Revitalizing Poor Neighborhoods: Gentrification and Individual Mobility Effects of New Large-Scale Housing Construction*

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Abstract

Using over three decades of full-population yearly registry data on individuals and residential buildings in Sweden, this paper examines whether new large-scale housing construction is a suitable policy tool for revitalizing poor neighborhoods. The answer is yes. New market-rate condominiums, which increased the population in the poorest quartile of neighborhoods by 15%, reduced the excess poverty rate by 44% and the mean income gap relative to the city as a whole by 52%. The effect was not only driven by richer people moving into newly built apartments but also by higher incomes in pre-existing homes. We rule out other types of concurrent housing-stock changes, such as demolitions or renovations, suggesting that the new supply of owner-occupied homes made the areas more attractive. The gentrification effect was stronger in the poorest neighborhoods. In terms of migration, we find no displacement of incumbent poor residents. Instead, gentrification was driven by high-income people moving in from richer areas. However, locals were over-represented in the new homes, which provided housing-career opportunities even to incumbents. Finally, our findings show that there were smaller gentrification effects in areas with new rental properties; but unlike the case with condominiums, there were offsetting cannibalizing effects on nearby areas.

Keywords: Housing construction, Gentrification, Individual Mobility, Displacement, Spatial inequality, Poor neighborhoods

JEL Classification: R31, R21, R52

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1 Introduction

Growing up and living in areas of concentrated poverty negatively impacts health and labor market outcomes (see Chyn and Katz, 2021 for a review), highlighting the need to support disadvantaged individuals while also improving local conditions to reduce spatial inequalities. In response, many countries have recently taken steps to identify and assist marginalized areas—for example, France's "priority districts," the Netherlands' "extraordinary measures for urban problems," Denmark's "ghetto list," and Sweden's "vulnerable areas list." Place-based policies, often implemented in distressed areas, have shown mixed results and could involve the stimulation of local businesses and job creation (e.g., Busso et al., 2013; Briant et al., 2015), the promotion of economic and social inclusion (e.g., Romero and Noble, 2008; Alonso et al., 2019), and the enhancement of public spaces and infrastructure (e.g., González-Pampillón et al., 2019; Balboni et al., 2021). Governments also attempt to revitalize struggling areas through physical interventions, such as new housing construction, demolitions, and renovations of existing homes. While housing policies may yield significant benefits by attracting a socioeconomically diverse population, a gentrifying process might increase local living costs, potentially displacing incumbent residents and undermining efforts to uplift the entire community.

A growing body of empirical research examines the neighborhood effects of housing policies, including demolitions (Aliprantis and Hartley, 2015; Almagro et al., 2023), large-scale new housing construction (Diamond and McQuade, 2019; Singh, 2020; Pennington, 2021; Li, 2021; Asquith et al., 2023), multi-family housing renovations (Dahlberg et al., 2023), and comprehensive revitalization programs that integrate these and other interventions (Rossi-Hansberg et al., 2010; Staiger et al., 2024). Studies on new housing construction have primarily examined its causal effects on housing prices and rents, as these indicators reflect changes in location value and housing costs. Only Asquith et al. (2023) has focused on low-income areas, investigating the impact of new rental properties with an emphasis on rent effects. However, a primary aim of revitalization policies is to enhance the residential mix in segregated areas by attracting new population groups without displacing current residents. Data limitations have hindered direct causal analysis of residential sorting effects. Our study aims to fill this gap by analyzing how new owner-occupied and rental apartments affect the residential composition in low-income neighborhoods and influence individual migration patterns.

We use yearly registry-based microdata that are unusually rich in geographic and other background information on all housing properties and all individuals in Sweden over a long time period (1992–2022). This allows us to precisely measure the spatial distribution of residents in terms of income and other socio-demographic characteristics, as well as year-to-year migration streams between residential buildings and areas. Pennington (2021) and Asquith et al. (2023) overcame many limitations of prior research, which used

U.S. decennial census data at aggregated geographical levels, by utilizing longitudinal address histories for individuals. This approach enabled them, for example, to approximate gentrification as in-migration from affluent areas and displacement as out-migration to low-income areas. In ongoing work related to ours, Staiger et al. (2024) accesses annual individual-level income and residential data to evaluate the HOPE VI revitalization program in the U.S., which funds the demolition of public housing projects and the construction of new subsidized and owner-occupied homes in low-income areas.

We focus on urban neighborhoods in the bottom quartile of disposable income, with the highest poverty rates (defined by the population share in the lowest citywide decile). We employ a difference-in-differences strategy, comparing 141 areas with large housing developments (containing more than 100 residents), which resulted in population gains of approximately 15%, to control areas from the same city. We analyze how new housing affected the residential composition in the immediate surroundings (DeSO areas with approximately 2,000 residents) and adjacent areas in the wider neighborhood (RegSO areas with about 5,000 residents), as well as migration streams to the new buildings and between neighborhoods.

Our main finding is that co-ops, the Swedish equivalent of market-rate condominiums, had strong gentrifying effects. The estimated impact on the area share of poor residents in the bottom decile of the city is a reduction of 1.7 percentage points, corresponding to a 44% decrease in the excess poverty rate. New co-ops also increased mean income by 7.7%, narrowing the income gap relative to the city mean by 52%. The gentrification effect was driven not only by richer individuals moving into the new condominiums but also by changes in the residential composition of pre-existing homes, where the share of poor residents fell by 0.97 percentage points, and mean income rose by 2.7%. Socio-demographically, new co-ops improved the population mix by reducing the share of immigrants born outside Europe, raising education levels, and increasing the share of the working-age population. Regarding effect heterogeneity, the estimated gentrification effect was more substantial in the poorest neighborhoods, which are of the most significant policy interest, and we also find positive effects in areas with high shares of rental units or residents born outside Europe.

An analysis of housing outcomes reveals that the new condominiums were not accompanied by housing demolitions, renovations, or conversions to a different tenure type. Potential reasons for the gentrifying effect of the new supply of owner-occupied homes include the new buildings and accompanying infrastructure enhancing the area's physical appearance, the addition of better amenities such as restaurants, cultural activities, schools, and other public services, as well as an initial shift in the socioeconomic composition that makes the area more appealing to richer residents. With both supply and demand effects at play, theoretical predictions on housing costs remain ambiguous. We estimate small but imprecise effects on rents and sales prices of existing owner-occupied apartments, but mechanically higher sales prices for new homes.

Regarding individual mobility, we find no changes in out-migration patterns, neither in the number and income distribution of out-movers to different destination areas nor in the income levels in these destination areas. Thus, we find no signs of displacement. Instead, gentrification was driven by richer people moving in from more affluent areas outside the wider neighborhood, both to new and existing homes. Given that the new supply increases the total housing stock, we also do not observe fewer low-income in-movers, although their share of in-movers is lower. While local incumbents made up a small share of all residents in the new homes, they were over-represented in the new homes by a factor of five. Thus, the new homes provided housing-career opportunities, even to incumbents, allowing higher-income locals to improve their housing standards without leaving the area.

Finally, we also found local gentrification effects in areas with new rental properties; these effects are about half the magnitude of those observed with new co-ops and were entirely driven by residential changes in existing homes. However, unlike the case with new co-ops, there were offsetting cannibalizing effects on nearby areas, resulting in a zero net impact for the wider neighborhood as a whole.

Our findings are broadly consistent with previous U.S. studies showing positive gentrification effects of new housing on sales prices of owner-occupied homes in low-income areas (Rossi-Hansberg et al., 2010; Diamond and McQuade, 2019), but counteracting supply effect preventing rising rent (Pennington, 2021; Asquith et al., 2023; Li, 2021). Our results also align with the lower probability of out-migration to poor areas following new housing construction in San Francisco that Pennington (2021) found, although we more forcefully dismiss displacement by showing that low-income people remain in the area. Like Asquith et al. (2023), we find that new homes are attractive for high-income families, even when located in low-income areas. However, we find rising in-migration from more affluent areas (and can confirm that in-movers have high incomes) rather than from low-income areas as they observe. This difference may be explained by their focus on new rental developments.[1]

Taken together, our results demonstrate that the new construction of privately owned homes can be a very successful policy tool for revitalizing deprived neighborhoods and preventing residential socioeconomic segregation. One concern in the broader previous research on place-based policies is that interventions empowering individuals might not revitalize communities due

[1]One can also connect our study to the moving chain literature studying how different population groups across space take advantage of vacancies created by households moving into newly built homes. Several studies showed that even poor people from poor neighborhoods benefit from moving chains created by expensive new homes in affluent areas (Rosenthal, 2014; B. et al., 2023; Mast, 2023; Kindström and Liang, 2024). Our mobility results alleviate potential concerns about a segregated housing market (Piazessi et al., 2020), where certain types of homes in specific areas primarily benefit specific socioeconomic groups, thereby limiting citywide benefits.

to the out-migration of successful individuals (R.-A. and V., 2022). The typical pattern is that once individuals in distressed neighborhoods succeed, they tend to move to better housing in more attractive neighborhoods. In contrast, we find that new market-rate homes allow successful residents to make a local housing career. The primary fear driving resistance to new housing construction among residents in targeted areas is that the benefits of the policies might not accrue to or even harm them, especially the economically vulnerable. However, we find no signs of rent increases or changes in out-migration patterns; thus, we observe improved neighborhood quality without displacement. Another potential side effect of new housing is the risk of adverse citywide effects on other socioeconomically weak areas and people. However, since the gentrification effect is driven by the in-migration of richer residents from more affluent areas, it reduces, rather than raises, residential segregation elsewhere in the city. Finally, we also refute concerns about adverse distributional effects on low-income residents in other areas, who might lose the opportunity to move into an existing home in the target area when the share of rich in-movers increases—an inherent effect of gentrification through housing renovation or regeneration.

Given that other housing improvements were not more common in our treated areas, we estimate an unusually clean effect of new construction. This effect applies to in-fill developments in densely populated areas or neighborhood expansion in areas with more available land, such as those close to city borders. Our results are also useful for evaluating the new construction component of multi-intervention programs that may involve demolishing existing homes to make room for new housing.

In Sweden, municipalities exert tight control over the housing supply, possessing extensive discretionary power over development plans and building permits. However, we find only weak patterns in the location of new large residential properties across areas with different socioeconomic characteristics, suggesting a significant opportunity to use housing construction more strategically to influence residential sorting patterns.

While we believe our results apply to other countries considering government-driven housing construction or the strategic regulation of new home locations, Sweden has a unique rent-setting system in which rents are renewed annually through national negotiations between landlord and tenant representatives. In practice, rapid rent increases in response to higher demand are likely more difficult, and we cannot rule out displacement risks under free-market rent regimes. Nevertheless, previous research showing a dominant supply effect that suppresses rents following new housing construction under freer rent-setting systems helps mitigate this caveat.

The rest of the paper is organized as follows: The next section provides an institutional background. Section 3 describes the data and Section 4 provides the empirical strategy. Results are reported in Sections 5 and 6, and the final section concludes.

2 Institutional background

Swedish local governments (municipalities) are responsible for city development, as outlined in detailed development plans, where they also set upper limits for new housing construction. This authority, commonly known as the "plan monopoly," grants them significant control over urban planning. By law, municipalities are required to ensure the availability of adequate high-quality housing (Swedish Code of Statutes, 2000). They also influence the housing supply by selling land. Currently, municipal housing companies own about half of the total rental stock, operating in 270 out of 290 municipalities.

A residential property is a defined plot of land that typically includes a primary residential building (or multiple semi-attached buildings) along with auxiliary non-residential structures, such as a recycling station or parking garage. Residential properties are formally categorized into two main types: one- or two-family properties (detached or semi-detached houses) and multi-family properties, which accommodate at least three households. For simplicity, we will use the terms "residential property," "building," and "house" interchangeably.

In Sweden, there are three primary types of housing tenure. First, individuals can own their own home in a one-family or two-family house. Second, ownership may involve a home, typically an apartment, within a housing cooperative (also known as a co-op). Membership in a co-op grants individuals the right to live in the apartment (corresponding to their share in the co-op) indefinitely. These apartment shares can be freely bought and sold on the market. This type of tenure is akin to owning a condominium in the U.S. context. Third, individuals can rent their homes either from a private landlord or a public (municipal) landlord. In the absence of social housing, as seen in the U.S. or U.K., lower-income residents often reside in rental apartments owned by municipalities. The tenure type in multi-family buildings is typically either co-op or rental.[2]

The classification of townhouses is more complex, as they can be either semi-attached or semi-detached, depending on factors such as shared entrances, ventilation systems, and attics. Most townhouses are categorized as one- or two-family properties, with each home individually owned. However, a significant number are classified as multi-family properties with co-op or rental units.

A significant proportion of homes today were built during the government-driven "Million Homes Program" (1965–1974). However, construction rates dropped sharply after the financial crises of the 1990s. Following deregulations during this period, many housing companies privatized a large share of their stock by converting rental apartments into co-ops. Since the early 2000s, construction has gradually increased again, primarily driven by private equity financing new co-op developments. In 2017, 52% of the population lived in

[2]In mixed-tenure buildings, it is common for a co-op and its members to collectively own the rental apartments.

owner-occupied detached or semi-detached houses, 16% in co-ops, and 32% in rental apartments.[3]

In Appendix A1., we describe the often lengthy process involved in building new multi-family houses in Sweden, highlighting the need to account for potential anticipation effects.

3 Data and descriptive statistics

3.1 Data

We use annual end-of-year data covering the entire Swedish population from 1992 to 2022, along with data on all residential properties from 1998 to 2022, sourced from Statistics Sweden. By linking individuals to their residential properties through social security numbers and registered addresses, we can track their residence at the end of each year. Additionally, we incorporate apartment sale price data from 2005 to 2021, provided by Svensk Mäklarstatistik AB—a company owned by two brokerage firms and two trade associations for brokers. We also utilize a smaller sample of rent data from Stockholm's public housing agency, covering the years 2005 to 2014.

The individual-level data come from RTB and LISA, microdata registries at Statistics Sweden, which are accessible to researchers at Swedish universities. These datasets encompass a diverse range of economic and demographic variables, including income sources, taxes and transfers, birth year, country of birth, educational attainment, and marital status.

Data on residential properties are sourced from the Property Registry, which is maintained by the Swedish Mapping, Cadastral, and Land Registration Authority. This dataset includes a property identifier to which addresses can be linked, as well as geographic coordinates with a resolution of 100 meters and property type (multi-family building or not). Additional details—such as the judicial owner, construction year, size, standard, and assessed value—are obtained from the Property Taxation Registry, which is compiled and used by the Swedish Tax Agency for taxation purposes. While a multi-family property can include both co-ops and rental units, we focus solely on the vast majority of new residential properties with a single tenure type.

The apartment sales data cover approximately 95% of all sales in Sweden, with prices reported by real estate agents after transactions close. We have access to final sale prices and property characteristics, including living area, number of rooms, construction year, and monthly fees. Additionally, the dataset includes home addresses. Our rent data cover homes that changed tenants between 2005 and 2014 in Stockholm County. While we cannot directly link sales and rent data to individuals or properties in our registry data, we can construct area-level datasets that enable meaningful analysis.

[3]These statistics are based on our own calculations using data from Statistics Sweden, which we present in the next section.

3.2 Treated areas with pioneering buildings

In 2018, Statistics Sweden divided Sweden into approximately 6,000 DeSO areas, which we use as the definition of (micro) neighborhoods. These areas had an average population of about 1,700, though some were sparsely populated or uninhabited in 1992 (31 DeSO areas). The DeSO division ensures similar population sizes while also considering natural spatial barriers such as streets, railroads, and water bodies. Additionally, the borders align with the 290 municipalities and respect previous urban boundaries. However, DeSO areas serve no administrative purpose and do not have official names.

At the request of the Swedish government and in collaboration with municipalities, Statistics Sweden also aggregated DeSO areas into 3,363 RegSO areas, which we use to define wider neighborhoods. The purpose of this classification is to standardize the collection of socioeconomic statistics at this level and to monitor trends in segregation over time. RegSO areas are named and closely align with various previous formal and informal definitions of city districts, as well as popular perceptions of neighborhoods. A typical RegSO area typically includes an elementary school and a district center offering public and private services, such as a medical center, postal services, and shops.[4]

We restrict ourselves to the main urban area of each municipality, which gives us 4,324 DeSO areas located in "cities" or "towns." We operationally define multi-family buildings as properties with over 50 residents, and this criterion excludes most detached and semi-detached houses as well as townhouses.[5]

Large developments are typically initiated and tightly controlled by municipalities and are less endogenous to decisions of smaller actors, such as firms and households building new homes in areas with specific trends. They also have more significant impacts on the neighborhood. This motivates an empirical focus on them, and we limit our attention to developments involving more than 100 residents living in new multi-family buildings in the same DeSO area. Each of these developments may involve one or several multi-family buildings constructed within a six-year time span.

Defining event year 0 as the construction year of the first multi-family building in a large development, we track developments over a twelve-year window—six years before and six years after construction (i.e., event years -6 to +5). We can follow each large development built between 1998 and 2017 for the entire time window. Of the 3,701 multi-family buildings constructed during this period, 2,149 belong to large developments in 958 unique DeSO areas and 682 wider RegSO areas.

New homes can be built continuously over a long time in a neighborhood,

[4]Other administrative and non-administrative area definitions exist for elections, religious purposes, schooling, and the housing market. However, these definitions—such as election districts, parishes, SAMS, and NYKO—are not standardized across municipalities or over time.

[5]Our results are insensitive to using the formal multi-family classification used by authorities, which somewhat arbitrarily include some townhouses and not others.

Table 1: Treated DeSO areas with large developments by city size and period

	(1)	(2)	(3)	(4)
Years/Population:	>250k	50k-250k	<50k	Total
1998-2001	18	38	15	71
2002-2005	29	54	27	110
2006-2009	41	45	33	119
2010-2013	28	44	26	98
2014-2017	29	80	44	153
Total	145	261	145	551

Note: A large development is defined as a new residential project that provides housing for more than 100 people.

and large developments in a DeSO area might have 12-year time windows that overlap. Spatial overlap with other large developments in an adjacent DeSO within the same RegSO area is also common. We work with treated DeSO areas where the first building—henceforth the pioneering building—is the first new multi-family building in its DeSO and RegSO area in 12 years. This restriction ensures that there is no preceding large development with an overlapping DeSO area window in the same RegSO area.

Several DeSO area time windows with large developments initially had little or no population. Since our focus is on the impact on existing neighborhoods rather than the creation of new ones, we include only areas with at least 500 residents each year. Our empirical strategy, detailed in the next section, also requires each treated area to have at least one similar untreated control area within the same municipality, with over 500 residents and a comparable income level. Applying these criteria, we obtain a final sample of 551 treated areas with non-overlapping DeSO area time windows where the pioneering multi-family building in each case contains either co-ops or rentals.

Table 1 presents the distribution of treated areas with large developments across cities (and towns) of varying sizes and time periods. Of the 551 treated areas, 145 were located in Sweden's three largest cities—Stockholm, Gothenburg, and Malmö (each with populations exceeding 250,000). Another 261 were located in mid-sized cities (with over 50,000 residents), while 145 were located in smaller cities (with fewer than 50,000 residents). As of 2022, Sweden's population of 10.4 million was distributed as follows: 1.9 million in the three largest cities, 2.8 million in mid-sized cities, 3.2 million in small cities, and 2.5 million outside urban areas. Large housing developments were thus more frequent (per capita) in large and mid-sized cities than in smaller ones. Additionally, Table 1 shows that the construction rate has increased over time.

We are interested in people's living conditions, and to this end, we focus on disposable income, which reflects their purchasing power. Disposable income is defined as pre-tax income minus taxes, plus transfers. Pre-tax income encompasses all recorded sources of income, with labor and capital income being the primary components.

We use individual income rather than family or household income—not only because it is more consistently recorded over time, but also to avoid several complications: (i) the large number of unmarried cohabiting couples, with or without children, in Sweden; (ii) the varying number of members across families and households; and (iii) the instability of family units over time, largely due to Sweden's high and changing divorce rates. When constructing area income measures, we include individuals aged 21 and older. [6]

Our primary area-level outcome variable is the poverty rate, defined as the share of residents in a DeSO area who belong to the bottom citywide decile of the disposable income distribution. This measure captures the concentration of individuals from the lower end of the *citywide* income distribution within each area.[7]

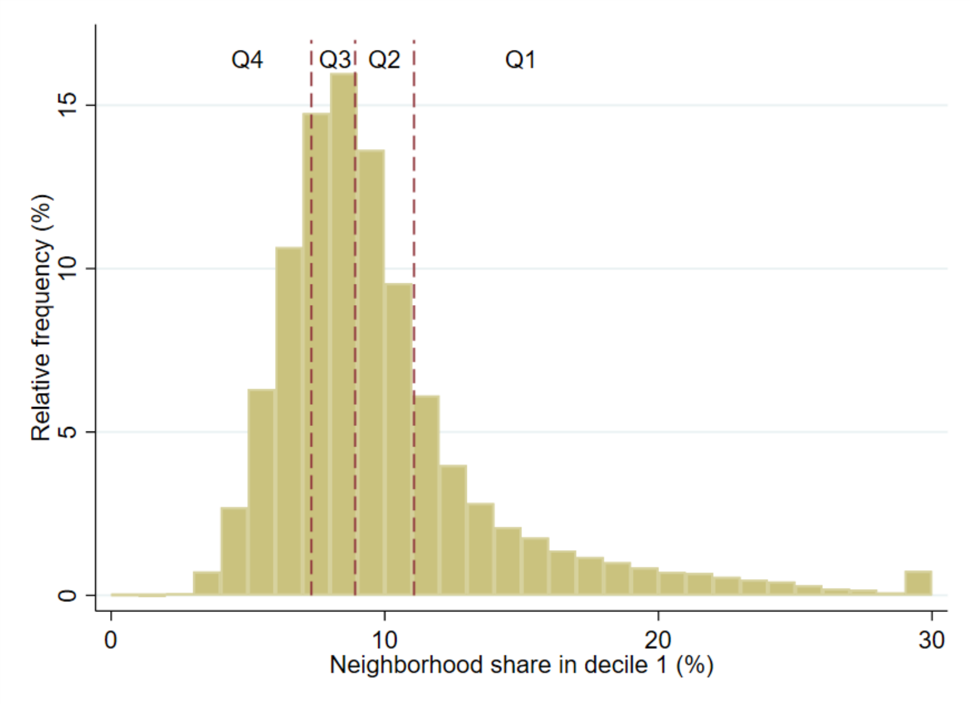
In addition to the poverty rate, we also report results based on the mean disposable income at the area level (deflated to the 2022 price level), which reflects the entire income distribution within an area. Given that income distributions are typically right-skewed, area mean income places relatively greater weight on the presence of high-income earners than low-income earners. For ease of interpretation, we primarily use the logarithm of area mean disposable income, ln(income), which allows for estimates to be interpreted in terms of proportional effects.[8]

To identify pre-treatment poor areas of interest, we rank areas across Sweden based on their poverty rates. Specifically, we consider all areas located in cities that include treated areas in event year -2, which we define as the base year (choosing event year -2 rather than -1 to avoid potential anticipation effects). When ranking areas, we apply population weights to account for differences in area size. Figure 1 presents the resulting distribution of area poverty rates, including the quartile cutoffs. For our analysis, we focus on poor areas in the lowest income quartile—that is, areas with the highest poverty rates (Q1 areas)—where poverty rates range from just above the citywide average of 10% to over 30%.

[6]In Appendix A4., we show that our results are robust to alternative income measures.

[7]We consider the spatial distribution of a city's relatively poor residents to be potentially influenced by urban planning policies. Moreover, we view relative poverty within the city as more relevant than relative poverty based on the national income distribution.

[8]Since mean area income is never zero—unlike individual income—we do not face the issue of the logarithm of zero being undefined.



Note: Q1 to Q4 refer to areas classified by income quartile, where Q1 corresponds to the lowest income quartile with the highest poverty rate. The poverty rate is defined as the share of individuals in an area who belong to the bottom citywide decile of the disposable income distribution. The sample includes all areas located in cities with treated areas in event year -2. The x-values are right-censored.

Figure 1: Share of poor in urban neighborhoods

To investigate patterns in the spatial distribution of large residential developments in Sweden between 1998 and 2017, Table 2 reports, by area income quartile, the number of treated areas that received new pioneering co-op and rental multi-family buildings. The table shows that more new co-ops were built than rentals, reflecting, in part, the liberalization of Swedish housing policy since the early 1990s. Additionally, a somewhat higher number of new developments have been built in lower-income areas compared to higher-income areas. However, the overall pattern suggests that the placement of new developments has not systematically targeted either the revitalization of disadvantaged neighborhoods or the exploitation of high-demand areas to maximize profits.

Table 2: Pioneering buildings by income quartile and tenure type

	(1) Q1 areas	(2) Q2 areas	(3) Q3 areas	(4) Q4 areas	(5) All
Co-ops	73	96	81	83	333
Rentals	68	73	39	38	218
Total	141	169	120	121	551

Note: See the note in Figure 1 for a description of how area income quartiles are constructed.

Figure 2 presents a map of the 77 treated urban areas (out of 504) in Stockholm, Sweden's capital. Areas are color-coded by income quartile, ranging from dark red (lowest quartile) to dark blue (highest quartile), and patterned to distinguish the type of pioneering building: co-ops are shown without patterns, while rentals are marked with a grid pattern. The figure shows that new co-ops have been less prevalent in the two lowest quartiles compared to the two highest quartiles (23 vs. 29 treated areas). In contrast, new rentals have been more frequently built in lower-income areas, with 15 treated areas in the bottom two quartiles (Q1 and Q2) compared to 10 treated areas in the upper two quartiles (Q3 and Q4).

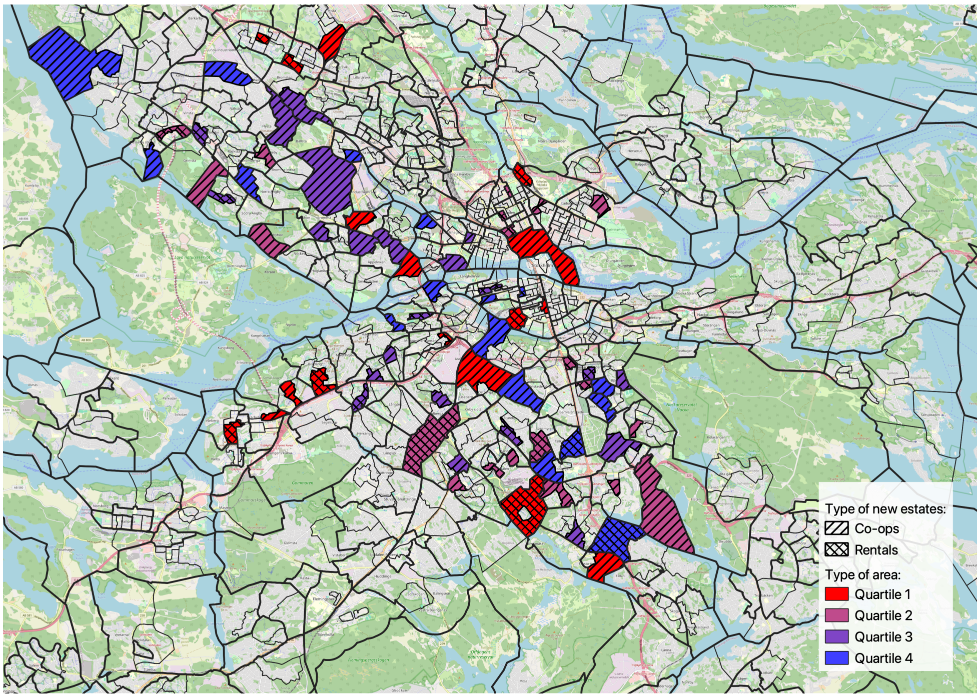


Figure 2: Areas with new large developments in Stockholm 1998-2017

In Appendix A2., we report and discuss summary statistics. The tables reveal, among other findings, that new co-ops and rentals in affluent areas tend to attract fewer low-income residents and more high-income residents compared to similar developments in poorer areas. However, the income premium associated with new co-ops in affluent areas is surprisingly modest, given that high-income individuals typically have the means to avoid areas with concentrated poverty. This finding is encouraging for urban planners aiming to promote social mixing in disadvantaged neighborhoods through new housing developments.

4 Empirical strategy

4.1 Difference-in-differences with area-specific controls

When examining how new housing transforms a neighborhood, it is essential to acknowledge that the location of new developments is rarely random; instead, it is often influenced by local characteristics and trends. Developers tend to build where expected housing prices are highest relative to land costs—whether in desirable, high-priced neighborhoods or in less attractive, lower-cost areas. Local governments, which issue building permits and control significant portions of land, also play a crucial role in shaping development patterns by either promoting or restricting new housing in specific locations. In Sweden, we believe that government policy may have a more significant influence than market forces in determining where large residential projects are located. However, whether the government has strategically used new housing developments to influence neighborhood composition remains an open empirical question. In urban areas, factors such as land availability and ownership structures likely played a key role.

Because we have access to panel data, we can track treated areas with large housing developments over time, both before and after the introduction of new housing. However, simple before-and-after comparisons in treated areas capture not only the effect of new housing but also other time-varying factors, such as general income growth. To address this, we employ a difference-in-differences strategy. This approach approximates the counterfactual trend of the treated areas (had new housing not been added) by using the observed trend of a comparable group of control areas. It then isolates the treatment effect by subtracting the before-after difference in the control group from the corresponding difference in the treated group. The key identifying assumption underlying this strategy is that, in the absence of treatment, the treated and control areas would have followed parallel trends over time.

Previous studies have often employed a "ring difference-in-differences" approach, which compares an inner treated ring located very close to a new building with an outer control ring situated slightly farther away (e.g., Diamond and McQuade, 2019; Pennington, 2021; Asquith et al., 2023). The idea is that within a limited geographic area, developers face limited options for new construction sites, making the precise placement of new buildings plausibly unrelated to underlying trends. Typically, the inner ring is defined with a radius of approximately 200 to 500 meters. This literature consistently finds that the effects of new housing developments are highly localized.

A potential limitation of the ring difference-in-differences approach is that the spatial proximity between the inner and outer rings increases the risk of spillover effects, which can contaminate the control group. For example, new housing may attract additional services that benefit households in both rings or in-migrating residents who settle in the inner ring may have otherwise chosen to live in the outer ring. Expanding the radius of the rings to avoid such spillovers reduces the comparability between treated and control areas.[9] In the Swedish context, this issue is further compounded by the relatively small size of cities, which means that outer rings often encompass non-urban areas that differ substantially from urban neighborhoods. For these reasons, we argue that defining areas in a way that aligns more closely with residents' perceptions of neighborhood boundaries is preferable. Additionally, because we select areas with new housing at the bottom of the area income distribution, neighboring areas located slightly farther away are likely to have higher income levels and may exhibit different trends over time.[10]

Given our focus on the poorest areas within the bottom income quartile, it is natural to use untreated Q1 areas as controls for treated Q1 areas. However, the prevalence of large new buildings is higher per capita in larger cities (see Subsection 3.2, Table 1), implying that the city size of treated and control areas may not be balanced. Additionally, low-income areas in different cities may exhibit divergent trends over time. To address these concerns, we restrict the control group for each treated Q1 area to untreated Q1 areas within the same city.

Furthermore, as the wider RegSO areas are named and recognizable, we anticipate significant spillover effects from new developments in smaller DeSO areas to adjacent areas within the same RegSO. Therefore, we exclude all areas adjacent to treated areas from the control group. We will also separately analyze the effects on these adjacent areas as well as on the wider RegSO neighborhoods.

In addition, we exclude from the control group any areas that underwent large multi-family housing developments during the period from 11 years before to 11 years after the treatment event in the treated area. This restriction prevents overlapping treatment windows for treated and control areas. We also exclude control areas that are adjacent to areas undergoing such developments.

Figure 3 illustrates an example of the selected control group for a treated area located in Uppsala, Sweden's fourth-largest city. The treated Q1 area, which features a pioneering co-op building established in 2000, is highlighted

[9] An alternative strategy, used by Li (2021) and Asquith et al. (2023), is to compare areas surrounding new buildings with areas that will receive new housing at a later point in time.

[10] In Appendix A4., we show that pre-trends are not parallel when applying a ring difference-in-differences specification.

in red. Thick boundary lines delineate the wider neighborhoods that are excluded from the pool of potential control areas, as these neighborhoods contain new buildings constructed between 1998 and 2011. The 13 selected control areas are marked in blue. When analyzing potential spillover effects on adjacent areas, we will apply the same selection procedure to identify appropriate control groups for each of the two neighboring areas.

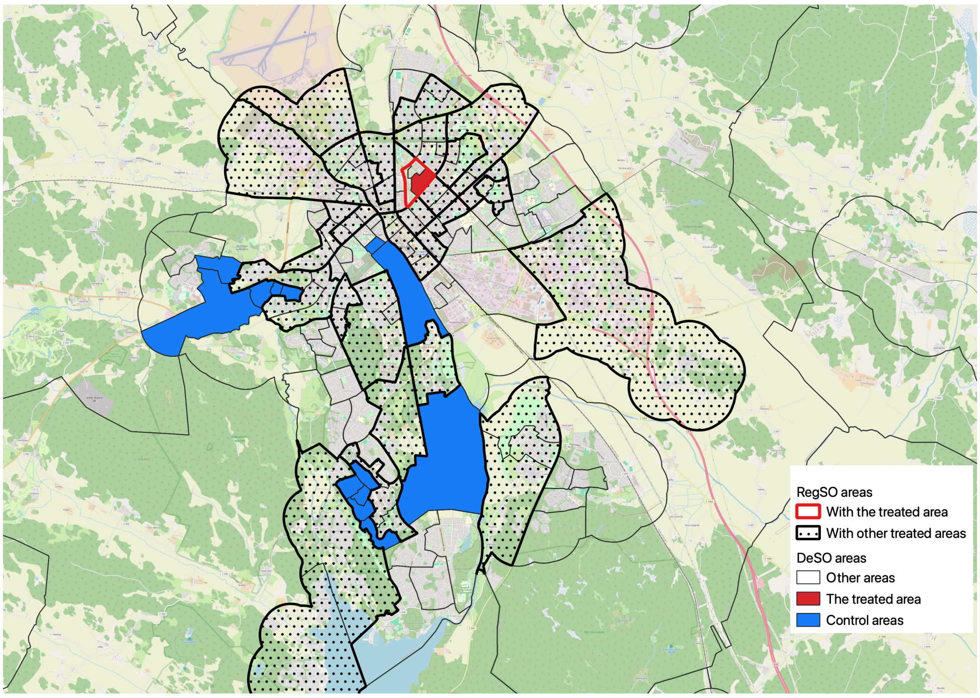


Figure 3: Control group selection for a Q1 area with new co-ops in Uppsala

Figure 4 presents the trends in poverty rates and log mean income separately for treated poor Q1 areas with new pioneering co-ops and rentals. The figure displays trends for all homes (thick lines) as well as for existing homes, defined as those constructed prior to event year 0 (long-dashed lines). For comparison, we also plot the corresponding trends for the selected control areas (thin solid lines) and the city as a whole.

In all panels, poverty rates and log mean income exhibit parallel trends prior to event year –2. In panels A and B, poverty rates decline beginning in event year –1, while in panels C and D, mean incomes begin to rise in the same year. These changes are further amplified following event year 0, coinciding with the completion of the pioneering building. This pattern suggests the presence of both anticipation effects and substantial post-treatment effects associated with the new housing, particularly as similar developments are not observed in the control areas or the city as a whole. The effects appear more pronounced in areas with new co-ops than in areas with new rentals. Moreover, the figures indicate that poverty rates decline and mean incomes increase not only overall but also within the stock of pre-existing homes. This

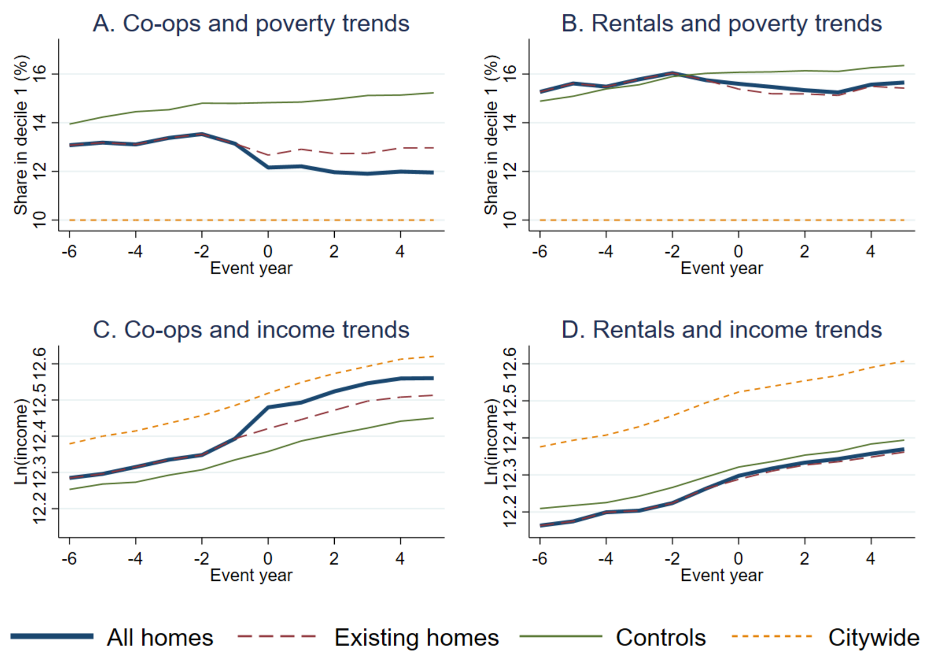


Figure 4: Income trends in treated and control areas in quartile 1

suggests that the new housing developments enhanced the attractiveness of these previously disadvantaged areas. Notably, in areas with new co-ops, the changes observed in the pre-existing housing stock are smaller than the total area-level effect, whereas, in areas with new rentals, the effects on the existing stock are of a similar magnitude to the total effect.

4.2 Regression specifications

In estimating the treatment effect with diff-in-diff, we stack treated areas and their individually selected control groups (selected according to the previous subsection), and run a two-way fixed effects regression with the following regression equation:

$$y_{itd} = \beta T_{itd} + \gamma_{id} + \mu_{td} + \alpha_d + \varepsilon_{itd}, \tag{1}$$

where y_{itd} is the outcome, typically the share of poor residents in decile one or log mean income, of an area i in year t for dataset d, where each treated area and its selected control areas form a dataset. T_{itd} is a treatment dummy taking the value of one for treated units in the post-treatment period with event year ≥ 0 and zero before that. We drop observation in event year -1 from the data since there is some evidence of anticipation effects as we saw in Figure 4.

For each dataset, we rely on within-area variation by accounting for time-invariant area fixed effects γ_{id} absorbing differences across areas that remain constant over time. Time trends are captured by dataset-specific year fixed effects μ_{td}. The term α_d is a dataset-specific constant, and ε_{itd} is an idiosyncratic error. We weight regressions by the population in the pre-treatment base year (event year -2). To account for serial correlation within areas and that a particular control area-year observation can occur multiple times as they can be controls for several treated areas (in different datasets), we report standard errors allowing for clustering at the area level.

We are interested in the estimate of the coefficient β, which represents the treatment effect. The identifying variation comes from the fact that that treatment is switched on in the treated areas in the post-treatment period, but not in the control areas, and thus T_{itd} varies by area-year interactions.[11]

Our specification corresponds to estimating the effect (the average effect across treated years) for each treated area separately and then aggregating the estimated effects into an average treatment effect on the treated. Given our regression weights, the effect we estimate can be interpreted as an average effect for residents in treated areas.

We also estimate event-study versions of the stacked difference-in-differences:

$$y_{itd} = \sum_{n \neq -2} \beta^n T^n_{itd} + \gamma_{id} + \mu_{td} + \alpha_d + \varepsilon_{itd}, \qquad (2)$$

where n indexes event years such that $n = 0$ is the treatment year and $-6 \leq n \leq 5$. Indicator variables T^n_{itd} take the value of one in event year n for treated areas and zero otherwise. We let $n = -2$ be the omitted base year as this allows one anticipation year. Whereas β^n for $n = -1$ provide estimates of anticipation effects, we can think of T^n_{itd} for $n \leq -3$ as counterfactually placed placebo treatments in the pre-treatment period with β^n_{itd} representing estimates of placebo effects. Dynamic effects following new housing are given by β^n_{itd} for $n \geq 0$. Relatively small placebo estimates that are statistically insignificant support the validity of the identifying assumption.

In our application with staggered treatment, recent methodological studies show that the standard difference-in-differences analysis with two-way fixed effects and treatment indicators is biased if treatment effects are dynamic (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Sun and Abraham, 2020). Suggested solutions conceptually amount to first estimating multiple clean difference-in-differences, each involving only one group that switches treatment status and a never-treated control group, and then aggregating the estimated effects from the difference-in-differences. Our implementation does this and corresponds to the stacked regression method used by Cengiz et al. (2019) and Baker et al. (2022).[12]

[11] Formally, the identifying assumption of parallel trends between treated and control areas requires that T_{itd} is uncorrelated with ε_{itd} conditional on the fixed effects, i.e., $E(\varepsilon_{itd} \mid T_{itd}, \mu_{id}, \gamma_{td}, \alpha_d) = E(\varepsilon_{itd} \mid \mu_{id}, \gamma_{td}, \alpha_d)$.

[12] An advantage of the stacked regression compared to the other proposed methods is

5 Neighborhood effects of new housing construction

We present two main sets of results. While in this section we focus on the neighborhood effects of new housing construction, we provide the effects on individual mobility in Section 6.

5.1 Effects on neighborhood poverty rates and income levels

Using Equation (1), we start by estimating the effects of large residential developments on the share of poor residents in the neighborhood (defined as residents belonging to decile 1 of the income distribution) and the log of the mean neighborhood income. The results are presented in Table 3. Since we have a specific focus on the effects in the poorest neighborhoods, the estimations are conducted on the sample of neighborhoods in the bottom income quartile. We report the estimates by whether the new pioneering building contains privately owned co-ops (columns 1 and 2) or rental apartments (columns 3 and 4). To gain an understanding of whether the effects are only driven by the new housing or not, we also present the results separately for all homes (columns 1 and 3) and preexisting homes (ie, homes built before event year 0; columns 2 and 4).

Panel A shows an estimated effect on the share of poor of approximately -1.7 percentage points when the new housing construction in the poorest neighborhoods is for private ownership (column 1). The estimated effect is statistically significant.[13] To assess the economic significance of this reduction in the poverty rate, we note that the actual post-treatment poverty rate is 12.2%, which is 2.2 percentage points higher than the city mean of 10%, i.e., there is a 2.2 percentage point gap or excess poverty in these areas. The predicted counterfactual poverty rate is 13.8 percentage points (12.2 + 1.7), i.e., the counterfactual excess poverty is 3.8 percentage points. Thus, the reduction of 1.7 percentage points reduces excess poverty by 44% (1.7/3.8) meaning that new housing nearly halves the difference between the poor area and the rest of the city. In pre-*pre-existing* homes, column 2 shows that the poverty rate decreases by 1.0 percentage points (panel A, column 2), which is 58% of the total effect of 1.7 percentage points. The remaining rise is from in-movers into the new (large and small) buildings less often being poor people from the bottom decile compared to residents in existing homes.

that it allows one to specify a unique control group for each treated unit, instead of a pool of controls from which the estimator selects at least all never-treated units. Unlike other solutions, stacked regression is simple and efficient. This estimator constrains the weights assigned to different heterogeneous effects (both over time and across units) to the one imposed by OLS. As Baker et al. (2022) notes, there is no conceptually "correct" weighting scheme.

[13] In this paper, when we say that an estimated effect is significant, we mean that it is significant at least at the 5% significance level.

Table 3: Effects on neighborhood poverty rate and income level

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Outcome:	All	Existing	All	Existing
		A. Poverty rate		
Share in decile 1 (pp)	-1.675**	-0.970**	-0.994**	-1.216**
	(0.256)	(0.258)	(0.353)	(0.371)
Counterfactual (%)	13.8		16.2	
Actual (%)	12.2		15.2	
City mean (%)	10.0		10.0	
Gap reduction (%)	43.7		16.1	
		B. Mean income		
Ln(income)*100	7.660**	2.677**	1.593	1.333
	(1.036)	(0.855)	(0.926)	(0.821)
Counterfactual (SEK)	254,804		225,682	
Actual (SEK)	275,089		229,306	
City mean (SEK)	293,854		287,142	
Gap reduction (%)	51.9		5.9	

Note: See Eq. (1) for the regression specification. Regressions are weighted by base-year population (21 years or older). The data used in estimating the effects of new co-ops include 73 treated areas, 493 clusters (unique treated or control areas), 1,232 dataset areas, and 13,552 observations. For new rentals, the corresponding numbers are 68 treated areas, 425 clusters, 881 dataset areas, and 9,691 observations. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

For the log of mean income in panel B, we estimate a positive mean-income effect of approximately 7.7% (column 1).[14] Using the observed log mean income (expressed in SEK), we can once again compute the counterfactual and the reduction of the gap relative to the city mean to get the economic significance of the results. As is clear from the last row in column (1) in panel B, we find that new housing reduced the income gap by 52%. In existing homes, the income rise is 2.7% (panel B, column 2), which is 35% of the total effect of 7.7%.

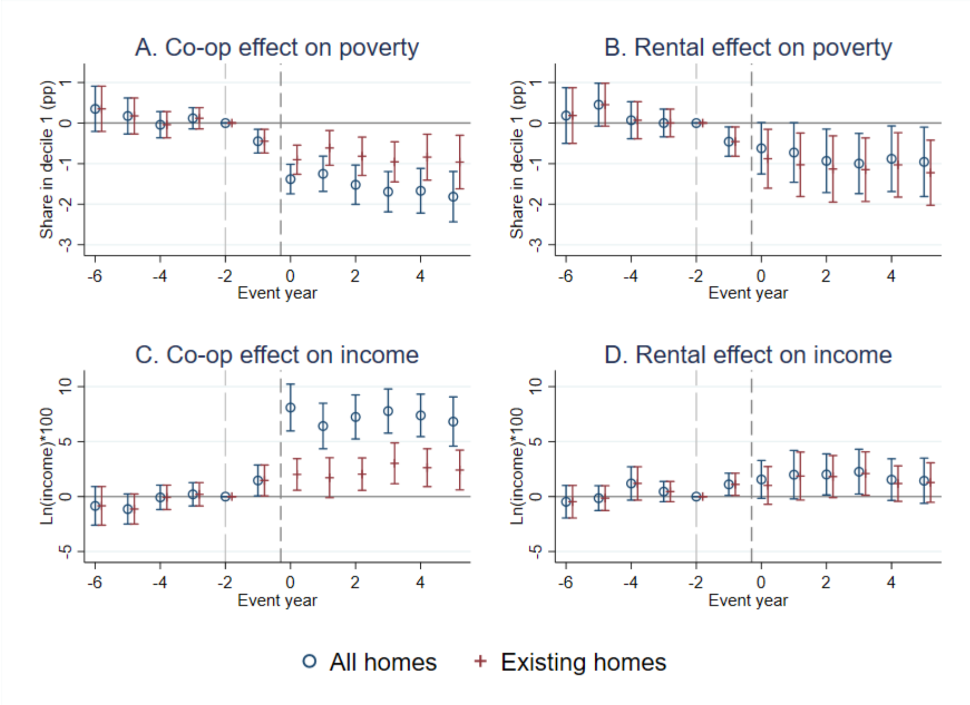
Turning to the construction of new rental apartments, our estimated effects are smaller. We find a reduction in the poverty rate of approximately 1.0 percentage points (panel A, column 3) corresponding to an excess poverty reduction of 16%. This reduction is entirely due to the poverty reduction of 1.2 percentage points (panel A, column 4) in existing homes. New rentals also raise mean income by 1.6% (panel B, column 3), which imply a reduction in the income gap of 6%, but this point estimate is not statistically significant.[15]

To check whether the parallel trends assumption holds, whether there are any anticipation effects of new housing, and explore dynamic effects, we provide event-study estimates based on Eq. (2). Figure 6 presents the results where, in each panel, we plot point estimates and 95% confidence intervals across event years from a regression with observations based on all homes and another regression with observations based on only existing homes. The figures reveal small and statistically insignificant placebo point estimates in the pre-treatment period (before event year -2, which is the base year).[16] In each regression, we cannot reject the null hypothesis that all point estimates for event years -6 to -2 are zero. In contrast, estimated treatment effects are large relative to the placebo estimates and statistically significant in each post-treatment event year. We also find small anticipation effects in event year -1 due to residential composition changes in existing homes. Post-construction effects kick in immediately in the construction year and then increase slightly over time. Overall, the effects gradually increase from the year before the completion of the pioneering building until event year 2, in which most of the long-run effects have materialized.

[14] The true effect on income is $e^{\beta}-1 \approx \beta*100\%$ for small β. We will use this approximation when referring to the percentage effect on mean income.

[15] Areas with new rentals are, on average, poorer with a higher poverty rate and lower mean income than areas with new co-ops. However, since our effect subgroup analysis in Section 5.6 does not reveal lower effects of new housing in the poorest of our poor Q1 areas for each property type, we interpret our results as smaller effects of new rentals than new co-ops.

[16] At the 5% significance level, we expect 1 out of 20 placebo point estimates to be statistically significant even when the true effect is zero. In our case, none of the 16 placebo point estimates are statistically significant.



Note: We plot point estimates and 95% confidence intervals. See Eq. (2) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level.

Figure 5: Event-study estimates of effects on area income

Our results indicate that large new buildings increase the attractiveness of poor areas. This could be due to an increase in the number and quality of neighborhood amenities, such as restaurants, cultural activities, schools, and public services, the new buildings themselves, or the socioeconomic composition of the new homes, making the neighborhood more attractive. It is especially interesting and encouraging to see the positive effects for pre-existing homes, which strongly support the interpretation that the attractiveness of the areas have increased.

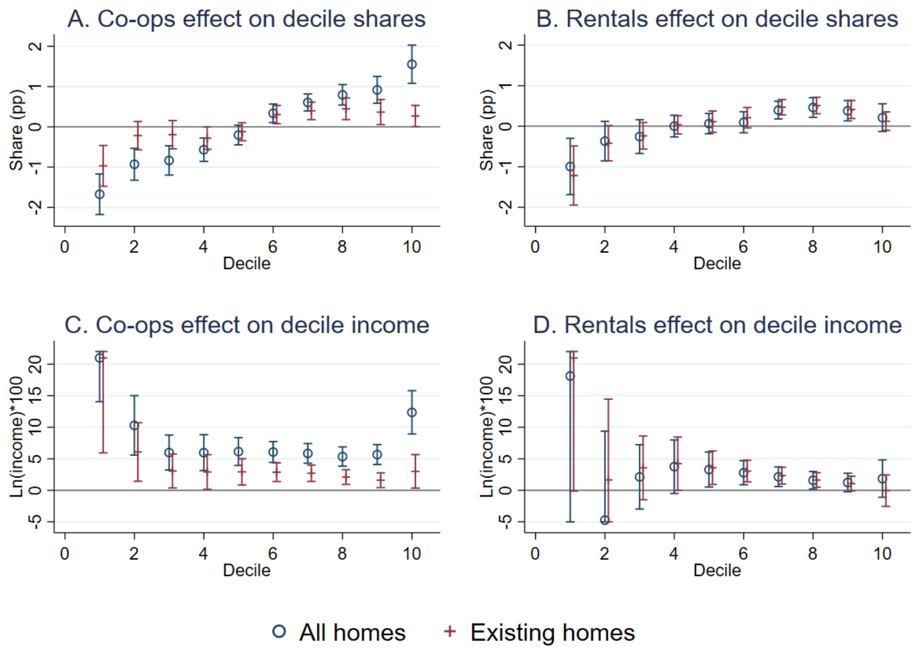
5.2 Effects across the income distribution

With access to full population data, we can construct precise measures of the entire income distribution beyond the poverty rate and mean income. We examine the effects of large new buildings on the income distribution in two ways. First, we estimate the impact on the share of residents in the neighborhood belonging to different *municipal* income deciles. Second, we estimate the effects on mean income in the area for residents in different *area* income deciles. Figure 6 visualizes these estimated effects on decile shares (upper panel) and income levels (lower panel). We plot estimated effects and 95% confidence intervals across deciles, where each estimated decile effect comes from a separate, decile-specific, estimation of Eq. (1).

Starting with the effects on decile shares, Figure 6A shows that new co-ops reduce the share of residents from the bottom five (municipal) income deciles. Instead, the low-income residents are replaced by residents from the top five deciles. In particular, the most pronounced increase is that of top decile residents. However, this increase is mostly driven by in-migration to newly constructed homes as the estimated effect in existing homes is much smaller. Overall, the pattern for new rentals in Figure 6B is similar but smaller. One difference is that new rentals do not attract many residents from the top decile.

Turning to the effects on income levels, Figure 6C shows that new co-ops increase income levels across every area decile, both when new homes are included and when they are not. The percentage increase is the largest in the extremes, specifically in the bottom two deciles and in the top decile when new homes are included.[17] For new rentals, the estimated effects, presented in Figure 6D, are largest in the lower end of the area income distribution.

[17]While the percentage effect in the bottom decile is large, they translate into smaller absolute effects as mean income is the lowest in this decile.



Note: In panels A and B, the outcome is the area share of individuals in municipal income deciles. In panels C and D, income is the area mean income for individuals in different *area* income deciles. In the lowest three deciles, the area mean income is sometimes zero. In these cases, we censor at 100 SEK, which is the minimum positive recorded income for individuals, to allow for taking the logarithm. We plot point estimates and 95 % confidence intervals. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level. The y-values are censored.

Figure 6: Effects on decile shares and incomes

5.3 Effects on socio-demographic composition

Policymakers often target neighborhood residential composition along other dimensions than income. In Table 4, we report our estimated effects of new housing on socio-demographic characteristics, including outcomes such as overall population, population shares in different age groups, share of residents born outside Sweden, and share of residents with different levels of education. Panel A shows that population increases by about 15% in our treated areas (column 1 for new co-ops and and column 3 for new rentals), an increase entirely driven by additional residents in the newly built homes (no effects on total population in pre-existing homes; c.f. columns 2 and 4).

Different types of housing with varying standards attract different age groups; e.g., new single-family houses generally attract more children families with high-income working-age parents, whereas most student-aged residents live in rentals. It is possible that the poverty reduction effects primarily reflect a demographic transformation of neighborhoods, e.g., from student-

Table 4: Effects on socio-demographic outcomes

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Outcome:	All	Existing	All	Existing
	A. Population			
Ln(population)*100	15.35**	-1.548	13.83**	-0.327
	(1.573)	(0.957)	(1.650)	(1.259)
	B. Age			
Age 0-20 (pp)	-0.401	-0.0379	-0.361	-0.200
	(0.338)	(0.278)	(0.335)	(0.316)
Age 21-25 (pp)	-0.565	-0.577**	1.010**	-0.552
	(0.303)	(0.221)	(0.351)	(0.286)
Age 61-65 (pp)	0.282*	0.181	-0.136	0.0662
	(0.131)	(0.114)	(0.113)	(0.108)
Age $\geq$ 66 (pp)	-0.394	-0.255	-1.073**	-0.317
	(0.358)	(0.292)	(0.343)	(0.351)
	C. Born outside Sweden			
Born in Europe (pp)	-0.0413	0.0729	-0.226	-0.0774
	(0.213)	(0.201)	(0.259)	(0.259)
Born outside Europe (pp)	-1.614**	-1.162**	-1.426*	-1.319*
	(0.388)	(0.401)	(0.659)	(0.641)
	D. Education			
No high-school degree (pp)	-1.483**	-0.377	-1.642**	-0.662*
	(0.299)	(0.285)	(0.375)	(0.333)
University degree (pp)	1.903**	0.709*	2.082**	0.370
	(0.408)	(0.338)	(0.486)	(0.422)

Note: See Eq. (1) for the regression specification. Regressions are weighted by base-year population (all ages in panels A-C, 21 years or older in panel D). Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

dominated areas to children-friendly areas. Table 4B shows that new co-ops reduce the share of young adults (ages 21-25) by about 0.6 percentage points in the pre-existing stock (column 2) and increase near-pension-aged residents (ages 61-65) by about 0.3 percentage points (column 1). The effects can be compared to the base-year pre-reform shares which are 9.3 and 4.8 percentage points for the ages 21-25 and 61-65, respectively (see Table A1, column 1 in the Appendix A2.). In contrast, new rentals increase the share of young adults by 1.0 percentage points (column 3) at the expense of a 1.1 reduction of retired residents (age 66 or older). The pre-reform shares in ages 21-25 and 66 or older were 13.2 and 15.4 percentage points, respectively (see Table A1, column 3 in the Appendix A2.). Overall, the effects on the age-composition are not large enough to be the main explanation for the positive effects on area income that we reported above. In Appendix A3., we provide another piece of evidence on this by showing poverty reduction effects even in areas with low shares of both young adults and old residents.

In Sweden and other European countries, the concentration of poor foreign-born residents, particularly those born outside Europe, in certain areas is sometimes of political concern. This is not the least reflected in immigrant placement policies spreading out immigrants in space. Table 4C shows that new housing decreases the share of residents born outside Europe in the neighborhood by about 1.5 percentage points (a bit less in the existing homes). The pre-reform shares of residents born outside of Europe are 11.2 % in areas with new co-ops and 17.0 % in areas with new rentals (see Appendix A2., Table A1, columns 1 and 3), and much higher than in the rest of the city. Thus, the lower share of foreign-born residents in the poorest neighborhoods mitigates their concentration and decreases overall ethnic residential segregation.

Finally, we find in Table 4D that new housing replaces residents without high-school degrees with those with university degrees, results that align well with the estimated income effects. Residents in new homes contribute strongly to these changes (the estimated effects are larger in columns 1 and 3 than in columns 2 and 4). These changes mean a more diverse neighborhood population as low-education residents are overrepresented and high-education residents are underrepresented in low-income areas (compare columns 1 and 3 with columns 2 and 4 in Table A1 in the Appendix A2.).

5.4 Effects on adjacent areas (spillover effects)

A new building might not only have hyperlocal effects, especially if they are accompanied by improved neighborhood amenities benefiting a wider area. It is also possible that residents are attracted to areas around the new building rather than slightly further away. Therefore, we analyze spillover effects on adjacent areas and the wider neighborhood (treated and adjacent areas). Our definition of the wider neighborhood is the RegSO area, and adjacent areas are other DeSO areas in the same RegSO area. Using Eq. (1), we estimate the effect for adjacent areas by treating them as if they were the treated

Table 5: Spillover effects on adjacent areas

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Areas:	All	Existing	All	Existing
	A. Poverty rate (pp in decile 1)			
Treated area	-1.675**	-0.970**	-0.994**	-1.216**
	(0.256)	(0.258)	(0.353)	(0.371)
Adjacent areas	0.0526		0.224	
	(0.298)		(0.277)	
Wider area	-0.693**	-0.389	-0.228	-0.310
	(0.217)	(0.210)	(0.238)	(0.244)
	B. Mean income (ln(income)*100)			
Treated area	7.660**	2.677**	1.593	1.333
	(1.036)	(0.855)	(0.926)	(0.821)
Adjacent areas	-1.851*		-2.610**	
	(0.825)		(0.920)	
Wider area	2.256**	0.104	-1.049	-1.146
	(0.769)	(0.641)	(0.756)	(0.738)

Note: Adjacent areas are other DeSO areas in the same wider RegSO area as the treated DeSO area. Wider areas consist of treated and adjacent areas. See Eq. (1) for the regression specification. Regressions are unweighted. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

areas (selecting unique control groups for each of them). When estimating the effect for the wider area, we pool treated and adjacent areas; thus, we estimate a weighted average effect for areas within the wider neighborhood. The results are reported in Table 5.

Table 5A shows that adjacent areas are not statistically significantly affected in terms of the poverty rate. It follows that the average effect for the wider area as a whole is lower than in the treated area. But for new co-ops, the estimated 0.7 percentage point reduction in share from the lowest income decile is still statistically significant (column 1). However, we find in Table 5B negative effects of -1.9% and -2.6% on area mean income in areas adjacent to those with new co-ops and rentals, respectively (columns 1 and 3). Thus, part of the positive effect on the treated area is a cannibalizing effect affecting the adjacent areas negatively. Our interpretation is that richer individuals who want to live in the wider neighborhood instead of living in the adjacent areas now reside in the treated areas. While the effect on the wider area is still positive and statistically significant for new co-ops (2.3% in column 1), the cannibalizing effect on adjacent areas completely offsets the gain in the treated areas for new rentals.

Our spillover effect results have important policy implications: The positive effect is quite local, and this result is in line with findings from previous research. One needs to surgically build close to the poorest small micro-neighborhoods rather than slightly further away. In the Swedish policy debate, anti-segregation measures typically target entire wider RegSO areas containing several poor micro-neighborhoods. To lift the entire wider neighborhood requires larger developments spreading across several micro-neighborhoods. The positive effects we find apply to developments that increase area population by about 15%.

Spillover concerns are particularly important if one considers using new rentals for revitalization. Not only is the local gentrification effect smaller, but stronger negative spillover effects hurt nearby areas. Since one wants to avoid undesirable side effects on nearby poor areas, rentals are only suitable in poor areas surrounded by richer areas. In the case of a larger poor area, new rentals need to be spread out quite evenly over the area. In denser cities with land scarcity, revitalization through new construction of rentals likely presupposes the demolition of existing structures.

5.5 Effects on rents and housing prices

Since the earlier literature, mainly based on data from the USA, has focused on the effects on housing prices and rents of large new housing construction, it is of interest to examine these effects also in a Swedish setting.

Theoretically, an expanded housing stock generates supply effects pressing housing prices and rent downward. Of course, amenity improvements lead to opposing demand effects. Moreover, homes are not a homogeneous good. The supply of a different type of home, new ones, to a neighborhood also attracts demand from residents who otherwise would not demand other existing homes in the area. Since new homes typically have higher standards than older homes, they are, on average, more expensive. However, for some people, newer and older homes are to some extent substitutable, at least if there is a price difference and people have preferences for the living environment in an area. For these reasons, it is difficult to theoretically predict the price and rent effects of adding new homes in an area. Nevertheless, incumbent homeowners often fear a supply effect depreciating the value of their homes, and this fear often sparks resistance against new housing constructions in one's "backyard".

In Table 6A, we report estimated effects on apartment sales prices.[18] The estimated effect of new co-ops on apartment sales prices is 12.6% when including all homes (column 1). We find a smaller and not statistically significant effect for existing homes (column 2), but standard errors are large, and we cannot rule out sizeable effects. Despite an increase in total supply, we do not find negative effects, and this is consistent with the area becoming more

[18] We also have data on sales of single-family houses. However, such homes are less common in poor areas, and the turnover rate is lower; the number of sold objects per year is less than 10% of that of apartments and too low to proxy the house price level for a majority of our treated and control areas.

Table 6: Effects on housing prices and rents

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Tenure type:	All	Existing	All	Existing
	A. Apartment sales prices (ln(SEK/m^2)*100)			
Owned homes	12.60**	3.574	-1.793	-6.122
	(4.323)	(3.979)	(4.935)	(5.013)
	B.Rents (ln(SEK/m^2)*100)			
Rented homes	-0.00105		-1.639**	
	(0.450)		(0.427)	

Note: In panel A, we use data from 2005-2020 with treated areas in 2007-2017 (unbalanced panel with 35 new co-ops and 37 new rentals). In panel B, we use data from Stockholm county 2005-2016 with treated areas in 2007-2016 (unbalanced panel with 9 new co-ops and 7 new rentals). To retain as many treated areas as possible, we allow unbalanced samples. In panel B, we also include every untreated area in the same city as controls (rather than only Q1 areas). See Eq. (1) for the regression specification. Regressions are weighted by the total number of yearly observations from event years $\leq$ -2 in panels A and B. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

attractive after renewal. In areas with new rentals, we find negative point estimates (columns 3 and 4), possibly indicating some substitutability between new rentals and existing owner-occupied apartments; but these point estimates are not statistically significant.

In contrast to incumbent homeowners, incumbent renters might fear that neighborhood gentrification leads to rent increases and other higher costs of living, although a higher housing supply theoretically has a downward push on rents. With periodically negotiated rents that in the past were determined primarily to reflect the standard of rented homes in Sweden, rents cannot adjust quickly except after renovations.[19] However, amenities are a legally valid factor to evoke in rent negotiations. Because of the more rigid system that, in practice, may make rapid rent increases more difficult, it is an empirically open issue whether and how fast rents change to reflect demand factors after neighborhood renewal.

In Table 6B, we report estimated effects on rents for areas in Stockholm County. With only data for one part of Sweden for the years 2005-2016, we could only estimate average rent effects for 16 of our treated Q1 areas. While the results should be interpreted cautiously, we find no indicative evidence of rent increases in the treated areas; if anything, rents appear to decrease a little in areas with new rentals (column 3), which might indicate substitutability between new and existing rentals.[20]

[19]In Appendix A4. Table A3, we can rule out that any larger renovations occur concurrently with the construction of our new buildings.

[20]Unfortunately, we are not able to reliably distinguish newly built apartments from existing ones in the rent data

5.6 Effect heterogeneity within group of poor neighborhoods

Using variations within the group of the poorest quartile of neighborhoods, we have conducted a series of heterogeneity analyses on the estimated effects presented in Table 3. The main reasons for this is that we are interested in examining if the effects are functions of the initial (pre-construction) situation in the neighborhoods (detailed initial poverty rates, initial share of rental residents, initial share of foreign-born residents, and initial share of young and old residents).

We reach three main conclusions from these heterogeneity analyses.[21] Starting with the initial economic situation within the quartile of poorest neighborhoods, we find that the gentrification effects of both new co-ops and new rentals are stronger in areas with higher poverty rates, both in terms of poverty rate reduction and average income increase (c.f. Figure A1). For new co-ops in the areas with the highest poverty rates, residents in the new homes drive this pattern. Since renewal policies typically focus on areas with the most extreme concentration of poverty, our results are encouraging in terms of the potential for revitalizing these areas through new housing construction.

Turning to the share of residents living in rental apartments in the neighborhood, we find that an initial high rental share does not appear to limit the positive impact of new co-ops and new rentals found in the baseline analysis (c.f. Figure A2).

Finally, examining the role of the initial demographic situation in the neighborhoods, we find (i) no clear heterogeneous effects based on the initial share of immigrants in the neighborhood (c.f. Figure A3) and (ii) that the gentrification effect is weaker in areas with many young adults, but stronger for new rentals in areas with many old residents (c.f. Figure A4).

5.7 Sensitivity analysis and robustness tests

We have conducted three types of sensitivity analysis and robustness tests, examining if our baseline results are (i) contaminated by any concurrent housing policies that might have taken place at the same time as the new housing construction, (ii) sensitive to the income definitions and identification strategy used, and (iii) sensitive to new construction of other types of housing than the pioneering buildings. In this section we briefly describe our findings. The full results are presented in Section A4. in the Appendix.

Where there any concurrent housing policies taking place at the same time as the new housing construction?

Area-based programs often include a bundle of measures, including multiple changes to the housing stock. In densely populated areas without free land, demolishing older homes might even be a prerequisite for building new ones.

[21] The detailed results are presented in Appendix A3..

The new large buildings that we have selected are all inhabited by a substantial share of residents in the areas and thus have major impacts on the neighborhoods. While the effects that we estimate also capture the impacts of new accompanying services such as shops and schools, other policy interventions, such as labor-market programs, were rarely area-based during our study period. However, since a substantial part of the Swedish housing stock underwent renovations and tenure-type conversions from public rentals to private co-ops over the last decades, an important question is whether such major housing stock changes concurrently took place with the pioneering treatment buildings used in the baseline analysis.

When examining this, we find no indications that the baseline effects of new housing construction presented in Table 3 are contaminated by concurrent housing demolitions, tenure-type conversions, or major renovations (see Table A3 in Appendix A4.). We conclude that in terms of housing, we estimate a clean effect of adding new homes to a neighborhood rather than a compound effect that also reflects other changes to the housing stock. From a wider perspective, new multi-family residential buildings typically come with new public and private services and infrastructure; our estimates still include the effects of these changes on the neighborhood.

Alternative income definitions and identification strategies

In Appendix A4., we show that our baseline results are insensitive to using alternative income definitions (c.f. Table A4) and alternative identification strategies (c.f. Table A5), delivering event-study estimates that validate the parallel trends assumption (c.f. Figure A5).

New construction of other homes than the pioneering buildings

Our definition of existing homes excludes all new homes constructed after the pioneering building. Thus, new homes include not only the pioneering building but also other large buildings and smaller ones such as detached houses. It is possible that the pioneering building was bundled with or stimulated the construction of other large buildings.[22] Even if that is not the case, part of the total impact might be due to residential changes in the smaller new buildings. In the robustness analyses, we disentangle effects by types of new homes. We find that new pioneering buildings were followed by subsequent large buildings and increased production of smaller houses. These additional housing stock expansions were gradual. Moreover, the residential compositions in other new large buildings than the pioneering ones and in

[22]Housing stock changes typically occur to some extent all the time in most neighborhoods. From a policy perspective, we think the effects of additional measures relative to a baseline, here changes in the control group, are of greatest interest. In contrast, one might be interested in the effects of all housing stock changes relative to areas without any such changes; however, such areas in decline are often atypical.

small buildings provide important contributions to the total gentrification effect. However, we rule out that the increased supply of these other buildings began already before the completion of the pioneering building, driving the anticipation effect we observe in event year -1. The full results are presented in Table A6 and Figure A6.

6 Mobility effects of new housing construction

In the former section, we found that large new housing construction led to poverty rate reductions, income increases, and a more socio-demographically mixed population in the treated neighborhoods. In this section, we turn to the effects on individual mobility, enabling us to get a deeper understanding of the sorting mechanisms behind the neighborhood effects.

6.1 Effects on out-migration

Does the construction of new homes in poor areas lead to displacement of incumbent residents due to higher living costs? Some earlier research has estimated displacement in terms of more out-migration to other low-income areas. We can directly estimate whether poor people move out. Given that we found non-positive effects on rents in Subsection 5.5, we think displacement is not very likely.[23]

We analyze the out-migration patterns of incumbent residents living in our treated areas in the pre-treatment base year (event year -2). We group incumbents by where they live four years later (event year 2) in the post-treatment period and additively decompose the effect stemming from stayers living in the treated (DeSO) area, out-movers to adjacent areas in the same wider (RegSO) area, and out-movers away from the wider area. Of course, people move in and out of areas all the time, and we, therefore, compare the distribution between stayers and out-movers between event years -2 and 2 with a baseline pre-treatment distribution of incumbents over the course of four years between event year -6 and -2. To alleviate concern about a secular (non-treatment related) trend in out-migration over time in the treated area, we adjust for the trend in the control areas. Thus, we run two-by-two diff-in-diffs (Eq. 1) to estimate the effect of the new housing on migration streams.

Table 7 reports the estimated treatment effects on the numbers of out-movers by end-year destination. Panel A shows that the number of out-movers does not change much, and none of the changes are statistically significant. In fact, point estimates are negative, with seven fewer working-age residents from the Deso area leaving the wider area than in the counterfactual situation

[23]It is still possible that other costs of living, e.g., food costs, increase in the area. However, more nearby housing options also improve the outlook for staying in the area if the current housing situation is unsatisfactory. In addition to displacement, changes in neighborhood character and homeowners wanting to realize housing price gains are potential other out-migration motives.

Table 7: Effects on number of incumbents by end-year destination

	(1)	(2)
Destination:	New co-ops	New rentals
	A. Working-age population (# aged 21-65)	
All	-6.434	-5.366
	(8.129)	(10.64)
Stayers	0.383	2.472
	(6.134)	(6.894)
Adjacent	0.264	-0.962
(out-move)	(0.947)	(2.092)
Outside	-7.082	-6.876
(out-move)	(6.205)	(9.053)
	B. Poor residents (# aged 21-65)	
All	-3.535	-10.52**
	(3.151)	(3.801)
Stayers	-1.248	-5.349*
	(2.185)	(2.415)
Adjacent	-0.158	-0.253
(out-move)	(0.206)	(0.411)
Outside	-2.130	-4.922
(out-move)	(2.030)	(2.732)

Note: The effects apply to event year 2 for residents living in treated areas in event year -2. Destination areas refer to residential areas in event year 2. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

(-7.1 and -6.9 in areas with new co-ops and new rentals, respectively). Panel B shows that there are fewer incumbents in the area that are poor (belonging to the lowest municipal income decile) after the treatment, but the estimated effects are small. New rentals lead to about 5.4 fewer poor stayers and 4.9 fewer residents moving away from the wider area. Fewer poor stayers are in line with an endogenous positive income response to a gentrified neighborhood, and fewer out-movers are consistent with improved rental options nearby.

In Table 8, we report out-migration results in terms of effects on the poverty rate and mean income of incumbents by end-year destination. As in Table 7, estimated effects are small and we find no signs of more poor people moving out from the wider area.[24]

One concern often raised is that if neighborhood gentrification displaces poor people, they probably disproportionately move to and cluster in other poor areas, thus still exposed to concentrated poverty. Moreover, from a city-

[24]If anything, new housing leads to out-movers less often being poor and having higher mean income).

Table 8: Effects on income of incumbents by end-year destination

	(1)	(2)
Destination:	New co-ops	New rentals
	A. Poverty rate (pp in decile 1)	
All	-0.151	-0.827**
	(0.275)	(0.294)
Stayers	-0.291	-0.852**
	(0.298)	(0.317)
Adjacent	-2.580	0.756
(out-move)	(1.767)	(1.306)
Outside	-0.254	-0.595
(out-move)	(0.446)	(0.516)
	B. Mean income (ln(income)*100)	
All	-0.253	1.440
	(0.844)	(0.846)
Stayers	0.493	1.548
	(0.853)	(0.907)
Adjacent	-0.683	-1.621
(out-move)	(4.368)	(3.716)
Outside	0.122	1.381
(out-move)	(1.226)	(0.976)

Note: The effects apply to event year 2 for residents living in treated areas in event year -2. Destination areas refer to residential areas in event year 2. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

Table 9: Effects on destination area income of incumbents by end-year destination

	(1)	(2)
Destination:	New co-ops	New rentals
	A. Poverty rate (pp in decile 1)	
All	-0.980**	-0.610*
	(0.172)	(0.263)
Stayers	-1.516**	-0.948*
	(0.251)	(0.404)
Adjacent	-0.764	0.199
(out-move)	(0.674)	(0.334)
Outside	-0.0667	0.0495
(out-move)	(0.0656)	(0.0737)
	B. Mean income (ln(income)*100)	
All	4.064**	1.143
	(0.817)	(0.726)
Stayers	7.313**	2.055*
	(1.054)	(0.978)
Adjacent	0.854	-0.756
(out-move)	(1.119)	(0.914)
Outside	0.513	-0.0893
(out-move)	(0.438)	(0.369)

Note: The effects apply to event year 2 for residents living in treated areas in event year -2. Destination areas refer to residential areas in event year 2. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

wide perspective, the more mixed population in the treated area would then be accompanied by a higher concentration of poverty in other poor areas, leaving the city as a whole not necessarily less segregated. While we find no evidence of displacement, a gentrified neighborhood might still change the destination choices of out-movers. We analyze such effects in Table 9, which reports the effects on the poverty rate and mean income in the destination areas of incumbents after new construction. For out-movers to adjacent areas or outside the wider area, the table reveals estimated effects that are small and not statistically significant.[25]

In Appendix A5., we offer an alternative dynamic migration analysis focusing on year-to-year moves. Even in that analysis, we rule out new housing leading to a displacement of poor residents from the area. However, we do find indications of a temporary higher share of poor out-movers from existing homes into new homes in the same area, a standard upgrade that cannot be

[25]For stayers, we find lower exposure to area poverty and higher mean area income levels, which is simply our main gentrification result.

interpreted as displacement.[26]

6.2 Effects on in-migration

Finding no mobility effects among incumbents, the total gentrification effect must be due to in-migration patterns. We now move on to analyze in-migration by grouping residents in treated areas in event year 2 by where they lived four years earlier in event year -2. The three origin area groups are stayers living in the same (DeSO) area, in-movers from adjacent areas in the same wider (RegSO) area, and in-movers from other (RegSO) areas. As in the out-migration analysis in the previous subsection, we conduct two-by-two diff-in-diffs (with pre-reform in-migration constructed based on in-migration between event years -6 and -2).

Table 10 reports estimated treatment effects on the numbers of in-movers, according to the description above. We report estimated effects for all homes in the area as well as the effects on existing homes and new homes separately. Panel A shows that the population gains of 188 working-age residents (columns 1) in areas with new co-ops are entirely driven by in-moves to the area, mostly from outside the wider neighborhood (181 residents). Moreover, the population increase is due to the residents in new homes (columns 3); in fact, the point estimates for population change in existing homes are negative (columns 2), and mainly driven by 11 residents in the area replacing their older homes with new ones (columns 2 and 3). The 201 additional residents in the new homes (column 3) mostly moved in from the outside (185). However, although only 16 residents (aged 21-65) in the new homes are locals from the same wider neighborhood $(11 + 5)$, they are still over-represented by a factor of approximately 5; while they make up 8.0% of the residents in the new buildings, they only make up 1.6% of the population in the municipality.[27]

The picture is similar in areas for new rentals: All the population gain of 189 (column 4) is due to the additional 199 residents in the new homes (column 6) and most of them moved in from the outside to the new homes; 13 are locals $(8.1 + 4.8)$ yielding an over-representation factor of approximately 3. We conclude that new housing improves housing options nearby, and

[26] Another possible strategy would be to follow base-year residents forward and backward in time year by year and estimate the out-move probability relative to the one in the control areas. This simple difference strategy suffers from potential secular trend differences in out-migration patterns between treated and control areas.

[27] The observant reader might have noted that the stayer estimates in Tables 7 and 10 differ slightly. The reason is some cases of property re-organizations, such as mergers and splits resulting in the same building being located in different DeSO areas across years. For instance, a rental property with two residential buildings assigned the coordinates of the largest buildings in one year could convert to two properties, one rental, and one co-op, each with one residential building with its own coordinates. Restructured properties are excluded from our sample of new large properties when selecting treated areas. But such properties do appear among existing homes. In Table 10, stayers are people living in a building in the area in the *base* year. In contrast, in Table 10, the stayer definition is residents living in a building in the area in the *end* year.

Table 10: Effects on numbers of in-movers by base-year origin

	(1)	(2)	(3)	(4)	(5)	(6)
		New co-ops			New rentals	
Origin:	All	Existing	New	All	Existing	New
			A. Working-age population (# aged 21-65)			
All	187.7**	-13.14	200.9**	188.9**	-9.794	198.7**
	(16.39)	(7.640)	(14.62)	(20.32)	(13.36)	(17.76)
Stayers	-0.377	-11.48	11.10**	2.927	-5.210	8.137**
	(6.253)	(6.994)	(3.034)	(6.882)	(7.477)	(2.439)
Adjacent	7.435**	2.388	5.047**	5.167**	0.322	4.845**
(in-move)	(2.856)	(2.770)	(0.917)	(1.828)	(1.767)	(0.769)
Outside	180.7**	-4.050	184.7**	180.8**	-4.906	185.7**
(in-move)	(17.32)	(7.596)	(14.20)	(20.06)	(11.93)	(17.25)
			B. Poor residents (# aged 21-65)			
All	4.707	-11.55**	16.26**	18.60**	-12.98	31.57**
	(3.755)	(3.212)	(1.816)	(6.860)	(6.676)	(3.596)
Stayers	-1.363	-2.225	0.863**	-5.248*	-6.156*	0.908**
	(2.201)	(2.250)	(0.314)	(2.408)	(2.480)	(0.253)
Adjacent	0.377	0.0387	0.339**	0.450	0.126	0.324**
(in-move)	(0.398)	(0.403)	(0.0796)	(0.474)	(0.479)	(0.0723)
Outside	5.692	-9.365**	15.06**	23.39**	-6.948	30.34**
(in-move)	(3.493)	(2.921)	(1.735)	(6.255)	(5.520)	(3.522)

Note: The effects apply to event year 2 for residents living in treated areas in event year 2. Origin areas refer to residential areas in event year -2. See Eq. (1) for the regression specification. Regressions are unweighted. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

this allows incumbents to make a housing career and live in homes of higher standards without moving away from the area.

Table 10B shows the estimated effects on the number of poor residents (from decile one). For areas with new co-ops, the change in the number of poor residents of 4.7 is small (column 1). Thus, the population gain of 188 (Table 10A, column 1) represents an addition of mostly non-poor residents. A decomposition reveals 12 fewer poor residents in existing homes but 16 more poor residents in new homes (columns 2 and 3). The total effect is driven by a different in-move behavior from outside the wider area with 9 fewer poor residents moving into existing homes and 15 more poor residents moving into new homes. Part of the in-moves to new homes merely replaces in-moves to existing ones, but positive net effects mean that more poor people get a chance to live in the area in absolute terms, alleviating concerns that poor outsiders are negatively affected by new housing.

The in-migration patterns of poor residents to areas with new rentals are qualitatively similar but stronger: We find a total increase in poor residents of 19 residents (Table 10B column 4), which is about 10% of the total population gain of 189 (Table 10A column 4), meaning that a representative share of in-movers are poor. An in-move of 30 poor residents from outside the wider neighborhood to new homes drives the total gain of poor residents.

For understanding our main gentrification result of new housing raising income levels in the area of construction, an analysis of the effects on the income distribution of different groups of in-movers is perhaps more illuminating than an analysis of absolute numbers. Table 11 reveals that not only does the number of in-movers from outside the wider neighborhood increase (Table 10 column 6), but the share of poor among those in-movers decreases (panel A), and their mean income increases relative to the situation without new housing (panel B). The in-mover effects are stronger than the total effects. For instance, following new co-ops, in-movers from the outside are 4.2 percentage points less likely to be poor and have 15% higher income compared to the net area effects of 1.5 percentage points lower poverty rate and 7.3 % higher mean income (column 1). The effects are qualitatively similar but smaller for in-movers from adjacent areas as well as to existing homes (column 2) and areas with new rentals (columns 3 and 4).

Where do the richer in-movers come from? Table 12 reports estimated effects on origin area income of in-movers, and the main result is that in-movers from the outside to areas with new co-ops come from areas with lower poverty rates (than in-migration without new housing) and higher mean income (column 1), and this effect is driven by in-moves to the new homes (small and not statistically significant effects for existing homes in column 2). The point estimates are smaller or not statistically significant for in-movers from adjacent areas and new rentals.

Table 11: Effects on income of in-movers by base-year origin

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Origin:	All	Existing	All	Existing
	A. Poverty rate (pp in decile 1)			
All	-1.539**	-0.839**	-0.960*	-1.161**
	(0.246)	(0.240)	(0.398)	(0.417)
Stayers	-0.337	-0.280	-0.846**	-0.867**
	(0.309)	(0.309)	(0.322)	(0.330)
Adjacent	-0.0432	0.460	-3.007	-1.383
(in-move)	(2.476)	(2.749)	(1.810)	(2.106)
Outside	-4.231**	-2.105**	-2.585**	-1.767*
(in-move)	(0.509)	(0.478)	(0.725)	(0.741)
	B. Mean income (ln(income)*100)			
All	7.269**	2.071**	2.060*	1.869
	(1.019)	(0.751)	(0.958)	(0.983)
Stayers	0.645	0.548	1.608	1.281
	(0.865)	(0.868)	(0.908)	(0.940)
Adjacent	10.79*	3.986	3.723	-2.054
(in-move)	(4.516)	(4.859)	(3.021)	(2.925)
Outside	15.44**	5.085**	4.742**	3.863*
(in-move)	(1.585)	(1.322)	(1.487)	(1.664)

Note: The effects apply to event year 2 for residents living in treated areas in event year 2. Origin areas refer to residential areas in event year -2. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table 12: Effects on origin area income of in-movers by base-year origin

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Origin:	All	Existing	All	Existing
	A. Poverty rate (pp in decile 1)			
All	-1.253**	-1.025**	-0.945**	-0.704*
	(0.182)	(0.188)	(0.275)	(0.278)
Stayers	-1.531**	-1.531**	-0.961*	-0.961*
	(0.246)	(0.246)	(0.399)	(0.399)
Adjacent	-0.639	-0.583	0.0261	0.163
(in-move)	(0.603)	(0.605)	(0.371)	(0.420)
Outside	-0.404**	-0.0229	-0.0909	0.0902
(in-move)	(0.0943)	(0.0891)	(0.101)	(0.108)
	B. Mean income (ln(income)*100)			
All	4.801**	3.990**	2.489**	1.262
	(0.831)	(0.773)	(0.713)	(0.716)
Stayers	7.219**	7.219**	2.063*	2.063*
	(1.020)	(1.020)	(0.959)	(0.959)
Adjacent	1.096	0.717	-0.135	-0.636
(in-move)	(1.016)	(0.998)	(0.879)	(0.950)
Outside	2.630**	0.285	0.803	-0.293
(in-move)	(0.489)	(0.372)	(0.444)	(0.428)

Note: The effects apply to event year 2 for residents living in treated areas in event year 2. Origin areas refer to residential areas in event year -2. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

The alternative migration analysis of year-to-year dynamics in Appendix A5. also contains in-migration results. The initial impact on the residential composition of in-movers is sharper and greater than the impact on the area's residential composition. However, eventually the lower share of poor people moving to the neighborhood bounces back and stabilizes at around the net poverty reduction effect.

7 Conclusions

We used full-population data on individuals and residential buildings in Sweden from 1992-2022 to examine the impact of new large-scale housing construction on neighborhood residential composition and the migration streams generating those effects. Using a difference-in-differences strategy comparing low-income areas with multi-family developments with those without such developments in the same city, we found that market-rate condominiums had strong gentrifying effects.

Our results showed that new co-ops, which resulted in population gains of about 15%, reduced the area share of poor residents in the bottom decile of the city by 1.7 percentage points, which translates to a 44% decrease in the excess poverty rate. Mean income also increased by 7.7%, narrowing the income gap relative to the city mean by 52%. The effect was not only driven by richer people moving into newly built apartments but also by higher incomes in pre-existing homes, suggesting that the areas became more attractive. Socio-demographically, new co-ops improved the population mix by reducing the share of immigrants born outside Europe, raising education levels, and increasing the share of the working-age population. Given that we found no other concurrent housing-stock changes, we estimate an unusually clean effect of new construction. The estimated gentrification effect was stronger in the poorest neighborhoods, and we find positive effects also in areas with high shares of rental units or residents born outside Europe.

In our migration analysis, we found unchanged out-migration patterns; the number and income distribution of out-movers remained the same, and they continued to move to the same type of destination areas. Rather than displacement of incumbent poor residents, immigration of people with higher incomes drove gentrification. However, the new apartments provided housing-career opportunities even to locals who moved into these homes to a greater extent than others.

For new rental buildings, we also found local gentrification effects. However, the impact was smaller and offset by cannibalizing effects in nearby areas.

To conclude, our results show that building new multi-family owner-occupied housing in the poorest neighborhoods is a very suitable policy if the aim is to revitalize these neighborhoods.

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Appendix

A1. The process of building new homes

Larger residential developments primarily involve contractors and municipalities. Contractors focus on building profitable housing, while municipalities regulate when and where construction takes place. Since municipalities often own public housing companies, they sometimes play a dual role as both builders and regulators.

The construction process begins with creating a project description, selecting a building location, acquiring the necessary land, and conducting a preliminary study on local area regulations. The contractor, whether private or public, submits this description to the municipality for review and approval. The municipality then evaluates whether the project aligns with existing development plans or requires an update.

If a new plan is needed, the municipality initiates a consultative process, gathering input on factors such as housing needs, city planning, geological conditions, and environmental impact. However, the final decision remains solely with the municipality. Any modifications made after the initial review are subject to final comments from consulting parties before the municipality decides whether to approve or reject the plan.

Once approved, the plan enters a three-week hold period, during which affected parties can appeal. If no appeals have been filed, the plan is validated, allowing construction preparations to proceed. These preparations often include forming new real property, building infrastructure, and finalizing construction plans.

After the detailed development plans have been established, the contractor can apply for a building permit. Like the development plan, the permit is reviewed by the municipality. Before finalization, a hold period is in effect, during which neighbors and other affected parties may appeal. Once finalized, construction can begin. Up to this stage, contractors remain heavily dependent on the pace of municipal processes.

Appeals against development and building plans significantly extend lead times. Between 2015 and 2022, the validation process for development and building plans took an average of four and a half years, with factors such as legal interpretation, conflicting interests, and outdated processes being major contributors (Federation, 2023). Statistics from 2016 to 2021 indicate that, on average, one in four development plans nationwide was appealed (Evidens, 2023).

In major cities like Stockholm and Gothenburg, appeal rates exceeded 40%, leading to an average delay of 14 months, although only 12% of appeals resulted in changes to the plan. The decentralized construction process, where 290 municipalities each have their own committees and procedures, creates significant variations in lead times. Given these delays, the preparation for new buildings often begins years before completion. This highlights the need to analyze effects before the construction year, accounting for potential anticipation effects.

A2. Descriptive statistics

Table A1 reports variable means for treated areas with new co-ops (column 1), new rentals (column 3), as well as the citywide means in those areas in the base year before new housing construction (event year -2). We report the means for a number of income, socio-demographic, and housing variables. We see that our treated areas have lower relative income than the city mean (91.5 % in areas with new co-ops and 80.4 % in areas with new rentals) and higher shares of residents from the bottom five deciles (decile cutoffs are city-specific). They also have higher shares of student-aged residents (21-25 years old), residents born outside Europe, those without high-school degrees, and residents living in rented homes.

Table A2 reports the mean population, poverty rate, and relative income in event year 2 after the construction of the pioneering building for treated poor areas in the bottom income quartile, and for comparative purposes also for all treated areas. We report means in new large buildings separately (columns 2 and 4). The table shows that the population in new large buildings is around 200, which constitutes approximately 12% of the population in the area. The share of poor in decile 1 is lower in new co-ops and higher in new rentals than in existing homes in treated areas. The pattern is similar for mean relative income: it is higher in new co-ops and lower in new rentals than in existing homes. A comparison of panels A and B shows that new co-ops and rentals in high-income areas attract fewer poor and more high-income residents relative to new co-ops and rentals in poor areas. However, the differences are surprisingly modest, suggesting that a high area poverty rate does not strongly deter high-income individuals.

Table A1: Variable means in event year -2

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Variable:	Treated	City	Treated	City
Income (SEK)	237,147	263,047	207,009	261,153
Relative income (% of city mean)	91.5	100	80.4	100
Decile 1 (%)	13.5	10	16.0	10
Decile 2 (%)	12.0	10	13.9	10
Decile 3 (%)	11.5	10	12.7	10
Decile 4 (%)	10.7	10	11.2	10
Decile 5 (%)	10.1	10	10.1	10
Decile 6 (%)	9.5	10	9.2	10
Decile 7 (%)	8.7	10	8.3	10
Decile 8 (%)	8.0	10	7.3	10
Decile 9 (%)	7.9	10	6.2	10
Decile 10 (%)	8.2	10	5.1	10
Population	1,353	187,928	1,585	177,084
Population ages $\geq$ 21	1,046	144,916	1,200	136,247
Age 0-20 (%)	22.5	25.5	23.5	24.4
Age 21-25 (%)	9.3	6.9	13.2	7.7
Age 61-65 (%)	4.8	5.2	4.2	5.3
Age $\geq$ 66 (%)	17.4	16.4	15.4	17.2
Born in Europe not Sweden (%)	11.0	9.1	12.0	8.9
Born outside Europe (%)	11.2	7.3	17.0	8.7
No high-school degree (%)	25.1	22.3	27.3	22.0
University degree (%)	34.4	35.9	33.6	35.9
Living in rentals (%)	31.0	19.2	51.8	27.6
Housing space (m^2)	40.2	39.8	36.5	39.8
Built year	1964.7	1967.1	1965.9	1965.2
Value year	1970.5	1971.3	1972.1	1970.2

Table A2: Variable means in event year 2

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Variable:	All	New	All	New
	A. Treated Q1 areas			
Population	1,640	206	1,873	209
Poverty rate (% in decile 1)	12.0	7.9	15.3	16.9
Relative income (% of city mean)	97.0	121.7	81.8	81.5
	B. All treated areas			
Population	1,715	192	1,773	204
Poverty rate (% in decile 1)	8.7	6.3	10.8	12.0
Relative income (% of city mean)	104.9	122.3	95.3	90.3

Note: In columns 2 and 4, only multi-family buildings are included.

A3. Heterogeneity results

We have conducted a series of heterogeneity analyses on the baseline results presented in Table 3. While the detailed results of these analyses are presented in this section, a brief summary of the results is provided in Section 5.6.

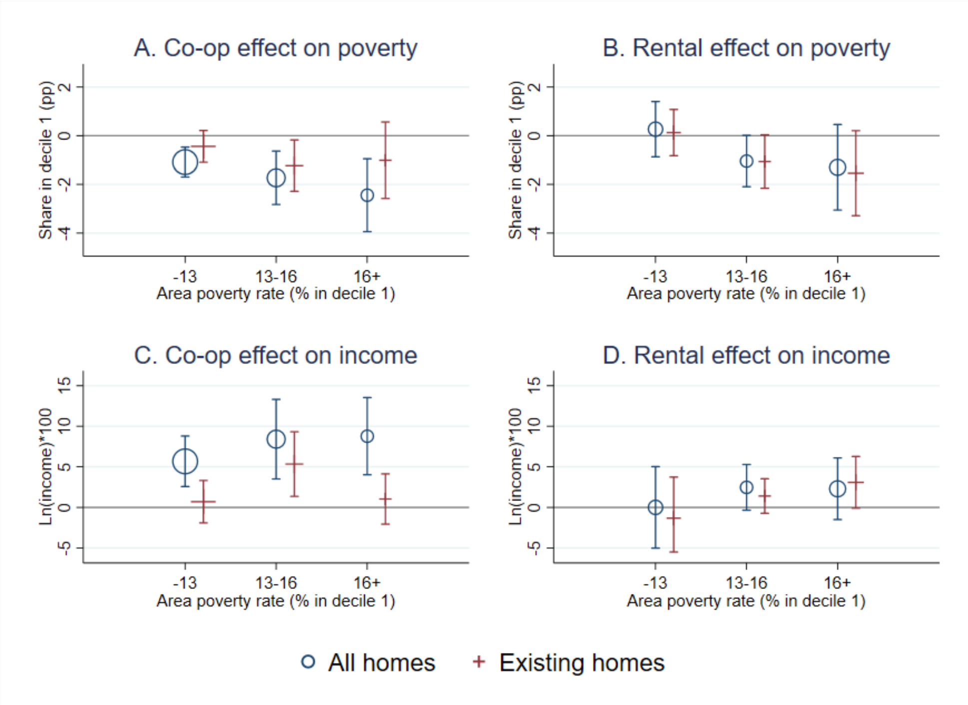
Initial neighborhood poverty rate

Let us begin with effects based on more detailed neighborhood poverty rates. We divide our sample of poor areas in the bottom income (top poverty rate) quartile into three (roughly equal-sized) poverty rate groups and estimate the baseline model (Eq. 1) for each subsample. For each treated area, say the one in the group with the highest poverty rates (above 16%), we select a control group in the same way as before (that is, other untreated areas in the bottom-income quartile in the same city), with the modification that control areas also need to have poverty rates above 16%.[28] The effect heterogeneity results are visualized in Figure A1 where the size of the point estimate marker is proportional to the number of treated areas. Since subsamples are much smaller than the full sample, we focus on the magnitude of the point estimates but bear in mind that precision is too low for us to statistically reject that the estimated effect is the same across subgroups.

We find that the gentrification effects of both new co-ops and rentals are stronger in areas with higher poverty rates, both in terms of poverty rate reduction and average income rise. However, for new co-ops in the areas with the highest poverty rates, residents in the new homes drive this pattern (as the effect difference for all and only existing homes is larger). Since renewal policies typically focus on areas with the most extreme concentration

[28]Thus, both the treated areas and their controls are subsamples of the full sample of bottom-quartile areas.

of poverty, our results are encouraging in terms of the potential for revitalizing these areas through new housing construction.



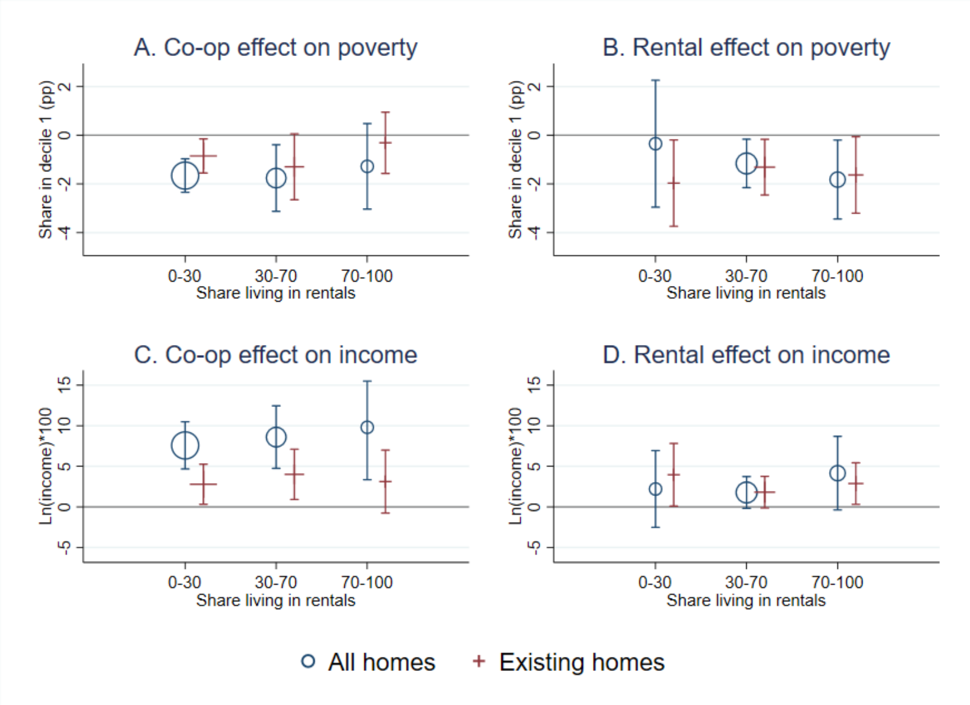
Note: We plot point estimates and 95 % confidence intervals. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level. Along the horisontal axis, the poverty rate applies to the base year. The y-values are censored.

Figure A1: Subgroup estimates by area poverty rate

Initial share of rental housing in the neighborhood

Since rental housing is overrepresented in poor areas, it is of interest to examine if the initial share of residents living in rentals in a neighborhood matters for the gentrifying results observed in the baseline analysis. In Figure A2, we present results that show that a high rental share does not appear to limit the positive impact of new co-ops or rentals.[29]

[29] However, a possible interpretation of panel A is that poverty reduction in the existing homes is more difficult in areas with rental shares above 70%

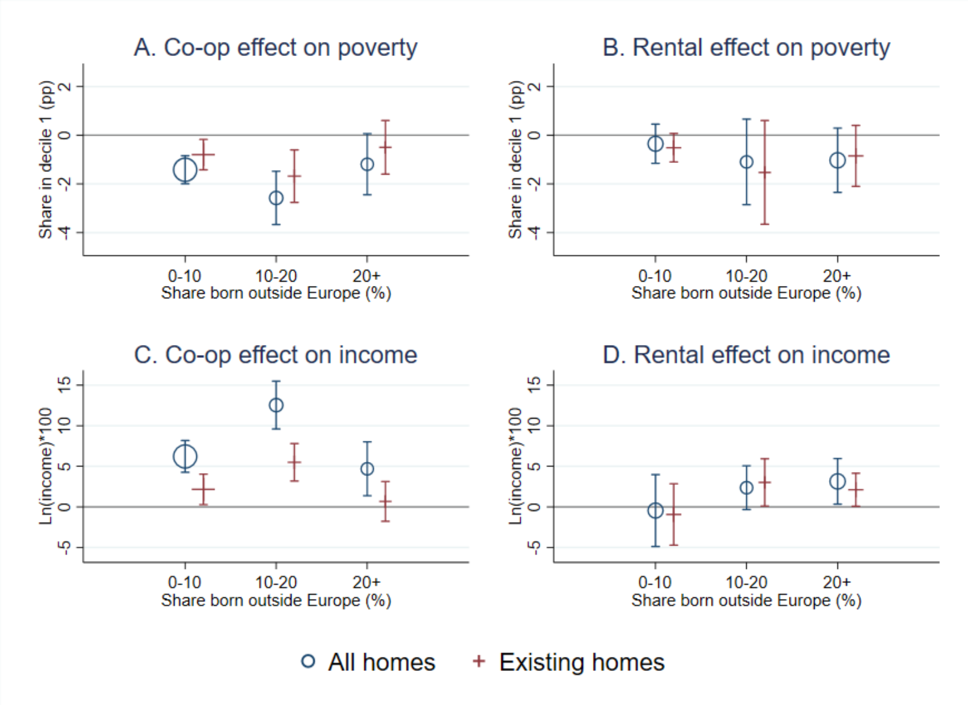


Note: We plot point estimates and 95 % confidence intervals. See Eq. (1) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level. Along the horisontal axis, the rental share applies to the base year. The y-values are censored.

Figure A2: Subgroup estimates by area rental share

Initial demographic situation

Following a period of high immigration rates in many European countries, marginalized neighborhoods are sometimes singled out partly based on the share of residents born outside of Europe. This is, e.g., the case when it comes to the Swedish police list of ”vulnerable areas” and the Danish ”ghetto list” of areas in need of public assistance. While we already showed in Section 5.3 that new housing reduces the share of foreign-born in the neighborhood, a related question is whether a high area immigrant share is a hurdle for gentrification. Figure A3 plots estimated effects across areas with different shares of pre-reform population born outside of Europe. We cannot identify any clear patterns of varying effects in the figures; the gentrification effects of new co-ops may be a bit weaker in areas with immigrant shares above 20%.

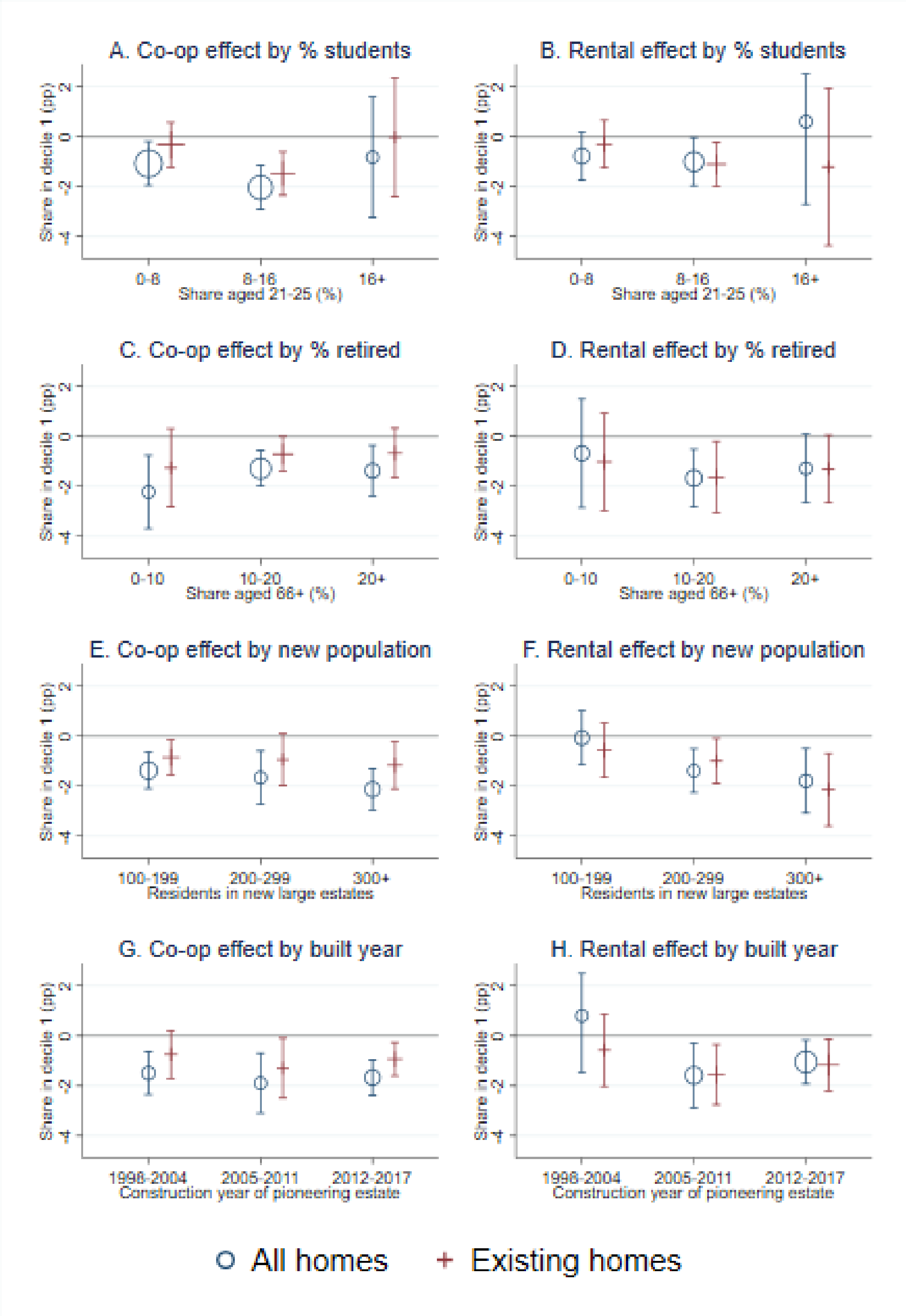


Note: We plot point estimates and 95 % confidence intervals. See Eq. (1) for the regression specification. Regressions are weighted by base-year population (all ages). Standard errors are clustered at the area (DeSO) level. Along the x-axis, the non-European share applies to the base year. The y-values are censored.

Figure A3: Subgroup estimates by area share born outside Europe

Figure A4 plots subgroup results by the area shares of young adults (aged 21-25) and old adults (age 66 or more), number of people living in new large buildings in event year 5, and time period. We find gentrification effects that are weaker in areas with many young adults (panels A and B), weaker for new co-ops (panel C) but stronger for new rentals (panel D) in areas with many old residents. Nothing indicates that total average effects across areas are driven by new housing effects in areas predominantly inhabited by students or retired people.

The estimated gentrification effect increases with the size of new buildings (panels E and F), but the fact that this increase is less pronounced for new co-ops might mean that even developments that are somewhat smaller in magnitude have an important impact on poor areas. The co-op effect is fairly constant across our three time periods (panel G), but rentals only had positive impacts after 2005 (panel H), which is the year in which rents in newly produced homes were deregulated to allow also reflecting production costs (rather than only the user value). Theoretically, one expects a freer rent-setting system to stimulate the construction of higher-quality rentals. Our heterogeneity results suggest that rents that are toughly regulated eliminate the gentrifying effects of new rentals. In 2011, another policy was implemented requiring previously non-profit public housing companies to operate on business-like principles, likely leading to freer and higher rents in the existing stock in many places. The gentrification effect of new co-ops or rentals that we find is not larger since 2012.



Note: We plot point estimates and 95 % confidence intervals. See Eq. (1) for the diff-in-diff regression specification. Regressions are weighted by base-year population (all ages in panels A-F). Standard errors are clustered at the area (DeSO) level. Along the x-axis, the variables apply to the base year. The y-values are censored.

Figure A4: Subgroup estimates by shares of students and retired, new population, and time period

A4. Robustness and sensitivity analyses

We have conducted three sets of robustness tests. These tests, summarized in Section 5.7, are presented in detail in this section.

Where there any concurrent housing policies taking place at the same time as the new housing construction?

To better isolate the causes of neighborhood gentrification, we report in Table A3 estimated effects on housing outcomes such as housing space, housing standards, and renovations (to examine potential effects from other housing policies). We provide, from here on, unless otherwise stated, basic diff-in-diff estimates of average effects across post-treatment years (Eq. 1).

In Table A3A, we quantify the expansion of the housing stock in terms of m^2 living space. We use the per-person addition to facilitate interpretation; in particular, the outcome is m^2 living space per pre-reform base-year resident, and this eliminates the influence of the rising post-treatment population.[30] We find that the additional housing space expansion relative to the control areas when the pioneering property was a co-op amounts to 7.8 m^2 per person on average, with the entire expansion coming from more owned space (column 1). In areas where the pioneering property was a rental, the corresponding number is 5.3 m^2 per person on average, with the main addition consisting of rented space (column 3). In comparison, the pre-treatment average housing space was 36-37 m^2 per person; thus, the treatment corresponds to housing additions of 15-20% of the pre-treatment housing space.

For existing homes, our effect estimates are small and not statistically significant, both when including all tenure types and for owned or rented homes separately (panel A, columns 2 and 4) Thus, new homes increased the housing stock and did not merely replace older demolished buildings in the treated areas. Neither was more new housing space added in ongoing renovations. Furthermore, although tenure-type conversions of rental apartments to owner-occupied apartments were common in our period of study, concurrent conversions were not more common in the treated areas relative to the control areas.

Table A3B shows that because of the newly added homes, the average construction year of the homes in the treated areas increased by 6.7 years in areas with new pioneering co-ops (column 1), decreasing the age of homes by the same amount. The increase is entirely due to the addition of new owned homes. Correspondingly, residents in treated areas, on average, inhabit homes built 5.0 years later in areas with new pioneering rentals (column 3).

To measure renovations, we make use of a variable called the value year.

[30] Using log housing space would have been another option, but that makes a comparison of changes to the owned vs. rented housing space harder; a small absolute change in, e.g., rented space could translate into a large percentage effect if the pre-reform rented housing stock is small.

Table A3: Effects on the housing stock: examining if there are any concurrent housing policies

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Tenure type:	All	Existing	All	Existing
	A. Housing space (m^2/base-year person)			
All types	7.758**	0.0756	5.261**	-0.0656
	(0.846)	(0.350)	(0.629)	(0.302)
Owned homes	7.662**	0.993	1.104*	-0.0276
	(0.808)	(0.535)	(0.440)	(0.242)
Rented homes	0.0955	-0.918	4.157**	-0.0380
	(0.558)	(0.512)	(0.457)	(0.368)
	B. Newer homes (built year)			
All types	6.691**	0.281	5.031**	-0.310
	(0.590)	(0.270)	(0.545)	(0.275)
Owned homes	8.676**	0.523	2.375**	-0.116
	(0.731)	(0.499)	(0.824)	(0.304)
Rented homes	0.939	-0.299	5.986**	-0.307
	(0.682)	(0.462)	(0.653)	(0.442)
	C. Renovation year gains (value - built year)			
All types		0.320		1.228
		(0.255)		(0.767)
Owned homes		0.303		1.752
		(0.357)		(1.084)
Rented homes		0.347		0.777
		(0.595)		(1.139)

Note: Value year equals built year initially but is later updated to reflect extensive renovations. In panels A-C, we use data from 1998-2022 with treated areas in 2000-2017 (a non-balanced panel with 63 new co-ops and 65 new rentals). To retain as many treated areas as possible, we allow unbalanced samples. See Eq. (1) for the regression specification. Regressions are weighted by base-year population in panels A-C. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table A4: Effects on alternative income measures

	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
Outcome:	All	Existing	All	Existing
	A. Family income per adult			
Poverty rate (pp in decile 1)	-2.079**	-0.952**	-0.838*	-1.180**
	(0.310)	(0.305)	(0.413)	(0.446)
Mean income (ln(inc)*100)	7.660**	2.677**	1.593	1.333
	(1.036)	(0.855)	(0.926)	(0.821)
	B. Working-age income (ages 26-60)			
Poverty rate (pp in decile 1)	-1.710**	-0.929**	-0.732	-0.830*
	(0.317)	(0.330)	(0.389)	(0.350)
Mean income (ln(inc)*100)	6.866**	2.724**	1.021	0.452
	(1.095)	(0.947)	(0.919)	(0.821)

Note: See Eq. (1) for the diff-in-diff regression specification. Regressions are weighted by base-year population (21 years or older in panel A and 26-60 years old in panel B). Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

The value year is the same as the construction year until there is an extensive renovation or reconstruction, after which the value year (but not the construction year) is adjusted to reflect the increased quality. The difference between the value year and the construction year reflects the degree to which a building has been renovated, and we use it as the outcome in our analysis in Table A3C.[31] The estimated renovation year gains are small (compared to the point estimates in panel B) and not statistically significant.

Alternative income measures and control groups

Panel A of Table A4 reports estimated effects using family income per adult (aged 21 or more), assigning the same mean disposable income per person to each adult in the family, assuming that family members share their resources. In panel B in Table A4, we instead restrict the individual sample to adults in the working ages (26-60) when calculating the area poverty rate and mean income. Overall, the estimated effects are close to those obtained when using our main individual income measure for all residents aged 21 or more (c.f. Table 3).

Table A5 reports estimated effects using alternative control groups that are more or less reasonable, and Figure A5 shows the corresponding event-study estimates. We focus here on the poverty rate outcome but the result patterns are similar for the log of mean area income. In panel A, we restrict

[31] For single-family detached houses, the value year is only updated after an expansion of the living area; in such cases, the value year is a weighted average of the construction year and the expansion year with weights depending on the amount of living area added.

Table A5: Estimated effects with alternative control groups

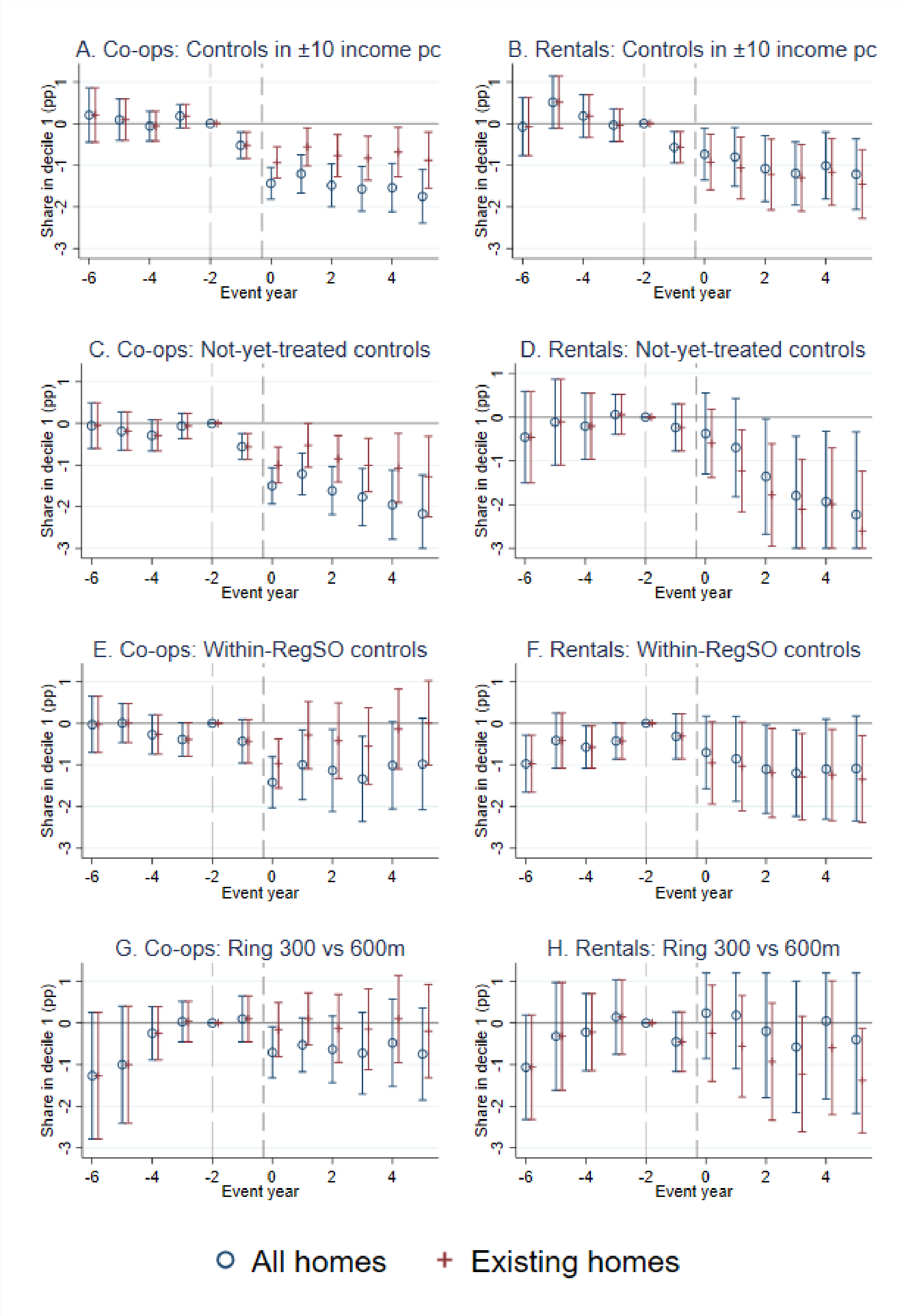
Outcome: Poverty rate	(1) New co-ops All	(2) New co-ops Existing	(3) New rentals All	(4) New rentals Existing
	A. Controls in ±10 income pc			
Share in decile 1 (pp)	-1.499**	-0.812**	-1.044**	-1.271**
	(0.254)	(0.252)	(0.349)	(0.386)
	B. Not-yet-treated controls			
Share in decile 1 (pp)	-1.512**	-0.823**	-1.055	-1.312*
	(0.302)	(0.306)	(0.653)	(0.560)
	C. Adjacent controls within wider area			
Share in decile 1 (pp)	-1.016**	-0.276	-0.548	-0.665
	(0.385)	(0.360)	(0.435)	(0.422)
	D. Inner vs. outer ring (300 vs. 600 m)			
Share in decile 1 (pp)	-0.451	0.0573	-0.547	-0.729
	(0.351)	(0.337)	(0.400)	(0.384)

Note: In panel A, for a treated area, only areas within ± 10 income percentiles in the same city are selected as controls. In panel B, for each tenure type, areas treated in 2015-2017 act as controls for treated areas in 1998-2013. In panel C, adjacent DeSO areas within the wider RegSO area serve as controls for the treated DeSO area. In panel D, a treated area consists of an inner ring with a radius of 300m from the pioneering building, and the control area consists of an outer ring between 300-600 m from the pioneering building. See Eq. (1) for the diff-in-diff regression specification. Standard errors are clustered at the area (DeSO) level in panels A-C and area*ring level in panel D. * p<0.05, ** p<0.01.

the control group for each treated area to other untreated areas in the same city within an income percentile of ±10 of that of the treated area. In this way, for each treated area, we get controls that are more similar in poverty rate than controls based on all areas in the entire poorest quartile in the city. In panel B, we use treated areas in 2015-2017 as controls for treated areas in 1998-2013. Thus, for each treated area with new co-ops in 1998-2013, we use all not-yet-treated areas in 2015-2017 that later will receive new co-ops as the control group, and similarly for each treated area with new rentals. The underlying logic is that developers choose sites in both groups for similar reasons, but one building is completed before the other for random reasons such as the timing of when sites are available for purchase. Of course, it is possible that like the selection of location, developers time their developments to maximize profits.

A popular assumption in the previous literature is that areas that are geographically the closest to a treated area are similar enough to constitute good control areas. In panels C and D of Table A2, we implement two possible choices of control areas based on this principle. In panel C, adjacent DeSO areas within the wider RegSO area serve as controls. In panel D, we implement a ring diff-in-diff defining an inner ring with a radius of 300m from the pioneering building as the treated area, and an outer ring between 300-600 m from the pioneering building as the control area.

In Figure A5, panels A-D show that pre-trends appear parallel and none of the placebo estimates are statistically significant. Thus, we think that the identification is credible in the specifications using controls within a tighter income band and not-yet-treated controls. Panels A and B in Table A5 show that these alternative control groups deliver similar estimated effects as our main results in Table 3. In contrast, placebo estimates are statistically significant in panel F, and in panels G and H, the pre-reform estimates are upward trending with some estimates being greater in magnitude than the post-reform estimates. Given that we have selected treated areas with the highest poverty rates, surrounding areas will typically have lower poverty rates. These placebo estimates in panels F-H suggest that they also have different trends in poverty rates. Hence, adjacent DeSO areas in the same wider RegSO area or outer rings are not suitable control areas. For completeness, we reported estimated effects using these control groups in panels C and D of Table A5. Estimated effects are quite different from our main results, but as discussed, the identifying assumptions are likely violated.



Note: We plot point estimates and 95 % confidence intervals. See Eq. (2) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level in panels A-F and area*ring level in panels G and H. See the note in Table A5 for additional details.

Figure A5: Event-study estimates with alternative control groups

Decomposition of effects by types of new homes

Table A6 reports estimated effects on residential composition by excluding different types of new homes, including the pioneering building, other new large buildings, and new small buildings. In areas where the pioneering building is a new co-op, as more categories of new homes are excluded (moving from column 1 to 4), the poverty rate and mean income effects decrease. Over half the gentrifying effect that can be attributed to the residential composition in new homes is due to the pioneering building; e.g, panel B shows that the pioneering building contributes with an effect of 2.8% (7.7 − 4.9) to the 5% total mean income effect (7.7 − 2.74). Subsequent large buildings and residential changes in new small buildings contribute approximately equally to the remaining part. In contrast, in areas with a pioneering rental, none of the effect is due to the pioneering building; in fact, removing the pioneering building alone increases the estimated effect (from 1.6% to 2.4%).

Figure A6 plots event-study estimates of the income effects distinguishing between types of new homes, focusing on estimates after the base year (dropping the placebo estimates from the figures, but not from the estimation). While most of the effect via the pioneering building kicks in immediately in the construction year (the difference between the estimates in circles and triangles remains constant thereafter), the contributions from residential changes in the rest of the housing stock grow gradually.

We report estimated effects on housing space and population in Table A7 and present corresponding event-study estimates in Figure A7. Panel A in the table shows that out of the 18.6% increase in housing space in areas with new co-ops, 6.9% (18.55 − 11.64) is due to the pioneering building, 6.0% is due to other large buildings, and 5.6% is due to more small buildings. The fact that we find no effects on housing space in existing homes implies that demolitions were equally common in treated and control areas. The patterns are similar in areas with new rentals and for population changes in the treated areas (panel B).

Figure A7 shows that space additions and population gains grow gradually among new buildings other than the pioneering ones. It also shows that these gains did not start before the pioneering buildings were completed (event year -1). This means that the anticipation effect we found before cannot be explained by smaller new buildings being completed before the large buildings. Instead, migration patterns to and from the existing homes must have changed in anticipation of not-yet-completed new housing.

Table A6: Estimated effects on income by types of new homes

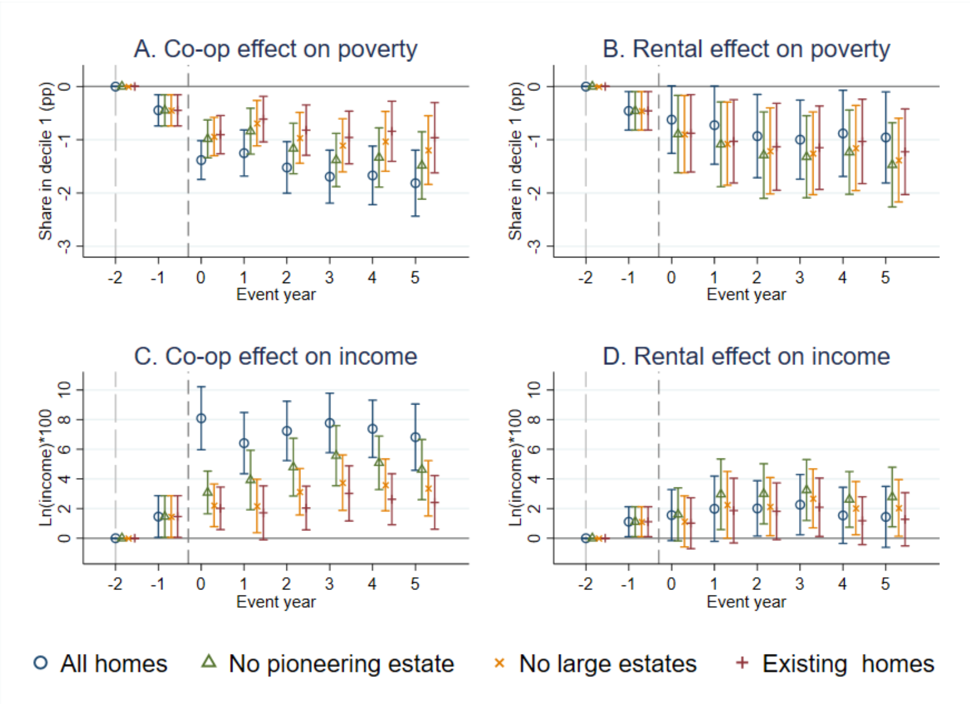
	(1)	(2)	(3)	(4)
Tenure type:	All	No pioneer	No large	Existing
	A. Poverty rate (pp in decile 1)			
New co-ops	-1.675**	-1.317**	-1.107**	-0.970**
	(0.256)	(0.257)	(0.259)	(0.258)
New rentals	-0.994**	-1.357**	-1.303**	-1.216**
	(0.353)	(0.370)	(0.368)	(0.371)
	B. Mean income (ln(income)*100			
New co-ops	7.660**	4.886**	3.411**	2.677**
	(1.036)	(0.947)	(0.884)	(0.855)
New rentals	1.593	2.495**	1.839*	1.333
	(0.926)	(0.940)	(0.888)	(0.821)

Note: In column 2, new pioneering buildings are excluded. In column 3, all new multi-family buildings are excluded. See Eq. (1) for the diff-in-diff regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table A7: Estimated effects on housing space and population by types of new homes

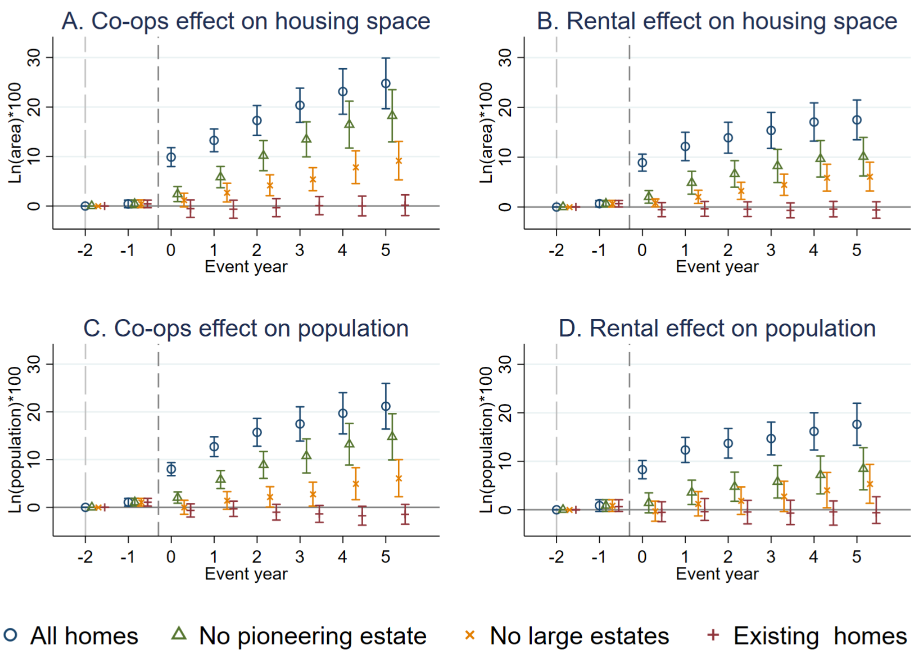
	(1)	(2)	(3)	(4)
Tenure type:	All	No pioneer	No large	Existing
	A. Housing space (ln(m^2)*100)			
New co-ops	18.55**	11.64**	5.593**	0.207
	(1.656)	(1.656)	(1.284)	(1.025)
New rentals	14.51**	7.383**	4.333**	0.0592
	(1.668)	(1.484)	(1.036)	(0.897)
	B. Population (ln(population)*100)			
New co-ops	15.35**	8.797**	2.456	-1.548
	(1.573)	(1.543)	(1.293)	(0.957)
New rentals	13.89**	5.225**	2.521	-0.327
	(1.681)	(1.641)	(1.562)	(1.259)

Note: In column 2, new pioneering buildings are excluded. In column 3, all new multi-family buildings are excluded. See Eq. (1) for the diff-in-diff regression specification. Regressions are weighted by base-year population (all ages). Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.



Note: We plot point estimates and 95 % confidence intervals. See Eq. (2) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level. We dropped estimates for event years ≤-3 in this Figure. Those are the same as in Figure 5.

Figure A6: Estimated event-study effects on income by types of new homes



Note: We plot point estimates and 95 % confidence intervals. See Eq. (2) for the regression specification. Regressions are weighted by base-year population (all ages). Standard errors are clustered at the area (DeSO) level. We dropped estimates for event years ≤ -3 in this Figure.

Figure A7: Estimated event-study effects on housing space and population by types of new homes

A5. Year-to-year migration analysis

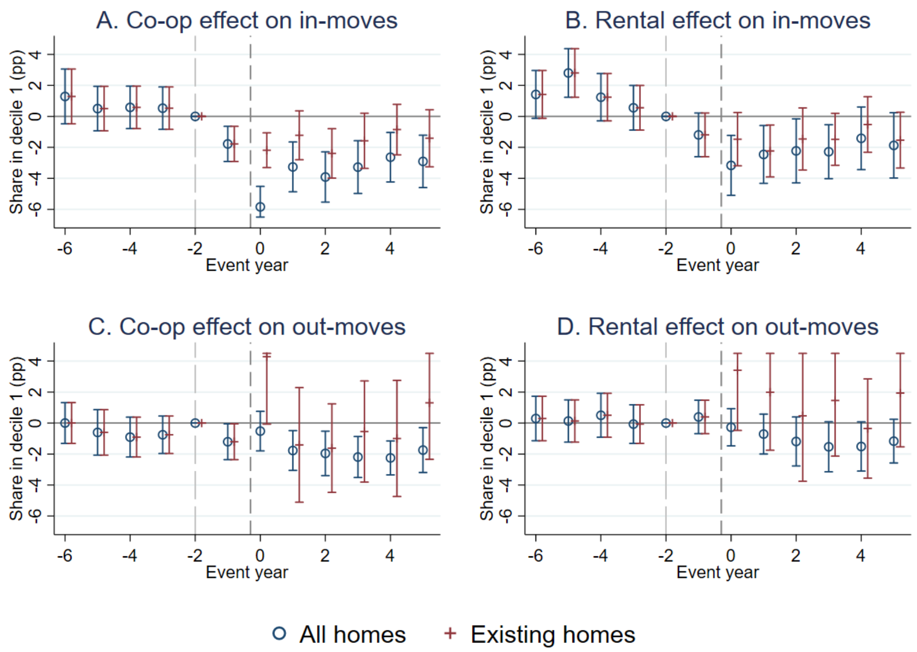
Year-to-year migration can be analyzed by defining in- and out-movers as people moving into or out of the area since a year ago. We provide estimated (average across-year) effects on year-to-year migration in Table A8 and corresponding event-study estimates in Figure A6. Table A8A shows a larger reduction of poor in-movers than the total poverty effect in the area (e.g., for new co-ops, the estimated effect is -4.3 percentage points in Table A8 column 1 vs. -1.7 percentage points in Table 3 column 1). Panels A and B in Figure A8 show initial sharp drops in the share of poor among in-movers, but the effect eventually wears off and stabilizes at around the total poverty reduction effect.

When it comes to out-migration, Table A8B shows that the share of poor out-movers *decreases* (by 1.3 in new co-ops). However, since year-to-year outmoves later in time, e.g., between event years 2 and 3, also include people who moved in after event year 0 leaving the area, this analysis does not rule out the displacement of pre-treatment incumbents (but our analysis in Subsection 5.7 did). The dynamic analysis in panels C and D och Figure A8 reveals a temporary hike in the share of poor moving out from existing homes in the area, but since the total effect for all homes is close to zero, we infer that the poor out-movers from existing homes move to the new homes in the same area once the pioneering building is completed.

Table A8: Estimated effects on year-by-year migration

	(1)	(2)	(3)	(4)
Outcome: Poverty rate	New co-ops		New rentals	
	A. In-moves			
Share in decile 1 (pp)	-4.306**	-2.186**	-3.084**	-2.653**
	(0.553)	(0.616)	(0.697)	(0.657)
	B. Out-moves			
Share in decile 1 (pp)	-1.256**	-0.00387	-0.996*	1.967
	(0.310)	(0.944)	(0.415)	(1.089)

Note: See Eq. (1) for the diff-in-diff regression specification. Regressions are weighted by base-year population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.



Note: We plot point estimates and 95 % confidence intervals. See Eq. (2) for the regression specification. Regressions are weighted by base-year population. Standard errors are clustered at the area (DeSO) level. The y-values are censored.

Figure A8: Estimates of event-study effects on year-by-year migration