 1998 but increased to 3.1 rank points by 2001. Similar increases can be seen for low concentration TOR neighborhoods (increase from 0.5 to 1.5 rank points) and non-TOR neighborhoods (increase from 2.8 to 3.4 rank points). Section 6.3 discusses how these differences have affected neighborhood inequality.

While Figure 8 documents clear trends in neighborhood-specific estimates across time and by neighborhood type, a key concern is whether these patterns reflect statistically significant effects (Mogstad et al., 2024). Table 3 shows the fraction of neighborhood-specific estimates that are significantly different from zero at a five percent level for 1993, 1998, and 2001. Panel A presents the tests for all estimates, Panel B for negative estimates, and Panel C for positive estimates, each broken down by neighborhood category (high-concentration TOR neighborhoods, low-concentration TOR neighborhoods, and non-TOR neighborhoods). The share of significant estimates gradually increased,

reaching over six percent by 2001. The highest share of statistically significant estimates is found in high-concentration TOR neighborhoods—five percent in 1998 and eight percent in 2001. However, around six percent of estimates in non-TOR and low-concentration TOR neighborhoods were also significantly different from zero, suggesting that the policy eventually impacted all types of neighborhoods. Panels B and C indicate that almost all statistically significant estimates in high-concentration TOR neighborhoods are positive, whereas significant estimates are more evenly spread between positive and negative in non-TOR and low-concentration TOR neighborhoods. Thus, while many neighborhood-specific estimates are statistically indistinguishable from zero, the statistically significant estimates follow the same patterns presented in Figure 8.

The TOR policy induced significant changes to the composition of residents in many neighborhoods, which unfolded gradually in the years following its introduction. However, it remains unclear whether the neighborhood-specific estimates reflect persistent changes in the trajectories of certain neighborhoods or if the effects gradually spread throughout the city, impacting some neighborhoods earlier and others later.[28,29] Figures 9A address this by showing the correlation between the estimated effects of the TOR policy on neighborhoods in Greater Copenhagen across 1998 and 2001. The figure reveals a strong correlation between post-policy estimates over time, with some neighborhoods experiencing a steady increase in the average earnings rank of residents while others see a gradual

[28] Mast (2023) and Bratu et al. (2024) show how the supply of new housing trickles down through different neighborhoods in "moving chains" where the moving behavior between some neighborhoods change thereby affecting moving behavior between other neighborhoods and so forth.

[29] To assess whether the policy effects on housing units and neighborhoods in Copenhagen are within a range of what could reasonably be expected from a policy shock to around six percent of the housing mass in a city, Appendix E presents a simple model of how the housing shock gradually expands to other housing units through spillover effects. Using the relative size of the TOR units in the total housing stock in Copenhagen and the average annual moving rate of 10 percent (see Figure A.2), the simulation results based on the model suggest that around 12 percent of all housing units are affected by the a policy shock within six years (corresponding to year 2001) with spillover effects to housing units that were not directly targeted by the shock accounting for around 75 percent of the effects.

decline. This suggests that the neighborhood-specific estimates reflect persistent changes in neighborhood trajectories rather than a ripple effect that spreads gradually between different areas.

We also examine the effects of the TOR policy on employment rates and the share of inactive individuals (i.e., those neither employed nor studying). Figures 9B and 9C show that these outcomes mirror the effects on earnings ranks.[30] Focusing on neighborhood-specific estimates in 2001, Figures 9B and 9C demonstrate that the correlations between the effects of the TOR policy on residents' earnings ranks and its effects on the share of residents in employment or inactivity are greater than 0.5. This indicates that the movement patterns of high- and low-resource individuals within Copenhagen, in response to the TOR policy, were consistent across different individual outcomes. These findings raise the question of what factors distinguished the neighborhoods that benefited from the policy from those that were adversely affected.

6.2 Neighborhoods Benefitting and Neighborhoods Losing

To investigate what characterizes neighborhoods that benefitted from the TOR policy and neighborhoods that appear to have lost from the TOR policy in terms of residents' earnings, we regress synthetic control estimates of the neighborhood-specific effect of the TOR policy ($\hat{\delta}_{j,t}$ in the previous subsection) on baseline characteristics of the neighborhood (X_j) measured in 1994 (i.e., before the policy):

$$\hat{\delta}_{j,t} = a + bX_j + e_{j,t}, \tag{4}$$

where an estimate of $b > 0$ indicates that neighborhood characteristic X_j is positively associated with the policy effects and vice versa.

[30]Figure A.12 illustrates the total variation in effects by plotting the prior distribution estimated using the hierarchical structure in (2) for the two outcomes in question.

Figure 10 presents estimates of b for all neighborhoods, non-TOR, low-concentration TOR, and high-concentration TOR neighborhoods separately. Panel A of Figure 10 shows results when X_j is a dummy indicating whether a neighborhood was among the 25% of neighborhoods in 1994 with the lowest average earnings rank of residents. Panel B shows results when X_j is a dummy indicating whether a neighborhood was among the 25% of neighborhoods with the lowest employment rate among residents, and Panel C when X_j is a dummy indicating whether a neighborhood was among the 25% of neighborhoods with the highest share of residents who are neither employed nor studying.

The figure shows that among high-concentration TOR neighborhoods, the estimates of b are positive, indicating that the positive policy effects were concentrated in the most disadvantaged neighborhoods (measured by pre-policy characteristics). In contrast, among non-TOR neighborhoods, the estimates of b are negative, suggesting that being a disadvantaged non-TOR neighborhood is negatively associated with experiencing a positive policy effect. Finally, in low-concentration TOR neighborhoods, the relationship between policy effects and pre-policy neighborhood characteristics falls somewhere between those observed for the other two neighborhood types.

The fact that the TOR policy did not impact neighborhood compositions through changes to the moving behavior of the original TOR residents or outflow from TOR neighborhoods leaves two potential contributors to the effects of the TOR policy, $\hat{\delta}_{j,t}$, for non-TOR neighborhoods: A) changes to the residential composition in non-TOR neighborhoods because they receive residents who would have moved to TOR neighborhoods had it not been for the policy, and B) changes to the residential composition in non-TOR neighborhoods because those who move to TOR neighborhoods would have lived in these neighborhoods in the absence of the policy.

To approximate these two counterfactual moving patterns, we consider changes to moving patterns from each neighborhood to high-concentration TOR neighborhoods in the pre- and post-policy

period.[31] Table A.2 presents the association between the proxies of the degree to which a non-TOR neighborhood is a type A or type B contributor (see previous paragraph) and the estimated neighborhood-specific effect of the TOR policy in the neighborhood in 2001 ($\hat{\delta}_{j,2001}$). The table shows that the TOR-policy effects are negatively associated with being a type A contributor and positively associated with being a type B contributor. This suggests that the "losing" non-TOR neighborhoods are those who receive the individuals who would have moved to TOR neighborhoods had the policy not been implemented, while the "winning" non-TOR neighborhoods are the areas where those who move to TOR-neighborhoods due to the policy would have moved in the absence of the policy.

6.3 Children's Exposure to Neighborhood Effects

As documented in the existing literature (see e.g., Chetty et al., 2016), neighborhood of residence during childhood can have important impacts on various later life outcomes. Therefore, the effects of the TOR policy on neighborhood composition and neighborhood quality may also have impacted the children growing up in affected neighborhoods. We investigate this next by investigating the association between the outcomes of children and their exposure to neighborhoods that were impacted by the TOR policy between 1995 and 2001.

---

[31] Specifically, we separate the data in a pre-policy period (1991-1994) and post policy period (1998-2001), and estimate the share of residents moving from each neighborhood j to high concentration TOR neighborhoods in the pre-period ($send_j^{pre}$), and the post-period ($send_j^{post}$). We approximate type A contributors by estimating the neighborhood specific propensity to receive residents in the post-policy period, from neighborhoods that were sending residents to high-concentration TOR neighborhoods in the pre-policy period, $E\left(send_j^{pre} | period = post\right)$. We approximate type B contributor by estimating the propensity to receive residents in the pre-policy period, from neighborhoods that send residents to high-concentration TOR neighborhoods in the post-policy period, $E\left(send_j^{post} | period = pre\right)$. To ease the interpretation of the estimates in Table A.3, the two variables $E\left(send_j^{pre} | period = post\right)$, $E\left(send_j^{post} | period = pre\right)$ are transformed to ranks (defined among all neighborhoods in the Greater Copenhagen area).

Because neighborhood effects on children increase with the duration of exposure to neighborhoods (Chetty & Hendren, 2018), and because the effects of the TOR policy on neighborhood residential composition do not stabilize until the end of the observation period, these estimates will likely constitute a lower bound of the total longer run effects of the TOR policy on children in the Greater Copenhagen area via, for example, changed peer groups in schools.

We focus on children born in 1986-1990 (i.e. those in school-age during the post-policy years 1995-2001) and the association between child outcomes y_i^c and the estimated empirical Bayes posterior TOR policy effects, $\delta_{j,t}^*$, (to obviate attenuation bias from using the noisy neighborhood estimates $\hat{\delta}_{j,t}$ as regressors) for residents' earnings rank in the neighborhoods where the children lived between years *t*: 1995-2001.

$$y_i^c = \alpha + \gamma \delta_{j,t}^* + X_{i,t}^{1992\prime} \beta_t + e_i \qquad (5)$$

Here, $X_{i,t}^{1992}$ are the pre-policy characteristics (measured in 1992) of residents in the neighborhoods that the child has lived in from 1995 to 2001, which we include to control for the overall association between child outcomes and the characteristics of the child's neighborhood of residence. We consider three child outcomes: 9th grade GPA, high school completion, and number of charges for crime committed between age 15-19.

Table 4 presents estimates of γ from Eq. 4, which captures the association between exposure to the TOR policy effects during childhood and later outcomes. We scale $\delta_{j,t}^*$ such that the estimate of γ corresponds to the effects of a 5 percentile point increase in the average earnings rank of residents from 1995 to 2001 (approximately the earnings rank increase in the neighborhoods with the largest effects, see Figure A.11).

Column (1) of Table 4 shows a strong association between exposure to policy effects and children's 9th grade GPA. Exposure to neighborhoods with 5 percentile points higher earnings rank during childhood is associated with almost 8 percent of a standard deviation higher GPA. Exposure to the policy effects is also significantly associated with high school completion as shown in column (6). Exposure to neighborhoods with 5 percentile points higher earnings rank is associated with 4.5 percentage points higher probability of completing high school. For crime as shown in column (11), estimates in the main specification are noisy and not significantly different from zero.

To assess whether the exposure effects are driven by the families who move between neighborhoods or those remaining in the same neighborhood, columns (2), (7), and (12) show the estimated exposure effects when we assign children $\delta^*_{j^{94},t}$ according to the neighborhood where they lived in 1994: j^{94}. These estimates follow the same pattern as the full exposure effects and are of similar magnitude suggesting that exposure effects are driven by children who remain in the same neighborhood throughout the period.[32]

Columns (3), (8), and (13) test for differences in exposure effect across neighborhood affluence by interacting $\delta^*_{j^{94},t}$ with an indicator for whether the neighborhoods' pre-policy average earnings rank was in the lowest 25% of neighborhoods in Greater Copenhagen. The columns show that exposure effects are larger for children from these low-resource neighborhoods. While the point estimate on GPA is insignificant, exposure effects on the two other outcomes are both significant and sizable: Exposure effects on high school completion is four times larger than in the remaining areas (0.077 +

[32] Table A.3 presents the estimated the effects of exposure to the policy separately for those who remain in the same neighborhood from 1994 (pre-policy) to 2001 and those who move between neighborhoods. Estimates are largest for stayers for GPA and high school completion and for movers for crime charges. However, as the decision to move during childhood is non-random (Table A.3 indicates that children who move are negatively selected as they have lower GPA and high school completion, and higher crime rates compared to children who do not move during school age), the differences in policy exposure estimates between stayers and movers should be interpreted with caution.

0.025 vs. 0.025) and effects on crime charges are solely driven by exposure effects in low-resource areas.

While it is difficult to compare estimates across different settings and treatments, we may assess the magnitude of the results from Table 4 in the light of the findings from the MTO experiment in Chetty et al. (2016). The closest comparison of outcomes are high school completion and college attendance, respectively. The exposure estimates suggest that children from low-resource neighborhoods experienced an increased likelihood of completing high school of 10 percentage points (0.025 + 0.077), which corresponds to 19 percent relative to the mean. In comparison, the MTO experiment increased college attendance by roughly 5 percentage points, which corresponds to 32 percent relative to the control mean (Chetty et al., 2016, pp. 877).

The remaining columns in Table 4 test for heterogeneity in the exposure effects across children's characteristics. Columns (4), (9), and (14) test for difference across age at exposure to the policy effects. While there are no differences for 9^{th} grade GPA, effects on high school completion are larger for older children (i.e., teenagers close to high school age). Similarly, effects on crime are larger for older children underscoring the sensitive age-range when it comes to the initiation of a criminal trajectory through peer exposure as, among others, shown in e.g., Damm and Dustmann (2014) and Dustmann and Landersø (2022). Finally, columns (5), (10), and (15) show that exposure effects are mainly driven by children whose parents have not completed high school, which points to the potential of neighborhood quality to either mitigate or exacerbate inequality in children's outcomes across parental background.

6.4 Inequality between Neighborhoods

The results from the Section 6.2 suggest that some neighborhoods, particularly disadvantaged high-concentration TOR areas, experienced positive policy effects, whereas disadvantaged non-TOR

neighborhoods saw negative impacts. Thus, it is not clear if the policy decreased or increased overall inequality between neighborhoods in Copenhagen – a question that is key to concerns relating to gentrification in many cities.

To investigate this concern, we use the observed average earnings ranks, share employed, and share active for each neighborhood in Greater Copenhagen and the corresponding empirical Bayes posterior TOR policy effects, $\delta_{j,t}^{*}$, to obtain the average outcomes in neighborhoods, with and without the policy, at the 10th, 50th, and 90th percentile in non-TOR, low-concentration TOR, and high-concentration TOR neighborhoods, on the one hand, and all neighborhoods in Greater Copenhagen, on the other hand.[33]

Figure 11 illustrates the percentage change in the P90/P10, P50/P10, and P90/P50 ratios resulting from the TOR policy in 2001 for the three outcomes in question in 2001.[34] Bars labelled "All" indicate the overall change in these ratios within Greater Copenhagen. Bars labeled "Non-TOR" indicate how the P10 or P50 in non-TOR neighborhoods changed relative to the P90 or P50 within Greater Copenhagen, bars labeled "TOR low" indicate how the P10 or P50 in low concentration TOR neighborhoods changed relative to the P90 or P50 within Greater Copenhagen and so forth.[35]

A negative estimate for the P90/P10 ratio indicates a reduction in the gap between disadvantaged and affluent neighborhoods. Similarly, a negative estimate for the P90/P50 ratio suggests a decrease in

[33] We focus on inequality measured as the average difference between neighborhoods, rather than total inequality, which includes both within- and between-neighborhood variation.
[34] Figure A.13 shows the policy effects on the P90/P10 ratios in the different neighborhood types from 1995-2001.
[35] Average earnings ranks at the 10th, 50th, and 90th percentile within each of the three neighborhood types c (non-TOR, low concentration TOR, and high concentration TOR), $P10_C^d$, $P50_C^d$, $P90_C^d$, and the average earnings rank at the 50th and 90th percentile in the full city of Copenhagen, $P50_A^d$, $P90_A^d$ in the counterfactual states with $(d = 1)$ and without $(d = 0)$ the TOR policy are based on the empirical Bayes posterior estimates presented in Section 6.1. We then estimate the ratios $\frac{P90_A^d}{P10_C^d}$; $\frac{P50_A^d}{P1\ \frac{d}{c}}$; $\frac{P90_A^d}{P50_C^d}$ for each neighborhood type and treatment state d. Finally, we compare the relative difference between each of these inequality measures between the control and treatment states, for example $\left(\frac{P9\ \frac{1}{A}}{P1\ \frac{1}{c}} \Big/ \frac{P90_A^0}{P10_C^0} - 1\right) \times 100$.

the gap between median and affluent neighborhoods. Finally, a negative estimate for the P50/P10 ratio signifies a reduction in the gap between disadvantaged and median neighborhoods, and vice versa.

Focusing first on the high concentration TOR neighborhoods, Figure 11 shows that the P90/P10 ratio—representing the gap between disadvantaged high-concentration TOR neighborhoods and affluent neighborhoods in Copenhagen—decreased by 7-14%, depending on outcome, due to the policy. Figure 11 also shows that this decrease is mostly due to a decrease in the P50/P10 ratio, which decreased by 7-14% for high-concentration TOR neighborhoods. This suggests that, based on the average earnings ranks of residents, the share employed, and the share who either have a job or study, the TOR policy significantly reduced disparities between the lower end of the distribution among high-concentration TOR neighborhoods and the middle of the distribution across all neighborhoods in Copenhagen.

In contrast, for non-TOR neighborhoods, the P90/P10 and the P50/P10 ratios have increased due to the reform. Thus, the disadvantaged non-TOR neighborhoods have fallen behind compared to the middle and upper tiers of the overall neighborhood distribution. In low-concentration TOR neighborhoods, inequality did not change substantially due to the policy. Finally, focusing on inequality among all neighborhoods in Greater Copenhagen, inequality has increased by 1% measured by earnings ranks, 2% measured by the share employed, and 4% measured by the share who either have a job or study with most changes being driven by an increase in the P50/P10 ratio (see bars labeled "All").[36]

[36] To get a sense of how many individuals that would have to live in a different neighborhood relative to the counterfactual state without the TOR policy to generate the effects from Fig. 11, Fig. A.14 shows the percent change in the P90/P10 ratio if a given fraction (from 1% to 100%) of residents from the poorest 10% of neighborhoods and the 10% most affluent neighborhoods switched residence. The figure shows that the estimated change in the p90/p10 ratio at city-level corresponds to around 6-7 percent of residents living in a different area than they otherwise would have without the policy.

In summary, Figure 11 illustrates how policies targeting specific neighborhoods can have contrasting effects on the target group and the broader city. Altering the composition of residents in some low-resource neighborhoods does not address inequality issues if these initiatives result in the relocation of disadvantaged groups to other low-resource areas. In fact, such policies may even worsen segregation.

7 Conclusion

This paper examines how a housing policy that converted around 6% of Copenhagen's housing stock from public to private housing affects the residential composition within the transformed units, the surrounding neighborhoods, and the entire city.

The policy led to a substantial change in the composition of residents in the target housing units where the average earnings rank of residents increased, while crime rates and the share of adult residents without a job or not enrolled in education dropped. These changes, in turn, enhanced the attractiveness of the immediate neighborhoods. For instance, real estate prices increased by approximately 10% within five years following the policy, suggesting that it successfully improved a set of disadvantaged neighborhoods. The policy did not directly affect the earnings or asset accumulation residents who lived in the affected units prior to the implementation, whether they stayed in the unit (and joined co-op ownership) or moved away except for crime rates.

Neighborhoods with a high proportion of converted housing units attracted younger, more affluent residents than those without the policy. However, neighborhoods not directly targeted by the policy but already among the most disadvantaged in the metropolitan area at the onset of the policy received a larger share of disadvantaged individuals than they would have otherwise. The policy thereby influenced moving patterns in other neighborhoods by altering the composition of residents throughout the city. We show that exposure to policy effects on neighborhood composition affected

children's GPA, high school completion, and crime rates – particularly for children from low educated parents.

Overall, the policy affected many areas and residents – some in good and others in bad. As the earnings of residents in many low resource neighborhoods fell further behind the rest of the city, the policy led to an overall increase in inequality between low-resource and high-resource neighborhoods. Our study thus reveals a previously overlooked aspect of housing policies. While fostering positive changes and enhanced opportunities in the targeted neighborhoods—outcomes that policymakers may seek to replicate—the policy also exacerbated challenges in other disadvantaged neighborhoods.

References

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program. Journal of the American Statistical Association, 105(490), 493-505.

Abadie, A., and Javier Gardeazabal. (2003). The economic costs of conflict: A case study of the Basque Country. American Economic Review, 93(1), 113-132.

Alves, Guillermo, William H. Burton, and Sebastián Fleitas. (2023). Difference-in-Differences in Equilibrium: Evidence from Placed-Based Policies. Distrito Capital: CAF- banco de desarrollo de América Latina y el Caribe.

Autor, David H., Christopher J. Palmer, and Parag A. Pathak. (2014). Housing Market Spillovers: Evidence from the End of Rent Control in Cambridge, Massachusetts. Journal of Political Economy 122(3): 661–717.

Asquith, Brian J., Evan Mast, and David Reed. (2023). Local effects of large new apartment buildings in low-income areas. Review of Economics and Statistics 105(2): 359-375.

Baum-Snow, Nathaniel, and Byron F. Lutz. (2011). School Desegregation, School Choice, and Changes in Residential Location Patterns by Race. American Economic Review 101(7): 3019-3046.

Bergman, Peter, Raj Chetty, Stefanie DeLuca, Nathaniel Hendren, Lawrence F. Katz, and Christopher Palmer. (2024). Creating moves to opportunity: Experimental evidence on barriers to neighborhood choice. *American Economic Review*, *114*(5), 1281-1337.

Billings, Stephen B., Eric J. Brunner, and Stephen L. Ross. (2018). Gentrification and Failing Schools: The Unintended Consequences of School Choice under NCLB. Review of Economics and Statistics 100(1): 65-77.

Billings, Stephen B., David J. Deming, and Jonah Rockoff. (2014). School Segregation, Educational Attainment, and Crime: Evidence from the End of Busing in Charlotte-Mecklenburg. Quarterly Journal of Economics 129(1): 435-76

Bjerre-Nielsen, Andreas, and Mikkel Gandil. (2024). Attendance Boundary Policies and the Limits to Combating School Segregation. American Economic Journal: Economic Policy 106(1): 190-227.

Blanco, Hector, and Lorenzo Neri. (2023). Knocking It Down and Mixing It Up: The Impact of Public Housing Regenerations (No. 15855). IZA Discussion Papers.

Boustan, Leah, Robert A. Margo, Matthew M. Miller, James Reeves, and Justin Steil. (2023). JUE Insight: Condominium development does not lead to gentrification. Journal of Urban Economics, 133, 103524.

Bratu, Christina, Oskari Harjunen, Tuukka Saarimaa. (2024). JUE Insight: City-wide effects of new housing supply: Evidence from moving chains. Journal of Urban Economics 133.

Chetty, Raj, Rebecca Diamond. , Lawrence F. Katz, Sonya Porter, Matthew Staiger, and Laura Tach. "Creating high-opportunity neighborhoods: Evidence from the Hope IV program." Unpublished working paper.

Chetty, Raj, and Nathaniel Hendren. (2018a). The impacts of neighborhoods on intergenerational mobility I: Childhood exposure effects. The Quarterly Journal of Economics, 133(3), 1107-1162.

Chetty, Raj, and Nathaniel Hendren. (2018b). The impacts of neighborhoods on intergenerational mobility II: County-level estimates. The Quarterly Journal of Economics, 133(3), 1163-1228.

Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. American Economic Review, 106(4), 855-902.

Chyn, Eric. (2018). Moved to opportunity: the long-run effects of public housing demolition on children. American Economic Review 108(10): 3028-3056.

Chyn, Eric, and Lawrence F. Katz. (2021). Neighborhoods matter: assessing the evidence for place-based policies. Journal of Economic Perspectives 35(4): 197-222.

COWI. (2000). Virkningerne af salget af Københavns Kommunes beboelsesejendomme solgt gennem Ejendomsselskabet TOR I/S.

Damm, Anna Piil, and Christian Dustmann. (2014). Does growing up in a high-crime neighborhood affect youth criminal behavior? American Economic Review, 104(6), 1806-1832.

Damm, Anna Piil, and Marie-Louise Schultz-Nielsen. (2008). Danish neighbourhoods: construction and relevance for measurement of residential segregation. Nationaløkonomisk Tidsskrift, 2008(1), 241-262.

Deutscher, Nathan. (2020). Place, peers, and the teenage years: long-run neighborhood effects in Australia. American Economic Journal: Applied Economics, 12(2), 220-249.

Diamond Rebecca, Tim McQuade, and Franklin Qian. (2019). The effects of rent control expansion on tenants, landlords, and inequality: Evidence from San Francisco. American Economic Review 109(9): 3365-3394.

Diamond, Rebecca, and Tim McQuade. (2019). Who Wants Affordable Housing in their Backyard? An Equilibrium Analysis of Low-Income Property Development. Journal of Political Economy 127(3): 1063–1117.

Disney, Richard, John Gathergood, Stephen Machin, and Matteo Sandi. (2023). Does homeownership reduce crime? A radical housing reform from the UK. The Economic Journal, 133(655), 2640-2675.

Dustmann, Christian, and Rasmus Landersø. (2021). “Child gender, young fathers’ crime, and spillover effects in criminal behavior." Journal of Political Economy 128(12): 3261-3301.

Dustmann, Christian, Rasmus Landersø, and Lars. H. Andersen, (2024). Refugee benefit cuts. American Economic Journal: Economic Policy, 16(2), 406-441.

Dustmann, Christian, Mikkel Mertz, and Anna Okatenko. (2023). Neighbourhood Gangs, Crime Spillovers and Teenage Motherhood. The Economic Journal, 133(653), 1901-1936.

Eriksen, Michael D., and Stuart S. Rosental. (2010). Crowd Out Effects of Place-Based Subsidized Rental Housing New Evidence from the LIHTC Program. Journal of Public Economics 94(11-12): 953-966.

Favilukis, Jack, Pierre Mabille, and Stijn Van Neiuwerburgh. (2023). Affordable housing and city welfare. Review of Economic Studies 90(1): 293-330.

Freeman, Lance, and Frank Braconi. (2004). Gentrification and Displacement: New York City in the 1990’s. Journal of the American Planning Association 70(1): 39-52.

Friday, Cameron, and Tucker Smith. (2023). Turning around schools (and neighborhoods?): School improvement and gentrification. Economics of Education Review 94(6).

Guerrieri, Veronica, Daniel Hartley, and Erik Hurst. (2012). Within-city variation in urban decline: the case of Detroit. American Economic Review 102(3): 120-166.

Guerrieri, Veronica, Daniel Hartley, and Erik Hurst. (2013). Endogenous gentrification and housing price dynamics. Journal of Public Economics 100(4): 45-60.

Hausman, Naomi, Tamar Ramot-Nyska, and Noam Zussman. (2022). Homeownership, labor supply, and neighborhood quality. American Economic Journal: Economic Policy 14(2): 193-230.

Municipality of Copenhagen. (2001). Redegørelse om Københavns Kommunes gæld og likviditet. https://www.kk.dk/dagsordener-og-referater/%C3%98konomiudvalget/m%C3%B8de-12062001/dagsorden/punkt-5.

Parliament. (1997). Beretning om oprettelsen og driften af Ejendomsselskabet TOR I/S af Rigsrevisionen. https://webarkiv.ft.dk/BAGGRUND/statsrev/1596.htm

Mast, Evan. (2023). JUE Insights: The effect of new market-rate housing construction on the low-income housing market. Journal of Urban Economics 103.

McKinnish, Terra, Randall Walsh, and Kirk White. (2010). Who gentrifies neighborhoods? Journal of Urban Economics 67(2): 183-190.

Mogstad, Magne, Joseph P. Romano, Azeem M. Shaikh, and Daniel Wilhelm. (2024). Inference for ranks with applications to mobility across neighborhoods and academic achievement across countries. The Review of Economic Studies 91(1): 476-518.

Neri, Lorenzo. (2024). Moving opportunities: The impact of mixed-income public housing regenerations on student achievement. Journal of Public Economics, 230, 105053.

Rosental, Stuart S. (2014). Are Private Markets and Filtering a Viable Source of Low-Income Housing? Estimates from a "Repeat Income" Model. American Economic Review 104(2): 687-706.

Sodini, Paolo, Stijn Van Nieuwerburgh, Roine Vestman, and Ulf von Lilienfeld-Toal. (2023). Identifying the benefits from homeownership: A Swedish experiment. American Economic Review, 113(12), 3173-3212.

Staiger, Matthew, Giordano Palloni, and John Voorheis. (2024). “Neighborhood revitalization and residential sorting.” Unpublished working paper.

Statistics Denmark. (2024). www.statistikbanken.dk, Table BUD32X.

Schwartz, Amy Ellen, Ingrid Gould Ellen, Michael H. Schill, and Ioan Voicu. (2006). The external effects of place-based subsidized housing. Journal of Urban Economics 36(6): 679-707.

Tach, Laura, and Allison Dwyer Emory. (2017). Public housing redevelopment, neighborhood change, and the restructuring of urban inequality. American Journal of Sociology 123(3): 386-739.

Vigdor, Jacob L., Douglas S. Massey, and Alice M. Rivlin. (2002). Does gentrification harm the poor? *Brookings-Wharton papers on urban affairs*, 133-182.

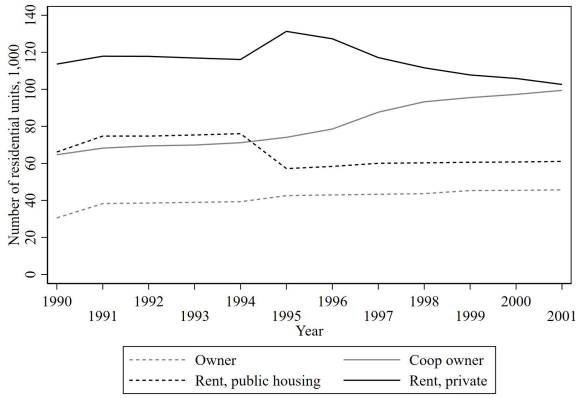
VIVE. (2002). ”Har salget af Københavns kommunes boliger forringet mulighederne for lav-indkomstgrupper? https://www.vive.dk/da/udgivelser/har-salget-af-koebenhavns-kommunes-boliger-forringet-mulighederne-for-lav-indkomstgrupper-qz5881z8/

Yang, Crystal S. (2017). Does public assistance reduce recidivism? *American Economic Review*, *107*(5), 551-555.

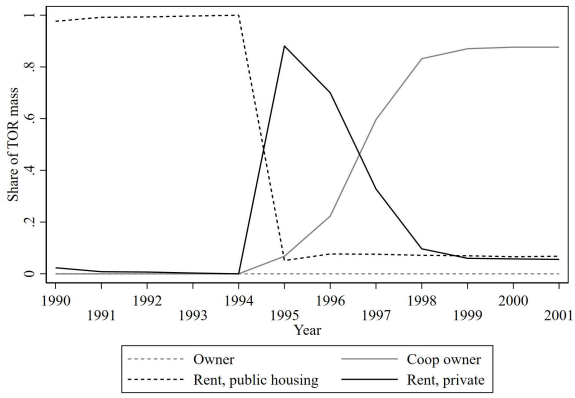
Walters, Christopher. (2024). Empirical Bayes methods in labor economics. *Handbook of Labor Economics*, vol. 5, 183-260, Elsevier.

Figure 1: Housing Composition in Copenhagen and the TOR Policy

(a) Residential units, CPH



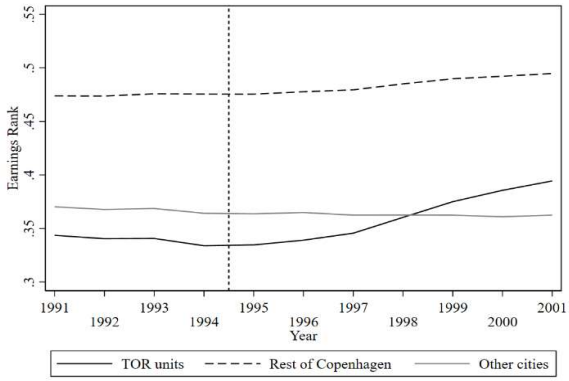
(b) Ownership, TOR units



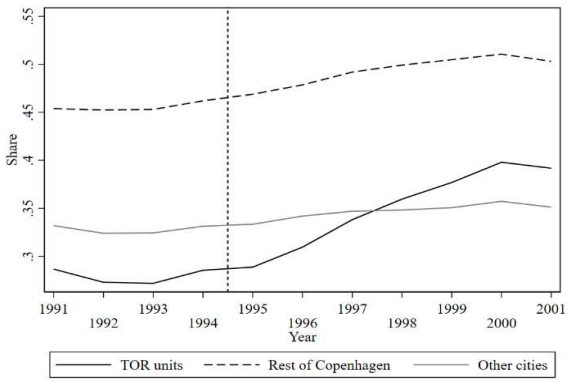
Note: Panel (a) shows the number of residential housing units in Copenhagen by year and housing type. Panel (b) shows the housing types housing units that were classified as TOR units in 1994 in different years surrounding the TOR policy in 1995.

Figure 2: Earnings and Employment of TOR units, the Rest of Copenhagen and Public Rental Units in Other Cities

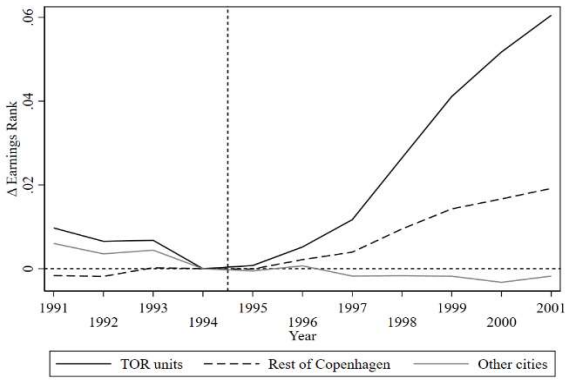
(a) Earnings ranks



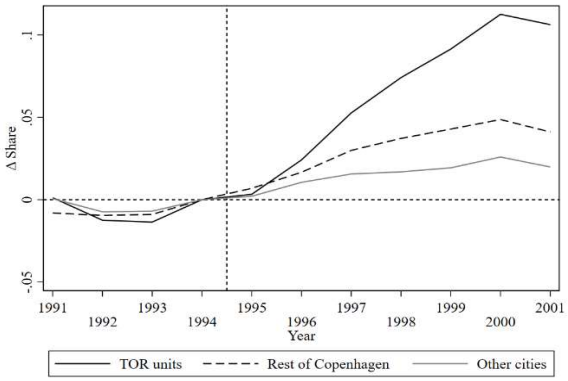
(b) Employment rate



(c) Change in earnings rank rate indexed relative to 1994

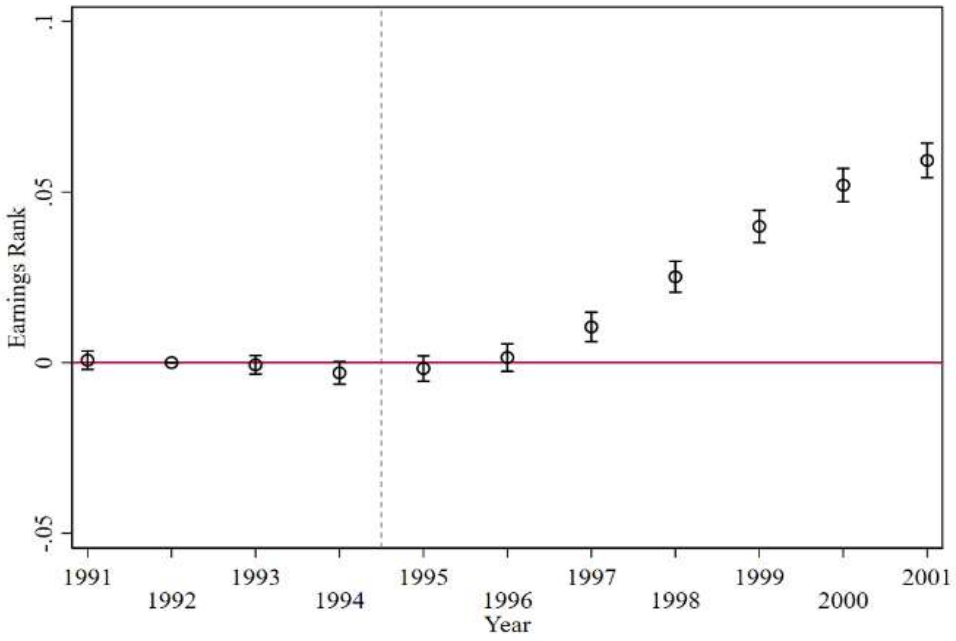


(d) Change in employment rate indexed relative to 1994



Note: Panel (a) shows the average earnings rank of residents in TOR housing units, residents in other housing units in Copenhagen and residents in public rental housing units in Aarhus, Odense and Aalborg in the years surrounding the TOR policy in 1995. Earnings rank refer to earnings ranks in the full population. Panel (b) shows the employment rate among residents in TOR housing units, residents in other housing units in Copenhagen and residents in public rental housing units in Aarhus, Odense and Aalborg in the years surrounding the TOR policy in 1995. Panel (c) shows changes in the average earnings rank of residents indexed relative to 1994 and panel (d) shows changes in in the employment rate of residents indexed relative to 1994.

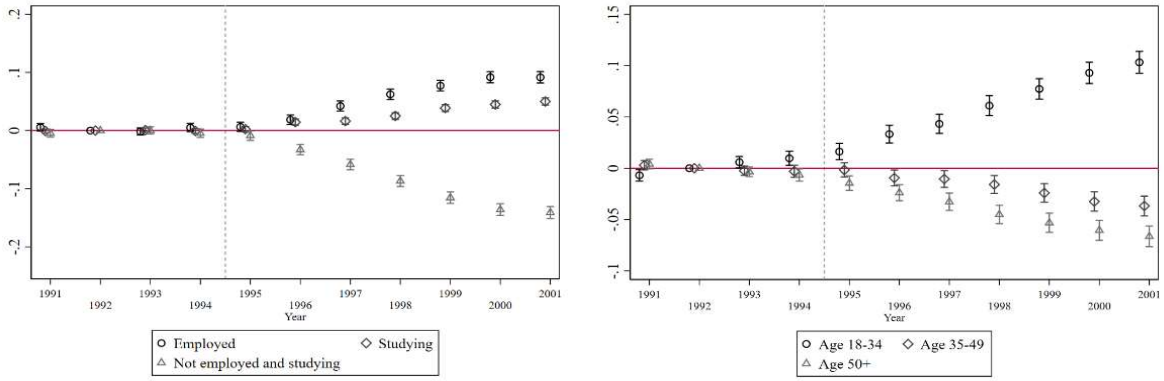
Figure 3: Effects of the TOR Policy on Earnings in TOR Units



Note: The figure shows estimates of the impact of the TOR policy on the earnings rank of residents in TOR housing units. Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure 4: Effects of the TOR Policy on Other Outcomes in TOR Units

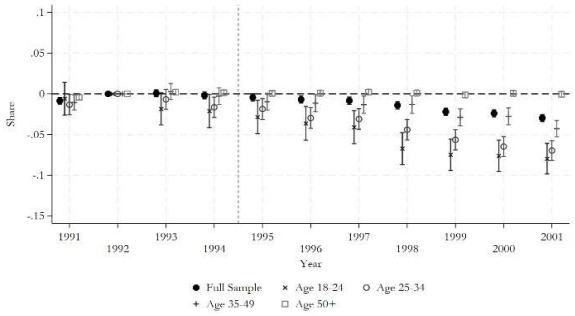
(a) Labor market status (b) Age Composition



Note: The figure shows estimates of the impact of the TOR policy on the labor market status (employed, unemployed, studying or outside the labor force and not studying) of TOR residents in Panel (a) and estimates of the impact of the TOR policy on the age composition of TOR residents in Panel (b). Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure 5: Effects of the TOR Policy on Offenders in TOR Units

(a) All crime, by age

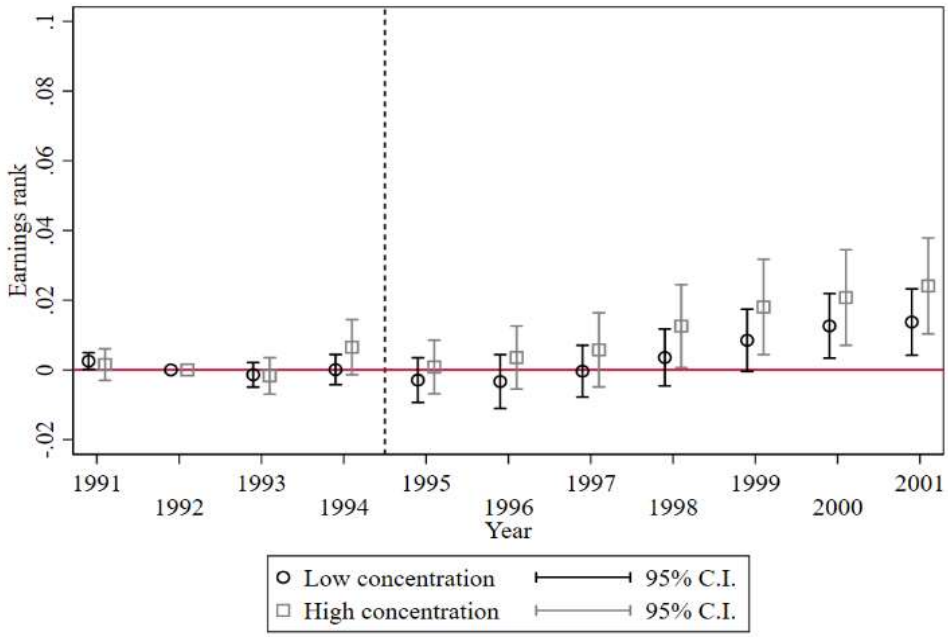


(b) Crime, by crime type



Note: The figure shows estimates of the impact of the TOR policy on the fraction of residents in TOR housing units with a crime charge in a given year. Figure (a) shows the results for all crime (excl. traffic offenses) for the full sample of adults and separately for individuals aged 18-24, 25-34, and 35-49 years. Figure (b) all crime (excl. traffic offenses) and separately for property, violent, and drug-related crime. Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

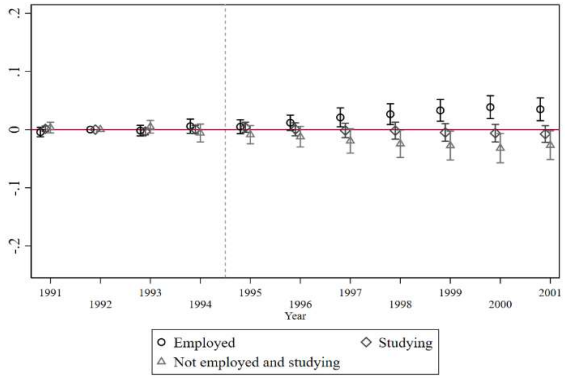
Figure 6: Effects of the TOR Policy on Earnings in TOR Neighborhoods



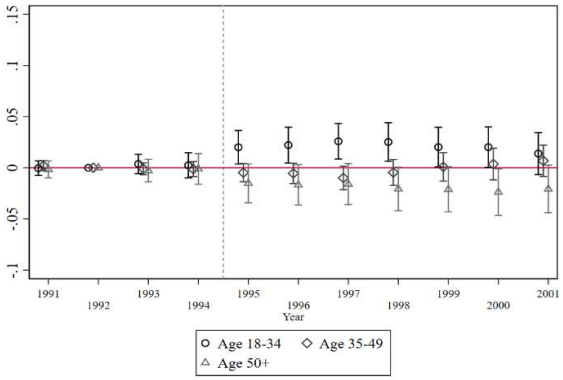
Note: The figure shows estimates of the impact of the TOR policy on the earnings rank of residents in TOR neighborhoods but not in TOR housing units. Estimates are from difference-in-differences specifications (Eq. 1) that compare the outcome trajectory of adults residing in TOR neighborhoods to the outcome trajectory of adults residing in neighborhoods with public rental housing in Aarhus, Odense or Aalborg. We differentiate between neighborhoods with 20% or fewer TOR (public housing) units and neighborhoods with more than 20% TOR (public housing) units. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure 7: Effects of the TOR Policy on Labor Market Outcomes, Age, and Crime of TOR Neighborhoods, Control: Other cities

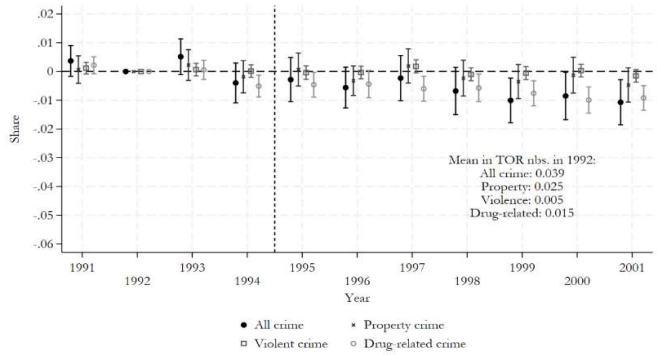
(a) Labor market status



(b) Age



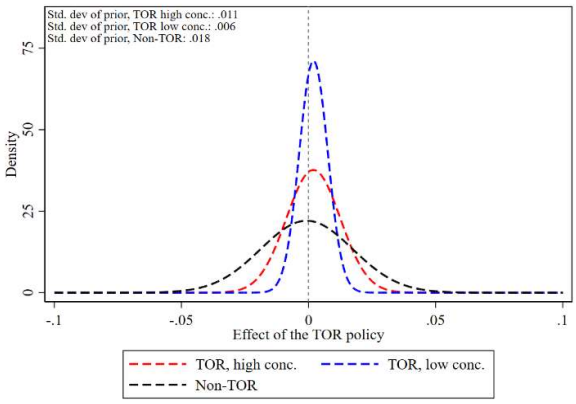
(c) Crime charges



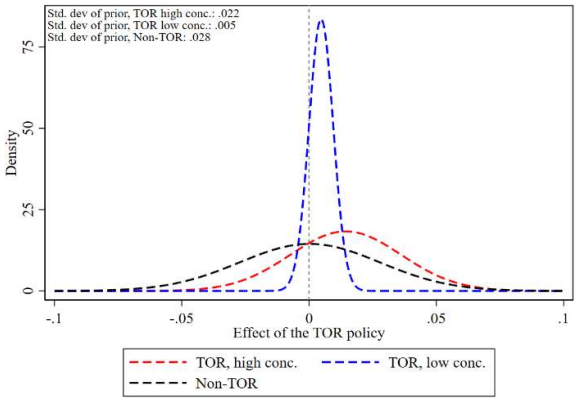
Note: The figure shows estimates of the impact of the TOR policy on the labor market status (employed, unemployed, studying or outside the labor force and not studying) in Panel (a), on the age composition of in Panel (b), and the fraction receiving a crime charge in Panel (c) for residents in TOR neighborhoods with a high concentration of TOR units (at least 20%), but not including TOR housing units. Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectory of adults residing in TOR neighborhoods to the outcome trajectory of adults residing in neighborhoods with public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure 8: Distribution of Effects of the TOR Policy on Neighborhoods in Copenhagen

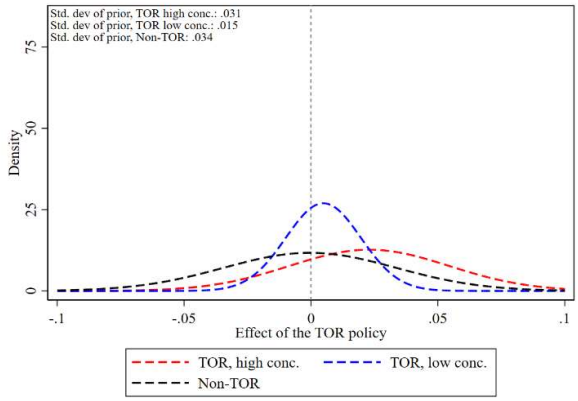
(a) 1993



(b) 1998



(c) 2001

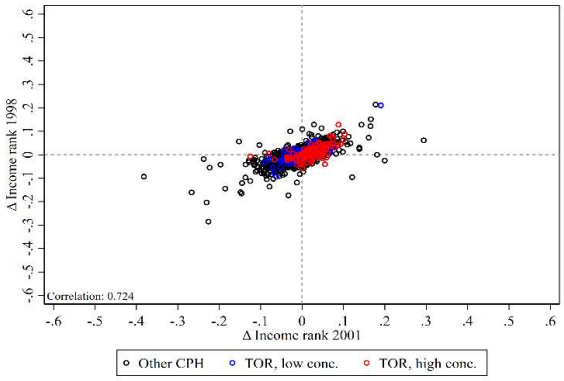


Note: The figure reports empirical Bayes prior distributions of the effect of the TOR policy on the average earnings rank of all neighborhoods in Copenhagen. These distributions are obtained from models fitting the distribution of neighborhood specific estimates separately by neighborhood type. Neighborhood specific estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectories of a given neighborhood in Copenhagen, with a synthetic control group constructed from neighborhoods in Aarhus, Odense and Aalborg.

Figure 9: Correlations between Neighborhood Estimates by Years Since the Reform

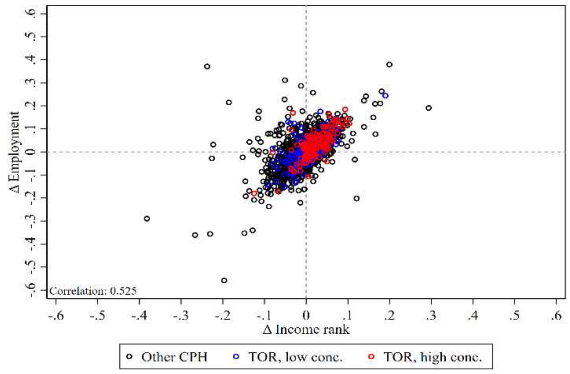
Correlations between neighborhood estimates across years

(a) 2001 vs 1998

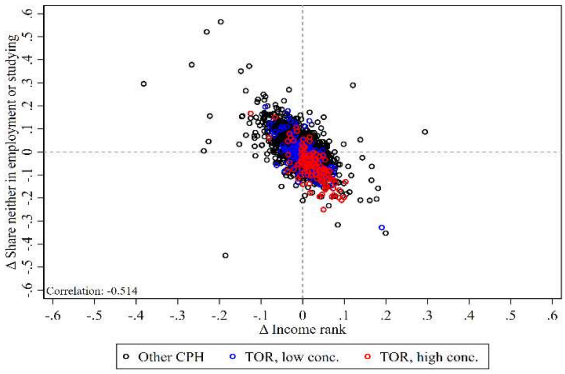


Correlations between neighborhood estimates for different outcomes

(b) Earnings rank and employment rate

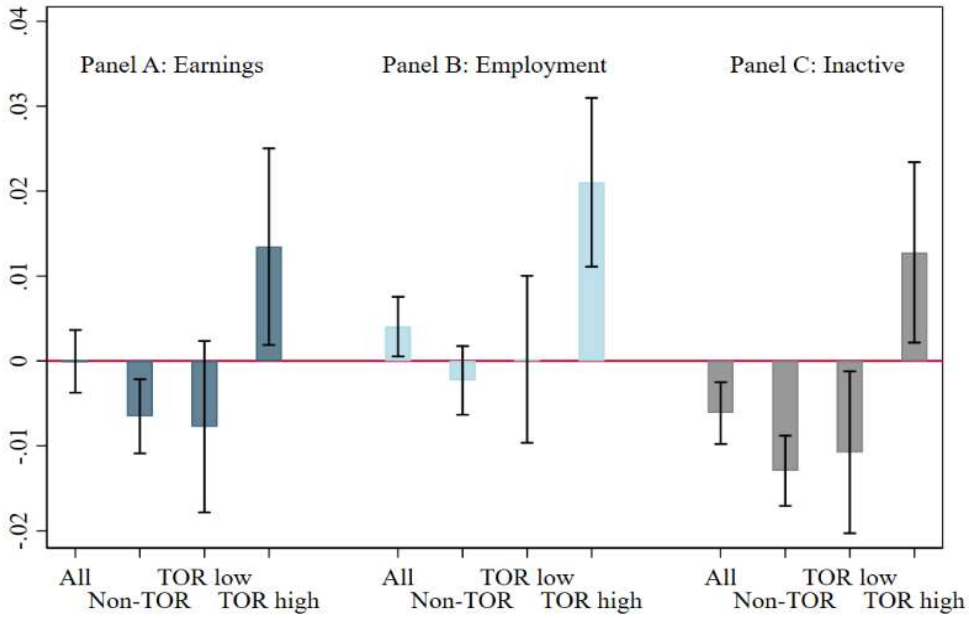


(c) Earnings rank and share neither employed or studying



Note: The figure reports estimates of the effect of the TOR policy on the average earnings rank of all neighborhoods in Copenhagen in 2001 against the estimated impact on earnings rank in 1995 (a), the estimated impact on earnings rank in 1998 (b), the estimated impact on employment rates in 2001 (c), and the estimated impact on the share of residents neither studying or in employment (d). Estimates are from difference-in-differences specifications that compare the outcome trajectories of a given neighborhood in Copenhagen, with a synthetic control group constructed from neighborhoods in Aarhus, Odense and Aalborg.

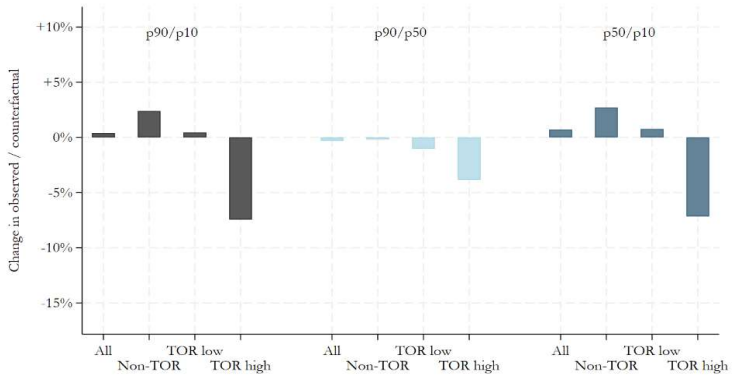
Figure 10: Characteristics of Winners and Losers from the TOR Policy



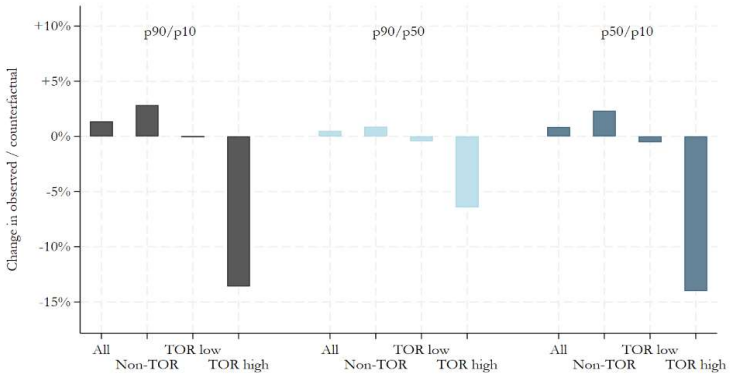
Note: The figure shows estimates of the relationship between neighborhoods being in the bottom quantile of the earnings distribution, having a low share of residents who are employed (bottom 25%) or having a high share of residents neither studying or being in employment (top 25%) in 1994. Specifically, the figure shows estimates from a regression where the estimated effect of the TOR policy on neighborhoods in Copenhagen is regressed on an indicator for being in the given part of the respective distribution. Estimates are shown for all neighborhoods in Greater Copenhagen, non-TOR neighborhoods in Copenhagen, neighborhoods with a low concentration (≤20%) of TOR units and neighborhoods with a high concentration (>20%) of TOR units. Vertical lines indicate 90% confidence intervals.

Figure 11: Vertical Inequality Effects of the TOR Policy in 2001

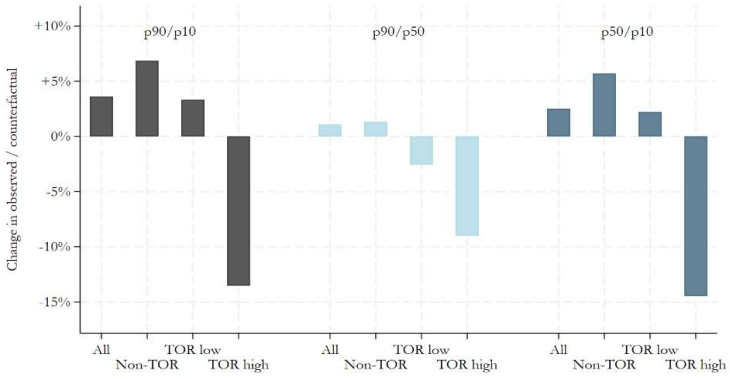
a) Earnings rank



b) Employed



c) Active



Note: The figure shows how the TOR policy affected the p90/p10, p50/p10, and p90/p50 ratios in the outcome distributions (earnings rank, share employed, share active) as a percentage of the counterfactual scenario. These measures are calculated by determining the average outcome at the 10th, 50th, and 90th percentiles both under the TOR policy and in the hypothetical scenario where the TOR policy was absent (using the empirical Bayes posterior estimates). We construct ratios for each neighborhood type —non-TOR, low-concentration TOR, and high-concentration TOR neighborhoods—and for all neighborhoods in Copenhagen. We compute the percentage change in the P90/P10 for a given neighborhood type c as:

$$\left(\frac{P90_A^1}{P10_c^1} \Big/ \frac{P90_A^0}{P10_c^0} - 1\right) \times 100$$

Change in p50/p10 and p90/p50 are computed in similar way. Estimates are shown for all neighborhoods in Greater Copenhagen (All), non-TOR neighborhoods (Non-TOR), neighborhoods with a low concentration (≤20%) of TOR units (TOR low) and neighborhoods with a high concentration (>20%) of TOR units (TOR high).

Table 1: Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
		1994			2001	
	TOR	CPH	Other	TOR	CPH	Other
Age 18-34	0.33	0.40	0.29	0.42	0.42	0.29
	(0.47)	(0.49)	(0.46)	(0.49)	(0.49)	(0.45)
Age 35-49	0.25	0.23	0.21	0.25	0.24	0.24
	(0.43)	(0.42)	(0.41)	(0.43)	(0.43)	(0.43)
Age 50-64	0.19	0.15	0.20	0.17	0.18	0.21
	(0.39)	(0.36)	(0.40)	(0.38)	(0.38)	(0.40)
Age 65+	0.24	0.22	0.30	0.17	0.16	0.26
	(0.42)	(0.41)	(0.46)	(0.37)	(0.37)	(0.44)
Studying	0.05	0.12	0.08	0.13	0.15	0.11
	(0.23)	(0.32)	(0.27)	(0.34)	(0.36)	(0.31)
Employment	0.29	0.46	0.33	0.39	0.50	0.35
	(0.45)	(0.50)	(0.47)	(0.49)	(0.50)	(0.48)
Not empl. or studying	0.51	0.33	0.50	0.42	0.30	0.48
	(0.50)	(0.47)	(0.50)	(0.49)	(0.46)	(0.50)
Earnings rank	0.33	0.48	0.36	0.39	0.49	0.36
	(0.24)	(0.29)	(0.25)	(0.25)	(0.29)	(0.25)
Disposable income rank	0.34	0.44	0.36	0.35	0.46	0.35
	(0.22)	(0.29)	(0.23)	(0.24)	(0.30)	(0.23)
Crime charge	0.06	0.02	0.02	0.03	0.01	0.02
	(0.23)	(0.14)	(0.15)	(0.17)	(0.12)	(0.13)
Observations	26,305	437,954	51,574	26,368	454,917	48,242

Note: The table reports descriptive statistics of residents in TOR housing units (columns 1, 4), other housing units in Copenhagen (columns 2, 5) and in public rental housing units in Aarhus, Odense and Aalborg (columns 3, 6). Characteristics are reported for 1994, the year before the TOR policy, and in 2001 six years after the policy. Standard deviations are reported in parentheses.

Table 2: Decomposing the Effect of The TOR Policy on TOR Units: Earnings Ranks

	(1)	(2)	(3)	(4)	(5)
	Baseline	Stayers	Composition	Weight on comp.	Original tenants
1995	-0.0017	-0.0074***	0.0333***	0.1389***	-0.0029*
	(0.0019)	(0.0019)	(0.0041)	(0.0028)	(0.0014)
1996	0.0015	-0.0128***	0.0456***	0.2451***	-0.0069***
	(0.0021)	(0.0021)	(0.0035)	(0.0028)	(0.0016)
1997	0.0105***	-0.0122***	0.0578***	0.3241***	-0.0050**
	(0.0022)	(0.0023)	(0.0033)	(0.0028)	(0.0017)
1998	0.0252***	-0.0094***	0.0790***	0.3909***	-0.0029
	(0.0023)	(0.0025)	(0.0032)	(0.0028)	(0.0019)
1999	0.0399***	-0.0062*	0.0971***	0.4462***	-0.0012
	(0.0024)	(0.0026)	(0.0031)	(0.0027)	(0.0020)
2000	0.0520***	-0.0036	0.1098***	0.4908***	-0.0002
	(0.0025)	(0.0027)	(0.0031)	(0.0027)	(0.0021)
2001	0.0593***	-0.0045	0.1166***	0.5629***	-0.0003
	(0.0026)	(0.0029)	(0.0031)	(0.0027)	(0.0023)
Observations	1,049,834	1,049,834	1,049,834	361.392	784.365

Note: The table reports estimates of the impact of the TOR policy on the earnings rank of residents in TOR units by years since the reform in column 1. Column 2 and 3 decomposes the effects into those due to stayers (column 2) and changes in the composition of residents (column 3). Column 4 reports the weights assigned to the composition estimates in the regression reported in column 1. The weight assigned to the stayers is 1 minus the weight in column 3. Column 5 reports the estimated effect of the TOR policy on the original TOR residents (residing in TOR units in 1994). Estimates are from difference-in-differences specifications that compare the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in public rental housing in Aarhus, Odense or Aalborg. Standard errors are reported in parentheses.

* p<0.1; ** p<0.05; *** p<0.01

Table 3: Statistically Significant Treatment Effects at the Neighborhood Level

Year	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	A) All estimates ≠ 0				B) Negative estimates ≠ 0				C) Positive estimates ≠ 0			
	All	non-TOR	low conc. TOR	high conc. TOR	All	non-TOR	low conc. TOR	high conc. TOR	All	non-TOR	low conc. TOR	high conc. TOR
1993	0.004	0.003	0.006	0.016	0.001	0.001	0.000	0.008	0.003	0.002	0.006	0.008
1998	0.036	0.038	0.017	0.047	0.013	0.015	0.000	0.000	0.023	0.022	0.017	0.047
2001	0.063	0.062	0.062	0.078	0.025	0.026	0.028	0.008	0.038	0.036	0.034	0.070
Observations	2,055	1,751	176	128	2,055	1,751	176	128	2,055	1,751	176	128

Note: The tables shows the share of the estimates of the impact of the TOR policy on neighborhoods in Greater Copenhagen that are significant at the five percent level. These shares are shown separately by year with the TOR policy occurring in 1995, by neighborhood type (all neighborhoods, non-TOR neighborhoods, low concentration TOR neighborhoods and high concentration TOR neighborhoods) and separately for all estimates (columns 1-4), negative estimates (columns 5-8) and positive estimates (columns 9-12).

Table 4: Child Outcomes from Exposure to Neighborhood Policy Effects on Earnings Rank (5 Rank Points)

	GPA (standardized to sd=1)					High school completion (0/1)					Crime charges				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Exposure (10 ranks)	0.076*	0.074**	0.059**	0.061	0.016	0.045***	0.046***	0.025**	-0.077**	0.014	-0.016	-0.027*	0.004	0.031	0.007
	(0.028)	(0.024)	(0.021)	(0.047)	(0.029)	(0.012)	(0.010)	(0.008)	(0.029)	(0.010)	(0.015)	(0.012)	(0.016)	(0.032)	(0.017)
Exposure (10 ranks) *1[Neighborhood earnings, 1st quartile]	-	-	0.069	-	-	-	-	0.077**	-	-	-	-	-0.080*	-	-
			(0.074)					(0.025)					(0.035)		
Exposure (10 ranks) *Age at exposure	-	-		0.012	-	-	-		0.014***	-	-	-		-0.009*	-
				(0.006)					(0.004)					(0.004)	
Exposure (10 ranks) *1[No high school, child's parents]	-	-		-	0.045	-	-		-	0.028*	-	-		-	-0.025
					(0.027)					(0.012)					(0.020)
Mean (s.d.)	0.049 (1.001)					0.524 (0.499)					0.263 (0.869)				
Observations	198,079					198,079					198,079				
Pre-policy characteristics	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
Fixing neighborhoods to 1994		X					X					X			

Notes: The table shows estimates of child outcomes regressed on the children's exposure to the neigborhood specific policy effects on residents' earnings rank (scaled to an increase in 5 rank points) from 1995-2001. Columns (1)-(5) show estimates on children's 9th grade GPA (standardized to mean of 0 and sd=1 at the national level), (6)-(10) show estimates on children's high school completion, and (11)-(15) show estimates on number of crime charges from age 15-18. Columns (2), (6) and (11) show the corresponding estimates while fixing neighborhood effects to the neighborhood where the children lived in 1994. Columns (3), (8), and (13) show estimates where expsoure is interacted with an indicator of the neighborhoods' pre-policy average earnings rank was in the lowest 25% of Greater Copenhagen. Columns (4), (9), and (14) show estimates where exposure is interacted with an indicator child age at exposure. Columns (5), (10), and (15) show estimates where exposure is interacted with an indicator of whether both of the child's parents have not completed high school. All estimates are controlling for pre-policy (1992) characteristics of residents in the neighborhoods the children lived in from 1995-2001. Effects are estimated at the child-year level using the neighborhood specificempirical Bayes posterior means of the estimated TOR policy effects. Estimates are clustered at the neighborhood level.

* p<0.1; ** p<0.05; *** p<0.01

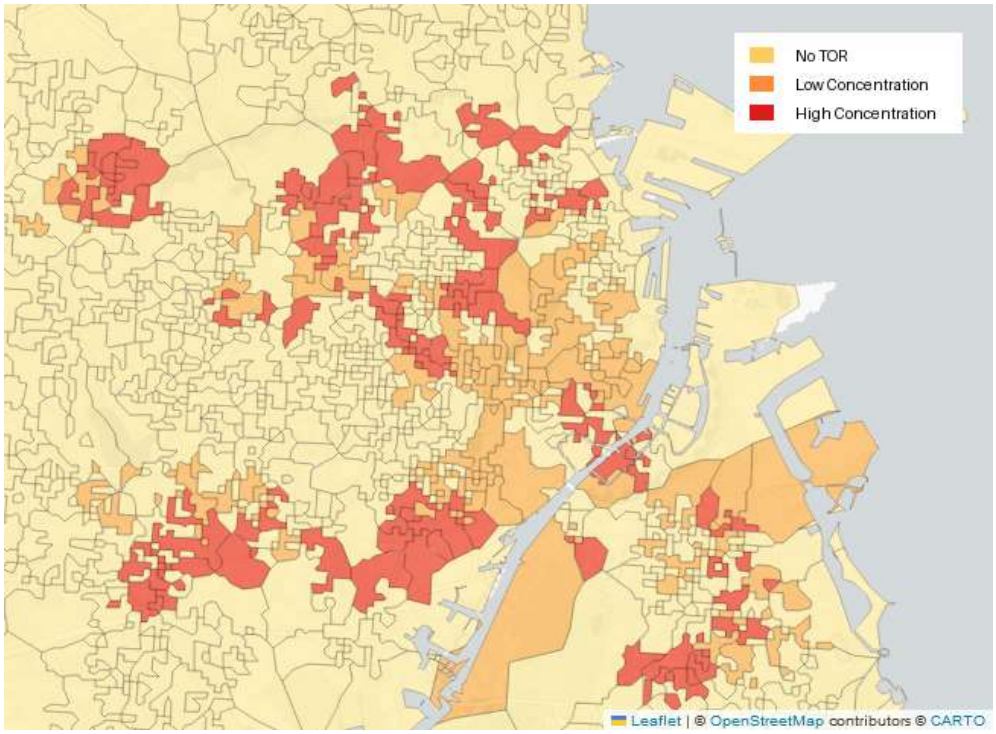
Appendix:
Direct and Indirect Effects of a Housing Policy on Neighborhoods, Residential Mobility, and Inequality

Christian Dustmann, Rasmus Landersø and Mikkel Mertz

March 2025

A. Further results

Figure A.1: Concentration of TOR Units by Neighborhood

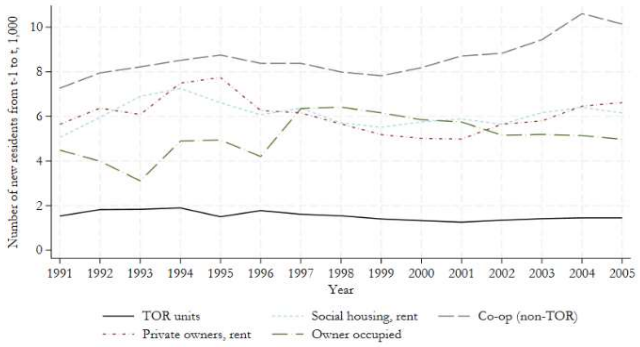


Note: The figure shows the share of TOR units in neighborhood clusters of 600 households; i.e., areas that contain on average four 150 household neighborhoods, which is the unit of analysis in the paper. Light yellow areas are areas without any TOR units. Orange areas are areas with TOR units, but without any 150 household neighborhoods that are high-concentration TOR neighborhoods (where TOR units compose 20% or more of the housing mass). Red areas are areas where at least one 150 household neighborhood is a high-concentration TOR neighborhood. Areas vary in size due to housing density with the largest areas also contain parks, recreational areas or industry (such as dock area next to the harbor).

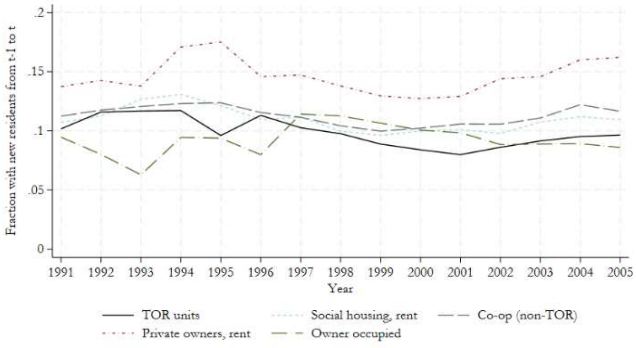
Figure A.2: Housing Units With New Residents From Year t-1 to t.

Housing Units with New Residents, by Housing Type

(a) Number of Housing Units With New Resident

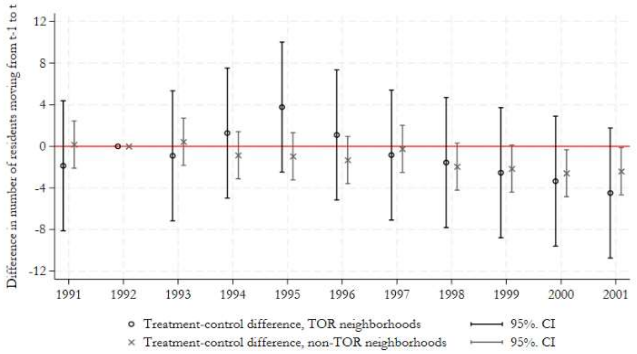


(b) Fraction of Housing Units With New Resident

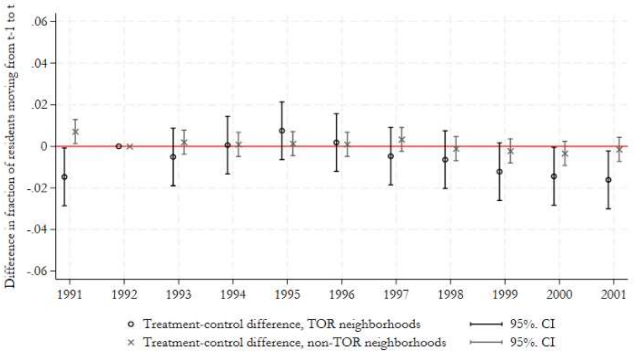


Housing Units With New Residents, Treatment-Control Differences at Neighborhood level

(c) Number of Housing Units With New Resident

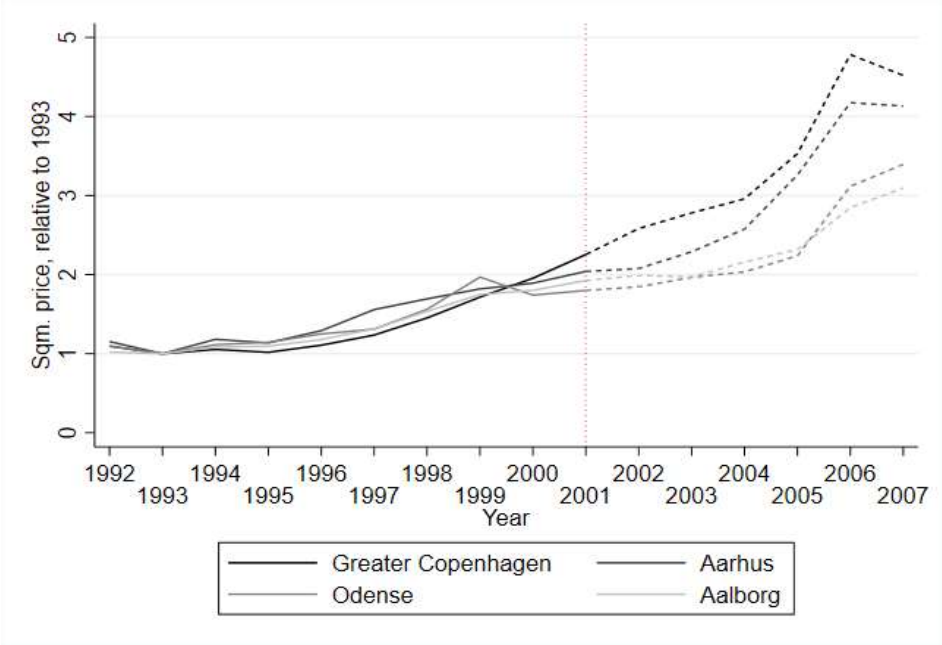


(d) Fraction of Housing Units With New Resident



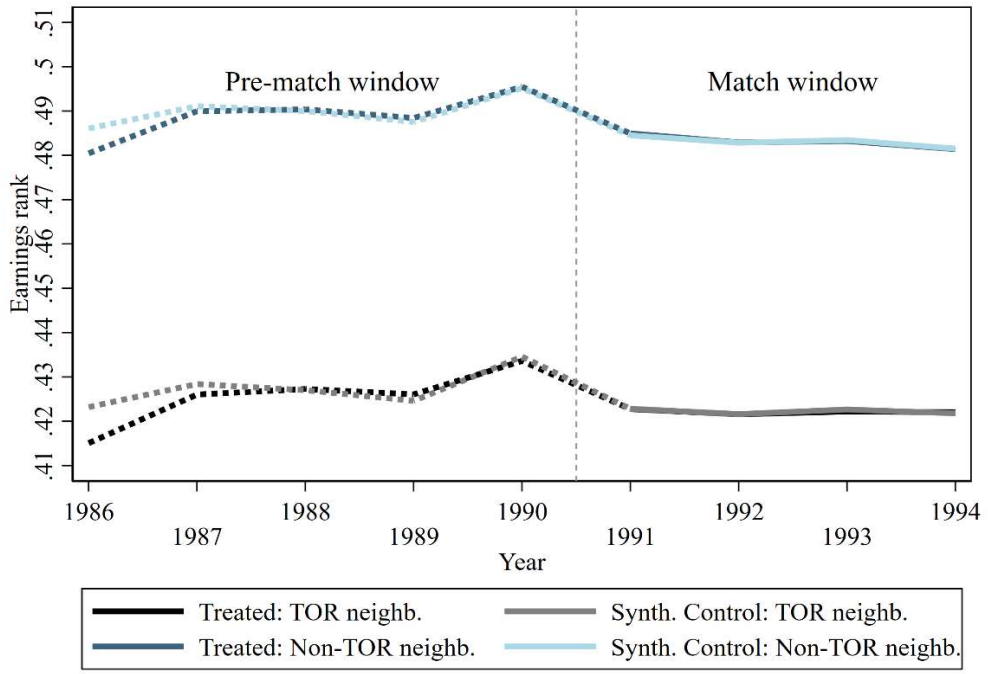
Note: Figures (a) and (b) show - by housing type - the number (a) and fraction (b) of housing units where all residents have moved out from year t-1 to year t. Figures (c) and (d) show - by neighborhood type - treatment-control differences in number (c) and fraction (d) of housing units where all residents have moved out from year t-1 to year t along with 95% confidence intervals. Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectories of a given neighborhood in Copenhagen, with a synthetic control group constructed from neighborhoods in Aarhus, Odense and Aalborg.

Figure A.3: Housing Prices in Copenhagen and Other Large Cities of Denmark



Note: The figure shows the evolution in the average price per square-meter of apartments sold in a year in Greater Copenhagen, Aarhus, Odense and Aalborg. Prices are set to one in 1993.

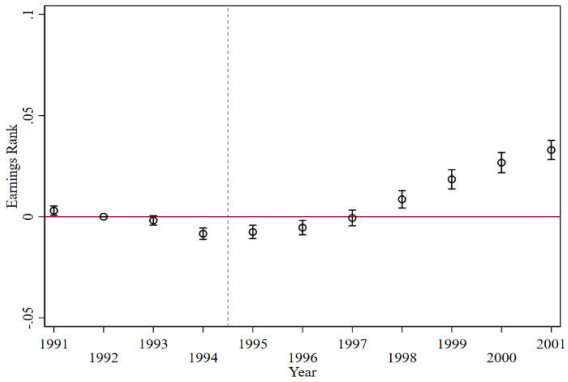
Figure A.4: Synthetic Control Validation by Neighborhood Type



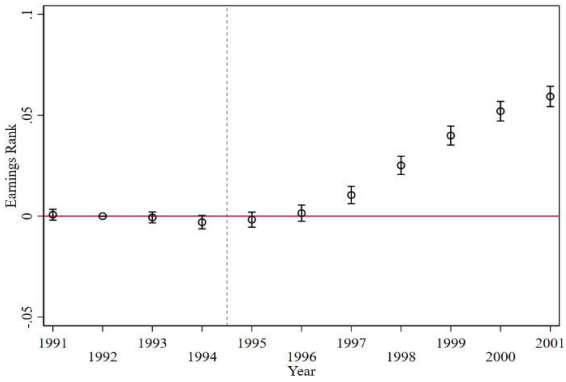
Note: The figure shows the mean earnings rank of neighborhoods in Greater Copenhagen, and of synthetic control groups of neighborhoods in Greater Copenhagen for the years 1986-1994. The synthetic control groups are constructed from neighborhoods in Aarhus, Odense and Aalborg. The synthetic control groups are constructed to match the earnings ranks of neighborhoods in Greater Copenhagen between 1991 and 1994. These statistics are shown separately for neighborhoods with TOR units, and for neighborhoods in Greater Copenhagen with no TOR units.

Figure A.5: Effects of the TOR Policy on Earnings in TOR Units

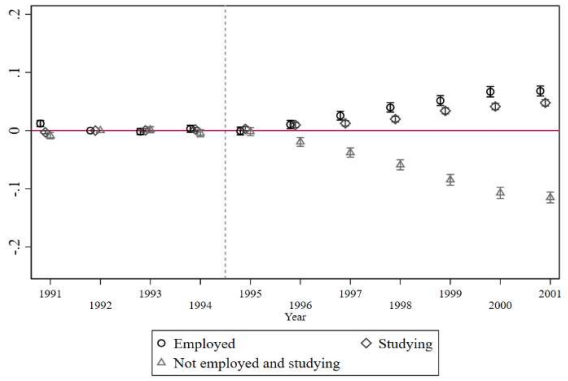
(a) Control: Rest of CPH. Earnings rank



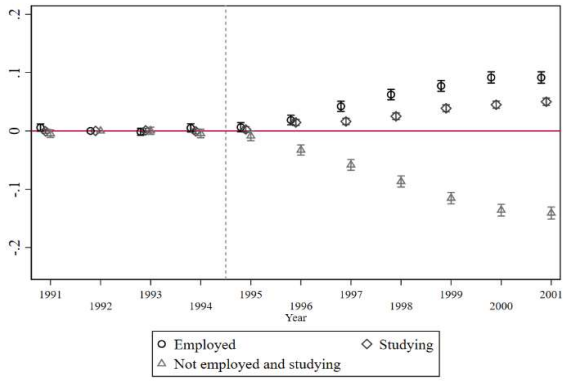
(b) Control: Other cities, Earnings rank



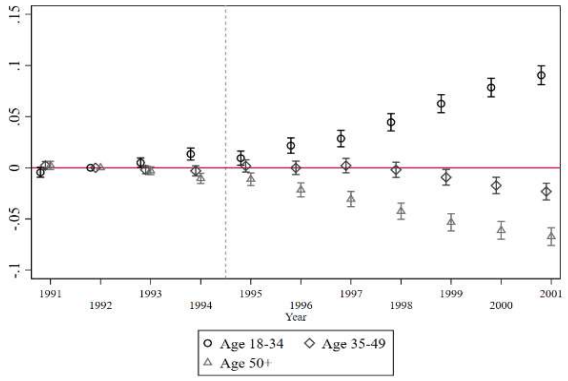
(c) Control: Rest of CPH, Labor market status



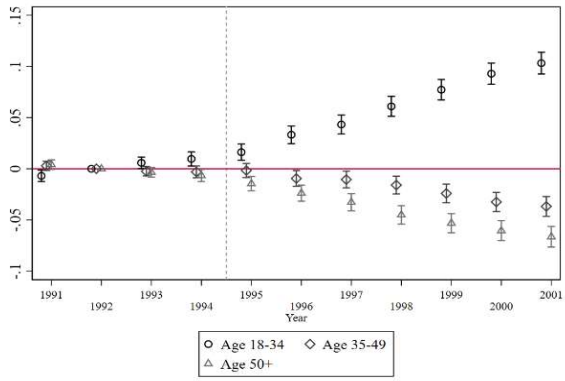
(d) Control: Other cities, Labor market status



(e) Control: Rest of CPH, Age



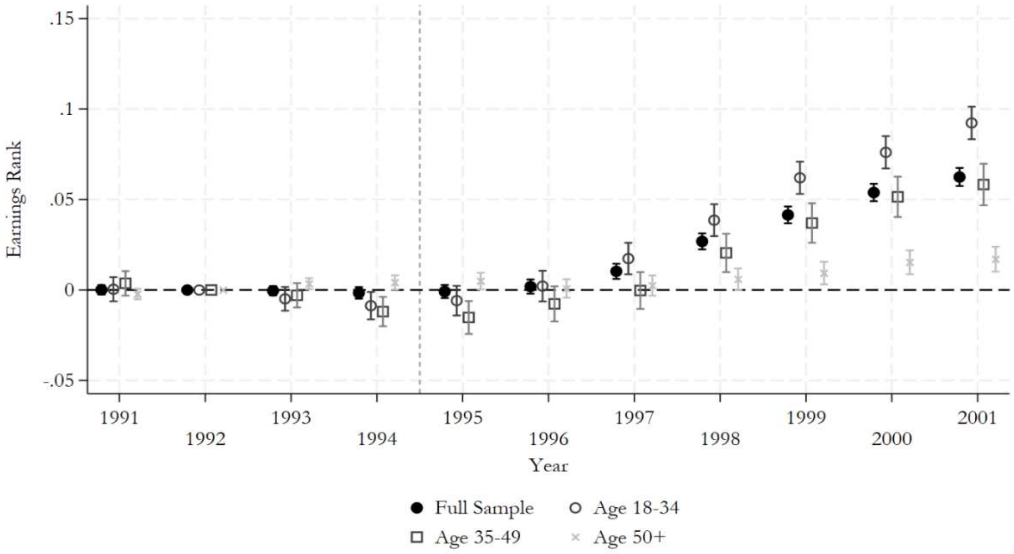
(f) Control: Other cities, Age



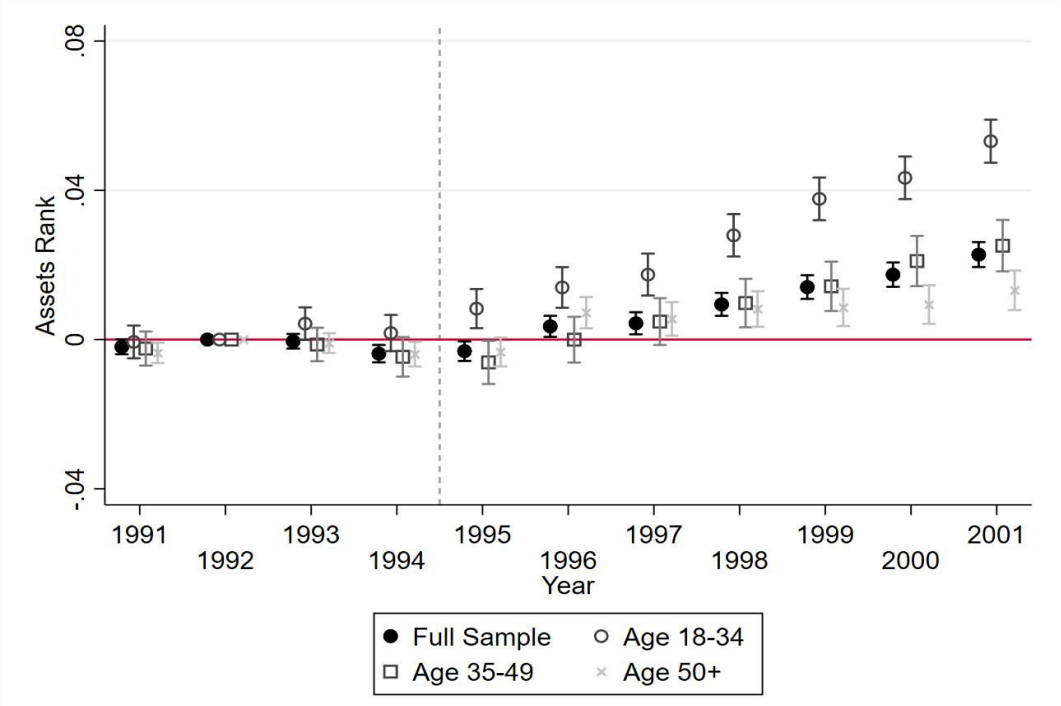
Note: The figure shows estimates of the impact of the TOR policy on the earnings rank, labor market status (employed, unemployed, studying or outside the labor force and not studying), and the age composition of TOR residents in Panels (a,b), (c,d), and (e,f), respectively. Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in other housing units in Copenhagen (panels to the left) or in public rental housing in Aarhus, Odense or Aalborg (panels to the right). Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure A.6: Effects of the TOR Policy on Earnings and Assets Ranks in TOR Units, by Age of Residents

(a) Earnings Ranks

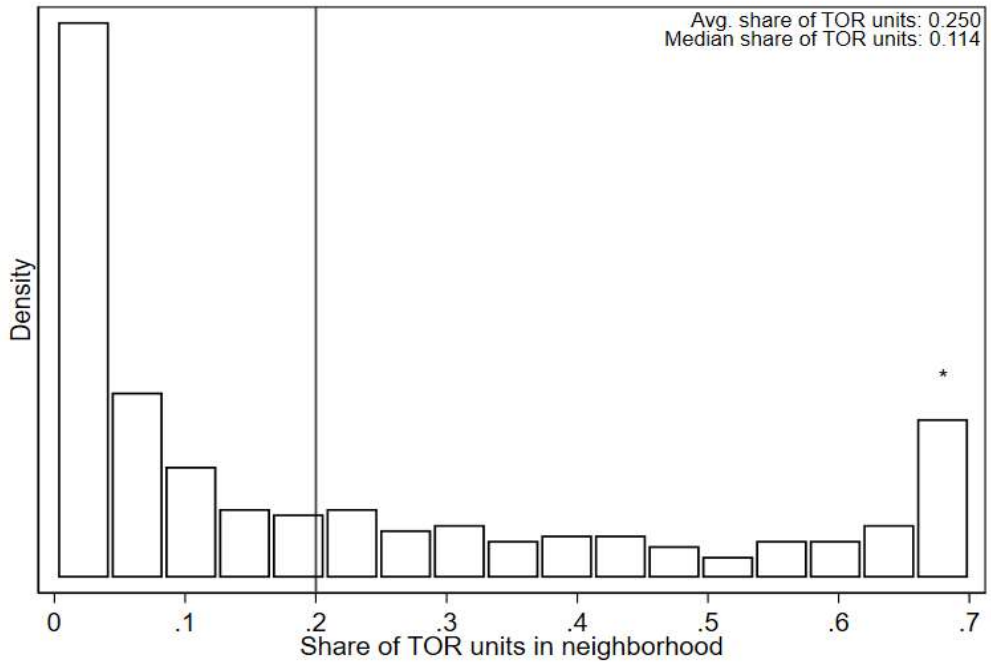


(b) Assets Ranks



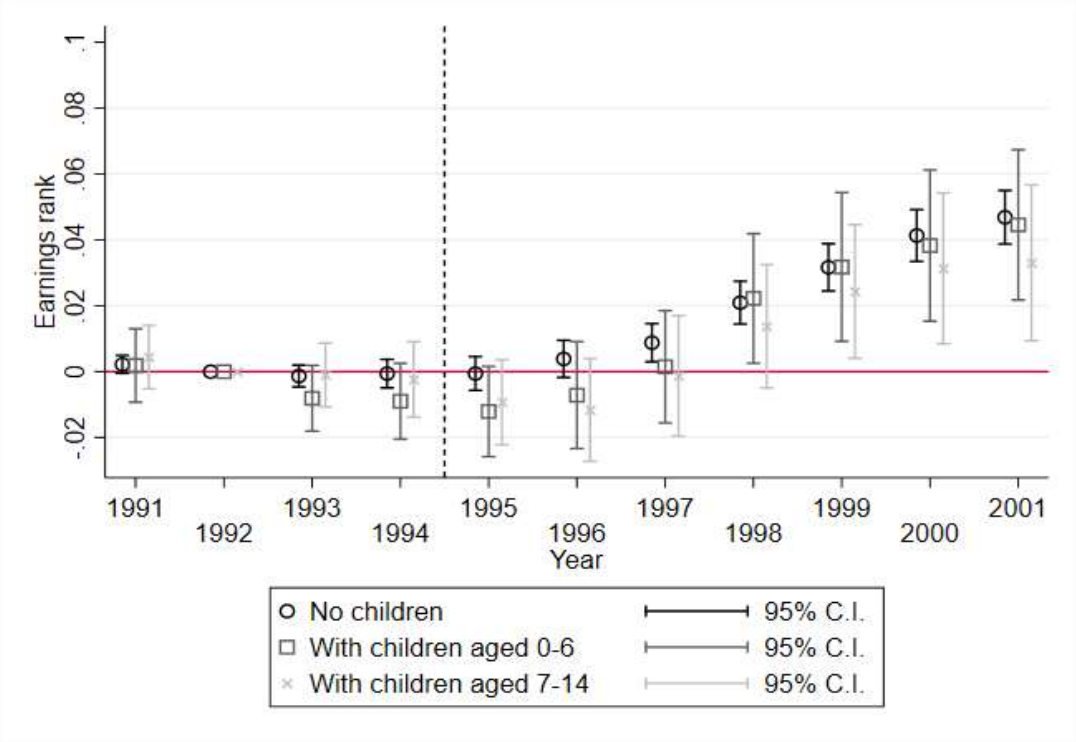
Note: The figure shows estimates of the impact of the TOR policy on the earnings and assets ranks of residents in TOR housing units separately for individuals aged 18-34, 35-49, 50+. Earnings and assets ranks for individual age groups are measured in the full population distribution (irrespective of age). Estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure A.7: Density of TOR Units in TOR Neighborhoods



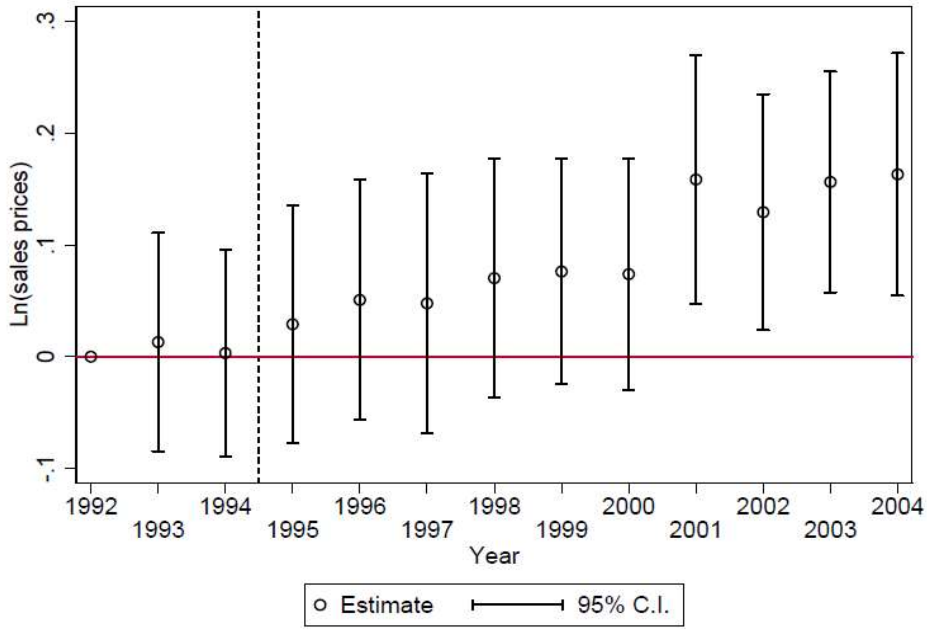
Note: The figure shows the share of TOR units in neighborhoods in Copenhagen with at least one TOR unit. The graph is censored at 0.7 as indicated with *.

Figure A.8: Effects of the TOR Policy on Earnings in TOR Neighborhoods, Families With and Without Children



Note: The figure shows estimates of the impact of the TOR policy on the earnings rank of residents in high-concentration TOR neighborhoods (but not in TOR housing units) according to whether they have children aged 0-6, children aged 7-14, or no children within those age-ranges. Estimates are from difference-in-differences specifications (Eq. 1) that compare the outcome trajectory of adults residing in high-concentration TOR neighborhoods to the outcome trajectory of adults residing in neighborhoods with similar fractions of public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

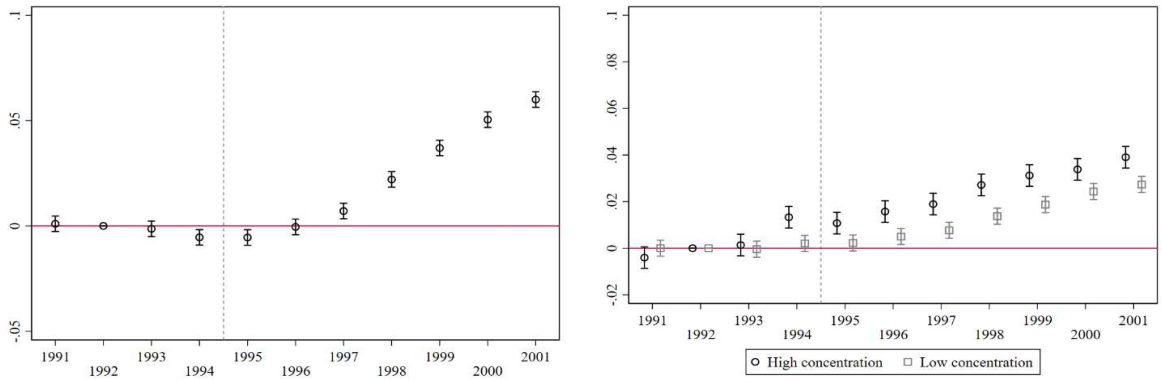
Figure A.9: The Impact of the TOR Policy on Housing Prices in TOR Neighborhoods



Note: The figure shows estimates of the impact of the TOR policy on the average sales prices of apartments in TOR neighborhoods excluding sales of TOR housing units in a year. Estimates are from difference-in-differences specifications that compare the outcome trajectory of housing units in TOR neighborhoods to the outcome trajectory of housing units in neighborhoods with public rental housing in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals. Standard errors are clustered at the housing unit level.

Figure A.10: Effects of the TOR Policy on Earnings using Synthetic Control Group

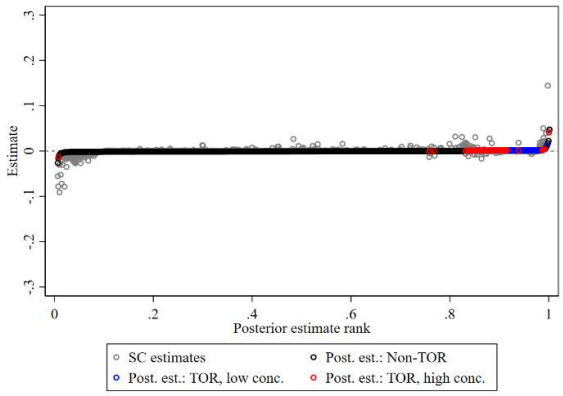
(a) TOR units (b) TOR Neighborhoods, excl. TOR units



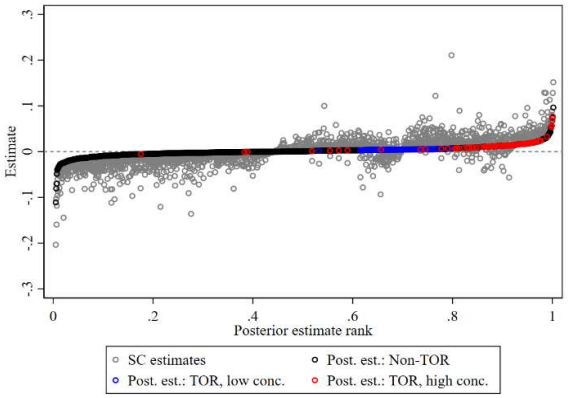
Note: The figure shows estimates of the impact of the TOR policy on the earnings rank of residents in TOR housing units (panel a) and on residents in TOR neighborhoods excluding residents in TOR units (panel b). Estimates are from difference-in-differences specifications that compare the outcome trajectory of adults residing in TOR units (panel a) or outcomes of adults residing in TOR neighborhoods (panel b) to the outcome trajectory of their synthetic control constructed from neighborhoods in Aarhus, Odense or Aalborg. Vertical lines indicate 95% confidence intervals.

Figure A.11: Effects of the TOR Policy on Neighborhoods in Copenhagen

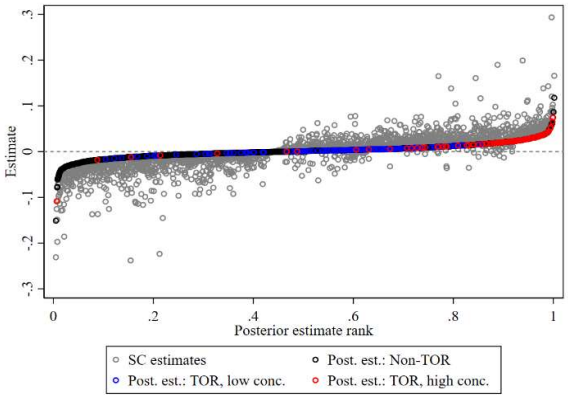
(a) 1993



(b) 1998



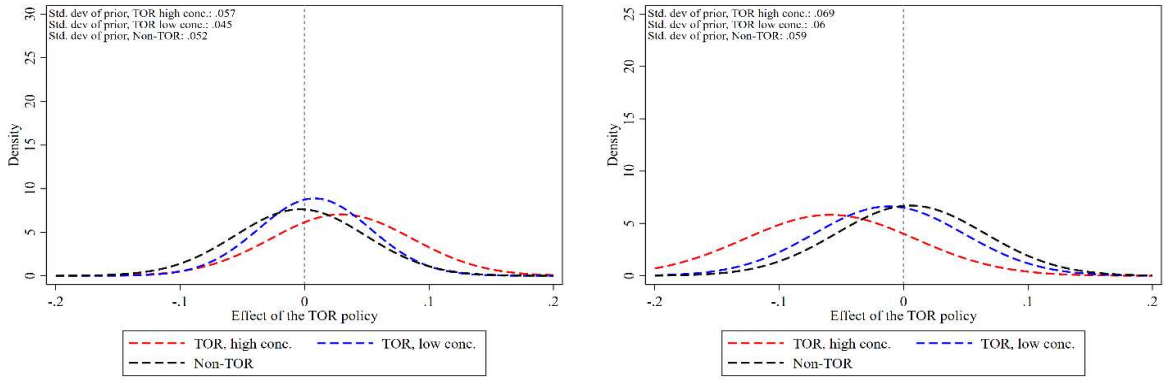
(c) 2001



Note: The figure reports estimates of the effect of the TOR policy on the average earnings rank of all neighborhoods in Copenhagen. Estimates in gray are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectories of a given neighborhood in Copenhagen, with a synthetic control group constructed from neighborhoods in Aarhus, Odense and Aalborg. Estimates in black, blue and red are empirical Bayes posterior estimates from models fit to the distribution of neighborhood specific estimates separately by neighborhood type.

Figure A.12: Effects of the TOR Policy on Neighborhoods in Copenhagen

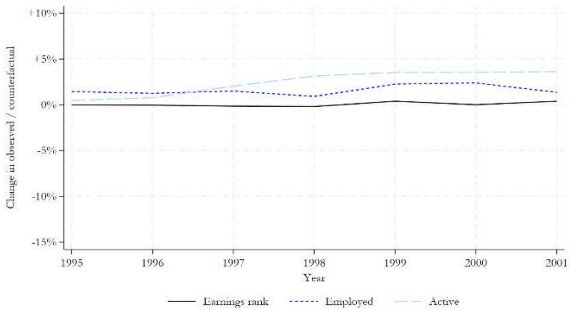
(a) Employment: 2001 (b) Not employed or studying: 2001



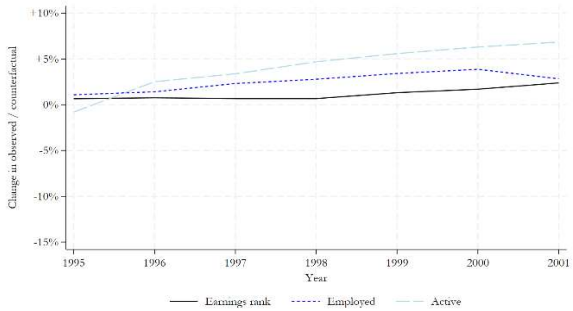
Note: The figure reports empirical Bayes prior distributions of the effect of the TOR policy on the employment rate (Panel a) and the share of residents who are neither employed or studying (Panel b) for all neighborhoods in Copenhagen. These distributions are obtained from models fitting the distribution of neighborhood specific estimates separately by neighborhood type. Neighborhood specific estimates are from a difference-in-differences specification (Eq. 1) that compares the outcome trajectories of a given neighborhood in Copenhagen, with a synthetic control group constructed from neighborhoods in Aarhus, Odense and Aalborg.

Figure A13: Vertical Inequality Effects of the TOR Policy, 1995-2001

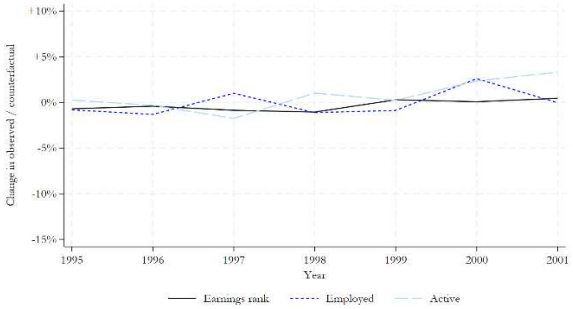
a) All neighborhoods



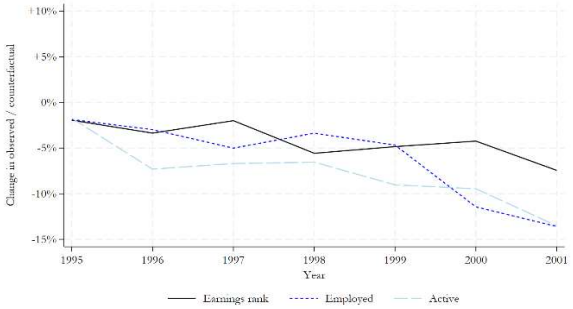
b) Non-TOR neighborhoods



c) Low-concentration TOR neighborhoods



d) High-concentration TOR neighborhoods

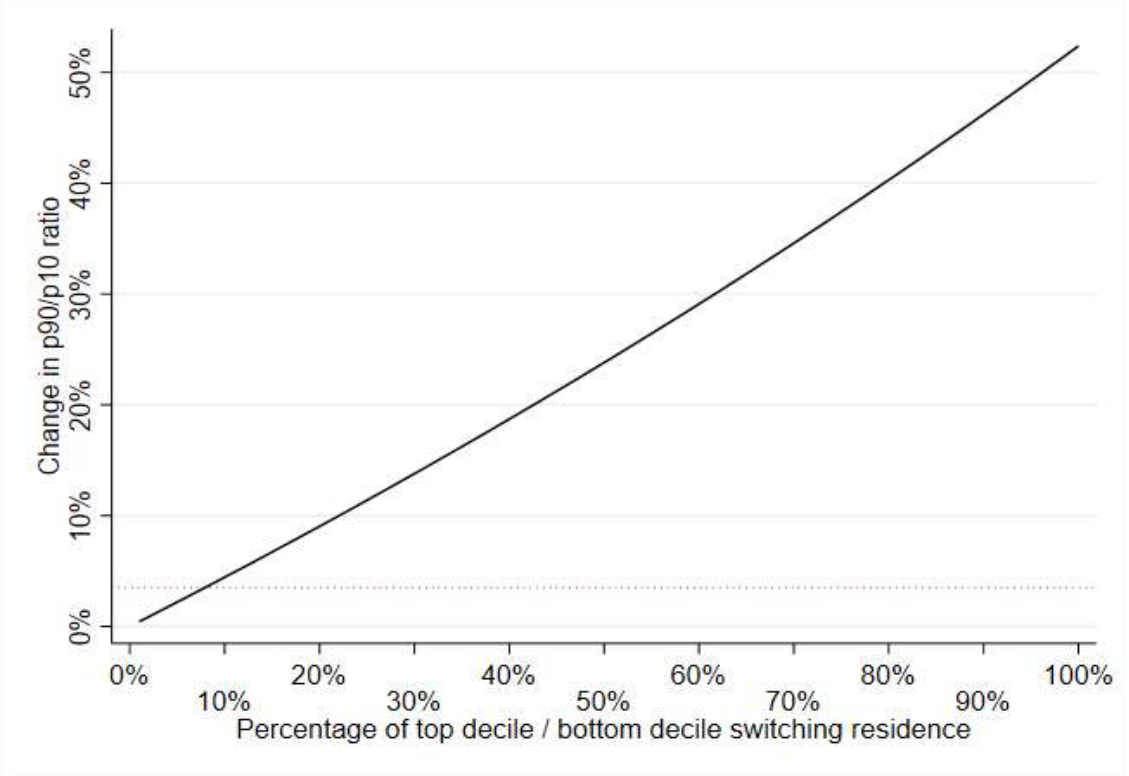


Note: The figure shows the estimates of the impact of the TOR policy on the p90/p10 ratios in the outcome distributions (earnings rank, share employed, share active) as a percentage of the counterfactual scenario fromm 1995 to 2001. These measures are calculated by determining the average outcome at the 10th, 50th, and 90th percentiles both under the TOR policy and in the hypothetical scenario where the TOR policy was absent. We construct these metrics for each neighborhood type—non-TOR neighborhoods, low-concentration TOR neighborhoods, and high-concentration TOR neighborhoods—and for all neighborhoods in Copenhagen. We compute the percentage change in the P90/P10 for a given neighborhood type c as:

$$\left(\frac{P90_A^1}{P10_c^1} \Big/ \frac{P90_A^0}{P10_c^0} - 1\right) \times 100$$

Estimates are shown for all neighborhoods in Greater Copenhagen (a), non-TOR neighborhoods (b), neighborhoods with a low concentration (≤20%) of TOR units (c) and neighborhoods with a high concentration (>20%) of TOR units (d).

Figure A.14: Change in p90/p10 at city-level if subset of individuals from top and bottom decile switch neighborhood of residence



Note: The figure shows the percentage change in the p90/p10 ratio between average earnings rank in neighborhoods in Copenhagen if a percentage of residents from decile and bottom decile of the earnings distribution swap neighborhood of residence varying from 1% to 100% of the top and bottom decile swapping neighborhood of residence. The figure is based on earnings and the residential patterns of individuals in 1994.

Table A.1: Decomposing the Effect of The TOR Policy on TOR Units: Crime Charges of 18-34 Year Olds

	(1)	(2)	(3)	(4)	(5)
	Baseline	Stayers	Composition	Weight on comp.	Original tenants
1995	-0.0222***	-0.0342***	0.0057	0.3004***	-0.0140***
	(0.0054)	(0.0054)	(0.0083)	(0.0043)	(0.0018)
1996	-0.0299***	-0.0389***	-0.0207**	0.4944***	-0.0186***
	(0.0054)	(0.0058	(0.0065)	(0.0041)	(0.0018)
1997	-0.0340***	-0.0405***	-0.0299***	0.6152***	-0.0197***
	(0.0053)	(0.0060)	(0.0060)	(0.0041)	(0.0018)
1998	-0.0494***	-0.0512***	-0.0487***	0.7091***	-0.0223***
	(0.0053)	(0.0063)	(0.0056)	(0.0039)	(0.0018)
1999	-0.0602***	-0.0551***	-0.0617***	0.7810***	-0.0243***
	(0.0051)	(0.0067)	(0.0053)	(0.0038)	(0.0018)
2000	-0.0661***	-0.0656***	-0.0661***	0.8324***	-0.0272***
	(0.0051)	(0.0066)	(0.0052)	(0.0037)	(0.0018)
2001	-0.0694***	-0.0651***	-0.0700***	0.8703***	-0.0303***
	(0.0050)	(0.0070)	(0.0051)	(0.0037)	(0.0018)
Observations	330,763	330,763	330,763	131,836	856,669

Note: The table reports estimates of the impact of the TOR policy on the fraction of residents aged 18-49 in TOR units charged for a crime by years since the reform in column 1. Column 2 and 3 decomposes the effects into those due to stayers (column 2) and changes in the composition of residents (column 3). Column 4 reports the weights assigned to the composition estimates in the regression reported in column 1. The weight assigned to the stayers is 1 minus the weight in column 3. Column 5 reports the estimated effect of the TOR policy on the original TOR residents (residing in TOR units in 1994). Estimates are from difference-in-differences specifications that compare the outcome trajectory of adults residing in TOR units to the outcome trajectory of adults residing in public rental housing in Aarhus, Odense or Aalborg. Standard errors are reported in parentheses.

* p<0.1; ** p<0.05; *** p<0.01

Table A.2: Changes to Moving Patterns and TOR Policy Effects on Non-TOR Neighborhoods

	(1)	(2)
Rank of Type A contributor,	-0.020*	-0.020*
where residents who would have moved to high-concentaration TOR neighborhoods live	(0.009)	(0.009)
Rank of Type B contributor,	0.046***	0.046***
where residents who move to high-concentaration TOR neighborhoods would have lived	(0.009)	(0.009)
Pre-policy neighborhood characteristics		X
Observations	1,628	1,628

Note: The table shows estimates from regressing neighborhood specific effects on earnings rank in 2001 on the proxies of being a type A and type B contributor for non-TOR neighborhoods. Column (1) shows the estimation results when regressing neighborhood specific estimates on the two proxies. Column (2) shows the corresponding results while controlling for neighborhood pre-policy characteristics (average earnings rank, share employed, share studying, share aged 18-24, and share aged 35-49). Estimates have been weighted by the inverse of the p-value for the neighborhood specific policy effects to place higher weight on the most precise estimtes. To estimate the degree to which a neighborhood is a type A or B contributor, we i) separate the data in a pre-policy period (1991-1994) and post policy period (1998-2001), ii) estimate the share of residents moving from each neighborhood to high-concentration TOR neighborhoods in the pre-period and the post-period, iii) estimate the neighborhood specific propensity to receive residents in the post-policy period, from neighborhoods that were sending to high-concentration TOR neighborhoods in the pre-policy period (proxying type A moves), and iv) estimating the propensity to receive residents in the pre-policy period, from neighborhoods that send residents to high-concentration TOR neighborhoods in the post-policy period.

* p<0.1; ** p<0.05; *** p<0.01

j

Table A.3: Child Outcomes from Exposure to Neighborhood Policy Effects on Earnings Rank (5 Rank Points)

	(1)	(2)	(3)
Exposure (10 ranks), Stayers	0.093**	0.058***	-0.019
	(0.030)	(0.012)	(0.015)
Exposure (10 ranks), Movers	0.070*	0.032	-0.042
	(0.030)	(0.017)	(0.046)
Movers	-0.070***	-0.040***	0.060***
	(0.014)	(0.007)	(0.015)
Mean (s.d.)	0.049 (1.001)	0.524 (0.499)	0.263 (0.869)
Observations	198,079	198,079	198,079

Notes: The table shows estimates of child outcomes regressed on the children's exposure to the neigborhood specific policy effects on residents' earnings rank (scaled to an increase in 5 rank points) from 1995-2001 separately for children who have lived in the same neighborhood from 1994-2001 and children who have moved between neighborhoods during the period. The row "Movers" show the estimated difference between the sample average and the children who moved between neighborhoods. Column (1) shows estimates on children's 9th grade GPA (standardized to mean of 0 and sd=1 at the national level), (2) shows estimates on children's high school completion, and (3) shows estimates on children's crime charges from age 15-18. All estimates are controlling for pre-policy (1992) characteristics of residents in the neighborhoods the children lived in from 1995-2001. Effects are estimated at the child-year level using the neighborhood specificempirical Bayes posterior means of the estimated TOR policy effects. Estimates are clustered at the neighborhood level.

* p<0.1; ** p<0.05; *** p<0.01

B. The TOR Policy and Co-ops

B.1 Co-ops

A co-op (co-operative) is a form of collective property ownership by a group of individuals. The group collectively purchases a set of residential units, such as a building or apartment complex, with each member owning a share that reflects their proportional contribution to the property's purchase—typically equivalent to the relative size of their unit within the total property. Consequently, co-op owners do not own a specific house or apartment outright (even though they are typically responsible for the day-to-day maintenance of their own unit), but instead own a portion of the entire property.

The co-op members jointly contribute to the property's running costs, which include maintenance expenses, interest on loans (such as those taken out to purchase the property or for renovations), and costs related to common areas.

The price of co-op shares is determined by the co-op itself (and during the period in question, was subject to strict regulations), based on the initial down payment made when the co-op was formed, ongoing running costs, and anticipated future renovations. As a result, co-op prices during this period did not align with fluctuations in the broader real estate market. Depending on the initial collective down payment, the cost of a co-op share might be relatively low, with most expenses directed towards running costs, such as interest on the original loan.

B.2 The TOR policy

The TOR policy was implemented in eight steps:

1. In the first half of 1994, the Danish state and the Municipality of Copenhagen (MoC) negotiated what would later become the TOR policy.

2. In June 1994, an agreement was signed between the Ministry of Housing and the MoC: the state and MoC would jointly establish a company, TOR I/S, with the state holding 15% ownership and the MoC holding 85%. The state also guaranteed coverage of the first DKK 3 billion in deficits.
3. Over the following months, the MoC evaluated potential candidate buildings for inclusion in the sale. A total of 234 buildings, encompassing approximately 19,000 housing units, were selected for the final group.
4. On November 14, 1994, TOR I/S was officially formed.
5. Shortly thereafter, leaflets were distributed to residents, informing them of the sale and their preemption right to purchase their units if they wished to form a co-op. If they chose not to buy, the units would be offered to third-party buyers.
6. On January 1, 1995, all the housing units were sold from the MoC to TOR I/S at a price determined by the state and the MoC. The units were then put up for sale at a price recommended by realtors to reflect the market value of similar units.
7. By 1998, almost all of the TOR units had been sold, with the majority converted into co-ops.
8. In 2000, TOR I/S was dissolved with a deficit of around DKK 800 million, which was covered by the state.

B.3 Consequences for the Original Tenants in TOR Units

B.3.1 Sale of TOR Properties

The TOR policy concerned the sale of 234 properties which included around 19,300 rental housing units. Of these properties 169, which included around 90% of the rental housing units, were sold to the original tenants and converted into co-ops. The remaining properties were sold to a third party (57 or 24%) or to public housing organizations (8 or 3%). An important component of the policy was

that the sale of the properties to the original tenants were conducted at market price. In most cases (43%) this was achieved by selling the properties to the original tenants at a price set by two independent real estate agents. In 40% of cases the properties were instead sold to the original tenants after an agreement was reached with a third party at the price negotiated by the third party. In the remaining 17% of cases the original tenants first formed a co-op housing association, after which they negotiated the price with TOR. Together this led to prices that broadly reflected market prices and an average price per property of around $2 million.[1]

B.3.2 Buying Co-op Shares

The formation of co-ops allowed residents to share the risks associated with loan-taking and real estate purchase. Yet, individual residents still had to buy their share of the co-op, which usually corresponded to the co-ops net-value (the difference between the value of the property and the co-op's mortgage) times the size of the share of the co-op (i.e., relative apartment size). Because of the condition of the properties, the average price of these co-op shares was relatively low, and an average sized apartment (62 sqm) has been estimated to have cost around $4,500 in 2020-prices.[2]

To make participation in co-ops feasible, even for low-income residents who were overrepresented in TOR units, residents were allowed to use their original deposit for when they moved into the flat as down payment. Furthermore, if residents had made advanced rental payments or if there was a surplus on the building's maintenance account these surpluses could be also used as down payments. A survey conducted among original tenants shows that around 90% of the original tenants who bought co-op shares financed at least some of their co-op share this way, and that around 50% of got at least 50% of the co-op share financed this way. Importantly, individuals who did not want to participate in

---

[1] These numbers are based on 143 properties sold before 1998.
[2] The average price per sqm was estimated to be $73 in 2020-prices (COWI, 2001) and the average apartment size 62 sqm.

the co-op were allowed to remain in their original apartment as renters, but now rented their apartment from the co-op rather than the municipality of Copenhagen.

Annual data on individual level wealth (assets – liabilities) further allows us to investigate what happens to the wealth of original tenants when the co-ops are created, and in particular if they take up large loans to finance the purchase of their co-op share.

Figure B.1 shows the change in average liabilities of the original TOR residents who remained in their TOR unit in the period surrounding the conversion to co-ops occurring between 1995 and 1998, relative to the change in liabilities over the same time period for TOR residents in co-ops converted in 1999.[3] Figure B.1 shows little change in the liabilities of original TOR residents around the conversion to co-ops, suggesting that the purchase of the co-op share was primarily covered through other means than loan taking.

In addition, Figure B.2 shows that among the residents in TOR units in 1994, the fraction of individuals moving during the subsequent years closely follows the churn observed in the control units in Aarhus, Odense, and Aalborg, and the overall moving rate in Copenhagen.

B.3.3 Changes to the Running Costs

While there appear to have been a limited financial burden for residents associated with the conversion to co-op's, the running costs associated with residing in a TOR housing unit changed as a result. Prior to the conversion the running costs consisted of a monthly rent, while the conversion to co-ops meant that the running costs now included interests on the co-op's joint mortgage, costs

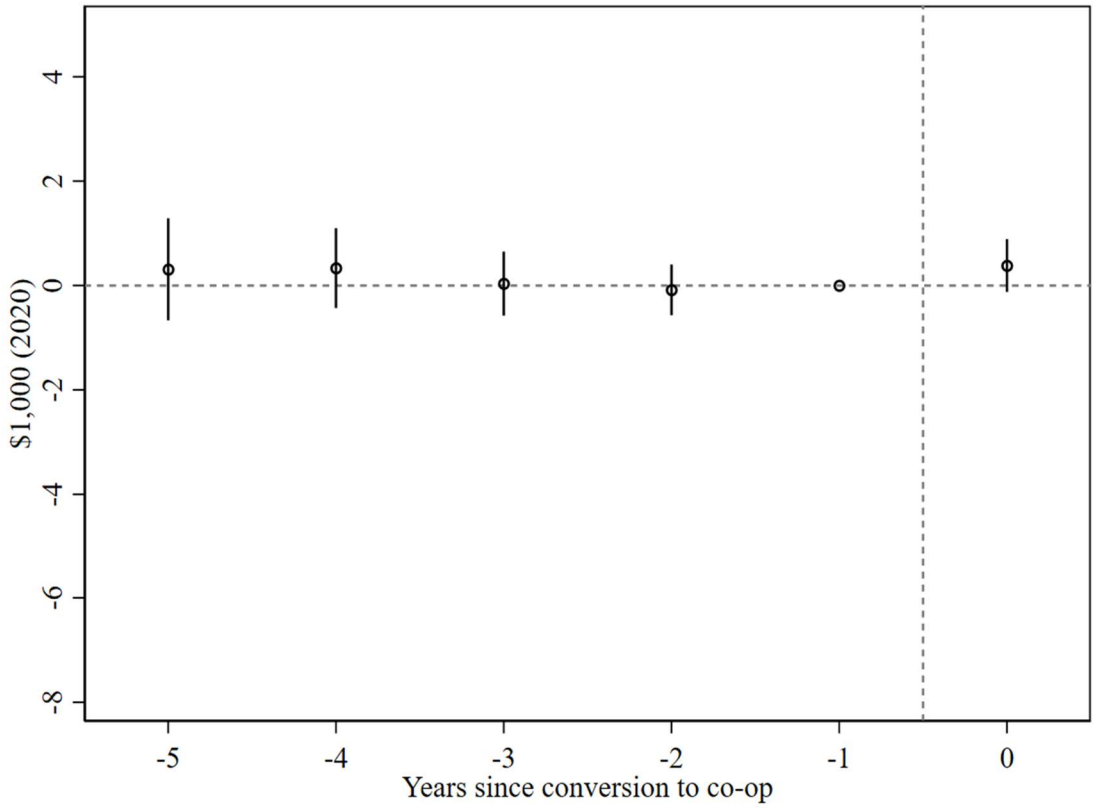
[3] Specifically, Figure B.1 shows estimates of δ_k for $k \in (-5; 0)$ from the following regression: $y_{it} = \sum_{-5,\neq -1}^{1} \delta_k D_{it}^k + f_c + f_t + e_{it}$ where y_{it} is the total liabilities of individual i in the end of year t. The D_{it}^k's are indicators equal to one if t is k years after the conversion to co-op, f_c are co-op conversion year fixed effects and f_t are year fixed effects. The sample covers the years between 1991 and 1998, the event time indicators are binned at -5 and 1 and set to zero for residents in housing converted to co-ops in 1999, such that these individuals are used to identify the year fixed effects.

associated with building management as well as potential interest payments on any loan taken to buy the co-op share (Figure B.1 suggests that this was not very prevalent). According to survey evidence (COWI, 2000) the annual payments related to the co-op mortgage and maintenance corresponded to on average $95 per sqm in 2020-prices, which would correspond to a monthly payment of $493.

B.3.4 Returns from Selling Co-op's

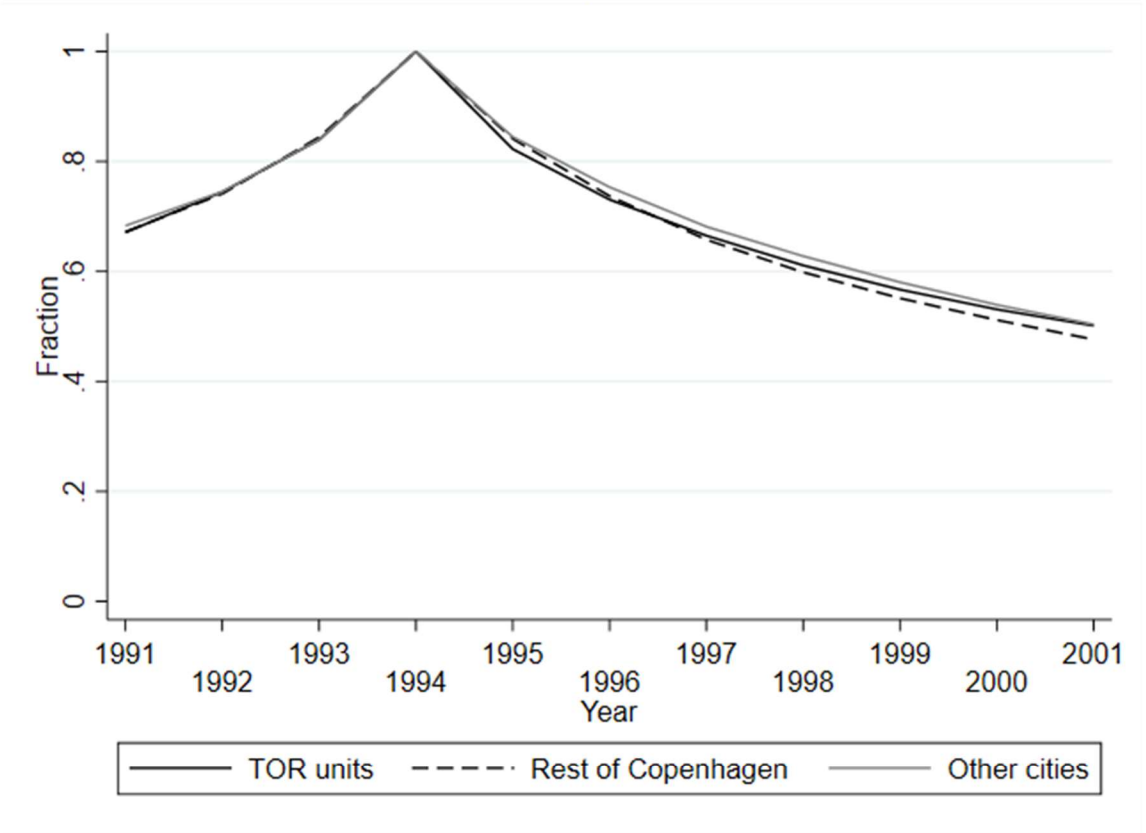
We investigate if there were substantial financial returns to selling the co-op share, by investigating changes to original tenants' wealth when they move away from the co-op. Specifically, we compare the wealth of original tenants in TOR units converted to co-op's between 1995 and 1999, who move away in the first year following the conversion, to the wealth of original tenants in the same housing units who sell one year later. Figure B.3, showing the estimates from this exercise, shows that there are no significant changes to the wealth of original TOR residents when they move away from their co-op. This suggests that the original residents had no financial gains to forming co-ops and selling the housing units.

Figure B.1: Liabilities and Co-op Purchase



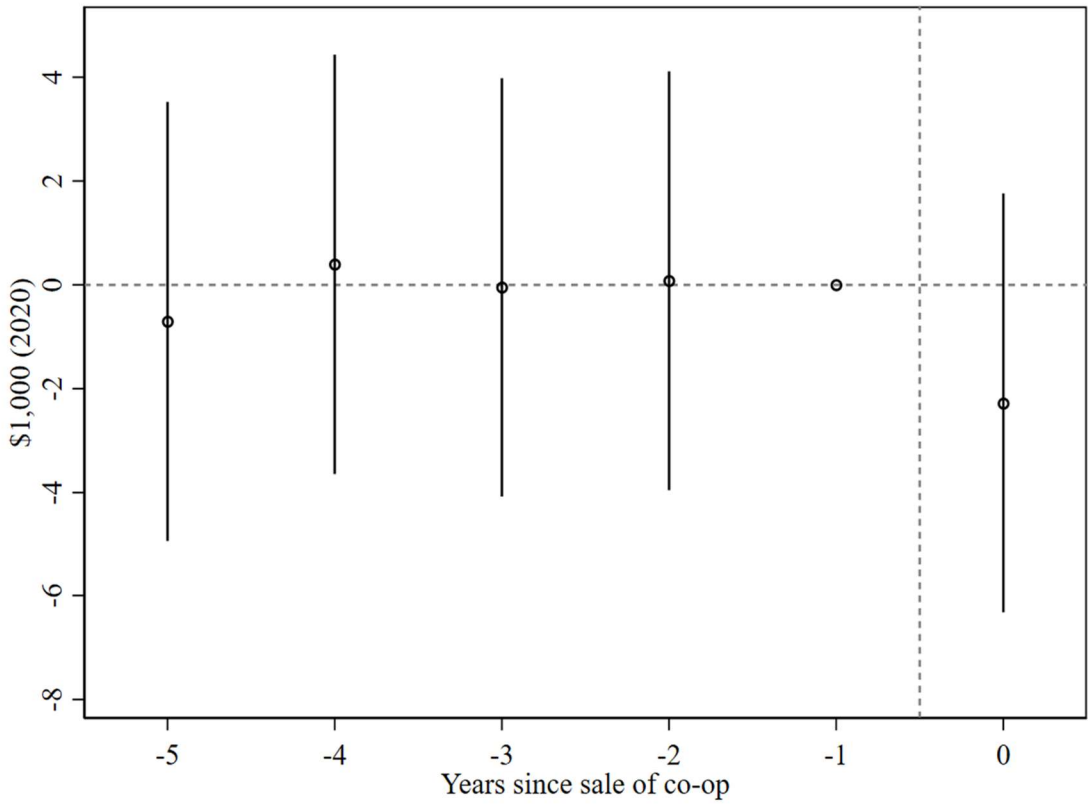
Note: The figure shows estimates of the change in liabilities among original TOR residents in the period surrounding the conversion of the TOR housing units to co-ops, among those converted between 1995 and 1998. Estimates are from difference in differences specifications that compare the liabilities of original TOR residents in housing units converted to co-ops between 1995 and 1998 to the liabilities of original residents in TOR units converted to co-ops in 1999. Vertical lines indicate 95% confidence intervals.

Figure B.2: Fraction of Households that Stay in Same Housing Unit, From 1994 Onward.



Note: The figure shows, from years 1991 to 2001, the fraction of individuals living in the same housing unit as they do in 1994 for residents in TOR units, in the rest of Copenhagen, and in the three control cities (Aarhus, Odense, and Aalborg).

Figure B.3: Wealth around Sale of Co-op



Note: The figure shows estimates of the change in wealth (assets-liabilities) of original TOR tenants who sell their co-op share in the first year after their housing unit is converted to co-op housing, among residents in TOR units converted to co-ops between 1995 and 1999. Estimates are from a difference in differences specification that compares the wealth of original TOR residents who sell their co-op share in the first year after the housing unit is converted to co-op, to the liabilities of original TOR residents who sell their co-op one year later. Vertical lines indicate 95% confidence intervals.

C. Data Appendix

Data Construction:

Housing Data and TOR Units:

Using the BOL register, which includes information for all properties in Denmark with unique (anonymized) addresses, we identify the entire residential housing stock in Denmark from 1991 to 2001. The register provides details such as municipality of location, property use (residential, business, industrial), ownership type (public by municipalities, public by the state, private, co-op), status (rental by private person, rental by company, rental by municipality or state, owner-occupied), property type (e.g., apartment or house), and year of construction. TOR units are identified based on ownership (the Municipality of Copenhagen), use (residential), status (rental by municipality), and year of construction, following the identification strategy used in previous reports on TOR units (VIVE, 2002).

Adding Individual-Level Data:

We merge housing data with individual-level data using the demographic register BEF, which contains unique address and individual identifiers for the entire population, along with demographic characteristics such as age. We supplement this with income and wealth data (earnings and disposable income, assets, and liabilities) from the income register IND, based on tax records and public transfer records, and information on labor market status and educational enrollment from the labor market register RAS. The sample is limited to individuals aged 18 and above. Based on this data, we calculate earnings and assets ranks (for the entire population each year) and construct indicators for whether individuals are employed, studying, neither employed nor studying, and categorize individuals by age groups: 18-34, 35-49, or 50 and older.

Neighborhood Classifications:

We add neighborhood identifiers from Damm and Schultz-Nielsen (2008). These classifications are based on grid data for the entire country, dividing it into clusters that are: i) compact (i.e., without borders such as lakes or highways separating parts of the area), ii) homogeneous in housing types (i.e., not a mix of residential and industrial or a mix of apartment complexes and villas), and iii) containing around 150 households. We also include parish information (larger units of around 2,000-4,000 households) for illustrating TOR exposure in Figure A.1.

Sales Prices:

Sales price data is obtained from the EJSA register, which includes information on all real estate sales, property addresses, and sale prices from 1992 onward. We convert sales prices to logs and calculate the average sales prices within each neighborhood on an annual basis.

Final Study Sample:

The study sample is restricted to the municipalities of Copenhagen, Frederiksberg, Gentofte, Gladsaxe, Herlev, Glostrup, Rødovre, Hvidovre, Tårnby (Greater Copenhagen), and Aalborg, Aarhus, and Odense. This results in a sample of approximately 700,000 housing units.

Neighborhood Types in Greater Copenhagen:

In Sections 5.2 and 6, we consider three neighborhood types:

i) High-Concentration TOR Neighborhoods: Neighborhoods where more than 20% of residential units are TOR units.

ii) Low-Concentration TOR Neighborhoods: Neighborhoods where no more than 20% of residential units are TOR units.

iii) Non-TOR Neighborhoods: Neighborhoods with no TOR units. Since the Municipality of Copenhagen introduced the TOR policy, all neighborhoods in other municipalities are categorized as non-TOR by default.

D. Further Information on Econometric Framework

D.1 Synthetic Control Estimation and Validation

We are interested in estimating the impact of the TOR policy on housing units within each of the 2,055 neighborhoods in Greater Copenhagen. Let $Y_{j,t}^{d}$ denote the potential outcome of neighborhood j, in year t, if the TOR policy happened ($d = 1$) and in the case where the TOR policy did not happen ($d = 0$).[4] For each neighborhood in Greater Copenhagen, we are interested in $\alpha_{jt} = Y_{jt}^{1} - Y_{jt}^{0}$, that is the change in y_{jt} due to the TOR policy. For each neighborhood in Copenhagen, we observe Y_{jt}^{1} in the post policy period, but never Y_{jt}^{0}. Hence, we must estimate Y_{jt}^{0} to obtain an estimate of α_{jt}. We estimate Y_{jt}^{0} by constructing a synthetic control for each neighborhood in Copenhagen (see e.g. Abadie and Gardeazabal, 2003; Abadie, Diamond and Hainmueller, 2010).

Assume that we have a set of N neighborhoods that are unaffected by the TOR policy to which we refer as the donor pool. The Synthetic control for a given neighborhood j, is a weighted average of the outcomes of the neighborhoods in the donor pool:

$$\hat{Y}_{j,t}^{0} = \sum_{n=1}^{N} w_n Y_{nt} \qquad (D1)$$

and the treatment effect of interest can be estimated as:

$$\alpha_{jt} = Y_{jt} - \hat{Y}_{j,t}^{0} \qquad (D2)$$

The core of the synthetic control method is to select a set of nonnegative weights $(w_1^*, \ldots w_N^*)$ summing to one, satisfying that $Y_{j,t}^{0} = \sum_{n=1}^{N} w_n^* Y_{n,t}$.

---

[4] Hence, $Y_{jt}^{d} = \frac{1}{N_j}\sum_{i \in j} y_{it}^{d}$, where N_j denotes the number of housing units in neighborhood j, and y_{it}^{d} denotes the potential outcome of housing unit i in year t as described in Section 4.

D.1.1 Computing the Synthetic Control Weights

Because all neighborhoods in Copenhagen may be affected by the TOR policy, we use 1,191 neighborhoods in the three largest cities in Denmark excluding Copenhagen, namely Aarhus, Odense and Aalborg as the donor pool. We construct a synthetic control separately for each neighborhood in Copenhagen. As proposed by Abadie and Gardeazabel (2003) and Abadie, Diamond and Hainmueller (2010), we choose the set of weights ($W^* = (w_1^*, \dots, w_N^*)'$) that minimizes the Euclidean norm between a given Copenhagen neighborhood (j) and neighborhoods in the donor pool:[5]

$$\left\|X_j - X_0 W\right\| = \left(\sum_{h=1}^{k} v_h\left(X_{hj} - w_1 X_{h1} - \cdots - w_N X_{hN}\right)^2\right)^{\frac{1}{2}} \qquad (D3)$$

subject to the restriction that weights are nonnegative and sum to one. In Eq. *D3* X_{hj} denotes one of the k predictors in X_j, and v_h the relative importance of the synthetic control put on reproducing the value of predictor k of the treated neighborhood. We include earnings rank, employment rates and average age of the neighborhood as separate predictors for each year between 1991 and 1994 as predictors and we choose $V = (v_1, \dots, v_N)'$ to minimize the mean squared prediction error between the synthetic control on the observed earnings rank of the treated neighborhood in the years leading up to the TOR policy.

[5] For computational feasibility, we draw a 20% random subsample of all the neighborhoods in Aarhus, Odense and Aalborg to use as a donor pool for each neighborhood in Copenhagen.

E. Benchmarking the Number of Affected Housing Units

In Sections 5 and 6 in the main text, we show that the TOR policy led to substantial changes in the composition of residents across housing units and neighborhoods in the subsequent years. For example, the fraction of 18-34-year-olds increased by more than 10 percentage points and the earnings ranks increased by around 6 percentile ranks among residents in TOR units, the earnings ranks in low- and high-concentration TOR-neighborhoods increased on average by 2 and 3 percentile points, respectively, and the average earnings ranks of residents changed among several non-TOR neighborhoods.

This appendix presents a simple framework, which benchmarks the number of housing units affected by a shock across time and illustrates the potential role of spillovers between the directly affected housing units and the surrounding city.

E.1 Framework

Let N_{shock} be the number of housing units in the policy shock, N_{Tot} the total housing mass in the city, π the ratio N_{shock}/N_{Tot}. Each year a fraction r of households are replaced. Following the shock, the inflow changes completely in the directly affected housing units while there is a spillover to the housing units where the new residents come from by $0 < p < 1$. Time since the shock is t. The fraction of housing units in the city affected by the shock at time t equals:

$$y_{t,p} = \underbrace{\pi * r * (1-r)^{t-1}}_{Direct\ effect} + \underbrace{y_{t-1,p} * p}_{Direct\ spillovers} + \underbrace{y_{t-1,p} * p^{t}}_{Indirect\ spillover} \quad \text{(E1)}$$

where $y_{0,p} = 0$. The number of housing units affected at a given time depends on three terms.

The first term, $N * r * (1-r)^{t-1}$, is the direct effect on the housing units subject to the shock. At each time t, a fraction of residents, r, are replaced out of the original residents, $(1-r)^{t-1}$, who are still residing in the units.

The second term, $y_{t-1,p} * p$, is the direct spillovers capturing that the changed residents from the direct effect of the shock would have resided elsewhere leading to changed composition in those housing units as well.

The third term, $y_{t-1,p} * p^t$, captures indirect spillovers when, for example, some housing units not directly affected by the shock become more or less attractive to live in. To capture this, an addition layer of spillovers given by a fraction $p^t < p$ is introduced at each time t.

E.2 Simulation

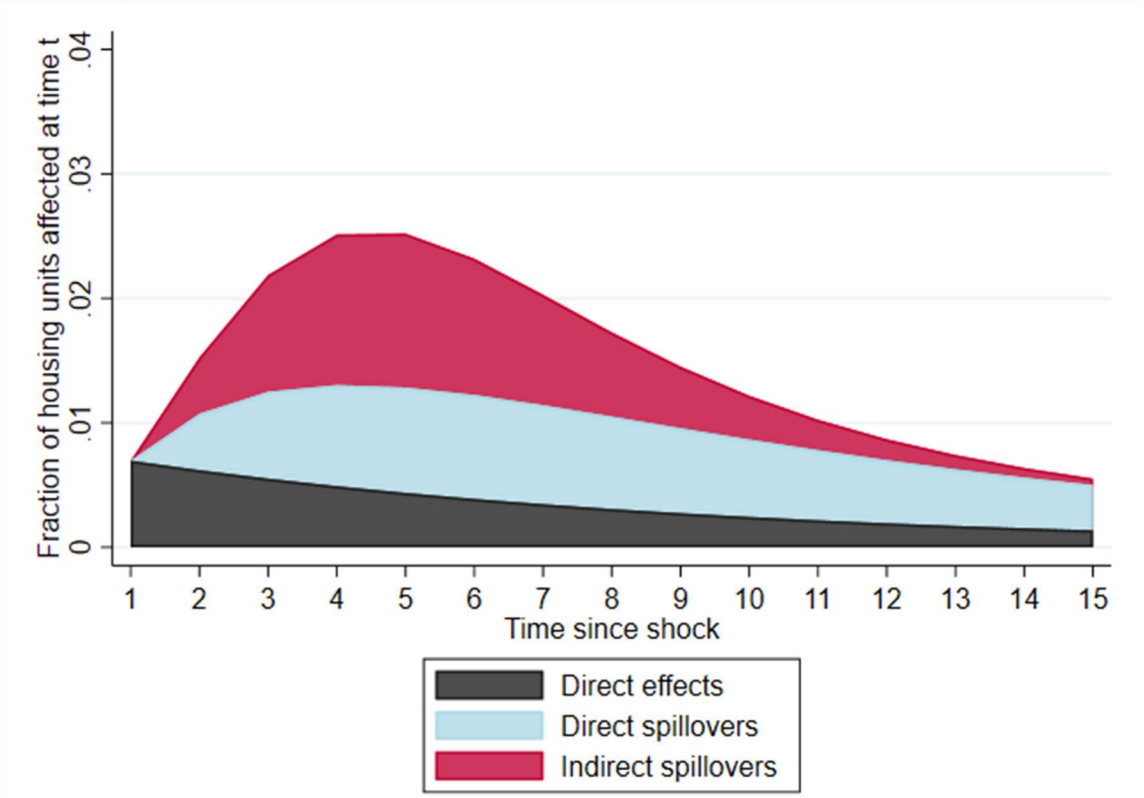
The TOR policy targeted approximately 19,000 housing units in Copenhagen with a total housing mass around 300,000 housing units providing N_{shock}, N_{Tot}, and π in Equation (D.1). Furthermore, we set $r = 0.1$ as Figure A.2 shows that around 11 percent of residents move each year.

The final parameter is *p*, which captures spillovers to the other housing units than the TOR units. While the effects of the shock in principle will continue indefinitely according to Equation (E1), the peak in the yearly fraction of housing units in the city affected by the shock is given by *p*, which we can use to obtain a reasonable range of *p*. Figure 6 shows that the effects of the TOR policy on non-TOR housing units in TOR neighborhoods gradually taper off between year 1999 and 2001, which corresponds to 4-6 years after the policy introduction. Setting $r = 0.11$ and $\pi = 19000/300{,}000$ thereby implies that *p* falls between 0.55 and 0.75. We chose the midpoint in this range in the figures below and set $p = 0.65$.

Figure E.1 shows the fraction of housing units affected by the shock across a 15-year period. The figure shows that all changes are driven by the direct effect during the first year following the shock. However, within a few years the spillover effects begin to dominate – initially the direct spillover effects and subsequently the indirect spillover effects. Finally, in the longer run, all effects die out gradually.

Summing over all years until year 6 (corresponding to 2001) implies that approximately 12 percent of all housing units have changed residents due to the policy. Around 3 percent of all housing units are the directly affected units (TOR units). The direct spillovers from the shock hit 4 percent of the housing mass, and the remaining 5 percentage points can be attributed to indirect spillovers.

Figure E.1: Fraction of housing units with new residents following a shock.



Note: The figure shows the simulated values of Equation (1) setting $\pi = \frac{19000}{300{,}000}$, $r = 0.11$, $p = 0.65$. The grey area shows the direct effects on the housing units hit by the shock: $\pi * r * (1 - r)^{t-1}$. The light blue area shows the direct spillovers, which have been estimated as the term $y_{t-1,p} * p$ in a model ignoring the indirect spillovers: $\pi * r * (1 - r)^{t-1} + y_{t-1,p} * p$ to avoid that the direct spillovers are gradually inflated by the indirect ones through $y_{t-1,p}$. Finally, the read area shows the indirect spillovers, which have been estimated as the difference between $\pi * r * (1 - r)^{t-1} + y_{t-1,p} * p$ and $\pi * r * (1 - r)^{t-1} + y_{t-1,p} * p + y_{t-1,p} * p^t$.