

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

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Abstract

Using almost three decades of full-population register data with detailed geo-coded information on how and where all individuals in Sweden live, on their moving patterns, and on their socio-economic characteristics, this paper examines if new large-scale housing construction is a suitable policy tool for revitalizing poor neighborhoods. The answer is yes. We reach four main conclusions. First, we find that new large developments of market-rate condominiums have strong gentrifying effects: the estimated effect on average income is 15% in the poorest quartile of neighborhoods. Second, the effect is not only driven by richer people moving into the newly built owned apartments, but also by average income rising by 10% in pre-existing homes. Since we do not find other concurrent housing-stock changes such as renovations and rent increases, this indicates that the areas become more attractive. Third, most of the gentrifying effects are due to high-income people moving in from richer areas outside a wider neighborhood. Fourth, we do not find any displacement of incumbent residents.

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

JEL Classification: R31, R21, R52

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1 Introduction

Most countries use different housing policies to revitalize poor and in other ways disadvantaged neighborhoods, such as new housing construction, housing demolitions, and large redevelopments (renovations). Since housing policies have both direct and indirect impacts on neighborhood development, they are popular instruments to address economic, social, and physical aspects of neighborhood revitalization. By improving the conditions in the poorest neighborhoods, the hope is to mitigate the adverse consequences of growing up and living in poor neighborhoods (for a recent overview of causal neighborhood effects on individual outcomes, see Chyn and Katz, 2021) and to combat spatial inequality.

Even though there are studies examining the neighborhood effects of housing demolitions (Almagro et al., 2023), new large-scale housing construction (Diamond and McQuade, 2019, Singh, 2020, Pennington, 2021, Li, 2021, Asquith et al., 2023) and renovations of multi-family housing (Dahlberg et al., 2023), consensus has not yet been reached on which housing policy that works for different purposes under what conditions. We will add to this literature by examining if and how new large-scale housing constructions affect poor neighborhoods in terms of revitalization, gentrification, and migration patterns in a rent-regulated system. While neighborhood revitalization in poor neighborhoods is of particular interest for policymakers aiming at reducing spatial inequalities, the study by Asquith et al. (2023) is the only earlier paper on the causal effect of new housing that has this focus.¹

We use yearly registry-based micro data that is unusually rich in geographic and other background information on all housing buildings and the universe of individuals in Sweden over a long time (1991-2017). While the small but growing literature on the very local neighborhood effects of new housing construction in some cities in the U.S. (Diamond and McQuade, 2019; Singh, 2020; Pennington, 2021; Li, 2021; Asquith et al., 2023) focus on effects on rents and housing prices, we can estimate the impacts on neighborhood residential composition. It has been difficult to study this outcome in the U.S. due to data availability; e.g., census data that are often used are collected only each tenth year. Nevertheless, changing the income mix of residents in poor neighborhoods by making the area more attractive also for richer households is often a major policy goal.

Another main contribution of our paper is that since we follow each individual over time, we can estimate effects on migration streams (building-level accuracy), including those of incumbent residents. While the intent of all types of place-based policies typically is to improve disadvantaged neighborhoods, the unintended consequence of displacement can undermine the goal of uplifting the entire community. This is also highlighted by Chyn and

¹They used data from 11 cities in the U.S. with varying renter protection systems. The fact that Sweden has a uniform rent-regulated system allow us to get a cleaner interpretation of the estimates in terms of an amenity effect.

Katz (2021) who conclude that "A final frontier research area involves the estimation of the impact of place-based policies to improve low-income neighborhoods on the intended beneficiaries – the incumbent (pre-existing) adult residents and their children." While mobility effects have been studied using longitudinal address histories for samples of individuals (Pennington, 2021; Li, 2021; Asquith et al., 2023), with the study by Asquith et al. (2023) being the only paper focusing on poor neighborhoods, only limited conclusions could be drawn due to the lack of individual income data.

Focusing on poor neighborhoods in urban areas, we apply a difference-in-differences (diff-in-diff) design to examine how large-scale housing construction (constructions with over 100 residents) affects residential income composition in the immediate surrounding (so called DeSO areas, which are neighborhoods with 700-2,700 residents in 2017), the adjacent areas in a wider neighborhood (RegSO areas, which are neighborhoods with 700-23,000 residents), and migration streams to the new building and its neighborhood. In Sweden, local governments (municipalities) control housing supply by having the right to approve development plans and issue building permits. Still, the location of new estates appears random relative to area income trends in our data, suggesting the lack of a strategy to build certain types of multi-family estates (for instance owned apartments) in certain locations (for instance poor areas) with the purpose of affecting the residential income mix.

We find that new large residential developments of co-ops, the Swedish version of market-rate condominiums, have strong gentrifying effects, and more so in poorer micro-neighborhoods (DeSO areas). The estimated effect on average income is 15% in the poorest quartile of areas, with even larger effects in the poorest percentiles of neighborhoods. In contrast, new rentals, which are rent-regulated in Sweden, have small effects on area income in poor areas. These results align with the positive effects of new market-rated homes that Pennington (2021) found for San Francisco.

Another main finding is that the gentrification effect in the poorest quartile of areas is not only driven by richer people moving into the newly built owned apartments, but also by average income rising by 10% in pre-existing homes. The estimated effect also persists in areas with high rental shares in which housing costs did not grow (as rents cannot easily respond due to rent regulation), indicating that the poorest areas become more attractive.

We also provide empirical evidence suggesting that the new constructions are not accompanied by housing demolitions, renovations, tenure type conversions, or rent increases. Potential reasons for the gentrification effects include a new socio-economic composition in the neighborhood, more or better amenities such as restaurants, cultural activities, schools and other public services, or that the new buildings themselves make the neighborhood more attractive.

A novel finding is smaller negative spill-over effects on adjacent areas within the wider neighborhood for all types of new estates and areas. The overall gentrifying effect of new market-rated homes on the wider neighbor-

hood is, therefore, smaller than the positive effects on the immediate surroundings due to those "cannibalizing" effects.

Our in-migration analysis shows that most of the gentrifying effects of new owned apartments are due to high-income people moving in from richer areas outside the wider neighborhood (RegSO area). In comparison, Asquith et al. (2023) found that new market-rated homes increased in-migration of households from poor areas in U.S. cities but they cannot observe whether these households are actually poor. More generally, new homes create moving chains; as households move into new residences, they generate vacancies in the existing stock, and as people move into these vacancies, additional vacancies are created. Several studies showed that poor people from poor neighborhoods often take part of those moving chains (Bratu et al., 2023; Mast, 2023; Kindström and Liang, 2024). For incumbents within the wider neighborhood, we find that high-income locals are over-represented in the new homes (by a factor of four). Thus, these homes do provide opportunities for richer residents to stay in the area in homes with higher standards.

Finally, we find no evidence of changes in out-migration patterns and thus no signs of displacement. This supports the hypothesis that we would not expect to see any displacement of pre-existing tenants in a rent-regulated system. In her study on data from San Francisco, Pennington (2021) also finds that new market-rate housing construction leads to a lower displacement risk.

Taken together, the results in this paper show that new construction of privately owned homes can be a very successful policy tool for revitalizing the poorest areas. One concern in the broader previous research on place-based policies is that interventions that lift individuals might not lift poor neighborhoods due to out-migration of successful individuals (Ruiz-Alejos and Prats, 2022). The typical pattern is that once individuals in poor and otherwise vulnerable neighborhoods succeed, they tend to move to better housing in, other, better-off neighborhoods. New market-rate homes instead allow successful residents to make a local housing career. Another fear often causing resistance against new housing construction among residents in targeted areas is that the benefits of the policies not only accrue to the original residents to a limited extent, but also harm them, especially the economically vulnerable residents, due to rising rents and subsequent displacement. However, we find no signs of rent increases or changes in out-migration patterns; thus, we have a case of gentrification (upgrade of neighborhood quality) without displacement.

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 Section 5 and the final section concludes.

2 Institutional background

2.1 Swedish housing policy

Swedish municipalities bear the responsibility for city development as outlined in detailed development plans in which they also set an upper limit for new housing constructions. This authority is commonly known as the "plan monopoly". By law, they should also supply qualitative and adequate homes (Swedish Code of Statutes, 2000). They also affect the housing supply by selling land. Today, about half the total rental stock consists of apartments owned by municipal housing companies that exist in 270 out of 290 municipalities.

There are primarily three types of housing tenure in Sweden. First, individuals can own their own house, typically involving single-family ownership and small-scale housing. Second, one can own an apartment, which usually means possessing an apartment (owning a share) in a housing cooperative.² This tenure form is equivalent to owning a condominium in the US context. Third, individuals can rent their apartment, either from a private landlord or a public (municipal) landlord. In the absence of social housing as in the US or UK, the poorer residents typically reside in rental apartments owned by municipalities. These latter two tenure types constitute the prevalent large-scale, multi-family housing in Sweden.

A disproportionate amount of homes today were constructed during the government-driven "Million-Homes Program" 1965-1974. The construction rate plunged after the financial crises in the 1990s. Concurrently with the deregulations after the crises, housing companies privatized a large part of their housing stock by converting rental apartments to tenant-owned cooperative apartments (co-ops). Since the turn of the millennium, constructions picked up some pace again, now mainly driven by private equity financing the construction of co-ops. In 2017, 52% of the population lived in privately-owned detached houses, 16% lived in co-ops, and 32% lived in rental apartments.³

2.2 The process of building new homes

The construction of new housing is a process primarily involving contractors and municipalities. Contractors aim to build profitable housing, while municipalities regulate when, where, and how construction occurs. Municipalities often own public housing companies, and in some cases, they act both as builders and regulators

²Membership in a housing cooperative grants individuals the right to inhabit the apartment (corresponding to the share of the co-op) indefinitely. The apartments (shares) can be freely bought and sold on the market. While it is possible to own a condominium, it is an extremely unusual form of tenure in Sweden.

³The statistics are from our own calculations based on the data from Statistics Sweden that we present in the next data section

The initial step in the construction process is creating a project description, determining the building location, procuring land, and conducting a pilot study on area regulations. The contractor, whether private or public, submits the project description to the municipality for review. During this review, the municipality assesses whether the project aligns with existing development plans or necessitates an update or a new plan. If a new plan is required, the municipality engages in a consultative process, gathering opinions on aspects such as environmental impact, housing needs, city planning, and geological prerequisites. Nevertheless, the ultimate decision on the plan rests solely with the municipality. Any modifications made after the initial review are subject to final comments from consulting parties, and the municipality then decides whether to accept or reject the plan. An accepted plan undergoes a three-week hold period, during which affected parties can appeal. If there are no appeals, the plan is validated, enabling construction preparations, often including the formation of new real estate, infrastructure building, and finalization of construction plans.

Upon establishing detailed development plans, the contractor may apply for a building permit. Similar to the development plan, the building permit undergoes reviews at the municipality, with a hold period during which neighbors and other affected parties may appeal before finalization. Construction commences upon finalization. Until this stage, contractors are heavily dependent on the speed of municipal operations.

Appeals against development and building plans significantly contribute to increased lead times. From 2015-2022, the development and building plan validation process averaged four and a half years, with factors like law interpretation, conflicting interests, and outdated processes identified as key contributors (The Swedish Construction Federation, Initiativet bygg i tid, and Fastighetsägarna, 2023). Statistics from 2016-2021 showed that, on average, one out of four development plans were appealed countrywide (Evidens, 2023). In major cities like Stockholm and Gothenburg, appeal rates exceeded 40 percent, resulting in a 14-month average increase in lead times, with only 12 percent leading to changes in the plan. The decentralized construction process, with 290 municipalities having their own committees and methods, contributes to significant variations in lead times. The lengthy construction process implies that planning and preparation for a new building commence years before completion. This underscores the importance of analyzing effects before the construction year, considering the possibility of anticipation effects.

3 Data and descriptive statistics

3.1 Data

We use annual data from GeoSweden, a registry-based database compiled (and anonymized) by Statistics Sweden and administered by the Institute for

Housing and Urban Research (IBF) at Uppsala University. The database covers the entire Swedish population from 1990 to 2017 as well as all residential estates. It links individuals, via their social security numbers and registered addresses, to registers containing information on the residential estates in which they reside.

At the individual level, GeoSweden contains data from RTB and LISA, microdata registries available at Statistics Sweden for researchers at Swedish universities, with various economic and demographic variables including income sources, taxes, birth year, country of birth, educational attainment, and marital status.

Data on residential estates, from Statistics Sweden's estate registries Fastighetsregistret and Fastighetstaxeringsregistret, include information on geographic location (coordinates down to a resolution of 100 meters), type (e.g., detached house or apartment building), construction year, size, standard, assessed value, and judicial owner.

While social scientists have frequently used good Swedish individual-level microdata before, geo-coded data down to the estate level is a near-unique feature of GeoSweden and allows us to follow how individuals move in detail. Previously, researchers have used GeoSweden to study neighborhood effects.

3.2 Treated areas with pioneering estates

In 2018, Statistics Sweden divided Sweden into 5,984 DeSO areas, the definition of (micro) neighborhoods that we use. DeSO areas had a population between 700 and 2,700 in 2018, but many were sparsely populated or had no population in 1990 (36 DeSO areas). In addition to being similar in size, this division accounts for natural spatial barriers such as streets, railroads, and water bodies. Moreover, the borders do not cut through those of the 290 municipalities and largely respect previous definitions of urban limits and election districts. However, the DeSO areas do not have any administrative purposes and do not have names.

In 2018, at the request of the Swedish government and in cooperation with the municipalities, Statistics Sweden also aggregated DeSO areas into 3,363 RegSO areas, the definition of wider neighborhoods that we use. The idea is that area-level socio-economic statistics would from now on be collected at this level and segregation between RegSO areas be monitored over time. RegSO areas are named and align closely with several different previous more or less formal definitions of city districts and with popular notions of neighborhoods. A RegSO area often contains an elementary school and a district center with public and private services such as a medical center, postal services, and shops.⁴

⁴Other administrative and non-administrative area definitions exist for elections, religious purposes, schooling, and the housing market, but none of them are standardized across municipalities and time (e.g., election district, parish, SAMS, and NYKO)

Since our focus is on urban areas, we restrict ourselves to the main urban area of each municipality, which gives us 4,324 DeSO areas. We focus on large new multi-family estates with more than 100 residents five years after the year of construction since they are more likely to significantly affect the neighborhoods. For the 1,126 estates built between 1996 and 2013, we follow each estate during a time window of eleven years, six years before and five years after the construction year. Letting the completion of a new estate be the treatment of interest, we define event year zero as the completion year, and our event-year variable ranges from a value of -6 to 4.

Many large estates were built in the same DeSO or RegSO areas and have overlapping time windows. We have 366 non-overlapping DeSO area windows (a combination of DeSO area and time window) with a pioneering estate, which we define as the first estate in its RegSO area in 11 years.⁵ Quite often, several new estates were constructed in the same DeSO area within five years, and our 366 DeSO area windows cover 732 new estates in total. Thus, (incidentally) exactly two estates were on average built in each DeSO area window, and the treatment effects of the pioneering estates that we estimate include the effects of the subsequent new estates.

Several of the DeSO area time windows with pioneering estates had little or no population initially. Since we are interested in how to affect an existing neighborhood rather than build a new neighborhood, we keep areas with at least 100 residents each year. Our empirical strategy, as will be explained in detail in the next section, also requires for each treated area, at least one similar untreated control area with over 100 residents and a similar income level in the same municipality. This leaves us with a sample of 315 treated areas (DeSO area time windows).

Table 1 shows the number of treated areas with pioneering estates across cities (or towns) of different sizes and time periods. We see that out of 315 treated areas, 105 areas were located in one of Sweden's three largest cities (Stockholm, Gothenburg, and Malmö with a population of more than 250,000), 139 areas were located in mid-sized cities (with more than 50,000 residents), and 71 in smaller cities (with less than 50,000 residents).⁶ Table 1 also reveals that the construction rate was higher after 2006.

We are interested in people's living conditions and for this purpose, we focus on disposable income which captures their purchasing power. Disposable income is pre-tax income minus taxes plus transfers. Pre-tax income includes income from all recorded sources with labor and capital incomes being the dominant components. We work with individual income rather than family or household income, not only because these are more consistently registered over time, but also to circumvent the following issues: i) the nu-

⁵For 3 DeSO areas, we have 2 pioneering estates built more than 11 years apart, and thus we have 363 unique DeSO areas with pioneering estates.

⁶In 2017, out of 10.1 million inhabitants, 1.8 million live in the three largest cities, 2.6 million live in mid-sized cities, 3.2 million live in small cities, and 2.5 million live outside the cities.

Table 1: DeSO areas with pioneering estates across cities (with different population sizes) and time.

	(1) >250,000	(2) 50,000-250,000	(3) <50,000	(4) Total
1996-2000	16	20	6	42
2001-2005	20	48	11	79
2006-2009	42	41	26	109
2010-2013	27	30	28	85
Total	105	139	71	315

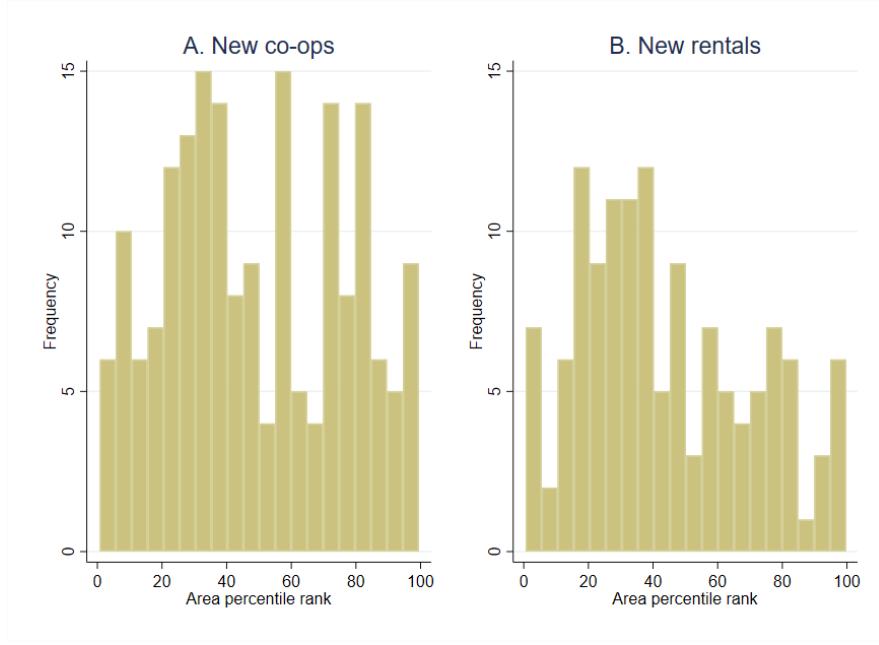
Note: A pioneering estate is the first estate in a RegSO area (covering multiple DeSO areas) in 11 year that has more than 100 residents five years after it is built.

merous unmarried cohabiting couples with or without children in Sweden. ii) varying number of members across families and households. iii) many family units were unstable over time due to the high and changing divorce rate. We include individuals above the age of 20 when calculating incomes.

To take productivity changes and inflation over time into account, we use the year 2017 price level and mostly work with the logarithm of disposable income, $\ln(\text{income})$, which also avoids giving outliers too much weight and allows simple interpretation of estimates in terms of proportional effects. We add a constant 1 before taking the logarithm to deal with those with zero income. Additionally, we also analyze the effects on the share of poor and rich residents in areas in which individuals in the lowest income quartile (bottom 25%) in the municipality are defined as poor and those in the highest income quartile (top 25%) are defined as rich. This analysis avoids the arbitrariness of adding a constant of 1 to income before taking the logarithm.

Income levels differ across areas in Sweden, with considerably higher income but also higher costs of living in larger cities. To measure a treated area's pre-treatment income level, we first construct the mean log income for the event years -6 to -2 (omitting event year -1 to avoid anticipation effects) and then divide it by the mean log income in the municipality to obtain the area's relative log income. We percentile-rank all urban neighborhoods based on this relative income, applying area population weights when making the ranking. Figure 1 shows the area-income percentile distributions for the treated areas with new pioneering co-ops and rentals.

We divide the area income distribution into four parts by income quartile cutoffs. Table 1 separately reports the number of treated areas with new pioneering co-ops and rentals in different area quartiles. We see that more new co-ops have been built than rentals, largely due to a liberalization of Swedish housing policy since 1990. Furthermore, somewhat more new estates have been built in poorer areas than in richer areas, although the overall picture is that new estates have not been strategically placed to revitalize deteriorating neighborhoods or endogenously in attractive areas to maximize



Note: We define area income as the mean disposable income (2017 years price level) in the pre-treatment period relative to the municipality mean. The percentile rank is based on all urban areas.

Figure 1: Pioneering estates by area income and tenure types

profit.

Figure 2 shows a map of the 52 treated urban areas (out of 455 areas) in Stockholm, the capital of Sweden.⁷ We color-coded areas by the different income quartiles (from dark red to dark blue) and patterned the areas depending on whether the pioneering estate is a co-op (no patterns) or rental (grid pattern). New co-ops are at least equally prevalent in the two lowest quartiles compared to the two highest quartiles (17 vs. 16 treated areas) but new rentals have been more often built in the two poorest quartiles (15 treated Q1 and Q2 areas vs. 5 treated Q3 and Q4 areas).

⁷No area was treated twice in Stockholm.

Table 2: Pioneering estates by area income quartile and tenure types

	(1) Q1 areas	(2) Q2 areas	(3) Q3 areas	(4) Q4 areas	(5) All
Co-ops	40	59	43	42	131
Rentals	36	48	24	42	184
All	76	107	67	65	315

Note: The area income quartile cutoffs are based on area percentile ranks. See the footnote in Figure 1 for a description of how area percentile ranks are constructed.

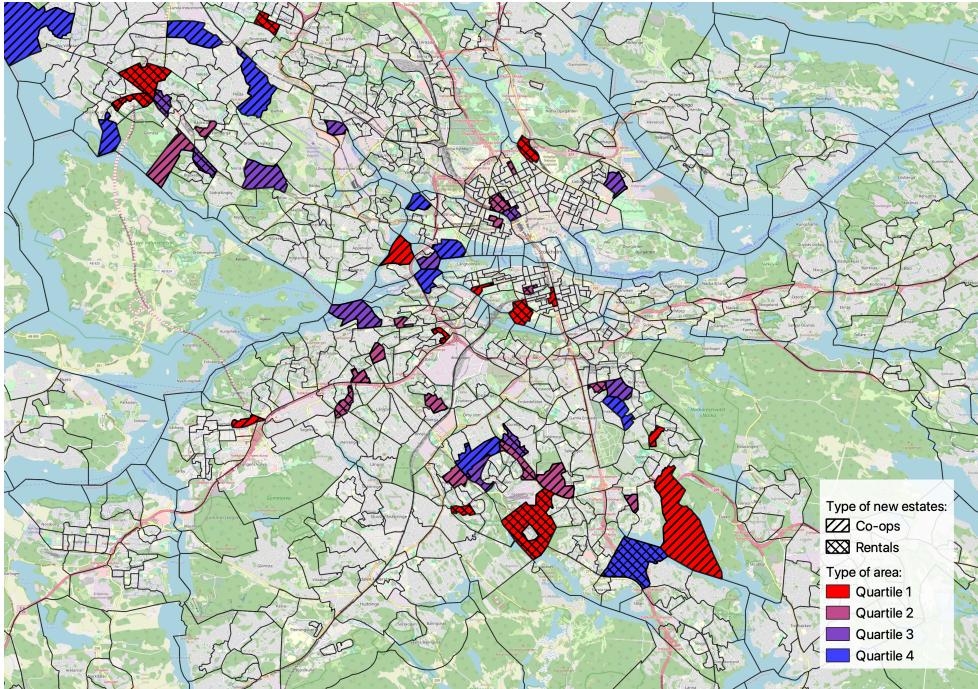


Figure 2: Areas with new estates in Stockholm 1996-2013

In the empirical analysis that follows, we focus on below-median income areas in the bottom two area quartiles, and particularly on poor neighborhoods in quartile 1, i.e., the areas with the 25% lowest percentile-ranked relative income. Since people in owned homes, on average, have higher income levels, new co-ops have higher ex ante revitalization potential, and some detailed analyses will be limited to treated Q1 areas with new co-ops.

4 Empirical strategy

4.1 Diff-in-diff design with area-specific control groups

For the investigation of how new housing transforms a neighborhood, it is important to account for the possibility that the locations of new housing are not random but rather determined by local characteristics and trends. Given building costs, developers try to build in areas with the highest projected housing prices relative to land costs, and such areas might be attractive areas with high prices or unattractive areas with low land costs. The local governments issuing construction permits and owning substantial land can also strategically stimulate the production of new homes in certain areas and prevent them in other areas. For large estates in Sweden, we think government policy rather than market conditions is of greater importance for the location of new homes. However, it is an open empirical question whether the govern-

ment used new housing strategically to affect areas, and in urban areas, land availability likely played a key role.

Since we have panel data, we can follow treated areas with new pioneering estates over time before and after new housing treatment. Because before-after differences among treated areas also reflect other time trends (e.g., general income growth), we apply a difference-in-differences strategy. This standard strategy approximates the counterfactual time trend of the group of treated areas by the trend of a group of control areas and removes the before-after difference of the control group from the same difference of the treated group. The identifying assumption that needs to hold is that treated and control groups would have moved in parallel had the treated areas not been treated.

Previous literature often applied a so called "ring diff-in-diff" method that compares an inner treated ring very close to a new estate to an outer control ring slightly farther away (e.g., Diamond and McQuade, 2019; Pennington, 2021; Asquith et al., 2023). The idea is that within a small area, developers have few available sites and that the placement of the new construction should be unrelated to underlying trends. Typically, the inner ring has a radius of about 200-500 meters, and the literature finds that new housing has hyperlocal impacts.

A potential problem with the ring method is that the spatial proximity between the inner and outer rings means that more diffuse effects of new housing may spill over to the control area; e.g., new housing might bring in new services benefiting households in both the inner and outer rings or in-moving residents to the inner ring might have otherwise chosen to live in the outer ring. On the other hand, larger rings could decrease the similarity between treated and control areas.⁸ Another issue in the Swedish context, where cities are small, is that outer rings often cover non-urban areas that are very different from urban areas. Therefore, we think using an area definition that correspond closer to residents' perceptions of an area is more suitable.

As a starting point, we consider the large pool of other below-median income urban areas in municipalities with treated below-median income areas to be potentially good control areas. However, the descriptive statistics in the previous section do show that large new estates are more common per capita in larger cities (Table 1), suggesting that the city size of the treated and control areas might not be balanced. Furthermore, different subgroup analyses of heterogeneous effects, e.g., the effect for the poorest areas in Q1, are important for us, and the large pool of areas might not be a good control group for the subgroup of the poorest treated areas. Therefore, we have

⁸One alternative strategy, used by Li (2022) and Asquith et al. (2023), is to compare an area around the new estate with areas that later will receive new estates. The underlying logic is that developers choose sites in both groups for similar reasons, but one estate is completed before the other for random reasons such as the timing of when sites are available for purchase. But like the selection of location, developers of course try to time their developments to maximize profits.

experimented with matching a specific group of control areas to each treated area, related to how the ring-defined diff-in-diff method selects a control area for each treated area.

Given that we found no strong systematic patterns in the location of different types of new estates across areas within cities (Table 1 and Figure 1), we try a simple city-matching of each treated area with other untreated (below-median income) urban areas within the same municipality with pre-treatment population above 100. Moreover, given that the wider RegSO areas have names, we think there could be important spillover effects of new developments in smaller DeSO areas on the adjacent areas in the same wider RegSO area. We therefore exclude all areas adjacent to treated areas from the control group. We will also analyze the effects on the adjacent areas and the wider neighborhoods (RegSO areas).

To allow effect heterogeneity analysis by area income, we further refine the control group by restricting it to other areas with a similar income level. In selecting city-income-matched control areas, we use an income band of plus/minus ten percentile rank around the treated area's percentile rank of log income relative to the municipal mean.

In Figure 3, we provide an example of the selected control group for a treated area in the city of Uppsala, the fourth largest city in Sweden. The treated Q1 area with a relative income percentile rank of 22 with a pioneering co-op in year 2000 is colored in red. Thick lines mark wider neighborhoods excluded from the pool of potential control areas since they have new estates with a construction year between 1990 and 2010 (and thus have time windows covering the time window of the treated area). The 13 selected control areas with a percentile rank between 12-32 are colored in blue. When analyzing spillover effects on adjacent areas, we will for each adjacent area apply the same procedure to select a control group.

Figure 4 plots the log income trends in treated areas (thick line) and the different control groups of areas we have experimented with. We report trends for the group of all untreated Q1 and Q2 urban areas (excluding treated or adjacent areas) in the same municipalities as the treated municipalities (short-dashed line), the pool of city-matched Q1 and Q2 controls, and the pool of city-income-matched controls. While there is a level difference in incomes between treated areas and other urban areas, the trends are quite parallel before event year -2. The level difference decreases with the stringency of the matching. Our preferred city-income-matched controls have an income level and trend that closely follow that of the treated areas before event year -2. Income rises continuously over time in all groups, but for the treated areas, there is a small relative rise (compared to the control groups) the year before treatment and a sharp rise once the pioneering estates have been completed. This suggests that there are some anticipation effects and strong post-treatment effects of new housing.

In Figure 5, we show income trends separately for each combination of tenure type and area income quartile group. We also distinguish between all

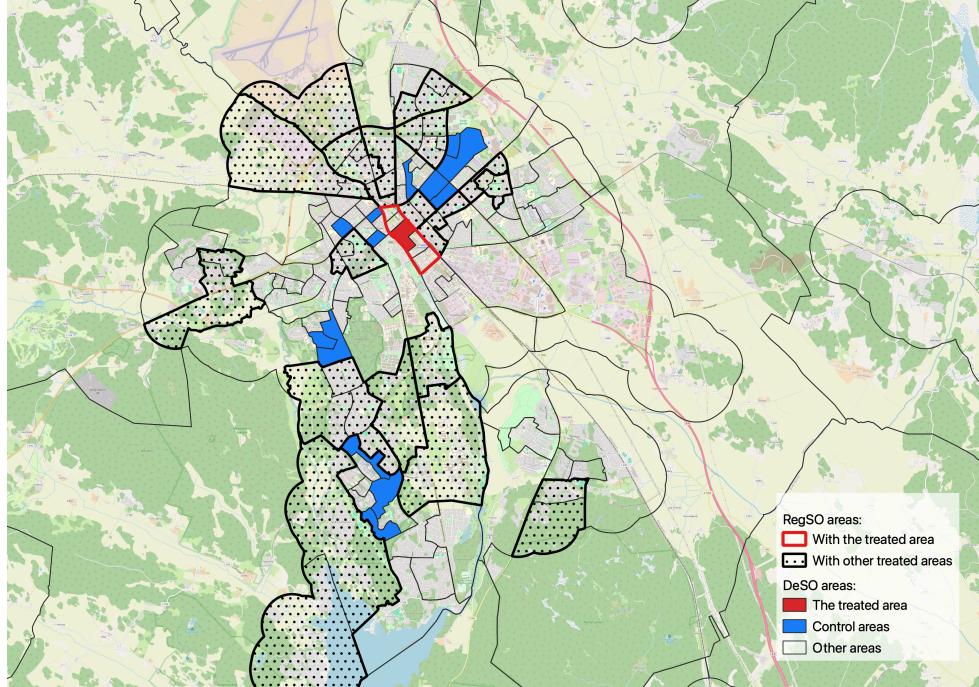


Figure 3: Control group selection for a Q1 area with new co-ops in Uppsala

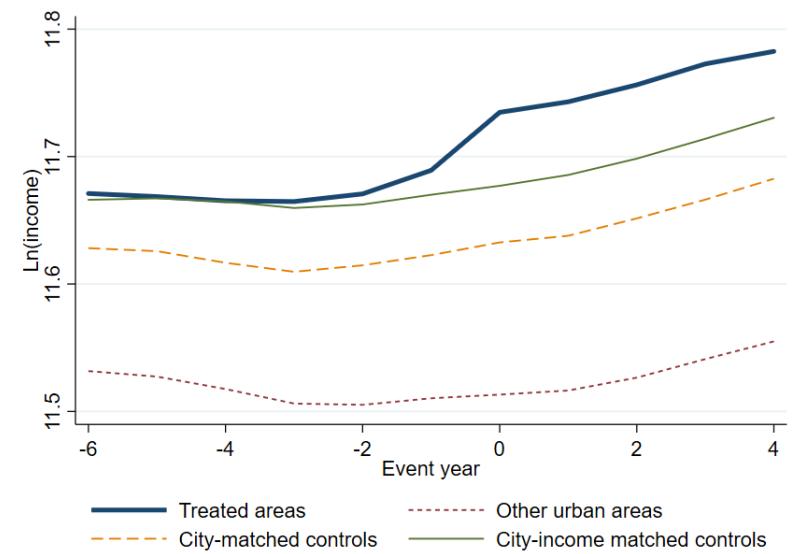
homes in the treated areas (thick solid lines) and existing homes in those areas (dashed lines) defined as all homes other than the pioneering large estate and other large estates built after the pioneering estate. We see a clear post-treatment income rise in poor areas with new co-ops (relative to the control group) but smaller or no effects in other treated areas. Moreover, the income level also increases in existing homes in Panel A, suggesting that new co-ops have strong gentrifying effects in poor areas.

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 description above), and run a two-way fixed effects regression with the following regression equation:

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

where y_{itd} is the outcome, typically area log 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 in Figure 5.



Note: The figure is based on areas with incomes below the municipal median located in municipalities with treated areas. Non-treated areas wider RegSO areas with a pioneering estate are excluded. City-matched controls include the set of individually-selected controls for all treated areas, where for each treated area the control group consists of untreated urban areas within the same municipality (an area might thus be the control area for several treated areas). In selecting city-income matched controls, we further restrict the control group for a treated area to areas within a percentile rank range of $\pm 10\%$.

Figure 4: Income in treated and control areas

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 mean pre-reform population in the ages 21-65 across event years -6 to -2, except when population is the outcome variable. To account for serial correlation within areas and that a particular control area-year observation can occur several 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 our estimated effect. The identifying variation comes from the fact 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. 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., $E(\varepsilon_{itd} | T_{itd}, \mu_{id}, \gamma_{td}) = E(\varepsilon_{itd} | \mu_{id}, \gamma_{td})$.

Our specification corresponds to estimating the effect (the average effect across treated years) for each treated area separately and then aggregating the

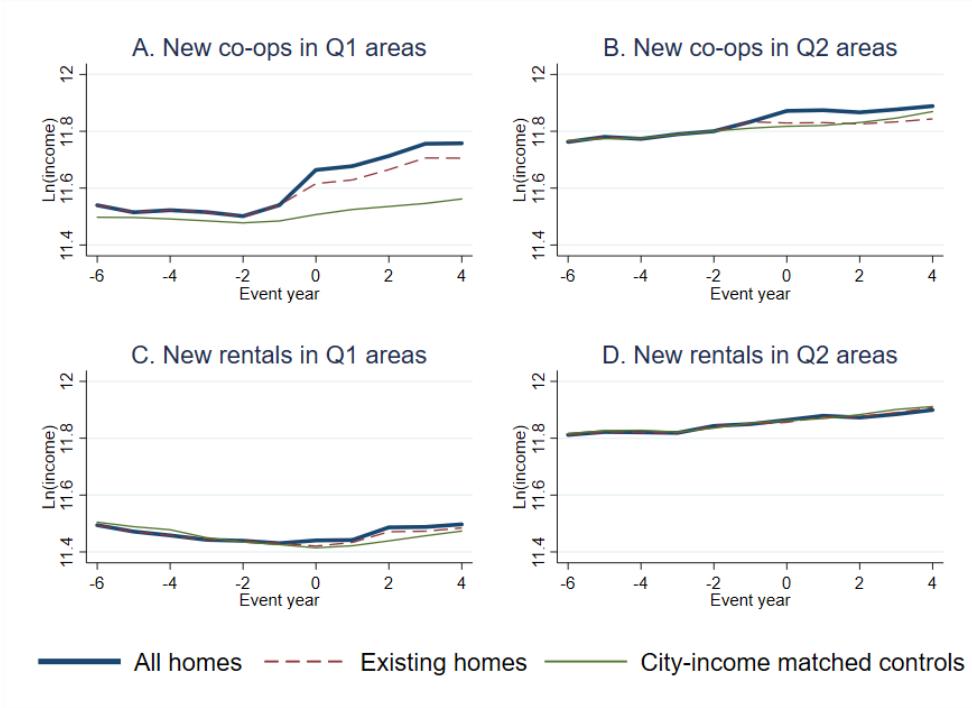


Figure 5: Income by new and existing homes vs. controls.

estimated effects into an average treatment effect on the treated. Subgroup analysis is straightforward by keeping any group of treated units and their control groups, and we will also analyze the distribution of treatment effects estimated separately for each treated area. 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 diff-in-diff:

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

where n indexes event years such that $n = 0$ is the treatment year and $-6 \leq n \leq 4$. Indicator variables T_{itd}^n take the value of one in event year n 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_{itd}^n for $n \leq -3$ as counterfactually placed placebo treatments in the pre-treatment period with β_{itd}^n representing estimates of placebo effects. Dynamic effects following new housing are given by β_{itd}^n for $n \geq 0$. Relatively small placebo estimates that are statistically insignificant support the validity of the identifying assumption.

To formally test for pre-treatment trends, we estimate a binned event-study specification:

$$y_{itd} = \beta^{Pla} T_{itd}^{Pla} + \beta^{-1} T_{itd}^{-1} + \beta^{Post} T_{itd}^{Post} + \gamma_{id} + \mu_{td} + \alpha_d + \varepsilon_{itd}, \quad (3)$$

where indicator variables T_{itd}^{Pla} , T_{itd}^{-1} , and T_{itd}^{Post} take the value one in event years $n \leq -3$, $n = -1$, and $n \geq 0$, respectively. If β^{Pla} is statistically different from zero, we reject the null hypothesis that the treatment and control groups follow parallel pre-trends.

While the event-study specifications in Eqs. (2) and (3) have important advantages over the constrained specification in Eq. (1) with only one treatment dummy, we lose degrees of freedom as more parameters need to be estimated. Moreover, the choice of event year -2 as the base week is somewhat arbitrary. Given that the event-study analysis will indicate that the parallel assumption holds, the pooled diff-in-diff specification in Eq. 1 will be the preferred one for inference about average post-treatment effects.

In our application with staggered treatment, recent methodological studies show that the standard diff-in-diff 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 diff-in-diffs, each involving only one group that switches treatment status and a never-treated control group, and then aggregating the estimated effects from the diff-in-diffs. Our implementation does this and corresponds to the stacked regression method used by Cengiz et al. (2019) and Baker et al. (2022) and thus avoids the methodological objections.⁹

5 Results

5.1 Main estimated effects on the neighborhood

Table 3 presents our main diff-in-diff estimates of the effects of large multi-family estates on the mean of log neighborhood income (estimated using Eq. 1) in below-median income areas. We report the estimates by tenure type of the new estates (co-ops and rentals) and area income (quartiles), as well as for all homes and existing homes separately. The table shows that the estimated effect is approximately 15% (column 1, all homes) and statistically significant for Q1 areas with new co-ops.¹⁰ We find a weaker effect of new co-ops in

⁹An advantage of the stacked regression compared to the other proposed methods is 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 controls. Unlike other solutions, stacked regression is simple and efficient. This estimator does constrain 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.

¹⁰The 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.

Table 3: Basic diff-in-diff estimates of effects on area income

Outcome:	(1) New co-ops		(3) New rentals		(4)
	Q1 areas	Q2 areas	Q1 areas	Q2 areas	
100*ln(income)					
All homes	15.130** (3.094)	4.292** (1.566)	3.457 (4.325)	-0.045 (1.325)	
Existing homes	10.190** (2.953)	-0.174 (1.676)	1.739 (4.377)	0.010 (1.169)	

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

Q2 areas, and no statistically significant effects of new rentals. Thus, we find that new co-ops substantially change the residential income composition and raise average income levels in the poorest Q1 areas.

Moreover, Table 3 also shows that the estimated effect is 10% for existing homes in Q1 areas (column 1), an effect that is about two-thirds of the total effect of 15% for all homes. The remaining rise is from residents moving into the new estates having higher income levels than in existing homes.¹¹ This implies that new estates increases the attractiveness of the poorest areas more generally. 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 new socio-economic composition due to richer residents in the new homes making the neighborhood more attractive.

An interesting question is whether the estimated income change is driven by a decrease in the share of poor or an increase in the share of rich individuals. Table 4 reports estimated effects on these shares. We see both a decrease in the share of poor people (-4.2%) and an increase in the share of rich people (+3.5%) in Q1 areas with new co-ops after the treatment.

5.2 Event-study estimates

To check whether the parallel trends assumption holds and whether there are any anticipation effects of new housing, we provide event-study estimates based on Eq. (2). The results, presented in Figure 6, reveal small and statistically insignificant placebo estimates in the pre-treatment period (before event year -2, which is the base year). In contrast, for new co-ops in Q1 areas, treatment effects are large and statistically significant in each post-treatment event year.

¹¹Existing homes thus defined include homes in small estates constructed after event year zero. This way of defining existing homes allow us to exactly identify the contribution of the new estates (the residual effect). However, defining existing homes as homes constructed before event year zero yield similar results.

Table 4: Estimates of effects on area share of poor and rich

Outcome:	(1)	(2)	(3)	(4)
	New co-ops		New rentals	
% poor in Q1	A. Share poor			
All homes	-4.232** (0.629)	-1.990** (0.373)	-1.558* (0.604)	0.068 (0.414)
Existing homes	-2.916** (0.600)	-0.905* (0.350)	-2.139** (0.602)	-0.254 (0.311)
% rich in Q1	B. Share rich			
All homes	3.461** (0.524)	3.002** (0.441)	0.349 (0.329)	0.645 (0.349)
Existing homes	1.604** (0.451)	1.062** (0.370)	0.618 (0.363)	0.676* (0.290)

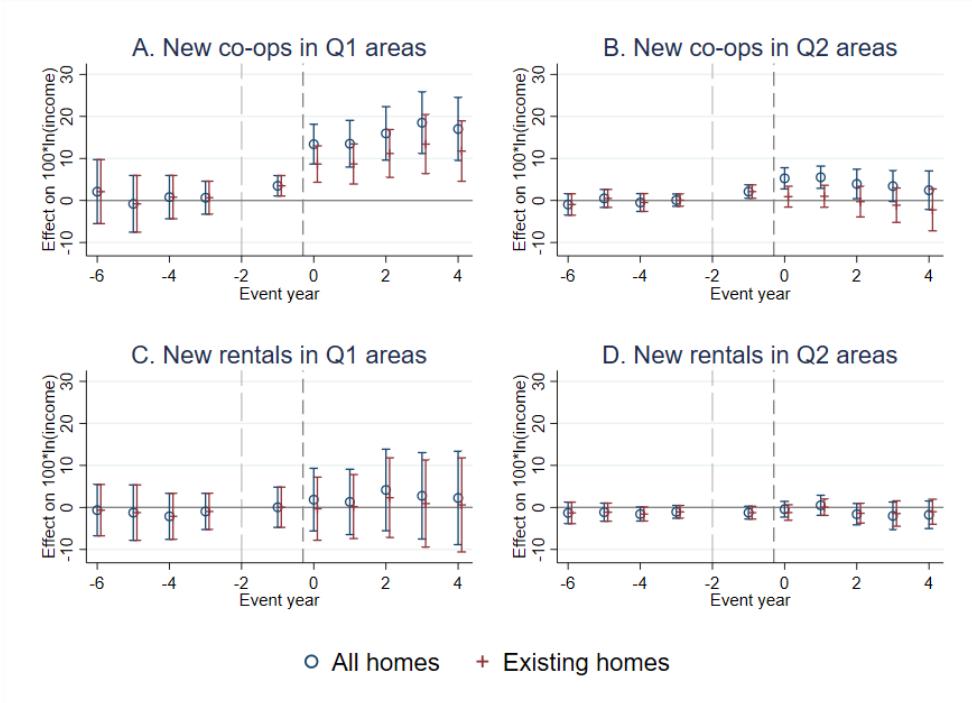
Note: Poor residents are defined as those in the lowest municipal income quartile and rich residents are defined as those in the highest municipal income quartile. See Eq. (1) for the regression specification. Regressions are weighted by pre-treatment population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table 5 report binned event-study estimates for all homes based on Eq. (3). None of the pre-treatment placebo estimates are statistically significant, which is reassuring. We also find that whenever the estimated effect is statistically significant, there are smaller anticipation effect estimates that are statistically significant.

5.3 Effects on the housing stock

Area-based interventions often include a bundle of measures. Policies targeting the housing stock include demolitions, renovations, and tenure conversions, in addition to new housing constructions such as in the Neighborhood-in-Bloom program in Richmond, Virginia, implemented in 1999–2004 (Rossi-Hansberg et al., 2010). In densely populated areas without free land, demolition of older homes might even be a prerequisite for the construction of new homes. Moreover, new businesses and public services typically also emerge in the wake of new housing, sometimes as a consequence of government stimulation packages. In fact, many area revitalization programs primarily target the local labor market and include social investments, with housing policies constituting a small part of total expenditures, e.g., the federal Empowerment Zone program implemented after 1994 in eight U.S. metropolitan areas (Busso et al., 2013).

The new large estates that we have selected are all inhabited by a substantial share of residents in the areas and thus have major impacts on the



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

Figure 6: Event-study estimates of effects on area income

neighborhoods. While the effects that we estimate also capture the impacts of new accompanying services such as shops and schools, labor-market interventions were rarely area-based during our study period. However, a substantial part of the Swedish housing stock underwent renovations and tenure-type conversions from public rentals to private co-ops; a question is whether such major housing-stock changes took place in our treated areas.

To better isolate the causes of neighborhood gentrification, we provide estimated effects on a number of housing stock outcomes, including changes in the existing stock of owned and rented homes, in Table 6. Given small or no effects in other areas, we report results only for treated Q1 areas with new co-ops, which is the subsample we focus on from now on, and we provide basic diff-in-diff estimates (Eq. 1). In Panel A, 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 resident and this eliminates the influence of rising post-treatment population.¹² We find that the housing space expansion amounts to $6.9 m^2$

¹²Using log housing space would have been another option, but that makes a comparison of changes to the owned vs. rented housing space harder since a high percentage effect is a smaller absolute effect when the pre-reform owned or rented housing stock is small.

Table 5: Binned event-study estimates for all homes

Outcome:	(1) New co-ops		(3) New rentals	
	Q1 areas	Q2 areas	Q1 areas	Q2 areas
100 * ln(income)				
Post-period	15.68** (2.935)	4.116** (1.475)	2.465 (4.293)	-1.057 (1.194)
Anticipation	3.493** (1.238)	2.119** (0.811)	0.0355 (2.441)	-1.245 (0.777)
Placebo	0.689 (2.757)	-0.220 (0.861)	-1.240 (2.588)	-1.265 (0.857)

Note: See Eq. (3) for the regression specification. Regressions are weighted by pre-treatment population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

per person on average, and the entire expansion is from more owned space, which is natural given that we here focus on the construction of new co-ops. In comparison, the pre-treatment average housing space is 31.7 m^2 per person; thus, the expansion is 22% of the pre-treatment housing space.

Table 6A also shows that a non-negligible part of the new housing space is due to a 2.0 m^2 per person expansion in homes other than the large new estates (column 2, existing homes). Thus, compared to control areas, additional smaller estates are also built.¹³ This result also means that our new estates do not merely replace older demolished homes; to the extent that demolitions are more common in the treated areas, they are more than compensated by other new homes than the new large estates. Although other smaller estates are added, the new large estates constitute the dominating addition to the area's housing stock. Column (3) shows that not much happened to rental space in the treated areas; thus, we rule out concurrent tenure-type conversions from rentals to co-ops. Table 6B shows that because of the newly added homes, the average construction year of the homes in the treated areas increases by 6.2 years, where 1.6 years of the total gain is from other homes than the new estates.

To measure renovations, we make use of a variable called the value year. 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.¹⁴ 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

¹³This include both smaller estates that might have been built at the same time as a part of a larger block by the same developer as the large estates and other smaller independent initiatives such as private persons building new detached homes.

¹⁴For single-family detached homes, 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 6: Effects on housing stock outcomes (new co-ops in Q1 areas)

Outcome:	(1) Total	(2) Owned space	(3) Rented space
$M^2/\text{base person}$			
		A. Housing space	
All homes	6.852** (1.086)	6.758** (1.083)	
Existing homes	1.871** (0.635)	1.987** (0.661)	0.0935 (0.558)
Built year			
		B. Construction year	
All homes	6.179** (0.840)	10.10** (2.068)	
Existing homes	1.581* (0.615)	2.811 (1.459)	1.400 (1.121)
Value yr - built yr			
		C. Renovation year gains	
All homes	-0.893 (0.506)	-0.447 (0.574)	-0.0271 (0.597)
$100*\ln(\text{rent}/m^2)$			
		D. Rent	
All homes			-7.993** (2.557)

Note: Base person is number of people in event year -2. Value year is built year updated to reflect extensive renovations. See Eq. (1) for the regression specification. Regressions are weighted by pre-treatment population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table 6C. The estimated renovation year gains are small and not statistically significant.

A common fear among incumbent residents is that neighborhood gentrification leads to rent increases, and this fear often sparks resistance against new housing constructions. With regulated rents primarily determined to reflect the standard of rented homes in Sweden, rents cannot adjust easily except after renovations. Since we ruled out larger renovations concurrent with the construction of our new estates, we do not expect rents to increase. We empirically test for rent changes using data from the Stockholm area's public housing agency. The data cover rents for homes involved in turnovers during 2005–2014. The data are only sufficient for constructing area-level rent over time for seven of our treated Q1 areas with new co-ops and their controls. The results, which we should interpret cautiously, are reported in Table 6D. We find no indicative evidence for rent increases in the treated areas; if anything, rents appear to decrease.

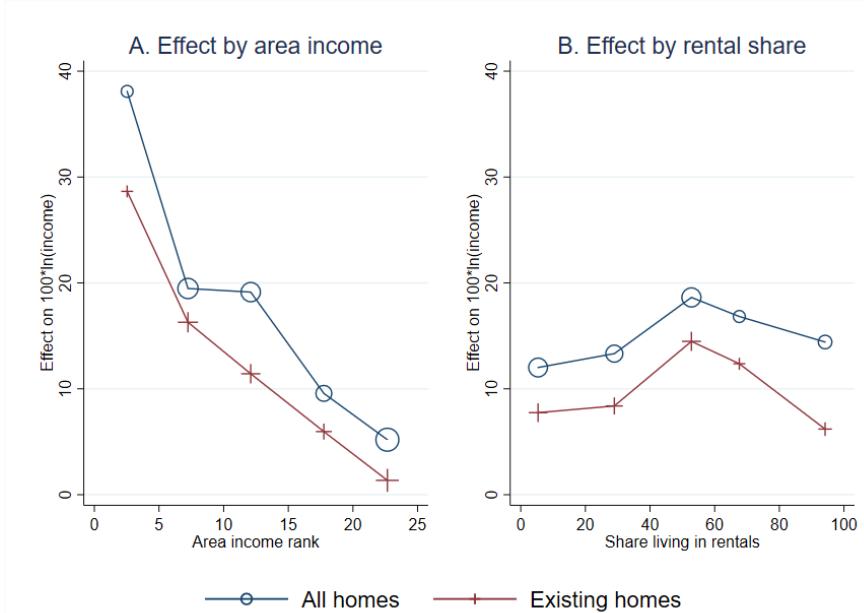
5.4 Effect heterogeneity

Since we have a specific matched control group for each treated area, we can estimate area-specific treatment effects. Figure 7A reports the distribution of estimated effects by area-percentile income rank. Each marker represents a weighted-average effect within a bin containing treated areas in five percentile ranks and the size of a marker depends on total population of the areas in the bin. The figure shows that the effects are the most pronounced for the poorest areas in the bottom decile where the estimated effect reaches slightly above 20% for all homes, and slightly less for existing homes.

How do new homes affect areas with different tenure types? In Figure 7A, we plot the distribution of estimated effects across areas with different population shares living in rentals. The estimated effects are stable and remain even in areas with only rentals. Since there is no confounding price effect, we take this to be a strong sign of the area becoming more attractive.

5.5 Effects on adjacent areas (spillover effects)

A new estate 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 estate rather than slightly further away. Therefore, we analyze spillover effects on adjacent areas and the wider neighborhood including the treated area and the 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 areas (selecting control groups for each of them). When estimating the effect for the wider area, we include both areas with new estates and their adjacent areas.



Note: Each marker represents a weighted average of area-specific effects within a bin containing treated areas in five percentile ranks (and the x -value is similarly the weighted average percentile rank in a bin). The weight used is the pre-reform population. The size of the marker depends on total population of the areas in the bin. See the notes to Table 3 for a description of how area-specific effects are estimated.

Figure 7: Effects by area income (new co-ops in Q1 areas)

The results, reported in Table 7, show a negative effect of -3.3% on area income in adjacent areas (Panel B, column 2). 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. The effect on the wider area is a weighted average of the effects on areas with new estates and their respective adjacent areas; the net effect is about 3.3% (Panel B, column 3) for the wider area but not statistically significant, and about half the effect comes from the existing homes.

5.6 Effects on in-migration

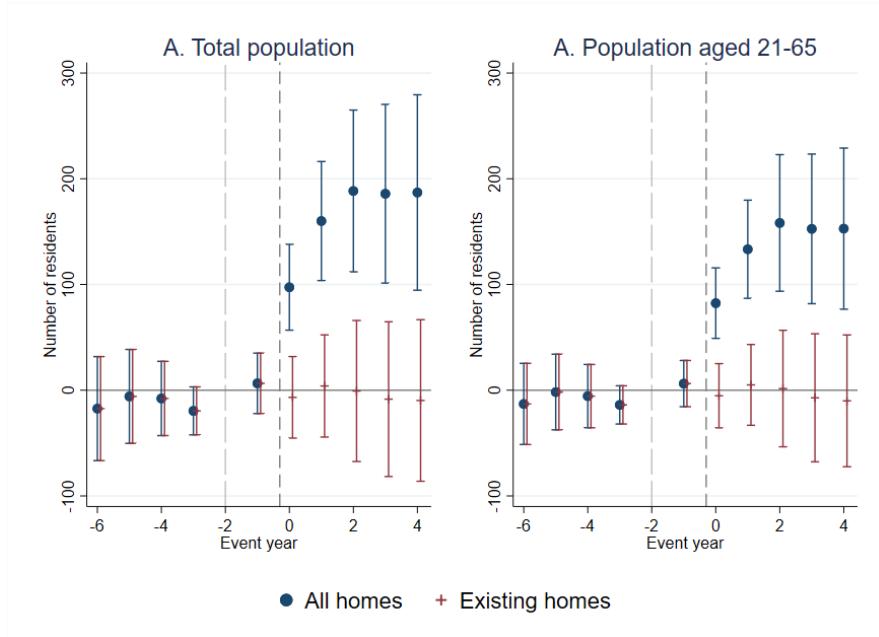
Before investigating migration streams, let us first understand in detail how area population changes over time in our treated areas. Figure 8 reports event-study estimates of the effects on area population over time. Population rises until event year 2 with an increase of about 190 people among which 160 are in the ages 21-65, and this corresponds to 12-13% of pre-treatment population and population in the existing stock (in event year 2). All the increase is from residents in the new large estates since the population in

Table 7: Spillover effects (new co-ops in Q1 areas)

	(1)	(2)	(3)
Outcome: $100 * \ln(\text{income})$	Treated area	Adjacent area	Wider area
All homes	15.13** (3.094)	-3.298* (1.601)	3.255 (1.727)
Existing homes	10.19** (2.953)		1.499 (1.612)

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

existing homes remain constant.



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

Figure 8: Effects on area population (new co-ops in Q1 areas)

We now move on to analyze the migration streams leading to the population gain and income rise that we have found. We start analyzing this question by grouping residents in a treated area in event year 2 (in which the effects stabilized) by where they lived four years ago in event year -2, which is the base year in the pre-treatment period. We additively decompose the number of residents in an area into stayers living in the treated (DeSO) area, in-movers from adjacent areas in the same wider (RegSO) area, and in-movers from other (RegSO) areas in the base year. Of course, people move

in and out of areas all the time, and we therefore compare the distribution between stayers and in-movers over a course of four years in event year 2 with a baseline distribution in event year -2 where the origin area is defined as the area of residence four years before that in event year -6. To alleviate concern about trends in how these distributions change over time within a 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 8 reports estimated effects in the treated areas in event year 2 (aged 21-65) where residents are grouped by their origin area in event year -2. Panel A shows that new housing increases the number of residents by 159 in the area with new estates (column 1) and the entire increase stems from in-moves from outside the wider neighborhood (161, column 4).

The population increase in treated areas are due to residents in the new co-ops (156, column 1), and mostly residents from the outside live in those new homes (146, column 4). However, although only 11 residents (aged 21-65) in the new co-ops are locals from the same wider neighborhood (4.9+5.7), they are still over-represented by a factor of 3.8; while they make up approximately 6.7% of the residents in the new estates, they only make up 1.8% of the population in the municipality.

While the new estates do enable 11 residents within the wider neighborhood to stay in the area, there are equal-sized counteracting negative effects of -14 in existing homes (column 2). Thus, there appears to be no displacement from the wider neighborhood. We conduct an out-migration analysis that more directly address the issue of displacement in the next subsection.¹⁵

Table 8B shows that new co-ops lead to in-moving residents from adjacent and other areas that are 30-40% richer (columns 3 and 4), and partly these richer residents move to pre-existing homes. Moreover, Table 8C reveals that those richer in-movers come from richer origin areas; the in-movers from outside the wider neighborhood come from areas that are 4.8% richer than if the new co-ops had not been built. Altogether, this subsection provide additional, in our opinion, strong evidence of co-ops making the area more

¹⁵An estate typically covers a main residential building and possibly a few auxiliary buildings, but in a few cases, they cover multiple main buildings. There are some cases where the same building switches estate over time due to estate restructuring. The residential area of an individual is based on the location of its residential estate. When an estate cut through the border of two DeSO areas, Statistics Sweden assigns the estate to the DeSO area in which most of the estate is located. This means that following estate restructuring, residents in the same building may be assigned to another adjacent DeSO area. This means that the exact -14 estimate for stayers, which is small and not statistically insignificant anyway, should be taken with a grain of salt. However, the sum of the estimates in column 2 and 3 together more precisely reflect the number of original locals. Another problem with the in-migration analysis in terms of what we can learn about displacement is that there might be minor differences between treated and control areas over time regarding the number of residents entering and leaving the ages of 21-65. For this reason, the out-migration analysis in the next section, focusing on only residents that lived in the treated area, is more suitable for capturing potential displacement.

Table 8: Effects on in-migration by base-year origin (new co-ops in Q1 areas)

Outcome:	(1) All residents	(2) Stayers	(3) From adjacent	(4) From outside
No. aged 21-65				
		A. Area population		
All homes	158.2** (32.94)	-8.789 (16.68)	6.258 (4.081)	160.8** (23.46)
Existing homes	1.993 (28.12)	-13.66 (16.54)	0.602 (4.265)	15.05 (17.94)
New co-ops	156.2** (12.89)	4.873** (0.683)	5.656** (1.067)	145.7** (12.42)
100*ln(income)				
		B. Area income		
All homes	16.46** (3.328)	2.910 (3.454)	39.80** (14.67)	34.99** (7.759)
Existing homes	11.56** (2.978)	2.546 (3.480)	33.14* (15.07)	22.35** (7.087)
100*ln(income)				
		C. Origin area income		
All homes	11.76** (1.992)	14.79** (3.021)	2.938 (4.625)	4.753** (1.045)

Note: The effects applies to residents and their income levels in the treated DeSO area in event year 2. Stayers, in-movers from adjacent areas (in the same RegSO area) and from other areas (outside the RegSo area) refer to the origin areas where the resident lived in event year -2. Origin area income is the income level of the origin area in event year 2. See Eq. (1) for the regression specification. Regressions are weighted by pre-treatment population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

attractive.

5.7 Effects on out-migration

Does new owned homes in poor areas lead to displacement of the incumbent residents? We analyze this question through an analysis of out-migration patterns in our treated areas. The analysis is related to the in-migration analysis in the previous subsection but has some important differences. We now group residents in a treated area in the base year (event year -2) by where they live four years later in the post-treatment period (event year 2). The grouping is stayers still living in the same (DeSO) area, out-movers to adjacent areas in the same wider (RegSO) area, and out-movers to other (RegSO) areas. As before, we conduct two-by-two diff-in-diffs.

Table 9 reports the estimated effect on out-migration. Panel A shows that out-migration patterns do not change much and none of the changes are statistically significant. The point estimates means that new co-ops do not induce any out-moves from the wider neighborhood (number of stayers and moves to adjacent areas within the wider area in columns 2 and 3 sum to zero).¹⁶ Moreover, Panel B shows that out-movers are somewhat richer than in the absence of the treatment (but the point estimates are not statistically significant), and Panel C show that out-movers move to equally or richer areas than if the new estates had not been built; in particular, out-movers to adjacent areas now move to 5% richer adjacent areas than if the the new estates had not been built. Together with the results in Table 6, that there are no concurrent major physical changes in existing homes and that rents did not increase, we think these results convincingly show that incumbent residents are neither displaced nor worse off following the new housing constructions.

¹⁶See also the previous footnote

Table 9: Effects on out-migration by event-year 2 destination (new co-ops in Q1 areas)

Outcome:	(1) All residents	(2) Stayers	(3) To adjacent	(4) To outside
No. aged 21-65		A. Population		
All homes	9.372 (18.09)	-3.758 (11.83)	3.805 (2.885)	9.325 (11.22)
100*ln(income)		B. Income		
All homes	4.289 (2.808)	4.670 (3.501)	10.65 (11.90)	3.797 (2.817)
100*(income)		C. Destination area income		
All homes	9.418** (2.169)	14.45** (3.165)	5.260* (2.296)	1.051 (0.832)

Note: The effects applies to residents in the treated DeSO area in the base year (event year -2). Stayers, out-movers to adjacent areas (in the same RegSO area) and to other areas (outside the RegSo area) refer to the destination area in event year 2, which is also the year the residents' incomes are measured. Destination area income is the income level of the destination area in event year 2. See Eq. (1) for the regression specification. Regressions are weighted by pre-treatment population. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

6 Conclusions

In this paper, we have examined how new large-scale housing construction affects poor neighborhoods in terms of income development and individual migration patterns. By combining information on all new housing construction in Sweden with yearly, full-population register data from almost three decades that contains detailed geo-coded information on how and where all individuals in Sweden live, their moving patterns, and their socio-economic characteristics, we are able to provide new and value-added evidence on the stated research question.

We reach four main conclusions. First, we find that new large residential developments of market-rate condominiums have strong gentrifying effects in the poorest neighborhoods: the estimated effect on average income is 15% in the poorest quartile of neighborhoods. In contrast, we do not find any evidence of gentrifying effects in poor neighborhoods from the construction of new rental housing.

Second, we find that the gentrifying effect is not only driven by richer people moving into the newly built owned apartments, but also by average income rising by 10% in pre-existing homes. Given rent regulation and no evidence of concurrent housing demolitions, renovations, tenure type conversions, or rent increases, this indicates that the poorest areas have become more attractive after the new housing construction.

Third, our in-migration analysis shows that most of the gentrification

effects are due to high-income people moving in from richer areas outside a wider neighborhood. For incumbent individuals in the treated areas, we find that high-income locals are over-represented in the new homes. This is an important result since it shows that new housing construction for private ownership provides opportunities for richer incumbents to stay in the area.

Fourth, our findings show that out-migration patterns are unchanged; thus, the revitalizing effect does not come with displacement.

To conclude, our results show that building new large market-rate housing in the poorest neighborhoods is a very suitable policy if the aim is to revitalize these neighborhoods.

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