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

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February 2024

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 three 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 16% in the poorest quartile of neighborhoods. 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 11% in pre-existing homes. Given rent regulation, this indicates that the poorest areas become more attractive. Third, our migration analysis shows that most of the gentrifying effects of new owned apartments are due to high-income people moving in from outside a wider neighborhood.

Keywords: Housing Construction, Gentrification, Individual Mobility, Displacement, Spatial Inequality, Poor neighborhoods

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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 gentrification and migration patterns in a rent-regulated system.

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 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 (preexisting) 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 (over 100 residents) affects residential income composition in the immediate surrounding (so called DeSO-areas with 700-2,700 residents in 2017), the adjacent areas in a wider neighborhood (RegSO-areas 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 (e.g. owned apartments) in certain locations (e.g. poor areas) with the purpose of affecting 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 16% 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, do not statistically significantly affect area income in poor areas (but negatively affect richer areas). These results are in line with the positive effects of new market-rated homes and the negative effects of new affordable housing that Pennington (2021) found for San Francisco.

Another main finding is that the gentrifying 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 11% in pre-existing homes.

The estimated effect also persists in areas with high rental shares in which housing costs are as good as constant (as rents cannot easily respond due to rent regulation), indicating that the poorest areas become more attractive. Potential reasons 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. This conclusion is strengthened by the fact that we also find that in-movers to the pre-existing rental apartments are richer following new market-rate construction.

A novel finding is smaller negative spill-over effects on adjacent areas within the wider neighborhood for all types of new estates and areas (RegSO-area). The overall gentrifying effect of new market-rated homes on the wider neighborhood is therefore smaller than the positive effects on the immediate surroundings due to those "cannibalizing" effects.

Our migration analysis shows that most of the gentrifying effects of new owned apartments are due to high-income people moving in from 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, previous studies of moving chains leading to newly-built homes also show that they often involve moves from other poor neighborhoods (Bratu et al., 2023; Mast, 2023). For incumbents within the wider neighborhood, we find that high-income locals are over-represented in the new homes (by a factor of four), and thus these homes do provide opportunities for richer residents to stay in the area in homes with higher standards.

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 out to better off neighborhoods. New market-rate homes instead allow successful residents to make a local housing career. Moreover, raising the income level in poor areas with new owned homes (and lowering incomes in affluent areas with new rentals) also reduces income differences across areas and thus mitigates spatial inequality in the city.

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.[1] 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. Concurrent with the deregulations after the crises, housing companies privatized a large part

[1] Membership 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.

of their housing stock by converting rental apartments to tenant-owned co-operative 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.[2]

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

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

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

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.[3] Statistics from 2016-2021 showed that, on average, one out of four development plans were appealed countrywide.[4] 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. Additionally, long lead times may open the possibility of changes in municipal rule, as the entrant political party may prioritize or view projects differently, potentially leading to repeals.

During construction, regulated events may halt projects, such as the discovery of ancient relics. In such cases, the project is immediately put on hold, and an assessment is conducted to determine whether it is possible to remove the relics and continue exploitation. If the relics hold historic value, an archaeological investigation may be required, potentially postponing production by years. Encountering the habitat of endangered species is another case, where the project's continuation depends on the potential impact on the species, with some projects potentially being postponed for years or, at worst, canceled.

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 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, mi-

[3] Nationellt ledtidsindex 2023

[4] Överklagande av detaljplaner - omfattning, effekter och orsaker 2023

crodata registries available at Statistics Sweden for researchers in 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 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.[5]

[5]Other 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, in which there are 2,287 large new multi-family estates with more than 100 residents five years after the year of construction. We focus on these large estates 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.[6] 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.

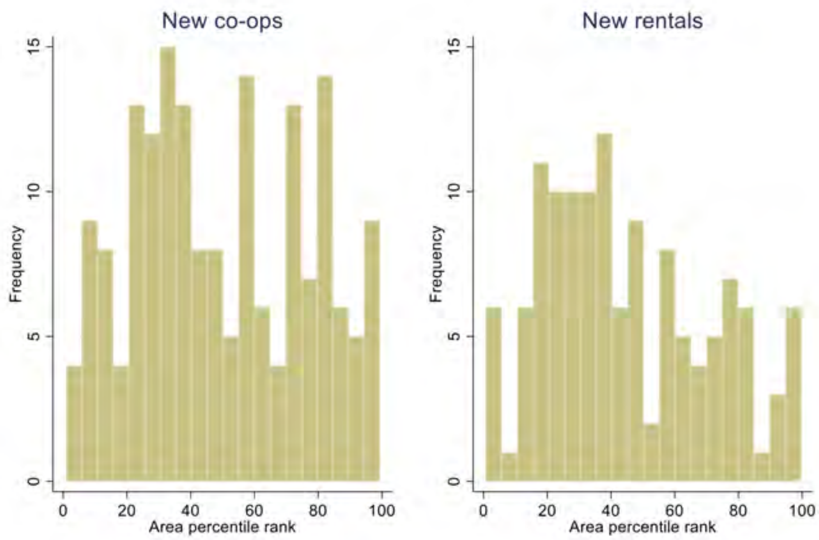


Figure 1: Pioneering estates by area income and tenure types

[6]For 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.

Table 1: Number of DeSO-areas with pioneering estates across cities and time periods

	(1) >250,000	(2) 50,000-250,000	(3) <50,000	(5) Total
1996-2000	16	20	6	42
2001-2005	20	46	11	77
2006-2009	39	39	26	104
2010-2013	26	28	28	82
Total	101	133	71	305

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 selected sample of 305 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 305 treated areas, 101 areas were located in one of Sweden's three largest cities (Stockholm, Gothenburg, and Malmö with a population of more than 250,000), 133 areas were located in mid-sized cities (with more than 50,000 residents), and 71 in smaller cities (with less than 50,000 residents).[7] 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 numerous unmarried cohabiting couples with or without children in Sweden. ii) varying number of members across families and households. iii) many

[7] In 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 2: Income in areas with pioneering estates

	(1) Q1 areas	(2) Q2 areas	(3) Q3 areas	(4) Q4 areas	(5) All areas
Co-ops	34	46	25	23	128
Rentals	36	58	42	41	177
Both types	70	104	67	64	305

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 mostly work with the logarithm of disposable income (log income), which also avoids giving outliers too much weight and allows simple interpretation of estimates in terms of proportional effects. We add a constant of one 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.

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. Our focus is on poor neighborhoods, which we define as areas in quartile 1 (Q1) with the 25% lowest percentile-ranked relative income. Table 2 separately reports the number of treated areas with new pioneering co-ops and rentals in different area quartiles, and Figure 1 shows the entire income distribution for the treated areas. 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 profit.

Figure 2 shows a map of the 49 treated urban areas (out of 544 areas) in Stockholm, the capital of Sweden.[8] 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 equally prevalent in the two lowest quartiles compared to the two highest quartiles (16 treated areas) but new rentals have been more often built in the two poorest quartiles (11 treated Q1 and Q2 areas, 6 treated Q3 and Q4 areas).

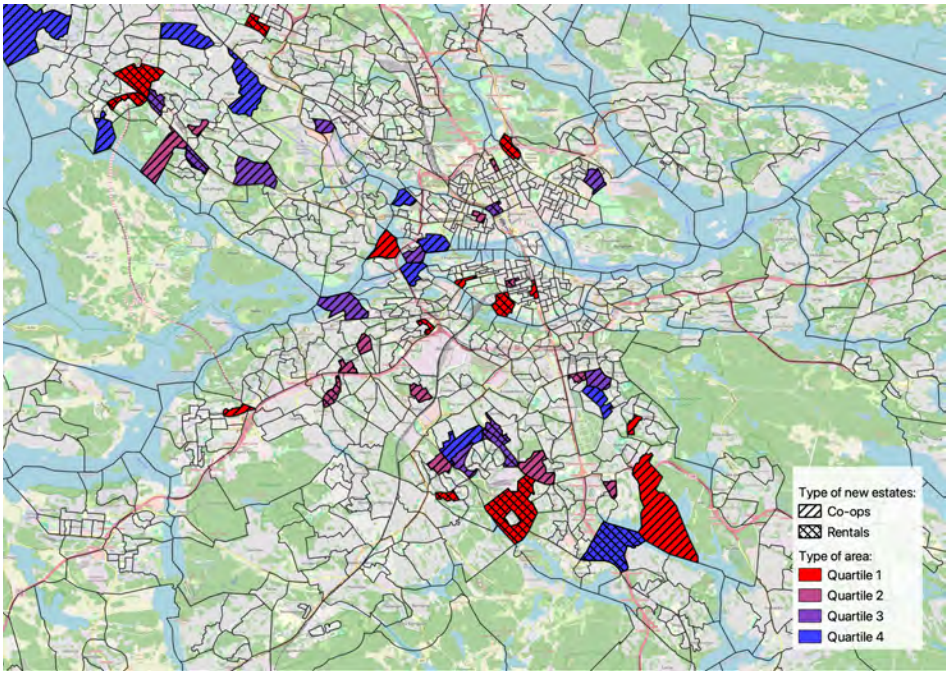


Figure 2: Areas with new estates in Stockholm 1996-2013

3.3 Area and estate development over time

How many residents live in new large multi-family estates? Figure 3 illustrates the population development in new estates in areas with pioneering estates. Given that the average income level is higher in co-ops than in rentals, we think new co-ops have the highest revitalization potential, and we show the development separately for Q1 areas with new co-ops and other treated areas. We plot separate graphs for the population in the pioneering estate (thin lines) and for all new large estates in the area (thick lines), and also separate graphs for the entire population (dashed lines), and residents

[8]No area was treated twice in Stockholm.

aged 21 or more (solid lines) who are the ones included when constructing income measures. In event year -2, the average population is 1,416 with 1,147 residents aged above 20 in Q1 areas with new co-ops and the corresponding numbers are 1,347 and 1,041 people in other treated areas. The figure shows that new estates increased population gradually by 193 people (160 above age 20) in Q1 areas with new estates and 222 people (175 above age 20) in other areas after event year 4. Thus, the residents in new estates correspond to about 14 to 17% of the pre-treatment population in the areas. About two-thirds of them live in the pioneering estate (although the pioneering estate only makes up half the new estates).

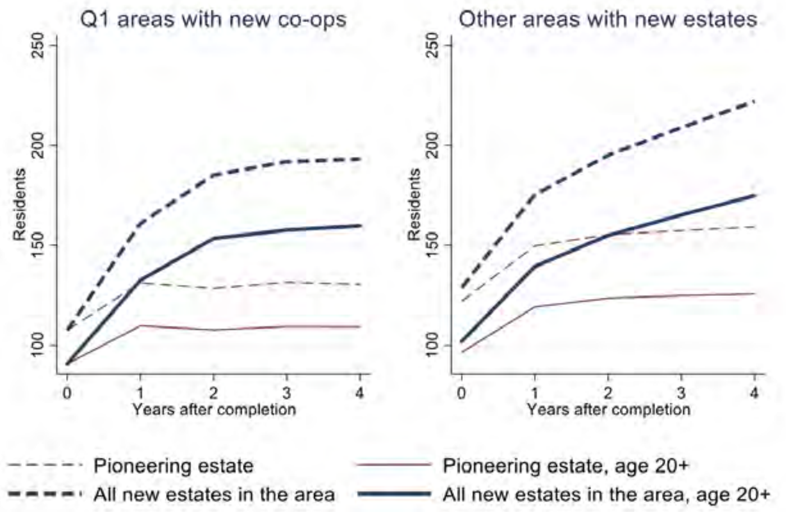


Figure 3: Population in new estates

Where do residents in new estates come from? Figure 4 plots the population composition over time in new estates by the residents' areas of origin in event-year -2, distinguishing between within-area moves from those living in the treated DeSO area (solid lines), moves from adjacent areas defined as other DeSO areas in the same RegSO area (long-dashed lines), and those from other areas (dashed lines). The figure shows that mostly outsiders access the new estates in the treated areas, and after event year 4, they make up 92-93% of the residents in those estates. Out of on average 11 residents above the age of 20 from the wider neighborhood (RegSO area) moving into the new estates in Q1 areas, half of them come from the same micro-neighborhood (DeSO area). While new estates cannot be said to give particularly many residents in the neighborhood a newer home, the locals are still overrepresented in the new estate; while they make up 7–8% of the residents in the new estates, they only make up 1.6% of the population in the municipality.

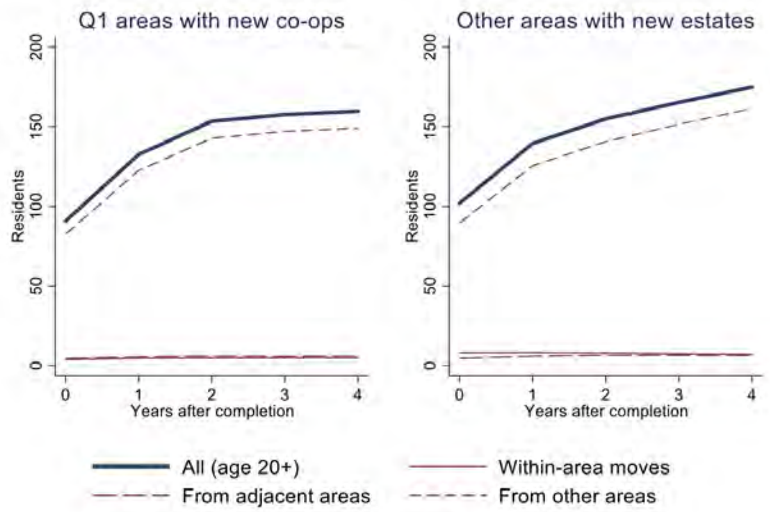


Figure 4: Population in new estates by origin area

In Figure 5, we explore the income development in areas with new estates (thick lines) after the completion of the pioneering estates. We also display income levels in both the new estates (thin solid lines) and existing homes defined as the remaining homes (long-dashed lines). For the new estates, we show the income level of local in-movers from the same area (dashed lines) and adjacent areas (short-dashed lines). The figures show that income rises over time in treated areas, partly driven by an income rise in existing homes. The income level is higher in the new estates than in existing homes, particularly in the treatment year, possibly due to a new home premium. Comparing Panels A and B, we see that post-treatment area income levels are similar although Q1 areas per definition had lower pre-treatment income levels, indicating that those areas are relatively gentrified by the new homes. Furthermore, richer individuals move into new co-ops in Q1 areas rather than into new homes in other treated areas, suggesting that new co-ops are attractive even in poor areas. Moreover, locals moving into new co-ops in Q1 areas have higher income levels than residents in existing homes, and thus new co-ops provide an attractive option for residents doing well in poor areas to upgrade their housing standards. The fact that locals moving into new co-ops in Q1 areas are poorer than other in-movers to the new co-ops might be due to locals selecting cheaper homes in the new estates.

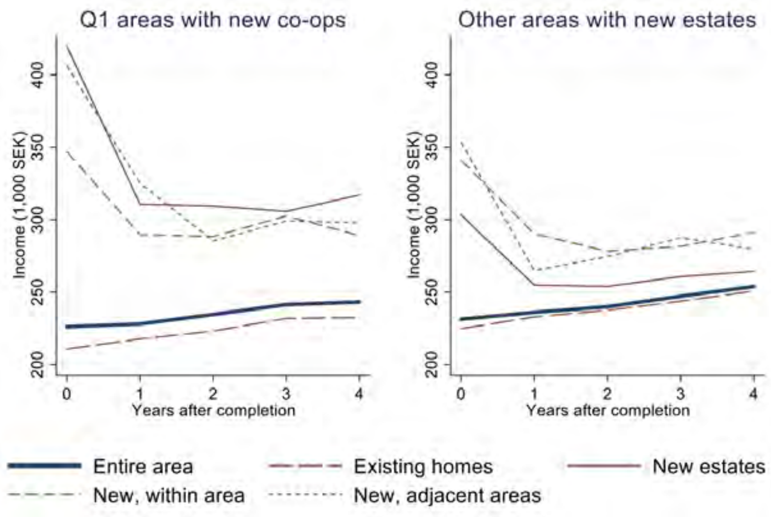


Figure 5: Income in areas with new estates

Table 3 summarizes income levels in areas with pioneering estates in event year 4 separately for new co-ops and rentals, areas in different income quartiles, and new estates and existing homes. While an analysis of post-treatment income trends in new estates can provide an idea about the neighborhood effects, new housing also affects the character of the neighborhood and thus residential composition in existing homes. Moving in the direction of exploring the causal effects, we also report income levels in event year -2 in Panel D. This panel shows that pre-treatment income levels are higher in areas from higher income quartiles (as expected) and that co-ops have been more often built in richer areas. Income levels are higher in existing homes post-treatment compared to pre-treatment (Panel B vs. D), either because income generally rises over time or because new homes have gentrifying effects. Income levels are higher in new estates than in existing homes (Panel A vs. B), but the income gaps between new estates in different areas are much smaller than the same gaps in existing homes.[9] Thus, while individuals moving into new homes in poor areas are poorer

[9]For instance, for co-ops in Q1 and Q3 areas (columns 1 and 3), the income difference is 9,000 SEK in new estates (332–323 thousand SEK) but 29,000 SEK in existing homes (362–233 thousand SEK)

Table 3: Income in areas with pioneering estates

	(1) Q1 areas	(2) Q2 areas	(3) Q3 areas	(4) Q4 areas	(5) All areas
			A. New estates in event year 4		
Co-ops	323	300	332	371	328
Rentals	211	203	228	234	216
Both types	263	259	296	319	280
			B. Existing homes in event year 4		
Co-ops	233	244	262	304	259
Rentals	201	224	256	287	235
Both types	217	234	259	297	249
			C. All homes in event year 4		
Co-ops	243	252	271	313	268
Rentals	203	222	252	280	233
Both types	223	238	264	300	253
			D. All homes in event year -2		
Co-ops	193	204	218	256	216
Rentals	175	193	217	241	201
Both types	185	198	217	250	209

Note: 1000 SEK

than those moving into new homes in rich areas, possibly because the new homes in poor areas have lower standards or because of fewer amenities or less attractive neighbors, new homes in poor areas do not appear much less attractive than in rich areas. When considering all homes, the income gaps between rich and poor areas decrease following new housing constructions.

4 Empirical strategy

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 government 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. 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.[10] 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 with practical implications that correspond closer to residents' perceptions of an area is more suitable.

As a starting point, we consider the large pool of other urban areas in municipalities with treated areas to be potentially good control areas. However, the descriptive statistics in the previous section do show that large

[10] One 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.

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 poor areas, are important for us, and the large pool of areas might not be a good control group for a subgroup of poor, treated areas. Therefore, we have 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 2 and Figure 1), we try a simple city-matching of each treated area with other untreated urban areas within the same municipality with pre-treatment population above 100. Moreover, given that the wider RegSO areas have borders with practical implications, 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 6, 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 7 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 urban areas (excluding treated or adjacent areas) in the same municipalities as the treated municipalities

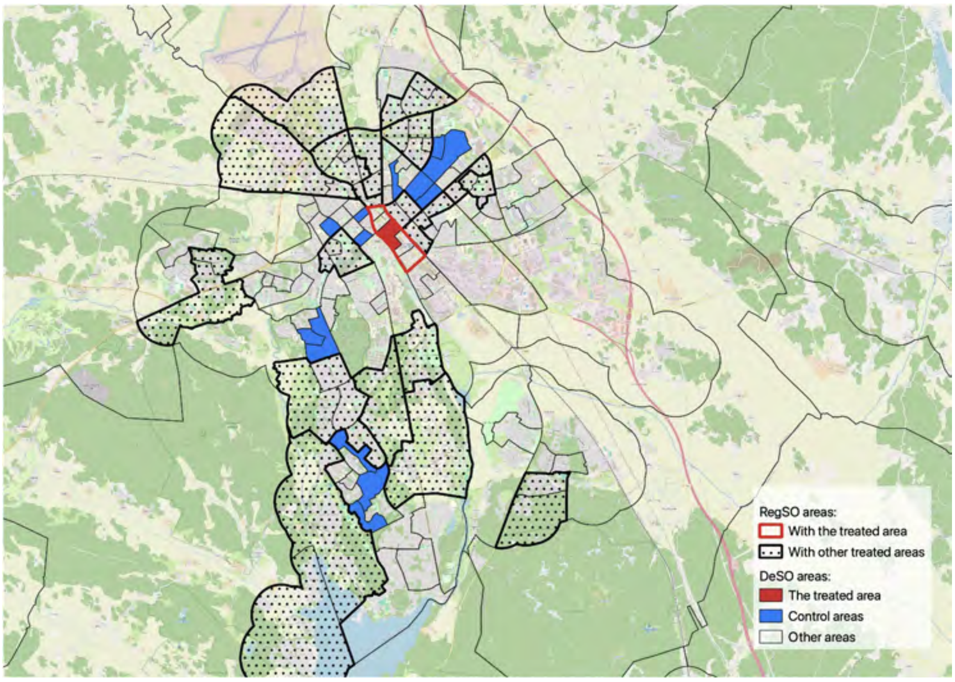


Figure 6: Selection of control group for a treated Q1 area with new co-ops in Uppsala

(short-dashed line), the pool of city-matched 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. With city-matched controls, the levels and trends follow each other before event year -2, and the co-movement is even closer for the city-income matched controls. 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 8, we show income trends separately for Q1 areas with new co-ops and other areas using city-matched controls. We also distinguish between all homes in the treated areas (thick solid lines) and existing homes in those areas (dashed lines). We see a clear post-treatment income rise in poor areas with new co-ops (relative to the control group) but more ambiguous effects in other treated areas. Moreover, the income level also increases in existing homes in Panel A, suggesting that new co-ops have

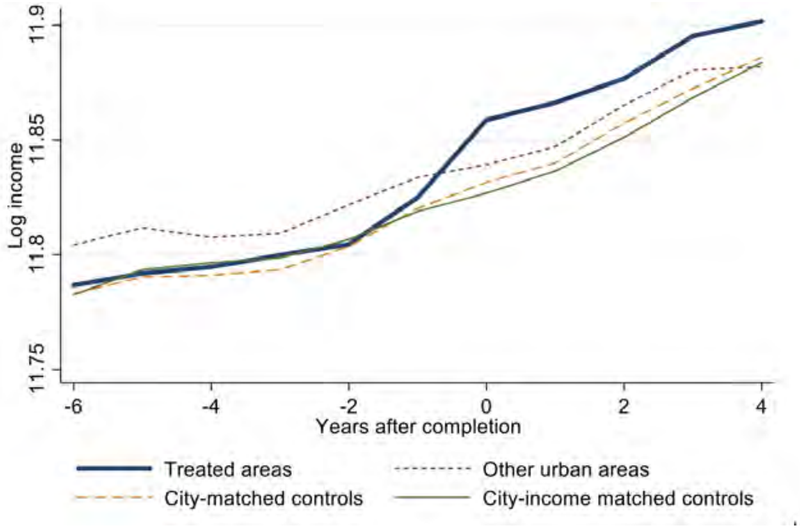


Figure 7: Income in treated and control areas

strong gentrifying effects in poor areas.

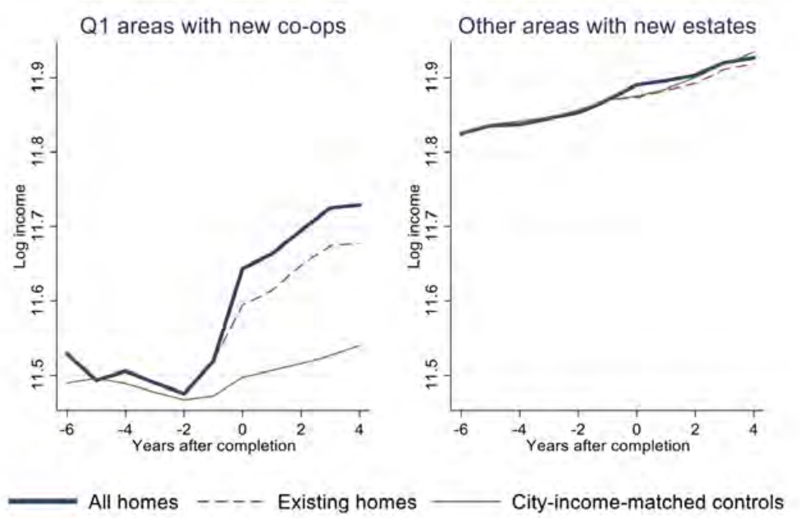


Figure 8: Income by new and existing homes vs. controls.

In estimating the treatment effect with diff-in-diff, we stack treated areas and their individually selected control groups, 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 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 8. 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. 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 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., $E\left(\varepsilon_{itd} \mid T_{itd}, \mu_{id}, \gamma_{td}\right) = E\left(\varepsilon_{itd} \mid \mu_{id}, \gamma_{td}\right)$. 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. 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. We will mostly weigh our regressions by the mean neighborhood population in the pretreatment period (event years -6 to -2), and the effect we estimate can be interpreted as an average for residents in treated areas.

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

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

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^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 pooled 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.[11]

5 Results

5.1 Main estimated effects on the neighborhood

Table 4 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)). We also report the estimates for all homes and existing homes separately, as well as by tenure type of the new estates (co-ops and rentals) and area income (quartiles). The table shows that the overall estimated effect is approximately 2.8% (Panel A, column 5, all types) and statistically significant. However, only the estimates for poor Q1 and Q2 areas and co-ops are positive and statistically significant. For existing homes in the areas,

[11] An 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.

Table 4: Basic diff-in-diff estimates of effects on area income

Outcome: 100* ln(inc)	(1) Q1 areas	(2) Q2 areas	(3) Q3 areas	(4) Q4 areas	(5) All areas
			A. All homes		
Co-ops	15.72**	4.347**	2.219	4.595**	6.538**
	(3.336)	(1.602)	(1.369)	(1.411)	(1.076)
Rentals	3.620	-0.227	−9.458**	-8.542	-2.217
	(4.268)	(1.251)	(2.612)	(4.593)	(1.594)
All types	10.08**	2.123*	-1.972	-0.0349	2.799**
	(2.748)	(1.065)	(1.390)	(1.998)	(0.952)
			B. Existing homes		
Co-ops	10.78**	-0.154	-0.633	3.124*	3.026**
	(3.187)	(1.715)	(1.366)	(1.243)	(1.038)
Rentals	1.866	0.106	−5.732**	-7.667	-1.763
	(4.407)	(1.176)	(1.403)	(4.850)	(1.568)
All types	6.625*	-0.0274	−2.463*	-0.679	0.980
	(2.712)	(1.073)	(1.058)	(1.985)	(0.914)

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

we find similar but weaker patterns.[12]

A striking result in Table 4 is that the estimated effect is the largest for new co-ops in Q1 areas, with a substantial effect of about 16% when including all homes (Panel A, column 1, co-ops) and 11% for existing homes (Panel B, column 1, co-ops); the latter effect is about 70% of the total effect. The remaining rise is from residents in the new estates having higher income levels than in existing homes, which we previously discussed in Section 3.3.[13] This implies that new estates have gentrifying effects by increasing the attractiveness of the poorest 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 new socio-economic composition due to richer residents in the new homes making the neighborhood more attractive.

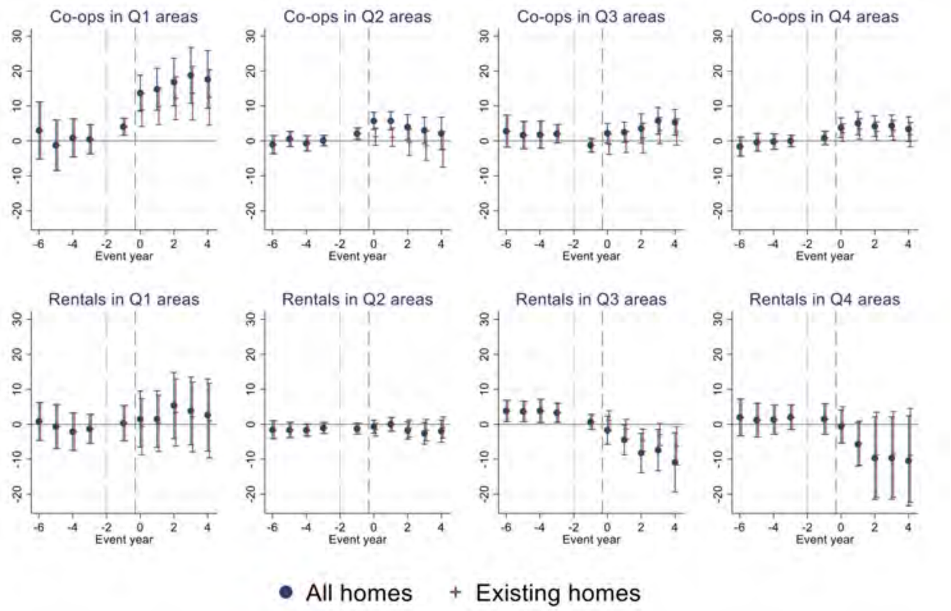
For the remaining areas in quartiles 1 to 4, we also find some evidence of gentrification effects. For rentals, we find some detrimental effects in richer

[12] In the pre-treatment period, all homes are, by our definition, existing homes.

[13] New estates neither existed in the pre-treatment period in treated areas nor exist in control areas; thus, a diff-in-diff analysis is not possible, and we refer to the descriptive statistics in Figure 4 and Table 3.

Q3 and Q4 areas, mostly driven by falling income levels in existing homes.

Does the parallel trends assumption hold and are there anticipation effects of new housing? In Figure 9, we provide event-study estimates based on Eq. (2), and we find small and statistically insignificant placebo estimates in the pre-treatment period before event year -2, which is the base year.[14] In contrast, for new co-ops in Q1 areas, treatment effects are large and statistically significant in each post-treatment event year. Furthermore, there is also a small but statistically significant anticipation effect in event year -1.



Note: We plot point estimates and 95% confidence intervals. See Eq. (2) for the regression specification.

Figure 9: Event-effects of new homes on area income $(100 * \ln(\text{inc}))$

Is the income change driven by changes in the share of poor or rich people? Table 5 reports estimated effects on these shares, separately for Q1 areas with new co-ops and other areas with new estates. We see both a decrease in the share of poor people (-4.2%) and a rise in the share of rich people (+3.4%) in Q1 areas with new co-ops after the treatment.

[14] The exception is new rentals in Q3 areas, where there are some smaller, but statistically significant placebo estimates.

Table 5: Diff-in-diff estimates of effects on share of poor in the bottom income quartile

(1)	(2)	(3)	(4)
Q1 areas with new co-ops		Other areas with new estates	
All homes	Existing homes	All homes	Existing homes
A. Outcome: Share poor (%)			
−4.164**	−2.887**	−0.755**	−0.573**
(0.669)	(0.639)	(0.190)	(0.169)
B. Outcome: Share rich (%)			
3.360**	1.534**	1.251**	0.708**
(0.551)	(0.477)	(0.204)	(0.168)

Note: Standard errors clustered at the area (DeSO) level are reported in parentheses.
* p<0.05, ** p<0.01.

5.2 Additional effect heterogeneity analysis

Since we have a specific matched control group for each treated area, we can estimate area-specific treatment effects (using Eq. 1). Figure 10 reports the distribution of estimated effects by area-percentile income rank, separately for tenure types (Panels A and B) and for all homes (solid lines) and existing homes (dashed lines). The graphs represent local polynomial fits to the estimated effects. They not only confirm that new co-ops in poor areas have the largest gentrifying effects, but also show that the effects are even more pronounced for the poorest areas in the bottom decile where the estimated effect reaches slightly above 20% for all homes, and slightly below 20% for existing homes. We also note the strongest negative effects of new rentals in the richest areas in the top decile.

How do new homes affect areas with different tenure types? In Figure 11, we plot the distribution of estimated effects across areas with different population shares living in rentals. We see that new co-ops have the largest gentrifying effects in areas with more owned homes, and the pattern is the most pronounced for existing homes. A likely explanation is that with rent regulation, rents cannot respond to increased demand. Without higher rent, there are no monetary incentives pushing poor residents away from these homes or discouraging them from moving into them, leading to lower effects in rented homes. Nevertheless, income levels increase even in areas with

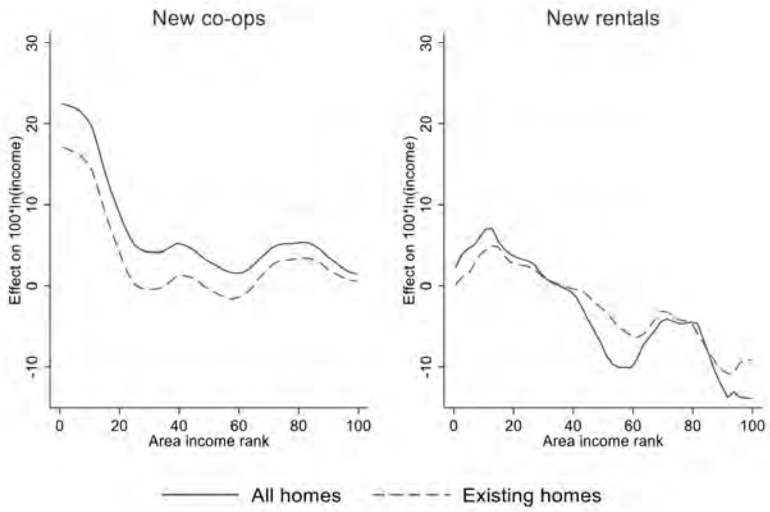


Figure 10: Effects of new homes by area income

only rentals, either due to more rich people staying or moving in. Since there is no confounding price effect, we take this to be a strong sign of the area becoming more attractive.

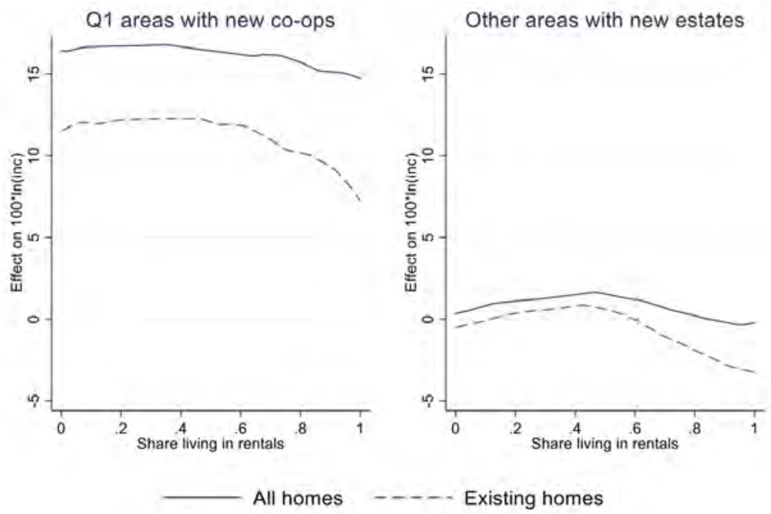


Figure 11: Effects of new homes by area rental share

One expects the effects of new homes on a neighborhood to depend on their effects on population, not the least since at least part of the effect is due to the new residents in the new homes. In Figure 12, we show the distribution of estimated effects of the new estates on area population, using our diff-in-diff specification but replacing the outcome variable by log population (aged 21 or more for whom we use for calculating area income). On average, the new estates increase neighborhood population by 13.9% in Q1 areas with new co-ops and, substantially more, by 23.8% in other areas. Figure 13 displays how the effect on area income depends on the effect on area population. For Q1 areas with new co-ops, we see a positive relationship between the two effects, with an income effect of about 10% when the population effect is 0% and an income effect of approximately 20% when the population effect is above 20%. If new estates only generate an additional in-move of residents richer than the area average, we expect the income effect to be roughly proportional to the population increase. Although the income effect depends on the population effect, we conclude that the mechanism is more complex than that, since we find an income effect when the population effect is zero.

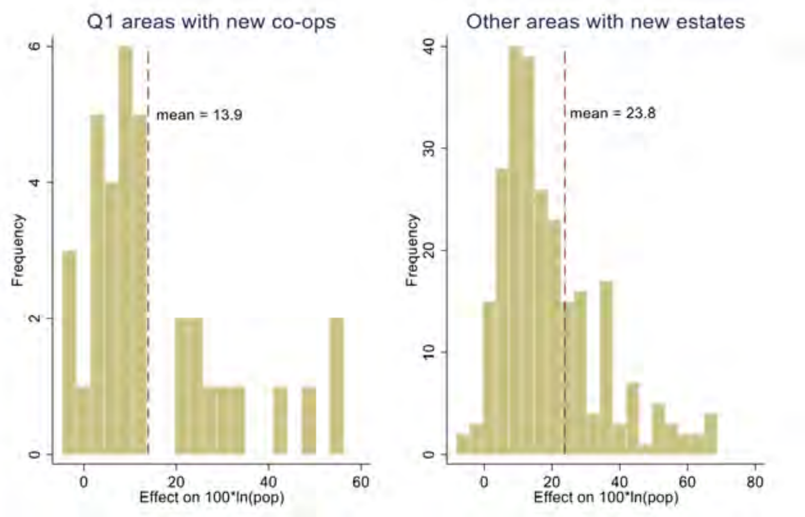


Figure 12: Effects of new homes on population

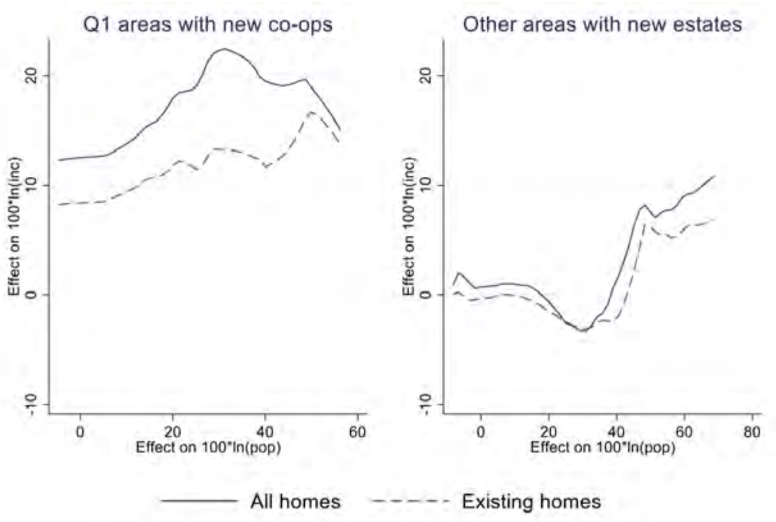


Figure 13: Income effects by population effects

5.3 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.

The results, reported in Table 6, show negative effects on area income in adjacent areas, although the point estimate is not statistically significant for Q1 areas with new co-ops. The effect on the wider area is a weighted average of the effects on areas with new estates and their respective adjacent areas. For Q1 areas with new co-ops, the net effect is about 3.7% (Panel C, column 1) for the wider area and statistically significant, and about half the effect comes from the existing homes (but this estimate is no longer statistically significant). For the other areas, we do not find a statistically significant effect for all homes, but a statistically significant negative effect on existing

Table 6: Effects on area income in adjacent (DeSO) areas and a wider (RegSO) area

(1)	(2)	(3)	(4)
Q1 areas with new co-ops		Other areas with new estates	
All homes	Existing homes	All homes	Existing homes
A. Area with new estate			
15.72**	10.78**	0.826	-0.515
(3.336)	(3.187)	(0.918)	(0.901)
B. Adjacent areas			
-2.668		-1.771^{*}	
(1.616)		(0.773)	
C. Wider area (A + B)			
3.732*	2.012	-0.879	-1.340^{*}
(1.795)	(1.675)	(0.630)	(0.625)

Note: Outcome in $100 * \ln(\text{inc})$. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

homes by about 1.3% (column 4).

5.4 Effects on individual migration

How do new homes affect the migration streams that together generate the total effect on area income? We start analyzing this question by grouping residents in a treated area in event year 2 post-treatment 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 area, in-movers from adjacent areas in the same wider area, and in-movers from other 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.

For new co-ops in Q1 areas, Table 7 reports estimated effects on the number of residents in the area in event-year 2 (aged 21 or more) by origin area in event-year -2. The table shows that new housing increases the number of residents by 151 in the area with new estates (Panel A, column 1) and the entire increase stems from in-moves from outside the wider neighborhood (170, column 4). However, the point estimates for stayers and in-movers from adjacent areas are actually negative (-14 and -4.8, respectively, in columns 2 and 3), indicating rising out-moves, although the estimated effects are not statistically significant. For existing homes, the estimated total effect on population is small (-1.8 in column 1), but the negative out-move estimates are even larger (total change in in-move of $-19 - 11 = -30$ from the wider neighborhood in columns 2 and 3). Thus, while the new estates do enable some residents within the wider neighborhood to stay in the area (the effect of all homes minus that of existing homes in columns 2 and 3) which we also saw in 2 before, counteracting negative effects in existing homes generate a total negative effect. We do not interpret this as (statistically insignificant) displacement at this point since rents cannot increase in response to area gentrification and since out-moves driven by a wish to capitalize on potentially rising housing prices of owned homes is rather voluntary, but further analysis is needed. Estimated effects on adjacent areas are small and not statistically significant.

In Table 8, we report estimated effects on the income level of stayers and in-movers. The Figure shows that rising income levels due to new estates are due to higher income levels among in-movers from other areas (adjacent or outside the wider area). In existing homes, the rise is 32% and 25%, respectively, for in-movers from adjacent areas and other areas (Panel A, columns 3 and 4). Given richer in-movers to the new estates, the total effects are higher, 39% and 37%, respectively. The income levels of stayers change only marginally (column 2).

Table 7: Effects on population in event-year 2 by origin area, Q1 areas with new co-ops

Outcome: Population	(1) All	(2) Stayers	(3) From adjacent	(4) From outside
	A. Area with new estate			
All homes	150.5**	-14.20	-4.788	169.4**
	(34.03)	(14.94)	(7.824)	(28.85)
Existing homes	-1.805	-18.98	-10.64	27.81
	(29.34)	(14.77)	(8.040)	(22.76)
	B. Adjacent areas			
All homes	4.563	-5.517	8.921	1.159
	(15.31)	(8.074)	(9.095)	(8.508)
	C. Wider area (areas in A and B)			
All homes	56.69**	-8.619	4.023	61.29**
	(17.50)	(7.880)	(6.887)	(13.90)
Existing homes	2.288	-10.33	1.931	10.68
	(15.07)	(7.853)	(6.942)	(9.992)

Note: Outcome in number of residents above 20 years old. Origin area refers to where the resident lived in event-year -2. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table 8: Effects on income in event-year 2 by origin area, Q1 areas with new co-ops

Outcome: $100 * \ln(\text{inc})$	(1) All	(2) Stayers	(3) From adjacent	(4) From outside
		A. Area with new estate		
All homes	17.32**	3.252	38.50**	36.64**
	(3.566)	(4.002)	(12.10)	(8.115)
Existing homes	12.50**	2.774	31.51*	24.50**
	(3.199)	(4.025)	(12.45)	(7.402)
		B. Adjacent areas		
All homes	-1.459	0.210	7.114	-4.396
	(1.669)	(1.348)	(9.398)	(3.876)
		C. Wider area (areas in A and B)		
All homes	5.166**	1.258	17.02*	10.74*
	(1.887)	(1.670)	(7.939)	(4.429)
Existing homes	3.465*	1.093	14.81	6.262
	(1.708)	(1.675)	(7.932)	(4.001)

Note: Outcome in $100 * \ln(\text{inc})$. Origin area refers to where the resident lived in event-year -2. 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 (gentrification) 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, on their moving patterns and on their socio-economic characteristics, we are able to provide new and value-added evidence on the stated research question.

We reach three 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 16% in the poorest quartile of neighborhoods. While we find that the positive income effects are even stronger for the poorest percentiles of neighborhoods, the effects are fairly weak/non-existent for neighborhoods richer than the poorest quartile. In contrast, we do not find any 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 11% in pre-existing homes. This indicates that the poorest areas have become more attractive after the new housing construction.

Third, our migration analysis shows that most of the gentrifying effects of new owned apartments are due to high-income people moving in from outside a wider neighborhood. For pre-existing 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.

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. New rental housing does not have these positive effects in poor areas.

7 Appendix

7.1 Binned event-study estimates

Table A1 reports binned diff-in-diff estimates using event-year -2 as the omitted base year. We find similar post-treatment effects as in the main results in 4 and anticipation effects (event = -1) of new co-ops in Q1 and Q2 areas. All placebo estimates (event ¡= -3) are small and statistically insignificant except for new rentals in Q3 areas. From 9, we see that the reason is that there are anticipation effects already in event-year -2 and that the estimated treatment effect therefore is downward biased toward zero.

Table A1: Binned diff-in-diff estimates of anticipation and pre-reform effects (vs. event-year -2)

Outcome: $100 * \ln(\text{Inc})$		(1) Q1 areas	(2) Q2 areas	(3) Q3 areas	(4) Q4 areas	(5) All areas
Co-ops	Post-period	16.33**	4.116**	3.861*	4.143**	6.887**
		(3.226)	(1.510)	(1.544)	(1.443)	(1.062)
	Event $= -1$	4.030**	2.070*	-1.243	0.815	1.458**
		(1.247)	(0.836)	(1.003)	(0.984)	(0.525)
	Event $<= -3$	0.764	-0.288	2.053	-0.566	0.436
		(2.938)	(0.880)	(1.829)	(1.016)	(0.879)
Rentals	Post-period	2.949	-1.423	−6.539*	-7.241	-2.125
		(4.268)	(1.096)	(2.729)	(4.380)	(1.542)
	Event $= -1$	0.315	-1.287	0.654	1.468	-0.0447
		(2.589)	(0.791)	(1.105)	(2.212)	(0.874)
	Event $<= -3$	-0.839	-1.495	3.649*	1.626	0.114
		(2.423)	(0.888)	(1.426)	(2.121)	(0.872)
All types	Post-period	10.09**	1.423	0.129	0.130	3.038**
		(2.684)	(0.983)	(1.454)	(1.914)	(0.923)
	Event $= -1$	2.298	0.438	-0.562	1.045	0.816
		(1.369)	(0.599)	(0.770)	(1.004)	(0.480)
	Event $<= -3$	0.0169	-0.875	2.626*	0.207	0.299
		(1.912)	(0.637)	(1.290)	(1.001)	(0.623)

Note: Outcome in $100 * \ln(\text{inc})$. Origin area refers to where the resident lived in event-year -2. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

7.2 Additional migration results

Tables A3 and A2 report estimated effects on migration streams for treated areas other than Q1 areas with new co-ops. We find positive and statistically significant population effects by each type of origin area (Table A2, Panel A, all homes). Those effects are larger than the effects for Q1 areas with new co-ops, with sizeable contributions from the effects in existing homes.

Moreover, we find a positive population effect in adjacent areas driven by in-moves from outside the wider neighborhood (Panel B). However, the effect on the income composition of residents are much smaller, although in-movers from the outside now have somewhat higher income levels (7.5% in Table A3, Panel A, column 4), largely driven by richer residents in the new estates (small and statistically insignificant estimate for existing homes).

Table A2: Effects on population in event-year 2 by origin area, other areas with new estates

Outcome:	(1)	(2)	(3)	(4)
Population	All	Stayers	From adjacent	From outside
		A. Area with new estate		
All homes	213.6**	23.91**	9.568**	180.1**
	(10.36)	(3.830)	(1.904)	(9.720)
Existing homes	56.96**	16.29**	2.904	37.77**
	(8.995)	(3.925)	(1.804)	(7.696)
		B. Adjacent areas		
All homes	8.126	-4.219	-3.682	16.03**
	(5.908)	(2.709)	(2.407)	(4.590)
		C. Wider area (areas in A and B)		
All homes	83.31**	6.075*	1.167	76.07**
	(6.586)	(2.390)	(1.783)	(5.504)
Existing homes	26.00**	3.285	-1.272	23.98**
	(5.259)	(2.389)	(1.758)	(4.162)

Note: Outcome in number of residents above 20 years old. Standard errors clustered at the area (DeSO) level are reported in parentheses. * p<0.05, ** p<0.01.

Table A3: Effects on income in event-year 2 by origin area, other areas with new estates

Outcome: 100 $*$ ln(inc)	(1) All	(2) Stayers	(3) From adjacent	(4) From outside
			A. Area with new estate	
All homes	0.937	0.0369	-6.107	7.478**
	(1.086)	(0.647)	(5.112)	(2.318)
Existing homes	-0.344	-0.0434	−13.79*	0.789
	(1.069)	(0.652)	(6.407)	(2.438)
			B. Adjacent areas	
All homes	-1.592	-1.010	-3.507	-0.624
	(0.876)	(0.574)	(4.000)	(1.891)
			C. Wider area (areas in A and B)	
All homes	-0.716	-0.650	-4.177	2.294
	(0.706)	(0.454)	(3.415)	(1.518)
Existing homes	-1.159	-0.678	-6.155	-0.115
	(0.701)	(0.455)	(3.566)	(1.531)

Note: Outcome in 100 $*$ ln(inc). Origin area refers to where the resident lived in event-year -2. Standard errors clustered at the area (DeSO) level are reported in parentheses. * $p<0.05$, ** $p<0.01$.

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