The Economic Effects of Public Housing Programs

by

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Abstract

This dissertation studies the economic effects of public housing programs. Public housing used to be the primary form of housing assistance throughout the 20th century in countries such as the United States and the United Kingdom. In recent decades, however, public housing has fallen out of favor, mainly due to the negative experience with large public housing developments, which concentrated high levels of poverty and crime. As a result, policymakers have shifted resources towards subsidized private housing in mixed-income developments, i.e., buildings that combine affordable with market-rate units. In the first two chapters, I examine the impact on local housing markets of demolishing and regenerating public housing into mixed-income developments. In the last chapter, I lay out a quantitative model to think about the distributional implications of shifting resources from public housing towards other housing assistance programs, such as housing vouchers or subsidies to low-income housing construction.

Chapter 1 estimates the effects of demolishing public housing on private house prices. I examine the impact of a large and negative housing supply shock caused by the demolition of public housing developments in Chicago in the 1990s and 2000s. Using a synthetic control method based on census tracts in distant parts of the city, I estimate that house prices increased by about 20 percent over a ten-year period in census tracts near the demolitions. A calibration exercise suggests that the upward price pressure associated with reduced housing supply cannot fully explain the observed price effect. This leaves room for a contribution from positive amenities generated by demolitions, which raised the demand for nearby housing units. The estimated importance of amenity effects is, however, sensitive to the way the affected housing market is defined. The results highlight that, while public housing can lead to lower local house prices for unsubsidized households by increasing overall supply, the way in which the public sector supplies housing –in this case, high-rises concentrating very low-income households– can impose significant adverse consequences on its neighbors.

Chapter 2 (joint work with Lorenzo Neri) studies the effects of regenerating public housing into mixed-income communities on the local housing market. We exploit a wave of public housing regenerations in London that not only demolish and rebuild existing public housing but also almost double the number of units on-site by adding new market-rate units. Over a six-year period, we estimate that regenerations significantly raise nearby house prices and rents, although house prices decrease slightly farther away. We also find that they attract higher-income households, increase positive amenities (e.g., cafés, restaurants), and reduce negative amenities (e.g., crime). The results are consistent with strong demand effects concentrated near the buildings and moderate effects from increased supply that persist in the broader area. We provide suggestive evidence that changes in a neighborhood's socioeconomic composition are important to explain price effects: regenerations in low-income areas and those adding a large number of market-rate units lead to larger price increases. Overall, our findings indicate that providing public housing through mixed-income housing can overcome some of the negative consequences on nearby areas associated with traditional public housing developments, as suggested in Chapter 1. However, the supply of additional market-rate units can reduce affordability in low-income neighborhoods, possibly due to an increased risk of gentrification and displacement of low-income neighbors.

Finally, Chapter 3 (joint work with Juliette Fournier) examines the distributional implications of the policy shift from public housing to subsidized private housing initiated by the U.S. government over the past few decades. This policy shift leaves a larger role to private developers and property owners in supplying low-income housing, who may end up capturing a substantial share of the benefits intended for disadvantaged households. We build a quantitative urban framework where housing assistance complements income taxation to redistribute across workers. We argue that the provision of affordable housing involves a trade-off between indirect pecuniary redistribution and direct amenity effects. On the one hand, public housing drives local rents down by increasing supply, while amplifying the spatial concentration of poverty. On the other hand, project- and tenant-based rental assistance enhances the local amenities of subsidized households by promoting mixed-income communities, but pushes private landowners' rents up. We estimate the key parameters of the model, which allows us to disentangle the forces behind this crucial trade-off.

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Contents

Chapter 1

Pecuniary Effects of Public Housing Demolitions: Evidence from Chicago

1.1 Introduction

Public housing had been the primary form of housing assistance for most of the 20th century, but in the 1970s the United States drastically shifted support to other housing programs. From the 1930s to the 1960s, the government built large public housing developments, usually consisting of multiple high-rise buildings in low-income areas, to provide affordable housing for low-income households. By the 1990s, however, these buildings showed high levels of poverty and crime and, in some cases, poor maintenance made them uninhabitable. As a result, policymakers shifted resources to other housing assistance programs that were not perceived as generating such negative consequences, such as housing vouchers. This trend led to a major cutback on the public housing program in the 1990s and 2000s, when most of the severely distressed public housing developments in the country were demolished.

In this paper, I study the impact of a large reduction in the public housing stock on private house prices, which mainly results from two mechanisms. First, demolishing public housing reduced the overall supply of housing and increased the residual demand for private housing, which should have raised local house prices. I refer to this as the *public supply effect*. Coate et al. (1994) observed that public provision of in-kind benefits can have such pecuniary effects in the market. Second, demolitions likely raised local house prices by changing local amenities (*amenity effects*), which indicates that the way in which the public sector supplies housing can have adverse effects on its neighbors, e.g., concentrating very low-income households in high-rises likely imposes a negative externality.

I show that public housing demolitions led to quantitatively large price increases of nearby houses. I examine the impact of a large, negative housing supply shock caused by the demolition of large public housing developments in Chicago in the 1990s and 2000s. I estimate that house prices increased by about 20 percent in census tracts near demolitions over a ten-year period. Next, I test whether the full price effect can be explained by the reduction in overall housing supply, i.e., the public supply effect. A back-of-the-envelope calibration of a simple supply and demand model suggests that both the public supply effect and amenity effects played a role, the importance of each being sensitive to the definition of a housing market.

Chicago provides an excellent setting to study the effects of public housing on private housing markets. One reason is that Chicago demolished the highest number of public housing units in the country –22,703 units between 1995 and 2010. In fact, this city accounted for about one-fifth of all units demolished under HOPE VI, a federal program meant to replace the nation's oldest public housing developments. Another reason is that only around one-third of demolished units were eventually rebuilt, less than a half of which were public housing. This was a clear negative public housing supply shock that led to a significant increase in private housing demand in the city through the relocation of tenants from public to private housing. Lastly, Chicago has rich address-level data on all demolitions and their timing, as well as project-level data on reconstruction.

The empirical strategy follows a synthetic control methodology (Abadie and Gardeazabal, 2003; Abadie et al., 2010), a novel approach in relation to prior research, which relies on more traditional spatial differences-in-differences (DID) methods. These methods usually compare the evolution of prices within an inner ring of a treated building to an outer ring surrounding the inner ring that serves as a control group.

Consequently, the cutoff distance between the two rings makes the implicit assumption that price effects are zero beyond that point. In contrast, synthetic controls allow me to abstract from determining the exact distance at which spillover effects disappear. This last point is especially important in the context of Chicago: demolitions were very concentrated both geographically and in terms of the timing of their announcement. These two features make it challenging to find a control ring –either in space or time– that is not contaminated by other demolitions. Synthetic controls overcome this issue by comparing house price trends in tracts near demolitions to a synthetic control consisting of a combination of tracts in *distant* parts of the city that match them on price pre-trends and baseline census tract characteristics.

Using this method, I find large effects of public housing demolitions on private house prices and long-run rents in nearby census tracts. I define three treatment groups according to their proximity to demolitions. One group includes tracts with 50 or more demolished units, while the other two include tracts in the first and second ring of tracts surrounding the demolitions. The results show statistically significant house price increases after the demolition announcement in tracts with demolitions and tracts in the first ring (34% and 18%, respectively), which become smaller in the second ring (10%) and fade out beyond this point. I also estimate that long-run rents go up in the three treated groups in a very similar magnitude.

Next, I find suggestive evidence that both the public supply and amenity effects largely contributed to the large price increases. First, I provide evidence that there is scope for a large public supply effect. I show that housing supply decreased by 35% in tracts with demolitions. Leveraging Infutor data, which can track the address history of households displaced by demolitions, I also show that most displaced households ended up in private housing within two adjacent tracts of the demolitions, which increased the residual demand for private housing. Second, I present suggestive evidence of potentially large amenity changes: households attracted to nearby areas after the demolitions were significantly less likely to be low-income and black. Using a simple supply and demand model, I derive an expression to isolate the part of the price change that is implied by the public supply effect, which only depends on the number of households relocating from public to private housing and the housing supply elasticity. I present a range of values for this expression using alternative elasticity estimates and several definitions of a housing market. For some values in this range, the public supply effect can fully account for the long-run price change when I define a housing market based only on geography, i.e., focusing on nearby *houses*. However, estimates are smaller when I define a housing market as tracts where unsubsidized *households* who lived near demolitions moved to in the pre-treatment period.

The findings in this paper have two main policy-relevant implications. First, the potentially large public supply effect can be used as an argument for public housing when assessing the recent policy shift to housing vouchers. While more public housing may decrease local house prices by increasing supply, housing vouchers can lead to the opposite effect by increasing the demand for private housing. Second, the importance of amenities supports the idea that the form in which the public sector supplies and manages housing can impose large, negative externalities.

This paper contributes to three related but distinct literatures. First, and more narrowly, I contribute to the literature on the impact of public housing demolitions on neighborhoods.[1] Prior research shows that demolitions in Chicago induced large crime rates decreases in nearby areas (Aliprantis and Hartley, 2015; Sandler, 2016) and HOPE VI impacted the neighborhood racial and economic composition (Tach and Emory, 2017). In contrast, this paper examines the impact on the local housing market. The two closest papers study the HOPE VI program more generally. Brown (2009) estimates that house prices increased up to 9% near demolished public housing compared to non-demolished buildings around completion in four cities (Atlanta, Baltimore, Charlotte, DC). In my context, the large magnitude of the demolitions and the rich data in Chicago allow me to study the importance of the public supply effect. In addition, I choose a control group acknowledging that areas near demolitions are selected and I allow the path of price effects to start at the announcement date. The second paper, Zielenbach and Voith (2010), finds mild house price increases using

[1] Other papers study the long-run effects of demolitions in the U.S. on the displaced population, e.g., Jacob (2004) and Chyn (2018) on education and employment. Similarly, Neri (2020) examines the impact of public housing regenerations on student achievement in London, U.K.

four public housing developments in Boston and DC as case studies.

Second, this paper builds on the literature studying the impact of place-based housing policies on local housing markets. Diamond and McQuade (2019) show that subsidies to low-income housing construction through the Low Income Housing Tax Credit program (LIHTC) have heterogeneous price effects that depend on the neighborhood composition, while Sinai and Waldfogel (2005) and Eriksen and Rosenthal (2010) find large crowd-out effects of LIHTC on new market-rate housing supply. Koster and van Ommeren (2019) find positive but small price effects of public housing quality improvements in the Netherlands. This paper adds to this literature by studying the price effects of a sizeable reduction in the public housing stock.

Third, and more broadly, the role of the public sector in delivering support to low-income households either through cash or in-kind transfers is a fundamental issue in public finance. Coate et al. (1994) argue that (publicly provided) in-kind transfers such as public housing, "by expanding the supply of a good, lower its price and transfer rents from suppliers to consumers". I think of public housing demolitions as a sharp reduction in overall housing supply that not only shifts private housing demand outwards due to tenant relocation from public to private housing but also due to changes in local amenities, likely caused by the poor performance of the public sector in providing housing –which has not been previously acknowledged in this literature.

1.2 Background and Data

1.2.1 Background

Chicago was the most affected city by the wave of public housing demolitions that took place under the HOPE VI program in the mid-to-late 1990s and 2000s. This federal program started a nation-wide trend to replace the nation's oldest public housing developments and, as a result, favor other housing assistance programs, such as housing vouchers.

I focus on the public housing demolitions in Chicago for two reasons. First, Chicago accounted for an exceptionally large share of the demolitions. Around 20% of all demolished units under the HOPE VI program were located in this city. Second, demolitions resulted in a large, negative housing supply shock. Appendix Fig. A-1 illustrates how only around 35% of the demolished units were rebuilt, of which 40% were public housing. As a result, thousands of public housing tenants were displaced and relocated within the city. This lead to a considerable increase in the demand for private housing, which is useful to study the contribution of reduced public supply to observed house price changes.

Congress passed the HOPE VI program in 1993 with the objective of either demolishing, rehabilitating or rebuilding "severely distressed" public housing developments.[2] Under this program, public housing authorities (PHAs) could apply for "Demolition only" and "Revitalization" grants. The former were awarded for the sole purpose of demolishing public housing, while the latter included funding for rehabilitation and reconstruction. From 1993 to 2010, 278 grants were awarded and around 97,000 and 11,000 units were demolished and rehabilitated, respectively. The program also created approximately 79,000 affordable housing units and 12,000 market-rate units. Households displaced by demolitions were mainly relocated to other public housing (50%) or housing vouchers (40%), while a smaller share were either evicted or left unassisted (10%).

Notably, the HOPE VI program awarded many grants to demolish and redevelop Chicago's public housing into mixed-income communities. The fact that public housing developments in Chicago received more funding –and, most of it, during the first years of the program– can be explained by two main factors. One of them is the fact that these buildings showed high levels of poverty and were plagued by drug trafficking and violent crime, which quite often made it to the local and even na-

[2] In 1989, Congress established the National Commission on Severely Distressed Public Housing to identify "severely distressed" public housing developments. In order for the National Commission on Severely Distressed Public Housing –established by Congress in 1989– to refer to a project as "severely distressed" they considered the following conditions: 1) residents living in dispair and generally needing high levels of social and supportive service; 2) physically deteriorated; or 3) economically and socially distressed surrounding communities. In its final report in 1992, the Commission counted 86,000 units nationwide as falling under that category (6% of US total public housing).

tional news.[3] In 1999, blocks with public housing high-rises experienced a mean of 0.27 homicides and 24 drug crimes, compared to city-wide means of 0.02 and 3.65, respectively (Aliprantis and Hartley, 2015). In addition, the bad state of the public housing stock also played an important role. Developments were allowed to deteriorate for several reasons, including lack of political clout, deliberate neglect and prejudice (Popkin et al., 2004). The poor physical conditions of the buildings further contributed to the concentration of poverty —only the most vulnerable households were willing to live there. In fact, before the approval of the program, the worst housing projects had an occupancy rate of 45% because some units had to be closed even before any demolition plan was approved (Buron and Popkin, 2010). To tackle these issues, the city passed the "Plan for Transformation" in 2000, with the objective of getting rid of old medium and high-rise public housing developments and replacing them with low-rise mixed-income housing.

1.2.2 Data

I use three main datasets. First, I introduce address-level data on public housing demolitions and reconstruction. Second, I construct a quality-adjusted house price index, the main outcome of interest in this paper, using residential transactions data. Lastly, Infutor data, containing address-level migration decisions of most adults living in Chicago, is used to study the displacement and demographic effects of demolitions.

Public housing demolitions. The first dataset combines information from several sources to obtain a comprehensive picture of all public housing buildings active at any point between 1995 and 2018 in Chicago. The Chicago Housing Authority (CHA) provided me with the full list of public housing buildings that were either demolished or constructed in that period, including addresses, development name, number of units, as well as start and end demolition dates, and end date of construction, where applicable. The CHA also shared a list of new units replacing demolished buildings

[3] Some of the most known cases include:
https://www.chicagotribune.com/news/politics/chi-chicagodays-dantrelldavis-story/
http://www.chicagotribune.com/news/local/chi-941015-eric-morse

by type (public housing, affordable and market rate units), development and year from 1998 to 2017. Lastly, I use data from 1996 HUD-951 forms,[4] which contains a snapshot of all public housing building addresses, units and geographical coordinates for developments in that year, as well as a publicly available dataset containing the same information for all active public housing developments in 2018.[5]

I complement this information with data from HOPE VI on "Demolition only" and "Revitalization" grants. For the former, there is publicly available data containing the development name, number of demolished units and award year.[6] For the latter, I have administrative data on the award year and the timing of demolitions by development. Most demolished units received HOPE VI funding –18,899 out of a total of 22,703 demolished units (83%). Most developments with HOPE VI funds received both "Demolition only" and "Revitalization" grants. Appendix Table A.1 lists all developments and the relationship of grants, award years and demolition dates.

House prices. I construct the main dependent variable, the house price index, using data from Corelogic, a company that collects house transaction data from register of deeds officers. For all residential arms-length transactions in Chicago, this dataset contains the sale date, sale price, mortgage information, foreclosure status and the geolocated address of the transacted property. I merge it with other property characteristics from Zillow Ztrax, which are obtained from local assessor officers.[7] Finally, I drop outliers by excluding transactions in the top percentile of the yearly price distribution.

Next, I construct a quality-adjusted house price index at the census tract level. The facts that I only observe house prices for transacted properties and that demolitions might affect both the quantity and quality of sales poses a challenge to my

[4] These are forms that public housing authorities (PHAs) were required to report to the Department of Housing and Urban development (HUD) containing information on all of their public housing buildings.

[5] https://hub.arcgis.com/datasets/756ab1b3c8374169898ad77d667636ee_1

[6] Available at https://www.hud.gov/sites/documents/DOC_9890.PDF

[7] Appendix A.3.1 provides a detailed explanation of this merge, which is based on the parcel number. Source of Zillow data is "ZTRAX: Zillow Transaction and Assessor Dataset, 2018-Q4", Zillow Group, Inc.

analysis. I address this issue by controlling for a comprehensive set of transaction and property characteristics. More specifically, the house price index, ρ_{ct}, is the result of running the following regression:[8]

$$\ln \mathrm{P}_{ht} = \rho_{c(h),t} + \alpha_m + \gamma' \mathbf{X}_{ht} + u_{ht} \tag{1.1}$$

The left-hand side is the logarithm of the sale price of property h (located in census tract $c(h)$) in year t. α_m are month-of-sale fixed effects that capture seasonality in sale prices, while $\mathbf{X}_{ht}$ is a vector of property characteristics. This includes building type, building age dummies, lot size, lot size squared, number of stories, number of bedrooms, number bathrooms and roof cover type.[9] Finally, the house price index is given by the census tract-year fixed effects, $\rho_{ct} \equiv \rho_{c(h),t}$, which represent quality-adjusted house price trends at the census tract level.

Infutor data. I exploit Infutor data to obtain a detailed picture of the timing and magnitude of displacement induced by demolitions. This dataset, collected by Infutor Data Solutions from a number of private and public record sources, contains information on the address history of almost all adult individuals in the United States since the 1980s. It includes information on their name, date of birth, gender, full addresses and lived dates at each address. The coverage of the dataset is increasing in the earlier years and achieves its highest level of coverage in the year 2000. Appendix A.3.2 provides a more detailed description of the coverage and shows that Infutor covered around 55% of the adult census population in 1990 and jumped to approximately 80% by 2000.

To keep track of the relocation patterns of tenants displaced by public housing demolitions, I construct a novel dataset containing all tenants who left the demolished

[8] The construction of the house price index follows an approach similar to Baum-Snow and Han (2020).

[9] Since some property characteristics are missing from some transactions, I generate dummy variables for missing values for each property characteristic except building type (which is never missing) and re-code missing values as zeros. In the regression, I include a term interacting each characteristic's missing dummy variable with building type to flexibly account for heterogeneity in that characteristic across property types when data is missing.

buildings and appear in Infutor, as well as their history of living addresses. I refer to it as the *displacement dataset*, which is described in detail in Appendix A.3.3. The dataset is restricted to the sample of tenants leaving a demolished address from 7 years before the demolition of that address started and up to 1 year after this date.[10]

Other. I also collect census data from the 1990 decennial census on several demographic and economic variables: population, race, gender, age, employment, income, poverty rate, median rent, occupancy rates, etc.[11] Crucially, I use these characteristics to match units treated by demolitions to their respective synthetic control.

1.3 Empirical Strategy: Synthetic Controls

I use synthetic controls to estimate the effect of public housing demolitions in Chicago on nearby house prices. I compare house price trends in census tracts near demolished buildings to those in tracts farther away in the city that are similar on observables.

1.3.1 Why Synthetic Controls?

In contrast to prior research examining the price effects of place-based policies, I follow a novel approach to study this question: synthetic controls. Previous literature uses more traditional spatial differences-in-differences (DID) methods that compare the evolution of prices in an inner ring of a certain radius around the treated building to an outer ring surrounding the inner ring that serves as a control group. Such methods rely on the choice of a cutoff distance between the two rings beyond which price effects are assumed to be zero. However, synthetic controls abstract from this issue by using distant, yet similar on observables areas of the city as controls.

[10] This strategy allows for progressive relocation of public housing buildings, which might happen before the start of the demolition, as shown in Appendix Fig. A-2. I choose to count as displaced those individuals living in these addresses up to 1 year after the start of the demolition because Infutor data might reflect address changes with a lag.

[11] Census data and shapefiles were obtained from Steven Manson, Jonathan Schroeder, David Van Riper, Tracy Kugler, and Steven Ruggles. IPUMS National Historical Geographic Information System: Version 15.0 [dataset]. Minneapolis, MN: IPUMS. 2020. http://doi.org/10.18128/D050.V15.0.

In the context of Chicago, the high concentration of demolitions both geographically and in their announcement timing makes it even more difficult to assess the distance at which price effects fade out in a ring methodology. Fig. 1-1a shows the spatial distribution of demolished public housing addresses in Chicago by number of units. The majority of them, except for Cabrini-Green in the near North Side, were concentrated in specific neighborhoods of the West (28%) and South (55%) Sides. As a result, the overlapping of rings belonging to different demolition events arises as a serious concern of a more traditional spatial DID approach: the outer ring is likely to be contaminated by other demolitions. Moreover, Fig. 1-1b shows that most units were announced for demolition under the HOPE VI program between 1994 and 2000 –the announcement date is the relevant treatment period for house prices because they are forward-looking. Thus, an identification strategy that compares rings around buildings being announced for demolition earlier to those being announced later is also unfit to study the long-run impact of demolitions on house prices.[12]

These facts highlight the convenience of synthetic controls to study spillover effects in this setting: farther away areas of the city are a more plausible control group. Synthetic controls will compare house price trends in areas near demolitions, which experienced a clear negative housing supply shock, to those in distant, yet similar on observables areas of the city.

1.3.2 Definition of Treatment

Hence, I run the analysis at the 1990 census tract level and define the treatment groups and the treatment period in the following way.

Treatment groups. I define three treatment groups based on their distance to the demolitions (Fig. 1-3 below). First, I use the term "Demolition" tract for census tracts where 50 or more units were demolished between 1995 and 2018. The other two treatment groups are denoted as "Neighbor×1" and "Neighbor×2". The former

[12] Prior research used the quasi-random timing of public housing building closures as exogenous variation, since closures were spaced over time (Aliprantis and Hartley, 2015; Sandler, 2016).

includes census tracts in the first ring of tracts adjacent to Demolition tracts, while the latter includes tracts adjacent to that first ring. These definitions of treatment are also consistent with most displaced households relocating within two census tracts of the demolitions (Appendix Fig. A-17). Lastly, I drop from the analysis treated tracts corresponding to the Altgeld-Murray development, which was announced for demolition in 2016 and, therefore, does not include enough post-treatment years.

Treatment period. As discussed above, I use the year when demolitions were announced as the treatment period. This choice is consistent with rational expectations models of house prices (Poterba, 1984; Sinai and Waldfogel, 2005), in which prices should jump when information about the demolitions first arrives. Furthermore, the path of price effects can be used to assess whether and by how much such models hold in this particular context.[13]

More specifically, I define the announcement year in the following way. For demolitions that received a HOPE VI grant, I use the minimum between the award year and the start year of the first demolition within a public housing development.[14] Although it is usually the case that the grant award occurs earlier than the first demolition, in some cases a grant was awarded for later stages of the demolition process for a development. For demolitions without a HOPE VI grant, I use the start date of the first demolition within a development.

Summary statistics

Table 1.1 reports some summary statistics for two samples within each treatment group. The "Full" sample includes all census tracts in any treatment group (N=274), while the "Analysis" sample only includes tracts with a positive number of sales in the last two pre-treatment periods (N=207). When I examine the effects of demolitions on house prices, I use the "Analysis" sample to focus on a subset of tracts with better

[13]Deviations from the rational expectations model would imply that house prices jump not only at the time of announcement but also in the following years.

[14]In particular, the start of a demolition is defined as the notice-to-proceed date for demolition. The notice-to-proceed notified tenants that the building was going to be torn down and had to be issued at least 90 days before the demolition.

matching on pre-trends, since some treated tracts experience very few or no sales. The differences in characteristics between both samples are not very large and, hence, the "Analysis" sample is fairly representative of the "Full" sample.

The table reveals that treated tracts are remarkably different than untreated tracts (N=637). Treated tracts have a higher share of black, low-income and low-educated population. In addition, fewer sales take place in treated tracts and houses are transacted at a lower price. Lower prices might be explained by the fact that there is a higher share of renter households or that the transacted housing stock is older. All of these differences are greater for tracts closer to demolitions.

1.3.3 Estimation: Penalized Synthetic Controls

I estimate the effect of public housing demolitions on house prices for each treatment group using synthetic controls (Abadie and Gardeazabal, 2003; Abadie et al., 2010). This method constructs a control unit for each treated tract as a convex combination of untreated tracts (i.e., synthetic control) that best fits on aggregate some pre-treatment characteristics of the treated tract. The fact that synthetic controls provide a data-driven procedure to choose the control group is especially important in this context because, as discussed above, treated tracts are considerably different than the average untreated tract. This approach allows me to overcome this issue by matching not only on house price pre-trends but also census tract characteristics.

I use the penalized synthetic control method (PSCM), recently introduced by Abadie and L'Hour (2021), to estimate the average treatment effect on the treated (ATET) of demolitions on house prices, which is helpful in two ways. First, optimal synthetic control weights in traditional synthetic control methods (SCM) may not be unique. In contrast, PSCM achieves uniqueness by prioritizing the inclusion in the synthetic control of units that are more similar to the treated units, thereby reducing the risk of worst-case interpolation biases. Second, my setting comprises multiple units treated at different times, while the traditional SCM literature laid out estimation and inference methods only for the case of one treated unit. Abadie and L'Hour (2021) introduce a convenient and transparent way of thinking about the

ATET and inference in such cases.

PSCM computes optimal synthetic control weights as follows. Assume that there are n_0 control tracts. For a given treated tract i, PSCM solves the following problem:

$$\begin{aligned} \min_{W_i(\lambda)\in\mathbb{R}} \quad & \|X_i - X_0 W_i\| + \lambda \sum_{j=1}^{n_0} \|X_i - X_j\| W_{i,j} \qquad (1.2)\\ \text{s.t.} \quad & \sum_{j=1}^{n_0} W_{i,j} = 1 \\ & 0 \leq W_{i,j} \leq 1 \ \ \forall \text{ i,j} \end{aligned}$$

where W_i is the $n_0 \times 1$ vector of weights with which each control tract contributes to the synthetic control of treated tract i. Each element of this vector is denoted as $W_{i,j}$, i.e., the weight of control tract j on treated tract i's synthetic control. W_i is restricted to add up to one and each of its elements must be between 0 and 1. X_i is the $k \times 1$ vector of pre-treatment matching variables of treated unit i and X_0 is a matrix $k \times n_0$ of those variables for control tracts. Finally, the operator $\|A\|$ indicates a weighted quadratic distance.[15]

The main difference between PSCM and traditional SCM is the second term in the minimization problem of Eq. (1.2), which is governed by λ. When $\lambda = 0$, the problem above is equivalent to traditional SCM. That is, it chooses the weight combination of the control group that best fits the matching variables of the treated tract on aggregate. If $\lambda > 0$, however, the minimization problem incorporates a penalty for pairwise matching discrepancies between the treated tracts and each of the tracts that contribute to the synthetic control. That is, the value of λ trades off aggregate fit of the synthetic control and the fit of the matching variables between the treated tract and each of the tracts in the synthetic control. In practice, I follow Abadie and L'Hour (2021) and select λ using cross validation techniques.

I compute the ATET for each treatment group as follows.[16] First, I use PSCM to

[15] That is, $\|X_i - X_0 W_i\| = (X_i - X_0 W_i)' V_i (X_i - X_0 W_i)$, where V_i is a $p \times p$ diagonal matrix that assigns importance weights to the different components of the covariates vector. Appendix A.4 provides more details on the choice of this matrix.

[16] Appendix A.4 provides a more detailed explanation of the penalized synthetic control method-

obtain the vector of optimal weights $W_i(\lambda)$ for the synthetic control of each treated tract i by matching on two types of variables. The first matching variable consists of pre-trends in the outcome variable from 5 to 2 years before the announcement of the demolitions to ensure that the synthetic control was on the same time trend as the treated tract (I only include the pre-trend up to 2 years before the announcement to avoid anticipation effects in the year previous to announcement). The second type of matching variables are census tract characteristics in 1990: population density, black share, education level, median income, and poverty rate.

Second, I construct the outcome series for the synthetic control of each treated tract i. Let Y_{it} denote the outcome variable of tract i in year t relative to the demolition announcement. The outcome for the synthetic control of tract i, Y_{it}^{SC}, is the average of this variable in the control group, weighted by the contribution of each control tract to the synthetic control of tract i, $W_{i,j}^*(\lambda)$, as computed above. Then, I normalize the series with respect to $t = -2$ ($\tilde{Y}_{it} = Y_{it} - Y_{i,-2}$) and take the difference between the treated ($\tilde{Y}_{it}$) and control series ($\tilde{Y}_{it}^{SC}$) to obtain the treatment effect for i:

$$\tau_{it} = \tilde{Y}_{it} - \tilde{Y}_{it}^{SC}, \qquad \text{where} \quad \tilde{Y}_{it}^{SC} = \sum_{j=1}^{n_0} W_{i,j}^*(\lambda)\tilde{Y}_{jt}$$

Since the main outcome will be expressed in logarithms, the normalization above provides a convenient interpretation. For instance, $100 \times \tau_{it}$ can be interpreted as the percentage difference in house prices between tract i and its synthetic control at t relative to their respective value in $t = -2$.

Lastly, I report the ATET, τ_t, of each treatment group by year relative to announcement. I weight each treated tract by the number of private housing units in 1990, H^{1990}. Let n_1 be the number of treated units in the treatment group, then:

$$\tau_t = \frac{1}{\sum_{i=1}^{n_1} H_i^{1990}} \sum_{i=1}^{n_1} H_i^{1990} \times \tau_{it} \tag{1.3}$$

ology, including how to estimate λ.

To test the significance of the results, I run the permutation test described in Abadie and L'Hour (2021). In particular, I am interested in testing for the significance of the aggregate effects on treated tracts for each separate treatment group. The main idea of the test is the following. First, I compute the treatment effect under the original treatment assignment. Then, I randomly assign treatment in the dataset $B = 1,000$ times and compute the ATET for each of them. After this, I generate a p-value that reports the fraction of the B iterations with an ATET value higher than that in the original treatment. Appendix A.4 provides the details.

1.4 Main Results: Effects on Local Housing Prices

I find that demolitions led to large and persistent house price increases after their announcement in immediately surrounding areas and that long-run rental prices increased in a similar fashion. The results are robust to several alternative specifications.

1.4.1 Effects on House Prices

Houses in the first ring of tracts around the demolitions experienced quantitatively large price increases over a ten-year period after their announcement, while the price effect was smaller in the second ring of tracts. Fig. 1-2 plots the path of price effects by treatment group and the first columns of each group in Table 1.2 report price effects and p-values by period. Demolition tracts show an average long-run price increase of 34%, although one should be cautious interpreting this estimate because Demolition tracts are difficult to match on pre-trends given the few number of transacted houses. Despite this, the estimate is consistent with the nearest houses being the most affected by demolitions. In Neighbor$\times$1 tracts, prices slowly increased until they level off at a statistically significant 18% approximately four years after the announcement. The price effects for houses in Neighbor$\times$2 tracts was smaller (10%). When I run the same analysis on the third ring of surrounding tracts (Neighbor$\times$3), I find that the effects fade away and are very close to zero.

The results are not consistent with a rational expectations model in which all of the information about the policy change was revealed at the time of its announcement.[17] In these models, house prices reflect the present discounted value of the stream of expected future rents (Poterba, 1984; Sinai and Waldfogel, 2005). Hence, buyers and sellers incorporate future rent changes into house prices when information first arrives. In my context, about half of the long-run price effect in the first ring of tracts realizes one year after the initial announcement (10% vs. 18%), which implies that some information is capitalized into house prices right after demolition news are revealed. However, the gradual price increase suggests that either not all of the information was revealed at first (e.g., there was further good news about amenities) or that there was uncertainty or mistrust around demolition plans.[18]

Census tracts contributing to the synthetic controls are observably similar to treated tracts, geographically not very far from them, and were not significantly impacted by displacement. Altogether, these facts suggest that the synthetic controls are a plausible comparison group. First, not only synthetic controls reproduce the values of treated tracts' characteristics used to match in PSCM, but they are also similar across a wide range of other census and sales characteristics (see Appendix Table A.3). Another feature of the control group is that most tracts with higher weights for the synthetic control are located only slightly farther away from demolitions. Fig. 1-3 highlights untreated tracts by the sum of weights with which they contribute to the synthetic control of Neighbor×1 tracts, with darker blue colors indicating a higher contribution.[19] Finally, tracts in the synthetic control did not receive a large share of displaced households, which could have affected their house prices and bias my results. In fact, most displaced tenants relocated to treated tracts or untreated tracts

[17] See Appendix A.5 for a detailed description of an application of these models to this context.

[18] A good example of information arriving at different times is that some developments received more than one HOPE VI grant for different stages of the demolition process. For instance, Stateway Gardens was awarded one grant to demolish the projects in 2000 and another to revitalize the area in 2008. An extreme example of uncertainty or mistrust around demolitions plans is given by the last Cabrini-Green high-rise to be knocked down. While its demolition was announced in 1995, resident opposition delayed actual demolition until 2011, when other parts of the development had already been reconstructed. *Source*: https://www.chicagotribune.com/news/ct-bn-xpm-2011-03-30-29364731-story.html

[19] Controls being close to treated tracts usually holds for each separate public housing development as well. As an example, Appendix Fig. A-3 reproduces this map for the Henry Horner Homes and shows that the synthetic controls for Neighbor×1 tracts of this development are geographically close.

not contributing to the synthetic controls in a significant way.[20]

The results do not seem to be particularly driven by changes in the quality of transacted properties. Running PSCM with the inverse hyperbolic sine (asinh) of the number of sales as an outcome, I find that the number of sales increased in the two neighboring groups of tracts by 24 to 32% after the demolitions (Appendix Fig. A-5 and Table A.4).[21] Since I only observe prices for transacted houses, this result raises the concern that demolitions may affect the average quality of transacted houses in a way that the house price index is unable to account for. Nevertheless, Appendix Figs. A-6 and A-7 shows that the evolution of several house characteristics of sold houses is similar for treated tracts and their corresponding synthetic controls.[22]

The price effects above are significantly bigger than other estimates in the literature. Previously, Brown (2009) estimated house price increases of up to 9% and 5% within 0.5 and 0.5-1 miles, respectively. There are three reasons for the smaller magnitude. First, Brown (2009) studies four other cities, all of which demolished a considerably smaller amount of public housing than Chicago. Second, that paper misses part of the effect by defining the treatment period as the reconstruction completion date, while this paper shows that the path of price effects starts at the announcement date. Third, Brown (2009) uses non-demolished public housing as a comparison group in a spatial DID. However, it is plausible that demolished public housing was on different house price trends than non-demolished buildings, e.g., due to the persistence of poverty and crime –which would underestimate the effect. In contrast, synthetic controls are on the same pre-trend by construction.

Finally, there are two caveats to the findings above. One is that I ignore general equilibrium effects. I find that there is an increase in prices in treated tracts *relative*

[20] A comparison between Fig. 1-3 and Appendix Fig. A-4, which shows the pattern of displacement, supports this statement.

[21] The inverse hyperbolic sine (asinh) is defined as asinh(a) $= \ln(a + \sqrt{1 + a^2})$. This function preserves the interpretation of the logarithm and accounts for the cases in which there are zero sales.

[22] Although the share of single-family residences went up in Neighbor×1 tracts with respect to their synthetic control, this characteristic is comprehensively accounted for in the quality-adjusted house price index. Furthermore, when I construct the house price index using only single family residence sales, I obtain qualitatively similar results (Appendix Fig. A-8) –although the estimates are much noisier because the number of single-family sales is much smaller.

to farther away areas in the city. The results provide evidence of strong price effects that fade out with distance to the demolitions. The second caveat is that the results speak to a very specific counterfactual: I compare the evolution of house prices in treated areas, which experienced a sharp decrease in public housing supply, to that of similar areas where housing supply follows a trend without an exposure to such large shock. However, policymakers might also be interested in other counterfactuals, e.g., a context where the private sector was free to build any number of units on demolished sites or where demolished units were fully replaced by new public housing.

1.4.2 Effects on Long-Run Rents

Although I focus on house prices due to the availability of rich transaction-level data, demolitions should have had a more direct impact on rents: only renter households were displaced, most of which used housing vouchers on the private market. House prices are affected to the extent that they reflect the net present discounted value of these rents, which suggests that the impact on rents may have been even higher.

Using decennial census data on rents, I show that demolitions led to similar long-run rent increases in nearby tracts. Columns (1) and (5) of Table 1.3 report changes in median contract rents in treated areas from 1990 to 2000 and 2010, respectively. Panel A shows the results for a cross-section regression of rent changes on dummies for each treatment group, while Panel B runs PSCM. While effects are mostly concentrated in Demolition tracts in the OLS specification, the PSCM method yields statistically significant rent increases for the Demolition (37%), Neighbor×1 (15%), and Neighbor×2 (9%) groups that are consistent with the house price increases above. This difference between the two methodologies highlights the importance of using a comparison group that closely resembles the (highly selected) tracts near demolitions.

1.4.3 Robustness of the Results

The results hold when considering several robustness checks. First, I run the same analysis for a subset of tracts with an expected better match on pre-trends. In particular, I restrict the sample to tracts with an average of at least four sales per year in the pre-treatment period. The results, which are shown in Appendix Fig. A-12 and reported in the second column of each treatment group in Table 1.2, are very similar to the full sample version. Second, the results hold when I use the average house price index in the pre-period to construct the synthetic control, instead of each separate pre-period year as in the main specification (Appendix Fig. A-13). Third, the results are nearly identical when I use traditional synthetic control methods, i.e., $\lambda = 0$ (Appendix Fig. A-14 and Table A.6).

Fourth, the results are consistent when I exclude developments that experienced more reconstruction. Appendix Fig. A-1 shows that most developments reconstructed less than 40% of demolished units as either public or private housing. The exceptions were Cabrini-Green, Henry Horner Homes and Lake Michigan Homes, all of which reconstructed at least 80% of the units. The first two of these three demolitions were located in areas close to downtown and near high-income neighborhoods. Land is more valuable at these two demolitions than at any others. To explore whether the estimated house price effects for the full demolition sample were driven in large part by these sites, Fig. A-11 and Table A.5 re-run the main analysis without these three developments. The results are very similar to those including them.

Lastly, Fig. A-15 plots the results for two event study designs at the house sale level using the census tract-based definitions of treatment. Given that synthetic controls showed that results fade out in the second ring of tracts ("Neighbor×2"), I use house sales in the third ring of tracts as the control group ("Neighbor×3"), which gives a flavor of a more traditional spatial DID design but avoiding issues arising from overlapping rings. Panel (a) plots the coefficients for a specification in which the treatment period is 1994 for all treated tracts, the year when the first demolition is announced. This exercise gives a sense of how long-run house prices evolved in calendar time in each of the treatment groups relative to slightly farther away areas. Panel (b) uses years relative to the announcement of the first demolition in the house sale's tract instead of calendar years. Both approaches lead to very similar results to synthetic controls: house price increases of around 35%, 20% and

0% in the Demolition, Neighbor×1 and Neighbor×2 tracts, respectively.

1.5 Mechanisms: Public Supply vs. Amenity Effects

Using a simple supply and demand framework, I assess the importance of two mechanisms contributing to price increases. First, the sharp reduction in public housing supply led to an outward shift in the demand for private housing units, which I refer to as the *public supply effect.* This effect highlights a policy-relevant property of public housing –and, more generally, publicly provided in-kind transfers. The government, by building more public housing, increases housing supply, which should lower its price in the market. Demolitions, in contrast, should lead to the opposite effect. Second, demolitions likely further increased the demand for private housing in nearby areas by changing local amenities. Such *amenity effects* emphasize how the form in which in-kind transfers are publicly provided can also result in further pecuniary effects. In the context of Chicago, public housing mainly took the form of poorly maintained high-rises that concentrated poverty and crime, which likely generated a large disamenity.

1.5.1 Theoretical Framework

I introduce a simple supply and demand framework to explore whether the price effects can be solely explained by the public supply effect and, indirectly, assess the importance of amenity effects. To do this, I use the fact that the public supply effect induces a shift in the private unit housing demand that should be equal to the number of households relocating from public to private housing because of the demolitions.

Fig. 1-4 gives some graphical intuition on the consequences of demolitions on the private housing market. First, displacement from public housing and relocation to housing vouchers led to a sharp increase in the number of households demanding housing in the private market (ΔH^{S}), shifting the demand curve outwards from D to D'. Thus, prices increased by ΔP^{S}, i.e., the public supply effect. Second, demolitions likely changed amenities in two ways: by displacing very low-income public housing tenants to other areas –thereby changing the neighborhood composition–, and by removing a negative physical externality –due to the poor conditions of the buildings, they were likely to impose an eyesore effect on their neighbors. Hence, the private housing demand likely further shifted outwards from D' to D" due to an increased willingness to pay for amenities in these areas, leading to a price change of $\Delta \mathrm{P}^{\mathrm{A}}$.

The decomposition above makes the strong assumption that both mechanisms are additive. However, these effects will only be additive if there is no correlation between the number of displaced tenants relocating to a census tract and the extent to which that tract is affected by amenity effects. While this assumption might hold in the short-run, when amenity effects are not fully realized, there is no reason to think it holds in the long-run.

Hence, I derive an expression to test whether the public supply effect can explain the totality of the long-run price effects under the null hypothesis of no amenity effects ($\Delta \mathrm{P}^{\mathrm{A}} = 0$). The difference between the long-run price change and the public supply effect provides a sense of the importance of amenity effects. Given knowledge of the number of households relocating to private housing ($\Delta \mathrm{H}^{\mathrm{S}}$), I can back out the price change implied by the public supply effect under some additional assumptions. Assume an isoelastic housing supply curve with elasticity ε^s and that other supply factors remain constant. Since the public supply effect leads to a movement along the supply curve, it can be expressed as follows:[23]

$$\Delta \ln \mathrm{P}^{\mathrm{S}} = \frac{\Delta \ln \mathrm{H}^{\mathrm{S}}}{\varepsilon^s} \tag{1.4}$$

To estimate this quantity, I need (i) a definition of the housing market affected by demolitions, (ii) a measure of the number of displaced households relocating to the private market ($\Delta \ln \mathrm{H}^{\mathrm{S}}$), and (iii) an estimate of the housing supply elasticity

[23] A caveat of this derivation is that, in this context, the public supply effect includes an additional income effect. The vast majority of relocating households were issued housing vouchers. Families with a housing voucher only pay 30% of their income towards rent and the rest is covered by the government up to the Fair Market Rent, which is usually around 40% of the county's median rent. The public supply effect incorporates the fact that the decision of households to relocate to a certain private housing market was influenced by their increased purchasing power, which likely further raised house prices through higher rents.

in the market (ε^s). Given that these items are either difficult to define precisely or imperfectly observed, Section 1.5.3 estimates the public supply effect using alternative definitions of these items.

1.5.2 Descriptive Evidence

Before calibrating the public supply effect, I provide descriptive evidence that both mechanisms are likely at work. First, two facts suggest that there is scope for a large public supply effect. One is that demolitions dramatically depressed housing supply in affected tracts. Using PSCM, Column (6) of Table 1.3 shows that the number of housing units decreased by 35% in tracts with demolitions, while it was close to unaffected in the two surrounding rings of tracts. The second fact is that displaced public housing tenants mainly relocated in nearby private housing. Using the displacement dataset described in Section 1.2.2, Appendix Fig. A-16 reveals that around 85% of displaced tenants in Infutor ended up in private housing.[24] While 80% of tenants stayed within the city, a considerable share (above 40%) moved out to a housing unit within two adjacent tracts from the demolitions (Appendix Fig. A-17).

Second, large changes in the socioeconomic composition of nearby tracts point to potentially large amenity effects. Table 1.3 reports changes in median household income and black population shares by decade and treatment group. Focusing on the PSCM results, tracts with demolitions had increased their median income up to 58% by 2010. This figure is still significant for the first ring of surrounding tracts (30%) and becomes statistically insignificant for the second ring. A similar pattern holds for the black population share, with decreases of 15, 6, and 3 percentage points, respectively, for tracts in the closest to the farthest away treatment groups. While the effects on tracts with demolitions are likely due to tenant relocation, long-run

[24] A previous estimate of this percentage, based on tenants that were displaced between 1999 and 2008, suggests that 71% of households were relocated to non-public housing units. In particular, out of 16,551 households to be displaced in 1999, only 4,724 remained either in traditional or scattered-site public housing (*source*: University of Chicago School of Social Service Administration and Case Western Reserve University Mandel School of Applied Social Sciences (February 2012): "Chicago's Public Housing Transformation: What Happened to the Residents?" *Mixed-Income Development Study, Research Brief*). Alternatively, HOPE VI data reports 3,523 displaced households, of which 47% relocated to private housing.

increases in household income and decreases in the black share in neighboring tracts are informative of the type of households attracted to these areas after demolitions. Furthermore, when I explore heterogeneity of the price effects by these two variables, I find that tracts with low household income levels and higher black shares at baseline experienced slightly larger house price increases (Appendix Figs. A-9 and A-10). This result is consistent with these tracts having more potential for amenity improvements. Lastly, prior research on the decline of crime near the demolitions in Chicago further supports the idea of an outward shift of the private housing demand curve due to a reduction in disamenities (Aliprantis and Hartley, 2015; Sandler, 2016). Taken together, these findings suggest that these areas are becoming more attractive to higher-income households, likely due to better amenities.

The timing of relocation and demolition are informative of the relative importance of these two mechanisms only to a limited extent. The public supply effect should be fully realized after public housing tenants are relocated, while amenity effects should start at the moment of relocation (e.g., a decrease in population may decrease crime) and fully materialize after the demolition and reconstruction process is completed. First, I explore the timing of relocation by running PSCM on the yearly census tract population in Infutor.[25] Fig. 1-5 shows that relocation led to a population drop of about 25% in Demolition tracts over a twelve-year period, most of which happened within five years of announcement. Second, the demolition process was lengthier: 55% of units had been completely demolished within five years, a figure that increased to 90% after ten years (Appendix Fig. A-18). Given that most of the price effects are realized five years after the announcement, these facts suggest that the public supply effect and amenity effects related to relocation (e.g., crime decreases) play a central role in explaining house price increases compared to physical changes in the neighborhood (e.g., structural demolition and beautification of the area).

[25]In this case, I match tracts on pre-trends up to 5 years before the demolitions –as opposed to 2 years before–, acknowledging the fact that relocation may have started several years before announcement in some cases. More details on Appendix A.3.2.

1.5.3 Estimating the Public Supply Effect

I present a range of estimates for the public supply effect in Eq. (1.4) using alternative measures of the number of relocated households ($\Delta \ln \mathrm{H}^{\mathrm{S}}$), the housing supply elasticity (ε^s) and the housing market definition. For some values in the range of estimates, the public supply effect can fully account for the long-run price change when I define a housing market based only on geography, i.e., focusing on *houses* in tracts near demolitions. However, the estimates are smaller when I define a housing market as tracts where unsubsidized *households* who lived near demolitions moved to in the pre-treatment period. The second definition is grounded in the argument that mover households should be approximately indifferent between their before and after locations, which makes it possible to define the contours of a housing market using choices rather than geography alone.

Column (1) of Table 1.4 shows the average price effect in years 5 to 10 relative to announcement for each housing market definition, while Columns (2)-(4) report estimates of the public supply effect for two supply elasticities and two measures of $\Delta \ln \mathrm{H}^{\mathrm{S}}$. The last two columns show the minimum and maximum of the ratio of the public supply effect estimates in Columns (2)-(4) over the long-run price effect in Column (1) as a percentage. For the long-run price effect, I use the estimates in the previous section. Every treated tract that could not be used to estimate the price effect (i.e., not in the "Analysis" sample) is assigned the average price effect of their treatment group[26]. For the housing supply elasticity, I include two estimates from the literature. First, I use the metropolitan area-level housing supply elasticity for Chicago in Saiz (2010), which is 0.8. Second, I also report results using the tract-level housing supply unit elasticity estimates in Baum-Snow and Han (2020), which are generally lower than Saiz's estimate.[27]

[26]Note that some treated tracts could not be used because of very few sales taking place around treatment. For untreated tracts, I assume their long-run price effect to be 0.

[27]Baum-Snow and Han (2020) uses labor demand shocks to commuting destinations to identify the housing supply elasticity at the census tract level for the period 2000-2010. I use the predicted tract-level housing supply unit elasticities based on hedonic price growth from Table 7 in that paper, since this price index is the closest to mine. For Chicago, they find a mean tract-level housing supply unit elasticity of 0.32. For each housing market definition that I present, I aggregate these tract-level elasticities assuming that all tracts simultaneously experience identical housing demand shocks

For each supply elasticity, I construct lower and upper bounds for the outflow of households relocating from public to private housing ($\Delta \ln \mathrm{H}^{\mathrm{S}}$). To do this, I assume that each individual in the displacement dataset is a unique household.[28] The lower bound, $\Delta \mathrm{H}^{\mathrm{L}}$, uses the number of displaced tenants that I observe in Infutor as remaining in the corresponding private housing market. This is a lower bound because coverage of Infutor is incomplete in the 1990s, i.e., I only observe a subset of displaced tenants. The upper bound, $\Delta \mathrm{H}^{\mathrm{U}}$, uses the maximum number of displaced households relocating to private housing, which I construct in two steps. First, I proxy the total number of households as the number of units demolished adjusted by the occupancy rate in their census block group in 1990.[29] Then, I adjust this quantity by the share of displaced households relocating to private housing. Using Appendix Fig. A-16, I consider that 85% end up in private housing.[30]

Next, I present the results for three alternative housing market definitions. The first definition captures the effect on nearby houses; the second, on nearby unsubsidized households; and the last, most expansive definition considers the entire city.

Proximity-based definitions. The first two rows of Table 1.4 show that the public supply effect can explain from 30% to all of the long-run price effect when the housing market definition includes only tracts near demolished sites. First, I define the market affected by demolitions as only including Demolition and Neighbor×1 tracts. In this case, the public supply effect explains 43 to 178%. If I include Neighbor×2 tracts, this effect accounts for 30 to 122%. These results suggest that the public supply

(Section 6.1. of that paper). Baum-Snow and Han (2020) show that, as a result, the housing supply of a region r can be aggregated from tract-level (denoted by i) estimates using the following weighted sum: $\varepsilon_r = (\sum_{i \in r} H_{ir} \frac{\varepsilon_{ir}}{1+\varepsilon_{ir}})/(\sum_{i \in r} H_{ir} \frac{1}{1+\varepsilon_{ir}})$. For proximity-based definitions, I use the number of private housing units in 1990 as the weight H_{ir}, while for migration-based definitions I use the share of individuals migrating from areas affected by demolitions to census tract i as described below.

[28]This is a plausible assumption. Out of 13,917 identified displaced tenants, only 275 (2%) have the same last name, and same living start and end dates in the original building, which I use as a proxy for belonging to the same household.

[29]This accounts for the fact that some units were already vacant at the announcement time.

[30]For migration-based definitions of the housing market, these steps are slightly different. In those cases, I need an estimate of $\Delta \ln \mathrm{H}^{\mathrm{S}}$ for each destination tract. In practice, I (1) compute the number of displaced individuals by destination tract, (2) adjust this number by the ratio of total number of displaced measured by demolished units over the total number of displaced in Infutor –to account for the incomplete coverage of Infutor, and (3) multiply by 0.85 –share going to private housing.

effect has a large impact on houses closer to demolitions and can even account for the full price effect. This fact is consistent with the high rates of tenants relocating very close to demolished buildings, thereby exerting a higher demand pressure on the private housing market. However, the lower end of the range of estimates indicates that amenity effects are also important.

Migration-based definitions. The third and fourth rows of Table 1.4 use a revealed preference approach by defining the housing market based on migration patterns of households living in nearby areas before the demolitions. I construct a housing market index that weights every census tract in the city according to the share of individuals in Infutor moving in from tracts near the demolitions before their announcement. Intuitively, the weights indicate how important each tract was as an outside option for households living in private housing in affected areas prior to the demolitions.[31] I use two definitions for "affected areas": one includes Demolition and Neighbor×1 tracts, the second also includes Neighbor×2 tracts.

With migration-based housing market definitions, only between 18 to 85% of the long-run price effect can be explained by the public supply effect. This smaller magnitude of the public supply effect may be the result of tracts with higher weights in the housing market definition, i.e., tracts to which unsubsidized households moved out the most before the intervention, receiving less displaced households. Appendix Fig. A-19 provides suggestive evidence of this fact. While tracts with higher pre-treatment

[31] Formally, I define an outcome of the relevant housing market as follows. If m_{ij} indicates the number of moves from treated area i to tract j in the pre-treatment period, then the outcome of interest for the relevant housing market of treated area i, y_i, is expressed as:

$$y_i = \sum_j s_{ij} \times y_j, \qquad \text{where } s_{ij} = \frac{m_{ij}}{\sum_j m_{ij}}$$

That is, the housing market measure for a given outcome y_i weights every tract j in the city according to the share of moves s_{ij} from treated area i. Note that tracts in treated area i are also included in the weighted sum.

I obtain the share of moves from treated areas to each destination tract by restricting the sample of moves in Infutor in several ways. First, I only consider address moves within the city of Chicago. Second, I limit the sample to moves from the affected areas in the period 1985-1993. I choose 1993 as the last included year because most demolitions were announced in the period 1994-2000. Finally, I discard all moves where the origin or destination are a public housing address, since this paper is just concerned about price effects on unsubsidized housing. Thus, there is no need to multiply by 0.85 to adjust for the number of displaced tenants moving to the private housing sector.

migration shares from affected areas are also very close to demolitions (hence, amenity effects can still be large), they seem to receive a relatively smaller share of displaced households, driving down the public supply effect.

All Chicago. Lastly, I include all tracts in the city and use Saiz (2010)'s elasticity estimate to find that the public supply effect accounted for 30 to 48% of the observed long-run price effect (last row of Table 1.4).[32] These estimates indicate that the reduction in the public housing stock led to a significant burden on the city's private housing market: house prices rose by around 1% due to the relocation of thousands of public housing tenants to the private housing market.

Altogether, the results suggest that nearby *houses* experienced price increases relatively more through a (reduced) public supply effect than nearby unsubsidized *households*. The likely reason is that tracts receiving more displaced households were not the primary housing substitutes for nearby households –thus, the private housing demand increase was not as large in their market. In contrast, nearby houses necessarily bore the price of thousands of displaced households relocating to private housing in the nearby area. Intuitively, if the relocation pattern had been more dispersed throughout the city, the public supply effect would have been much less important in explaining price increases in proximity-based housing market definitions.

1.5.4 Discussion

The results suggest that both the public supply and amenity effects played an important role in increasing house prices, and that their relative importance is sensitive to the definition of a housing market.

These findings prompt two main policy-relevant implications. First, the potentially large contribution of the public supply effect in explaining price house changes

[32]For this case, I extend the computation method of proximity-based definitions to the whole city. The public supply effect is equal to the share of displaced households relocating to the entire city's private housing market over the Saiz's elasticity estimate, while the long-run price is a weighted sum of the tract-level price estimates in the previous section assuming that untreated tracts experience no change in prices due to the demolitions.

is relevant to inform the choice between public housing and other housing assistance programs, e.g., housing vouchers. While more public housing might decrease local house prices by increasing overall housing supply, the recent policy shift from public housing to housing vouchers can lead to the opposite effect. More vouchers, which allow subsidized households to rent a unit in the private market and pay only a fixed percentage of their income, increases beneficiaries' willingness to pay for housing, thereby increasing the private housing demand and likely raising local house prices. In fact, Susin (2002) and Collinson and Ganong (2018) provide suggestive evidence of vouchers inducing faster rent increases. A caveat is that any benefit of public housing coming from a public supply effect should be contrasted with the fact that supplying housing might come at a higher cost for the public sector relative to the private sector.

Second, and more generally, the results also point to significant pecuniary effects coming from the form in which housing is publicly provided. When Coate et al. (1994) examined the pecuniary effects of publicly provided in-kind transfers, they only focused on what I refer to as the public supply effect and regarded it as a welfare gain for unsubsidized households: "a program that builds housing for the poor, for example, is likely to result in a lower price of existing low-income housing than would an equally costly cash transfer".[33] However, I show that the public provision of housing can involve some features that may not arise with private provision and that may further impact prices –which I refer to as amenities. In the context of this paper, the public sector's poor management and underinvestment in maintenance led to the decay of public housing in Chicago. These conditions, together with the fact that developments consisted of high-rise buildings concentrating poor households in low-income areas, led to high poverty and crime rates, as well as an eyesore effect. These negative amenity effects associated with the provision of public housing had potentially adverse welfare effects –negative externalities– on households residing near public housing projects.

[33] Low-income households mostly rent. Since I do not have access to rent data, this paper uses house prices as an outcome. As discussed in Section 1.4.1, house prices can be interpreted as incorporating information of the expected future stream of rents. Therefore, house prices changes can be thought of as a proxy for changes in rents as well.

1.6 Conclusions

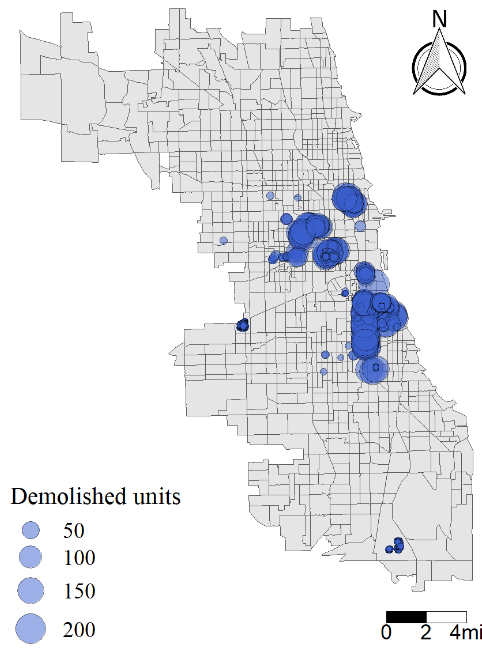
This paper shows that public housing demolitions in Chicago caused large house price increases in nearby areas over a ten-year period. Using a simple supply and demand model, I find that both effects from reduced housing supply and changes in amenities are important to explain observed price changes. In the context of Chicago, this last result can be explained by two facts. First, the large magnitude of public housing demolitions and the very low levels of reconstruction pushed thousands of public housing households into the private housing market, putting an upward pressure on house prices. Second, the particularly poor management of the buildings by the public sector generated a sizeable disamenity that translated into large amenity gains after their demolition.

Although this paper highlights that building more public housing can lead to a decrease in local house prices through the *public supply effect*, it also emphasizes the need for further research on the ways in which the public sector can provide it without generating large, negative externalities. In particular, future work should study the spillover effects and cost-effectiveness of providing public housing in alternative forms. For instance, scattering public housing throughout the urban landscape or partnering with the private sector to provide public housing within mixed-income communities might alleviate the adverse effects arising from the concentration of very low-income individuals in high-rise buildings.

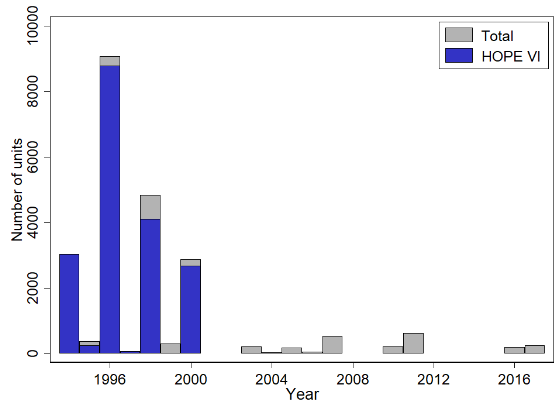
Moreover, this paper also stresses the importance of defining a housing market to evaluate place-based policies. Proximity-based definitions describe the consequences for the prices of nearby houses, which are relevant for the owners of these properties. Migration-based definitions, in contrast, capture the effects not only on owners of nearby properties, but of other properties that may be more remote geographically while still being part of the same housing market.

Figures

Figure 1-1: Public housing demolitions: location and timing



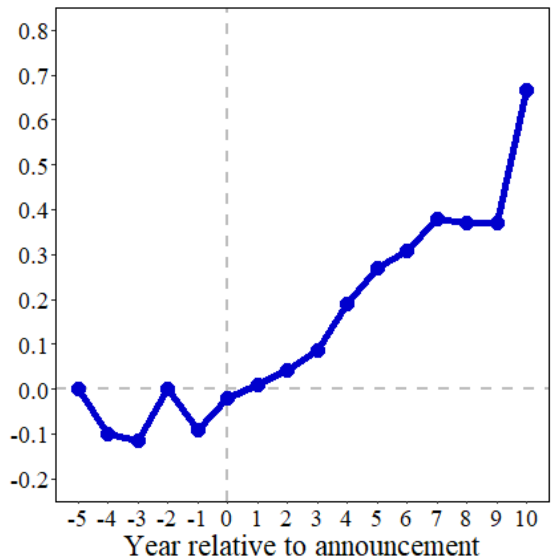
(a) Demolished addresses (1995-2018)



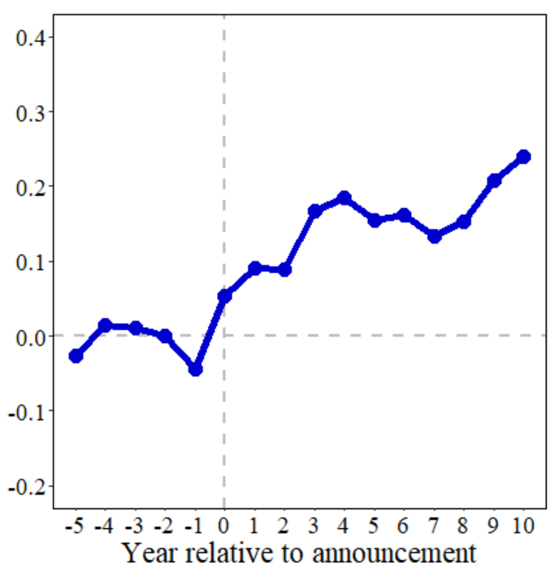
(b) Units announced for demolition by year

Note: The top map shows the city's division in 1990 census tracts. Every circle represents an address with a public housing demolition, and the size of the circle represents the magnitude of the demolition. The bottom histogram shows the number of public housing units announced for demolition by year and by whether they received a HOPE VI grant. For units in a development that received a HOPE VI grant, I use the award year as the announcement year. For units outside the scope of the program, I use the date when the Chicago Housing Authority notified residents that they were going to proceed with the demolition.
Source: Census tract shapefiles were obtained from IPUMS National Historical Geographic Information System (NHGIS) and demolished units by address are shown as reported by the CHA.

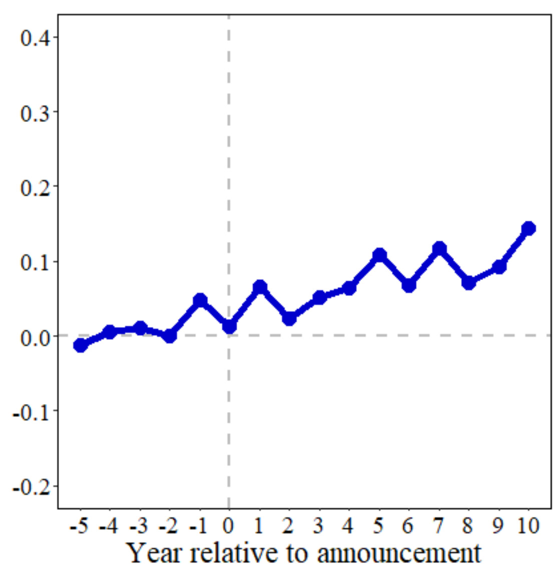
Figure 1-2: Effects of demolitions on the house price index, ρ_{ct}



(a) Demolition



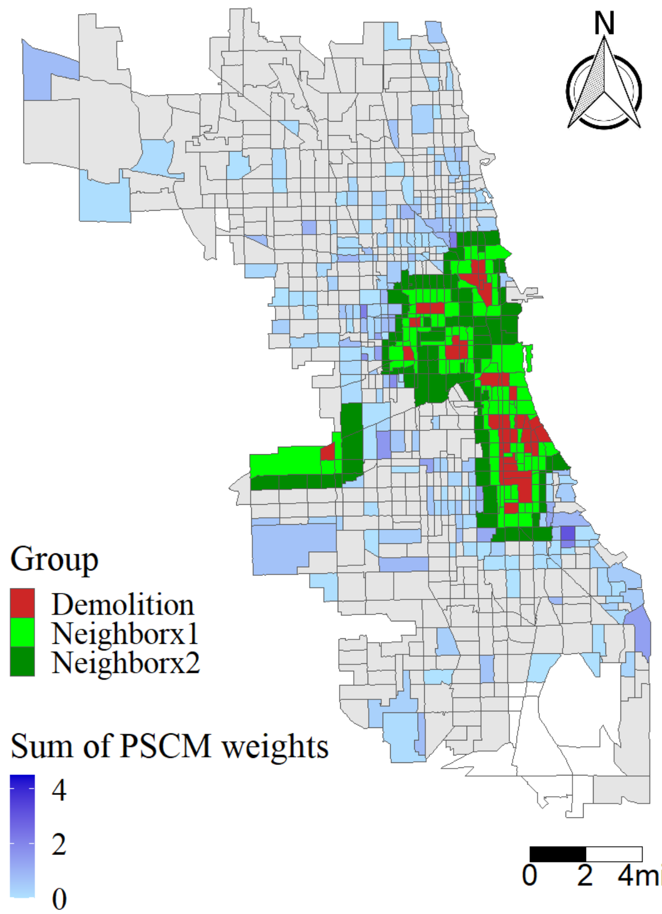
(b) Neighbor×1



(c) Neighbor×2

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the house price index ρ_{ct} as an outcome variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Analysis sample". The x-axis indicates the year relative to the first demolition announcement.

Figure 1-3: Treated tracts and contributors to synthetic controls for Neighbor×1 tracts



Note: This figure illustrates the contribution of each 1990 census tract to the creation of synthetic controls for the Neighbor×1 treatment group. In particular, it reports the sum of weights with which each tract i contributes to each of the synthetic controls of that treatment group ($w_{i,j}$), weighted by the number of 1990 private housing units of each treated tract j, H_j^{1990}. That is, it shows $\bar{w}_i = \sum_i (1/\sum_j H_j^{1990}) \sum_j H_j^{1990} \times w_{i,j}$. It also highlights Demolition (red), Neighbor×1 (light green) and Neighbor×2 (dark green) tracts.
Census tracts shaded in light gray, corresponding to the Altgeld-Murray development, are dropped from the analysis. The second ring of adjacent tracts are not excluded due to the large size of census tracts in that area. *Source*: Census tract shapefiles were obtained from NHGIS.

Figure 1-4: Private housing market before and after the demolitions

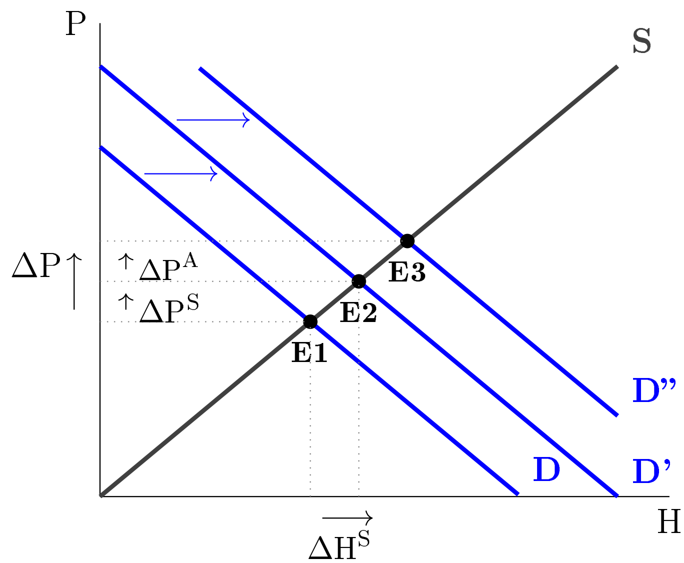
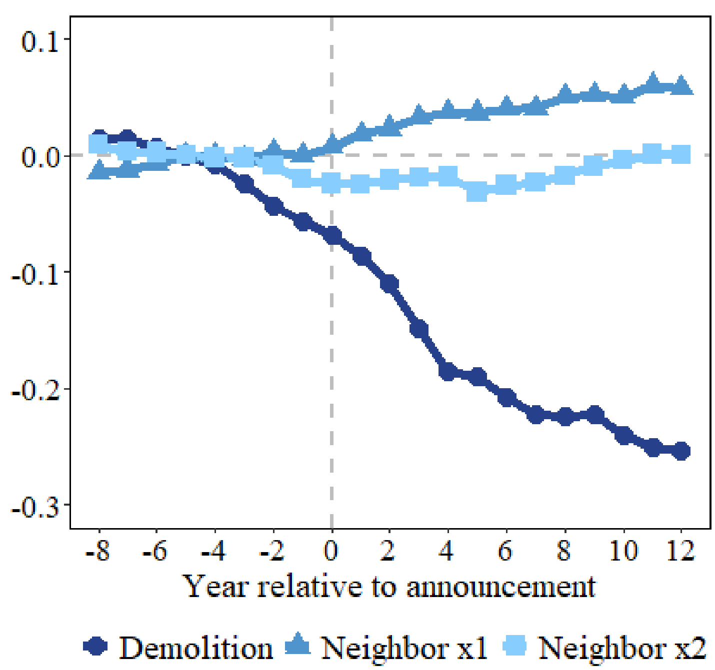


Figure 1-5: PSCM: Effects of demolitions on (Infutor) population count



Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the log of the census tract population as observed in Infutor as an outcome variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Full sample". The x-axis indicates the year relative to the first demolition announcement.

Tables

Table 1.1: Descriptive statistics by treatment group

	Demolition		**Neighbor×1**		**Neighbor×2**		**Other**
	Full	Analysis	Full	Analysis	Full	Analysis	Full
Panel A: Census characteristics 1990							
Population	2,500	2,531	1,527	1,811	2,359	2,607	3,887
Female (%)	0.56	0.54	0.50	0.51	0.50	0.51	0.52
Black (%)	0.91	0.88	0.55	0.52	0.34	0.33	0.35
Population under 18 (%)	0.37	0.30	0.24	0.24	0.24	0.25	0.24
Population over 65 (%)	0.10	0.15	0.12	0.13	0.11	0.11	0.12
Education: no diploma	0.55	0.54	0.41	0.45	0.42	0.42	0.35
Education: high school	0.24	0.24	0.22	0.22	0.20	0.21	0.26
Median household income	8,825	9,919	20,237	20,559	23,669	24,753	27,695
Public assistance (%)	0.52	0.40	0.24	0.24	0.18	0.17	0.14
Below poverty line (%)	0.65	0.54	0.34	0.30	0.26	0.27	0.18
Occupancy rate	0.78	0.79	0.84	0.84	0.88	0.88	0.92
Renter households (%)	0.71	0.68	0.62	0.60	0.59	0.57	0.47
Median rent	179	206	347	334	362	365	387
Distance to CBD (mi)	3.71	4.05	3.64	3.86	4.14	4.40	7.82
Panel B: House sales in 1994							
Sale price	93,435	82,597	112,898	116,323	114,196	112,406	115,596
Number of sales	5	8	13	17	27	29	47
Lot size sq. ft.	4.37	4.70	4.29	4.03	3.65	3.65	4.20
Condo (%)	0.03	0.04	0.12	0.11	0.18	0.18	0.12
Single-family (%)	0.19	0.21	0.20	0.25	0.29	0.29	0.54
Multifamily/Apartment (%)	0.34	0.28	0.35	0.37	0.38	0.38	0.30
Year built	1915	1921	1919	1918	1919	1919	1927
Panel C: Housing units							
Public housing demolished units	517	390	0	0	0	0	0
Total housing units in 1990	1091	1165	718	848	1055	1155	1539
Number of census tracts							
Sample	43	21	119	86	112	100	637
Restricted sample		20		69		94	

Note: This table reports some descriptive statistics of census tracts by treatment group and sample. The table excludes the census tracts (and neighboring rings) of the Altgeld-Murray development.
The "Full" sample column includes all census tract within the treatment group. The "Analysis" only includes tracts for which the last two pre-treatment periods have a positive number of sales taking place. At the end of these last columns, I also report the number of census tracts in a "Restricted" sample, that only includes tracts that have a yearly average of 4 or more sales during the four years previous to treatment.

Table 1.2: Price effects and permutation p-values by period

	Demolition		Neighbor ×1		Neighbor ×2		Neighbor ×3
	Analysis	Restricted	Analysis	Restricted	Analysis	Restricted	Analysis
Yrs. -5 to -3							
Price change	-0.04	-0.02	-0.00	-0.01	0.00	-0.00	-0.00
p-value	0.008	0.340	0.991	0.314	0.916	0.678	0.804
Yr. -1							
Price change	-0.11	-0.09	-0.04	-0.05	0.05	0.05	-0.03
p-value	0.052	0.060	0.063	0.077	0.047	0.024	0.173
Yr. 0							
Price change	0.02	0.08	0.05	0.06	0.01	0.01	0.00
p-value	0.644	0.102	0.029	0.028	0.586	0.453	0.989
Yrs. 1 to 5							
Price change	0.07	0.15	0.14	0.14	0.06	0.06	-0.02
p-value	0.004	0.001	0.001	0.001	0.002	0.001	0.311
Yrs. 6 to 10							
Price change	0.34	0.45	0.18	0.17	0.10	0.10	0.05
p-value	0.001	0.001	0.001	0.001	0.001	0.001	0.015
λ	0.01	0.01	0.03	0.01	0.01	0.01	0.03
Number of tracts	21	20	86	69	100	94	90

Note: The table reports the ATET on house prices in different periods by treatment group using PSCM. Instead of reporting τ_t as described in Eq. (1.3), we compute: $\tau_p = (1/\sum_{i=1}^{n_1} H_i^{1990}) \sum_{i=1}^{n_1} H_i^{1990} \times \tau_{ip}$ where p denotes both a period and the set of years included in that period, so $\tau_{ip} = (1/|p|) \sum_{t \in p} \tau_{it}$.
The first column of each treatment group uses the "Analysis sample" of treated tracts, while the second restricts the sample to those with an average of at least 4 sales per year in the pre-treatment period ("Restricted sample").

Table 1.3: Effects on long-run census tract characteristics (using 1990 as baseline)

	2000				2010			
	(1) Rent	(2) Units	(3) Income	(4) Black	(5) Rent	(6) Units	(7) Income	(8) Black
Panel A: OLS								
Demolition	0.066	-0.098	0.012	-0.042**	0.316**	-0.240**	0.239**	-0.096***
	(0.058)	(0.069)	(0.084)	(0.019)	(0.131)	(0.102)	(0.120)	(0.036)
Neighbor×1	0.001	0.018	0.015	0.001	0.042	0.041	0.044	0.001
	(0.026)	(0.032)	(0.040)	(0.011)	(0.036)	(0.053)	(0.054)	(0.013)
Neighbor×2	0.019	-0.036**	-0.060*	-0.000	0.046*	-0.036	-0.066	0.002
	(0.018)	(0.017)	(0.034)	(0.005)	(0.027)	(0.028)	(0.046)	(0.008)
Panel B: PSCM								
Demolition	0.069	-0.139	0.359	-0.032	0.370	-0.353	0.579	-0.153
	[0.009]	[0.001]	[0.001]	[0.116]	[0.001]	[0.001]	[0.001]	[0.001]
Neighbor×1	0.067	-0.019	0.174	-0.011	0.150	0.025	0.303	-0.059
	[0.001]	[0.049]	[0.001]	[0.052]	[0.001]	[0.785]	[0.001]	[0.001]
Neighbor×2	0.054	0.019	-0.057	-0.009	0.088	0.067	0.043	-0.032
	[0.001]	[0.819]	[0.017]	[0.463]	[0.001]	[0.014]	[0.393]	[0.010]

Note: The table reports the ATET on log rents, log housing units, log median household income, and black population share, in 2000 and 2010 by treatment group. Panel A regresses the change in the outcome variable between 1990 and the corresponding period (2000 or 2010) on dummy variables indicating the treatment group (Demolition, Neighbor×1, Neighbor×2) using Neighbor×3 tracts as the omitted group (i.e., tracts surrounding the Neighbor×2 ring). I include the number of housing units, black share, education levels, median income, poverty rates, occupied housing share, and renter households share in 1990 as control variables. Panel B uses PSCM and reports τ_t as described in Eq. (1.3). I use the outcome variable in 1990, in addition to the census tract characteristics mentioned in Section 1.3.3, as matching variables. For this exercise, I matched tracts in 1990 and 2000 to tracts in 2010 using the crosswalks in NHGIS.

Table 1.4: Estimates of the implied public supply effect by housing market

	Price effect	**Public supply effect**				$\frac{\mathbf{\Delta ln\ P^S}}{\mathbf{\Delta ln\ P}}$	
	Δln P	**Δln P^S**				**Pct (%)**	
	Period: 5-10 y.	$\varepsilon^{\text{Tract}}$ ΔH^L	$\varepsilon^{\text{Tract}}$ ΔH^U	$\varepsilon^{\text{Saiz}}$ ΔH^L	$\varepsilon^{\text{Saiz}}$ ΔH^U	Min	Max
Proximity-based							
Demolition + Neighbor×1	0.23	0.26	0.40	0.10	0.15	43	178
Demolition + Neighbor×1 + Neighbor×2	0.16	0.12	0.20	0.05	0.08	30	122
Migration-based							
Demolition + Neighbor×1	0.10	0.06	0.09	0.02	0.03	19	85
Demolition + Neighbor×1 + Neighbor×2	0.08	0.05	0.06	0.01	0.02	18	78
All Chicago	0.03			0.01	0.01	30	48

Note: Column (1) reports the average reduced-form price effect in years 5 to 10 after the demolition announcement, weighted by the number of private housing units in each tract, by housing market definition. For Demolition, Neighbor×1 and Neighbor×2 tracts, I assign the aggregate estimates of the corresponding treatment group from the previous section. For untreated tracts, I set it to 0.

Columns (2)-(5) present estimates of the public supply effect using the lower (LB) and upper (UB) bounds of $\Delta \ln \mathrm{H}^{\mathrm{S}}$ for two values of the housing supply elasticity. ε^{Tract} columns use the tract-level housing supply elasticity in Baum-Snow and Han (2020) and ε^{Saiz} columns use the metropolitan area level estimate for Chicago in Saiz (2010), 0.8. In these columns, I use the sum of the number of private housing units in 1990 in the tracts included in the housing market definition as the base level of housing units to compute $\Delta \ln \mathrm{H}^{\mathrm{S}}$ in Eq. (1.4). Columns (6) and (7) report the lower and upper bounds as the percentage of the price effect in Column (1) explained by the public supply effects estimated across Columns (2)-(5).

Chapter 2

The Local Impact of Regenerating Public Housing into Mixed-Income Communities

joint with Lorenzo Neri

2.1 Introduction

Over the past few decades, traditional public housing developments in countries like the United States and the United Kingdom have been demolished and replaced with mixed-income housing, i.e. a combination of affordable and market-rate units in the same building.[1] Public housing high-rises generated negative effects on nearby houses such as poverty clusters, high crime rates and low housing values. While prior research has examined the effects of public housing demolitions on affected neighborhoods (Aliprantis and Hartley, 2015; Sandler, 2016; Tach and Emory, 2017; Blanco, 2021), there has been little focus on their redevelopment as mixed-income housing. In this case, the effects on local housing markets are ambiguous: while an increase in market-

[1] Vale and Freemark (2012), Goetz (2012) and Fraser et al. (2013) provide a detailed description of this policy shift. Since the 1990s, the public housing stock in the United States has been reduced by about 300,000 units, while affordable units in subsidized private mixed-income developments have increased by 1.7 million units via the Low-Income Housing Tax Credit (LIHTC) program.

rate supply on-site can decrease local housing prices, demand effects from improved amenities can raise them. Moreover, concerns about gentrification and displacement of local residents are especially relevant in such low-income neighborhoods.[2]

In this paper, we study the effects of regenerating decaying public housing into mixed-income developments on local housing markets. We exploit the demolition and redevelopment of over 130 public housing *estates* (akin to US projects) between 2004 and 2018 in London, UK that approximately maintain the amount of public housing and add a similar number of market-rate units on-site. This context is particularly well-suited to answer two questions. First, can mixed-income housing prevent traditional public housing's negative effects even when preserving the number of public units? Second, what are the local effects of increasing market-rate supply in low-income areas?

Despite the fact that almost a fourth of households live in one of its units, London's public housing has faced similar challenges to those of distressed projects in the United States. We focus on a set of large public housing developments that were mostly built between 1950 and 1980, when the public sector supplied a significant amount of new affordable housing. From the 1980s, gradual disinvestment and poor maintenance led to the decay of many of these buildings –referred to as "sink estates" (Slater, 2018; Lees and White, 2020). Hence, public authorities started a wave of regeneration programs with the involvement of private developers, which led to the creation of mixed-income housing through the sale of additional units in the new buildings on the private market. In our sample, regenerations entail a large housing supply shock: about 31,000 public housing units are regenerated into 60,000 units, of which 34,000 are market-rate.

To study the impact of regenerations on the local housing market, we leverage a particularly rich set of data. We gather address-level data on the universe of real estate transactions, as well as rental listings from the UK's leading company in the online rental sector. The listings data is fairly representative of the distribution of

[2]Guerrieri et al. (2013) suggest that housing demand shocks such as urban revitalization programs can increase local housing prices in low-income areas by attracting high-income households.

private rents in London, in contrast to sources used in prior research that tend to overrepresent the high-end of the market or limit the study sample to apartment buildings.[3] Moreover, we construct a novel dataset by scraping online listings' descriptions in order to measure housing quality changes around regeneration.

Our main empirical strategy consists of a difference-in-differences design that defines the comparison group using variation in proximity to the estates. We assume that distance to the buildings determines treatment intensity. Hence, we compare housing units within an inner ring of a certain radius around an estate to units within an outer ring surrounding that inner ring, which serve as the comparison group. Intuitively, the only difference between units in the inner and the outer rings after controlling for observables is distance to the estate, since they belong to the same neighborhood. And, because proximity determines treatment, sufficiently far away units (i.e., in the outer ring) should not be treated. Reassuringly, an alternative specification that uses units in the inner ring of estates being regenerated later in the period as the comparison group yields remarkably similar estimates, which suggests that spillovers on the outer ring are negligible.

Following this method, we estimate that public housing regenerations significantly raise nearby house prices and rents, but moderately decrease house prices in the broader area. Over a six-year period, we find that house prices rise up to 6% within 100m of a regenerated estate and drop by 2-3% for housing units within 300-600m. Rents increase by 8% within 100m and up to 2% as far as 400m away from regenerated sites –we do not observe rent reductions at any distance. Moreover, the number of sales and listings of old units, as well as new market-rate construction, increases very close to the buildings, pointing to the growing attractiveness of these areas. Interestingly, we also find that landlords in the broader area upgrade the quality of their rental units to compete with market-rate housing in the new buildings, e.g., through refurbishment and renovation. Overall, these findings are consistent with strong demand effects very close to the buildings –leading to observed price increases–

[3]E.g., Asquith et al. (2021) find that Zillow rents are a 20% higher than estimates from the American Community Survey for low-income areas.

, and moderate supply effects that dominate farther away only in the sales market –explaining the negative result.

To explore the mechanisms behind these results, we collect additional data on neighborhood characteristics and amenities. For the former, we use data on primary school-age children's subsidized lunch eligibility to track the socioeconomic status of regenerated neighborhoods. For the latter, we use the listings' descriptions dataset to study changes in advertised local amenities, such as green spaces and businesses; as well as administrative data on crime.

Using the same empirical strategy, we document strong demand effects that support nearby price increases: new mixed-income housing significantly changes neighborhood socioeconomic composition and improves local amenities. Firstly, regenerations attract higher-income households. The number of children not eligible for subsidized lunch increases by 12.5% within 200m after the regenerations, while the number of eligible children does not change –we cannot reject the null of no displacement effects for nearby low-income households in the medium-run. Regarding local amenities, we estimate that the probability of a listing advertising general amenities, cafés, restaurants and green spaces significantly jumps for nearby rental units around the time of regeneration. Improvements in local amenities are also supported by sizeable crime reductions within 200m. Taken together, these results indicate that mixed-income housing can overcome the negative effects of traditional public housing on nearby areas while preserving the public housing stock.

Lastly, we provide suggestive evidence that neighborhoods undergoing large changes in their socioeconomic composition after regenerations experience larger price increases. To show this, we study the heterogeneity of our price results in two ways. First, we estimate that low-income neighborhoods account for most rent increases. Second, we find that housing price increases are considerably higher for regenerations that build relatively more market-rate housing. Since the new buildings attract richer households, these results suggest that regenerations are more likely to have the highest impact on prices in areas that considerably increase their ratio of high- to low-income households. These findings are consistent with high levels of new market-rate supply making neighborhoods less affordable by changing their socioeconomic composition.

This paper is related to several strands of literature. First, we contribute to the literature on the impact of public housing on surrounding neighborhoods. Prior research focuses on the demolition of US' most problematic projects, which resulted in a large, negative supply shock and its partial replacement with mixed-income housing through private subsidies. Demolitions led to large house prices increases (9-20%) in nearby areas (Brown, 2009; Zielenbach and Voith, 2010; Blanco, 2021), as well as sizeable crime rate decreases (Aliprantis and Hartley, 2015; Sandler, 2016) and changes neighborhood socioeconomic composition (Tach and Emory, 2017). In addition, Koster and van Ommeren (2019) estimate a mild positive reaction of house prices to a public housing quality improvement in the Netherlands. This paper examines a new setting in which redevelopment resulted in mixed-income comminities by preserving the amount of public housing and expanding market-rate housing supply. Thus, the housing price increases in this paper, which are smaller than those for US demolitions, are likely mitigated by increased supply.

Second, this paper builds on the literature studying the provision of affordable housing through mixed-income developments. Previous work focuses on the Low-Income Housing Tax Credit program (LIHTC) in the US, which subsidizes affordable housing in new market-rate buildings. Diamond and McQuade (2019) show that LIHTC buildings have heterogeneous price effects that depend on neighborhood composition –poorer areas experience price increases–, while Sinai and Waldfogel (2005) and Eriksen and Rosenthal (2010) find large crowd-out effects of LIHTC on new market-rate supply. In our context, we study mixed-income buildings as an alternative form of supplying public housing that may alleviate its negative effects nearby.

Third, a growing body of work has recently examined the local effects of new market-rate buildings on housing prices. While most studies point to a reduction in nearby rents (Li, 2019; Asquith et al., 2021; Pennington, 2021), others find significant rent increases near new construction (Singh, 2020). Damiano and Frenier (2020) suggests that low-end units experience rent increases, while high-end units bear rent decreases. We contribute by examining how supplying market-rate units through public housing affects the local housing market in deprived areas. A potential reason for our contrasting positive price effects is that we focus on previously decaying public housing –where amenity gains are probably much larger.

Finally, our paper is also related to the literature on the consequences of public housing demolitions on displaced and local residents. The literature on demolitions in the US shows that, although displaced children do no better in the short-run, they improve their labor market outcomes in the long-run after moving to less disadvantaged neighborhoods (Jacob, 2004; Chyn, 2018; Haltiwanger et al., 2020). In the case of London's regenerations, Neri (2020) shows that children staying in the neighborhood improved their academic performance, potentially by increased exposure to a more income-diverse population. In this paper, we seek to understand how public housing regenerations change neighborhood outcomes, which may lead to increased opportunities for nearby residents.

2.2 Background

Although public housing had been an important source of new affordable housing in London, gradual disinvestment led to the decay of most of its traditional developments (known as council estates; henceforth, *estates*) by the 2000s. To address this, local authorities started a wave of regenerations that resulted in new mixed-income housing by rebuilding existing public housing units and constructing additional market-rate units on-site. How these regenerations impact local housing markets is ambiguous: they increase nearby housing demand by making the area more appealing, which raises prices, but they also expand housing supply, driving prices down.

2.2.1 An Overview of Public Housing in London

Public housing is more common in London and the United Kingdom, more generally, than in other developed countries.[4] In 2011, there were about 786,000 public housing

[4] According to the OECD, 17% of UK dwellings were social rental dwellings in 2020, compared to the 7% OECD average. The share was only higher for the Netherlands, Austria and Denmark.

units in London providing affordable housing to about 24% of the 3.2 million households living in the city. Such units are subject to a range of rent levels, yet all of them are below the market price.[5] In contrast, the population share living in public housing is much lower for comparable metropolitan areas in the United States, a country that has faced similar challenges regarding large public housing developments. Some examples include New York (2.2%), Chicago (0.5%) or Atlanta (0.4%).[6]

The management of public housing is decentralized to the 33 Local Authorities in London (LAs, i.e., boroughs) and housing associations (HAs).[7] HAs are non-profit organizations, regulated and funded by the government, that cooperate with LAs in providing affordable housing –as of 2011, 45% of public housing units were managed by HAs. However, LAs set eligibility requirements for all public housing.[8] Once an individual meets the eligibility criteria, they join a waiting list and can apply for housing as properties become available. Priority is given to households with medical or welfare needs, those living in unsatisfactory conditions (e.g., overcrowding), and the homeless.

In this paper, we focus on a subset of public housing estates that had entered into a state of decay by the 2000s after gradual disinvestment by public authorities. These estates were mostly built between the 1950s and the 1980s, a period when LAs accounted for almost half of the yearly production of new housing units in England.

Social housing is defined as the rental housing stock provided at sub-market prices and allocated according to specific rules rather than market mechanisms.

[5]There are three main rent levels associated to public housing in London. The most common category is *social rent*, with a median rent of around 35% of that in the private market (Trust for London, 2020). The second category is *intermediate rent*, which includes rentals and shared-ownership housing that targets lower middle-and middle-income households. Lastly, *affordable rent* was introduced in 2011 with rents up to 80% of those in the private market. This last category accounted for a very small fraction of units in the regenerated buildings that are the focus of this paper.

[6]Based on the authors' calculations. London's number comes from the percentage of housing units classified as social housing according to the 2011 Census. For the US, public housing population was obtained from the Picture of Subsidized Households of the Department of Housing and Urban Development and total population from the Census.

[7]LAs – or 'local councils' – represent one of the local government units in England and are responsible for a range of services, such as education and housing. In London, there are 32 Local Authorities plus the City of London.

[8]Generally, any adult individual who has low income, has recognized housing needs, has lived for a certain number of years in the LA, and hasn't displayed situations of anti-social behavior or rent arrears, can apply for public housing.

By the early 1990s, however, this figure had dropped to below 1% –most new public housing production had been undertaken by HAs, accounting for 20% of total new construction.[9] The process of disinvestment in public housing started in 1980, when the government introduced the possibility for public housing tenants to buy their unit at a highly discounted price, i.e., the so-called *Right-to-buy* scheme (RTB). The RTB scheme considerably reduced the housing stock publicly maintained by LAs.[10] Furthermore, the government continued the cutback on public housing with the 1986 Housing and Planning Act, which allowed LAs to transfer the management of all their public housing stock to HAs. By the turn of the millenium, the ongoing decay –and a mounting need to increase housing density in major urban centres– fostered a large wave of public housing estate regenerations.

2.2.2 Public Housing Regenerations: towards Mixed-Income Housing

In response to the poor condition of public housing estates, LAs/HAs started a process of demolition and redevelopment ("regeneration") in the early 2000s. Before this, the word "estate" carried stigma: the press related it to crime, neglect and poverty – similarly to US projects.[11] Given the lack of investment from public authorities, some researchers even referred to the poor housing conditions and the estates' general air of disrepair as "managed decline" (Watt, 2009, 2013). Regeneration programs are seen, in the words of the Mayor of London, "as an opportunity to revitalize local communities rather than to move their residents away".[12] In fact, estates should be

[9]Ministry of Housing, Communities and Local Government, "Live tables on house building", Table 244.

[10]The *Right-to-buy* scheme helps public housing tenants to buy their home by benefiting from a consistent discount. House and flat tenants can benefit from a 35% and 50% discount, respectively after they have been public sector tenants for three years. After 5 years the discount increases by 1% and 2%, respectively, up to a maximum of 70%.

[11]Some examples include: "The word 'estate' has become synonymous with the term 'ghetto'. It's become a dirty word. Back in the '20s and '30s it didn't carry the same stigma", "The Aylesbury estate became journalistic shorthand for inner-city crime, squalor and deprivation, with the Daily Mail describing a walk around its precincts as 'like visiting hell's waiting room"', "The estate has been neglected for years". *Sources*: BBC (2012), The Guardian (2010).

[12]Mayor of London, *"Better homes for local people. The mayor's good practice guide to estate regeneration"*, 2018.

prioritized for regeneration based on their level of unfitness, i.e., poor design and physical conditions.

As a result, many public housing estates have been redeveloped as mixed-income communities, i.e., a combination of public and market-rate units in the new building.[13] Due to the lack of funding of LAs/HAs, regenerations are often carried out with the involvement of private developers. Hence, regeneration programs not only tend to preserve the amount of public housing originally present in the estate but also facilitate the sale of a substantial number of market-rate units in the new buildings. The details of these partnerships with private developers vary from regeneration to regeneration.[14] LAs/HAs retain the ownership of public housing units in new mixed-income buildings, although the management of these units is often transferred to a private entity. Note that the involvement of the private sector also implies that, in practice, LAs/HAs may have the incentive to prioritize estates in more "profitable" neighborhoods. In fact, public authorities are often accused of accelerating gentrification and displacing low-income households from the center of London via estate regeneration (Lees and White, 2020).[15] Despite our focus on mixed-income regenerations, a subset of small estates have been regenerated as public housing only.[16]

Regenerations take several years to complete and displaced households are gen-

[13] Appendix Fig. F.1 provides an example of a regeneration program in West London. In some instances, large regeneration programs can include the provision of new amenities for the area, such as new parks or playgrounds.

[14] The Myatts Field North estate is an example of this: "the local authority signs a contract with a private developer, which provides the upfront capital financing and subsequent management of the asset. The public sector repays the developer in monthly installments and, in residential developments, often with land and permission for private dwellings alongside the revamped social housing". *Source*: The Guardian (2017).

[15] See, e.g., "Regeneration –or pushing out the poor? Labour divides in bitter housing battle", The Guardian (2017); "The real cost of regeneration", The Guardian (2017)

[16] We focus on mixed-income housing for two reasons. First, mixed-income housing is especially policy relevant: policymakers argue that it can solve problems associated with traditional public housing. Second, estates regenerated fully as public housing are significantly smaller than those converted into mixed-income housing (77 and 248 units on average, respectively), which has several implications. One is that estates rebuilt as public housing only cannot serve as a counterfactual for mixed-income regenerations, since the two distributions of existing units do not overlap sufficiently. Another implication is that we have less statistical power due to their small size. Finally, the fact that mixed-income regenerations are larger indicates that this group reflects better the sample of estates that were in poor conditions and that are the focus of this paper. Appendix B.3.2 reproduces the main analysis for "public housing only" regenerations.

erally relocated nearby, which temporarily increases housing demand nearby. After permission is granted for the regeneration, it takes on average one year to start the regeneration and about four years to complete. During regeneration, tenants were moved to alternative public or private accommodation, located either in the preferred area or one that minimizes disruption to the household's work and schooling circumstances.[17] Due to this provision, public housing tenants tended to be initially rehoused in the surrounding neighborhood.[18] Life-time tenants had the right to be offered a flat in the new premises and homeowners who bought their home through the RTB scheme were offered a price for the flat.

2.2.3 Potential Demand and Supply Effects of Regenerations

Public housing regenerations can affect the local housing market through both demand and supply effects, which push prices in opposite directions. To study these effects, we treat housing units around each estate as a separate neighborhood within the city, following the previous literature (Diamond and McQuade, 2019; Asquith et al., 2021; Pennington, 2021).

On the demand side, nearby housing prices may rise if regenerations increase amenities and attract higher-income neighbors. First, redevelopment replaces run-down housing with new and higher-quality buildings, along with further beautification of the area (newly paved streets, green spaces, etc.). The old buildings' poor conditions likely depressed the values of nearby properties due to an eyesore effect. Second, households living in newly constructed market-rate units are presumably richer. Prior research suggests that households are willing to pay to live near higher-income and more educated neighbors (Bayer et al., 2007; Guerrieri et al., 2013; Diamond, 2016). Furthermore, the deconcentration of poor households in large estates may also bring amenities to the broader neighborhood such as crime reductions and increases in local economic activity, e.g., new businesses. Taken together, demand effects should be

[17] Households who had to move also had priority when bidding for vacancies advertised by the LA.
[18] Based on the authors' calculations, 80% of tenants with children moved within 1km of the regeneration site.

strong very close to the estates and still be present in the broader area, but decaying with distance to the estates.

Tenant relocation is an additional demand margin that plausibly increases local housing prices in the short/medium-run. Most displaced public housing tenants are at least temporarily rehoused within 1km of the estates, a fraction of which relocates to private housing. Hence, the reduction in public housing supply shifts the local private housing demand outwards, which pushes up prices. Rents should especially reflect this increase due to the temporary nature of the shock. This feature of regenerations, i.e., the provision (in this case, temporary reduction) of public housing as a way to affect local housing prices, links to the public finance literature on the pecuniary effects of in-kind transfers (Coate et al., 1994; Blanco, 2021).

On the supply side, estate regenerations shift the private housing supply curve outwards, which puts downward pressure on prices. In a simple supply and demand model, this shift implies that the marginal household's willingness to pay for living in the neighborhood is weakly lower after the regeneration. How the magnitude of the supply effect varies with distance is uncertain. Intuitively, supply effects should be stronger for closer substitutes of newly constructed units. If housing demand is strongly driven by distance to the estate, i.e., households really care about location within the neighborhood, we expect supply effects to be highly concentrated right around the estate. If housing demand reflects preference for the neighborhood more generally, as opposed to others, supply effects should persist also for units farther away from the estate.

The net price effect is therefore *ex ante* ambiguous, and an empircal question. It also likely varies with distance to the regeneration site, since the relative impact of demand-side and supply-side factors may vary with distance. If demand effects are strong relative to supply effects, regenerations may result in nearby price increases. Singh (2020) finds rent increases within 150m of new market-rate housing in NYC. On the contrary, if supply effects are stronger, we can expect lower sale and rental prices in the neighborhood. Li (2019), Asquith et al. (2021) and Pennington (2021) estimate rent decreases up to a distance from market-rate construction going from

0.15 to 1.5km for several US metropolitan areas. A third option is that the two effects dominate at different distances. For instance, highly localized demand-side factors can lead to price increases near the building but price reductions in the broader area as the relative importance of supply effects dominates.

2.3 Empirical Strategy

We estimate the effects of public housing regenerations on nearby housing units using a difference-in-differences design that compares units near regenerated estates to those located farther away. To do this, we gather a rich set of data on regenerations, house sales, rental listings, and local amenities.

2.3.1 Data

We identify public housing regenerations from a dataset containing the universe of planning applications in London. To explore the effects on the local housing market, we collect data on real estate transactions, rental listings, and new construction. Importantly, we build a novel dataset containing information on the quality of rental units and nearby local amenities by scraping rental listings' descriptions. Lastly, we further study neighborhood change using data on primary school-age children to track the socioeconomic status of nearby households, as well as data on crime.

Estate regenerations. We identify all estate regenerations in London between 2004 and 2018 using administrative records from the London Development Database (LDD). The LDD contains all housing planning applications filed to the planning authorities –represented by the 33 LAs– either approved or completed since 2004. Each application contains information on the permission, start and completion dates, exact location, the number of existing/proposed units by type (i.e., public or market-rate), and the provider of existing/proposed units (LA, HA or private entity). We identify buildings belonging to a estate regeneration as applications where the existing building contains public housing units whose provider was either a LA or a HA. There are 432 such buildings.

Given that buildings belonging to the same estate may be filed under different applications, we group them as follows. Buildings are grouped into the same estate regeneration if they share the same estate name in the application, were located within 400m of each other and their planning permission was approved within six years of each other.[19] We drop estate regenerations with less than 10 units in the existing building. This process leaves a sample of 239 estate regenerations.

Finally, we define mixed-income regenerations as estates where the new buildings include a percentage of public housing units of 80% or less. Panel (a) of Fig. F.2 shows that our analysis is not sensitive to this threshold because an overwhelming majority of regenerations above the 80 percent limit are capturing estates regenerated as public housing only. In addition, panel (b) illustrates that the number of regenerations is consistently spread throughout the sample period. The final sample consists of 135 regenerations.

House sales and rental listings. To measure house prices, we use administrative records from the UK Land Registry on all residential sales between 1998 to 2019. Every transaction records the date, price paid, unit type (detached, semi-detached, terraced, flats/maisonettes), age (newly built or established residential property), contract type (leasehold or freehold) and address.[20]

We complement house price data with the universe of rental listings posted between 2006 and 2019 on the website Rightmove, leader in the sector of online rental listings.[21] Every listing reports the date, rent, status (available or let agreed), house type, number of bedrooms, address and website link.[22] The dataset is fairly representative of rent levels in London: the correlation at the LA level between Rightmove

[19]The six-year limit is guided by the time window used in our empirical strategy.

[20]We geolocate houses using the latitude and longitude coordinates of the postcode. Postcodes in London are small and usually map into single buildings.

[21]As of 2021, Rightmove receives 127.5 million visits per month, while this figure stands at 50 million for Zoopla, the second leader company in the online rental sector. Source: Homeowners Alliance

[22]In the analysis, we do not include other house characteristics also present in the dataset such as floor area, number of bathrooms, and construction year, because they are missing for about two-thirds of the sample.

rents and official estimates is 0.99. While Rightmove rents are on average 10% higher, part of this is explained because Rightmove mostly captures asking rents, as opposed to agreed rents (only 24% of the sample). In our sample, agreed rents are 5-10% lower than asking rents, which explains most of the gap. Appendix B.3.1.1 provides more details.

To characterize rental listings, we construct a novel dataset by scraping the ad description in the listings' websites. In the description, agents usually advertise not only details about the unit but also about the neighborhood.[23] We use descriptions to generate dummy variables indicating the presence of certain keywords that refer to characteristics of the unit (refurbished, luxury, washing machine), the building (garden, gym, concierge), and the neighborhood (amenities, cafés, restaurants, parks).[24] This dataset allows us to proxy for rental housing quality changes in response to regenerations, as well as changes in advertised amenities.

Neighborhood composition and amenities. To measure changes in local demographics, we obtained administrative records from the National Pupil Database (NPD) on primary school-age students in England from 2002 to 2016 (approximately 600k per year). We use subsidized lunch eligibility to track the socioeconomic status of households at the block group level –children are linked to regenerations using their block group of residence. Regarding amenities, we use the listings' descriptions dataset above to study effects on new businesses and green spaces. We also employ crime data at the block group level from 2008 to 2018, which is publicly available from the London Metropolitan Police website and records the number of crime offenses broken down by category (e.g., burglary, theft, violence against the person).

Geography and others. The UK geography is defined by blocks (Output Areas, OAs), block groups (Lower Layer Super Output Areas, LSOAs) and census tracts (Medium Layer Super Output Areas, MSOAs). These are geographical units created by the Office for National Statistics (ONS) for Census reporting purposes and contain

[23] Appendix Fig. F.26 shows an example of such a listing.

[24] See Appendix B.3.1.2 for a more detailed description of the construction of this dataset.

an average of 130, 672 and 3,245 households in London, respectively. To construct statistics of the local areas that are targeted for a regeneration we use census data at the block level from the 2001 and 2011 UK censuses. Block-level statistics include detailed information on the population's socioeconomic and housing characteristics.

2.3.2 Summary Statistics

Estate regenerations almost double the total number of units in the new buildings while maintaining the amount of public housing. Columns 2-3 of Table 2.1 report the average number of units before and after regeneration by type for the full sample of regenerations and a balanced sample of 70 regenerations approved between 2004 and 2012 that we use in our main specification –both samples are similar on observables. Panel A illustrates that, on average, redeveloped buildings preserve the amount of public housing (206 units before, 197 after) and build around twice as much market-rate housing (218 units). Panel B shows that the change in market-rate units induced by regenerations is a big shock to the nearby area: it is equivalent to 41% of total housing units within 200m of the estates in 2001, and up to 3% of units within 800m. Finally, note that the average existing building contains about 17% of non-public housing units: some public housing tenants had bought their unit at a very discounted price through the RTB scheme.

Estate regenerations are also located in areas with lower socioeconomic status than the average London neighborhood (panel C). While Column 1 of Table 2.1 shows neighborhood characteristics for the average census block in London, Columns 2 and 3 do it for the full and the balanced sample of regenerations. For this table, we define a neighborhood as blocks within 800m of the reference block –consistent with our empirical strategy below. Estate regenerations were in poorer and less educated neighborhoods than the average London neighborhood, as well as in areas with more public housing and similar housing prices. The last fact can be explained by their location: Fig. 2-1 shows that, although regenerations were spread throughout the city, more mixed-income regenerations take place in Inner London, where housing prices are higher.

2.3.3 Empirical Specification: Using Variation in Proximity

The main empirical challenge is the selection of a plausible comparison group that describes the counterfactual trajectory of housing prices and other neighborhood outcomes in the absence of exposure to regenerations. An ideal experiment would compare housing units near estates randomly assigned to regeneration to those near similar estates not assigned to regeneration. Unfortunately, this experiment cannot be approximated because comprehensive data on non-regenerated estates is not available. Using data on regenerated estates only, we need to address the concern that regenerated areas are endogenous, e.g., private developers might decide to partner up to regenerate estates only in the most profitable areas.

To overcome this issue, we use a difference-in-differences design that uses *variation in proximity* to the estates to define the comparison group. This approach assumes that proximity determines treatment intensity, as argued in Section 2.2.3. We compare housing units in an inner ring of a certain radius around a regenerated estate to units in an outer ring surrounding that inner ring, which serve as a comparison group. The identifying assumption is that, in the absence of the regeneration, the outcome of interest would have changed in parallel in both rings. Intuitively, the only difference between units in the inner and outer rings after controlling for observables is distance to the estate, since they belong to the same neighborhood. And, because proximity determines treatment intensity, sufficiently far away units (i.e., in the outer ring) should not be treated.

We implement this strategy as follows. For each regenerated estate, we keep sales and listings of housing units within 1km.[25] We exclude housing units in newly regenerated buildings from the regressions, since our main goal is to study the effects on nearby houses.[26] Next, we construct an event year variable with respect to the

[25]For house sales and rental listings, we only include arms-length transactions and avoid outliers. We do so by dropping the top and bottom 0.5% sale/rental price transactions each year. This gets rid of a number of outliers and drops observations with zero or extremely low sale/rental price.

[26]When house prices are the dependent variable, we exclude sales of new houses occurring in the regenerated block after permission. In the case of rents, we exclude all listings in the regenerated block after permission because we do not have a perfect proxy to determine whether a house has been newly constructed.

permission year of the associated estate and restrict the sample to observations within 6 event years. Finally, we append all datasets. Note that some units may appear several times for different estates due to the overlapping of rings of different estates –Section 2.4.4 presents robustness checks where results hold even when dropping duplicated observations.

We start by interacting event year dummies from/to the regeneration event with multiple 200m rings up to 800m indicating the distance of each housing unit to the associated estate (treated rings). Housing units located between 800m and 1,000m are the omitted group (comparison ring). We estimate the following event study equation at the house h, estate e and year t level:

$$\mathrm{Y}_{het} = \alpha_{et} + \kappa_{e,r(h,e),g(h)} + \sum_{\tau=-6}^{6} \sum_{r \in R} \beta_{\tau,r} \; \mathbb{1}(t - E_e = \tau, r(h,e) = r) + \gamma' \mathbf{X_{ht}} + \epsilon_{het} \tag{2.1}$$

$\beta_{\tau,r}$ is the effect of interest, i.e., the evolution of housing prices over time in each treated ring with respect to the most outer ring, set to 800-1,000m. The indicator variable in the summatory interacts event years τ with dummy variables indicating the ring $r(h,e)$ in which housing unit h is located with respect to estate e. E_e denotes the year when the permission was approved for estate e, while the set of included rings r is defined as $R = \{\text{0-200m}, \text{200-400m}, \text{400-600m}, \text{600-800m}\}$. We weight each estate-year equally and, within each estate-year, we weight every block equally.[27] We cluster standard errors at the estate level.

We control for neighborhood time patterns and baseline levels using a rich set of fixed effects. Estate-calendar year FE (α_{et}) flexibly account for time patterns

[27]The first choice accounts for the fact that there are more sales and listings around estates in denser areas and, without weights, these estates would have higher weights than estates in less dense areas. The second choice addresses the fact that the number of sales and listings varies across years. Thus, we also need to weight each block equally to guarantee that $\beta_{\tau,r}$ reports the same weighted average for each ring across event years. In the absence of such weighting, estates with more sales or listings in event year τ for ring r relative to the comparison ring would contribute to $\beta_{\tau,r}$ with a higher weight. Note that this weighting does not matter when the outcome are house sales or rental listings counts per block because we run the regression at the block level, the number of which is constant across years.

across all rings around each estate e, while estate-ring-census tract g FE ($\kappa_{e,r(e,h),g(h)}$) control for baseline differences of units across each ring.[28] This combination of fixed effects ensures that $\beta_{\tau,r}$ captures differences in the evolution of the outcome across rings *within* each estate regeneration. Intuitively, $\beta_{\tau,r}$ is a weighted average of estate-specific treatment effects, i.e., the result of running Eq. (2.1) separately for each estate.

In the case of house prices, we include as control variables $\mathbf{X_{ht}}$ the unit type, tenure type, a dummy indicating whether the unit was newly constructed, month-of-sale dummies, a quadratic term for the average unit area in the postcode, census block characteristics in 2001 (density, number of households, public housing share, owner-occupied housing share), school market characteristics and a quadratic term for distance to the nearest tube station.[29] For rents, we also include the number of bedrooms and the listing status (available or let agreed). When we use outcomes at the block level, such as the number of sales, listings and newly approved units per block, we run the regression at the census block level i.[30]

We also report a pooled version of Eq. (2.1) that collapses post-treatment event year dummies into two periods: 0 to 3 years ($\text{Post}_{et}^{0\text{-}3}$) and 4 to 6 years ($\text{Post}_{et}^{4\text{-}6}$). This distinction reflects the fact that, on average, regenerations take 4 years to complete. Hence, we run the following regression:

$$\text{Y}_{het} = \alpha_{et} + \kappa_{e,r(h,e),g(h)} + \sum_{r\in R} (\theta_{0,r}\text{Post}_{et}^{0\text{-}3} + \theta_{1,r}\text{Post}_{et}^{4\text{-}6}) \times \mathbb{1}(r(h,e) = r) + \gamma'\mathbf{X_{ht}} + \epsilon_{het} \tag{2.2}$$

In our main specification, we restrict the sample to regenerations with a permission

[28]We include the census tract to account for differences across units around different parts of each ring.

[29]For unit area, we use the average unit area of sales in the postcode in 2008-2019 as reported on Energy Performance Certificates (EPC), a document that details the energy performance of a property that was introduced as mandatory for properties built, sold or let after 2008. When using listings data, we use unit area as reported in that dataset. However, we assign the average unit area of the postcode when this variable is missing. For school market characteristics, we include the number of highly and poorly rated schools within the unit's school catchment area.

[30]To this end, we compute the counts of those variables per block for each calendar year. Then, we assign blocks to a ring for each regenerated estate that falls within 1km of i's population-based centroid. Instead of estate-ring-tract FE ($\kappa_{e,r(h,e),g(h)}$), we use estate-block FE ($\kappa_{e,i}$) –the centroid of a block is always located within a single ring.

approved between 2004 and 2012 in order to obtain a balanced sample within 6 years of permission. In the case of rents, we use the period 2007-2012 because rental listings data is only available starting in 2006. Because the sample is unbalanced in relative years -2 and below, we only include rental listings between event years -3 and 6 when estimating the equations above.

Note that we define the year when the planning permission is approved as the treatment period for two reasons. First, house prices are forward-looking: the path of price effects should start at the moment when information about regeneration first arrives. Second, we expect rents to react to the relocation of displaced households in the nearby area and gradual improvements in local amenities (e.g., reduced crime), both of which increase housing demand before the completion of the project. Thus, using completion as the triggering event likely underestimates the impact.[31] Note that this choice is in contrast to prior research using the completion year as the relevant event to study the rent effects of market-rate construction (Asquith et al., 2021; Pennington, 2021).

Finally, a caveat of our empirical strategy is that it ignores general equilibrium effects: regenerations may have an impact on housing prices throughout the city. Regenerations increase the attractiveness of nearby areas relative to the rest of London, which may decrease relative demand for other neighborhoods in the city. In addition, they also increase housing supply in the city: in our sample, regenerations produce about 29,000 new units (0.9% of the number of households living in London in 2011). We argue that city-wide effects should be small and areas in close distance to regenerations concentrate the largest effects. This argument relates to the no price effects assumption in the outermost ring: if such city-wide exist and are significant, our estimates are downward biased but relative comparisons across rings are unaffected.

[31] In fact, when we define the completion year as the treatment period, we find no effects on rents at any distance from the estate. Appendix Fig. F.3 compares event study results when using both permission and completion years.

2.4 The Impact of Regenerations on the Local Housing Market

The regeneration of public housing estates into mixed-income housing significantly raises house prices and rents near regenerations, although house prices slightly decrease in the broader area. We also show that the quantity of sales and listings increases very close to regenerations and that rental unit quality goes up. We provide supportive evidence that our price results are likely not driven by changes in the quality of transacted housing stock.

2.4.1 Effects on Prices: House Prices and Rents

Public housing estate regenerations significantly increase house prices in their immediate surroundings but decrease them slightly farther away. Fig. 2-2 plots the results for the event study specification using the logarithm of the sale prices and rents as dependent variables. Panel (a) shows that housing units within 200m of the estates experience an increase of about 4% in house prices relative to the omitted group (units in the outermost ring at 800m to 1km), a figure that goes down to a zero effect in the second ring and becomes slightly negative within 400-600m. Although this last effect is not statistically significant in the event study specification, we show below that it becomes significant in the pooled DID regression. Price effects return to zero in the last treated ring (600-800m), which is consistent with effects fading out for sufficiently farther away units.

Rents also significantly increase in nearby areas (panel (b)). In contrast to house prices, the positive effect persists in the broader area. We find that housing units within 400m of an estate experienced rent increases of up to 4% when compared to the most outer ring (panel (c)). Rent effects are statistically undistinguishable from zero beyond that distance.

Fig. 2-3 summarizes the results using distance to regenerations as a continuous measure of treatment. The figure estimates Eq. (2.2): using housing units within 800-

1,000m as the comparison group, we pool event years into two "Post" dummies (0-3 and 4-6 years after permission) and interact them with either indicators for 100m rings or a third-order degree polynomial, instead of the 200m ring indicators in the event studies. The reason is that 200m rings do not account for the fact that the number of units within a given ring increases with distance. Hence, housing units closer to the estate within each ring, which are more intensely treated, are underweighted. The main results become starker. Panels (a) and (b) illustrate that, while there is no effect on house prices within 3 years of permission at any distance, house prices rise up to 6% in the long-run only within 100m of the estate. Furthermore, the mild negative effects within 300-600m (2-3%) are statistically significant at the 95% confidence level using both the 100m and polynomial specifications. Rents also go up by about 7% in the long run (bottom panels). In this case, rent increases are still significant up to 400m away from regeneration sites, yet decreasing with distance.

The time pattern of price effects in the event study specifications have three main insights. First, house prices do not fully incorporate all information about regenerations at the moment of announcement. Although house prices are forward-looking, i.e., represent the net present value of future rents, house prices do not jump upon permission approval but steadily increase after that. Some potential explanations are that new information may arrive after permission or that there is uncertainty around regeneration plans. However, the effects seem to be fully realized when the projects are completed (on average, event year 4). Second, house prices slightly go down right around permission, which suggests that homeowners place a large value on temporary disruptions arising from demolition and construction. Third, rents go up for units in the second ring right after the permission is approved. Fig. 2-3 (c) also shows that rents also go up to 2-3% in the short-run within 300m. This is suggestive evidence that displaced tenants relocating in the surrounding area temporarily increase nearby housing demand and, hence, exert an upward pressure on rents.[32]

[32]Temporary relocation is not driving the results in years 4-6 after permission. A concern is that, although regenerations take an average of four years to complete, some regenerations are not completed by that date (sd = 2.5 years). Fig. F.4 shows the result of estimating Eq. (2.2) adding

Overall, results are consistent with strong demand effects very close to regenerated estates and moderate supply effects that dominate farther away in the sales market. Price increases are considerably high within 100m of the regeneration site, likely because housing units within this distance benefit more from highly localized amenities: a higher-quality building replacing an eyesore, street repavement, new businesses, etc. Strikingly, supply effects dominate demand effects in the sales market in farther away distances but not in the rental market, which still shows positive effects.

There are several potential explanations for the contrasting results in the sales and rental markets in the broader area. First, the two markets are pricing different streams of payments. While house prices refer to the discounted value of all future rents, rents represent the one-year spot market. If households expect rents to go up in the short/medium term and then go down, this could explain the difference.[33] Second, there may be market segmentation. Market-rate units in regenerated estates can be closer substitutes to nearby owner-occupied units than to nearby rental units if, for instance, the latter were generally lower-quality at baseline. We explore this hypothesis using data on Energy Performance Certificates (EPC), a document that details the energy performance of a property by gathering data on several unit characteristics. EPCs were mandatory for buildings constructed, sold, or rented after 2008. Appendix Fig. F.5 regresses a dummy variable indicating whether a property is rented on several unit characteristics for the sample of old owner-occupied and rented units within 800m of a regeneration that were assessed in event years -3 to -1 –regenerations approved before 2009 are not included. The figure provides suggestive evidence that rental units were lower-quality at baseline: they had lower energy ratings, less habitable rooms and lower energy efficiency ratings for some physical elements (e.g., walls). Third, regenerations may push out more the demand for rental units near regenerated areas, e.g., if they attract more renters such as college grad-

event years 7-9, when most regenerations are completed. Rent effects for this period are identical to those during event years 4-6.

[33]This would be the case if households expect public housing to generate negative effects in the long-run. For instance, public housing was considered a "reward for good citizenship and focused admission on two-parent households with stable employment" in the United States between the 1930s and the 1950s (Vale and Freemark, 2012). Only after poor maintenance and changes in the sociodemographic composition of its tenants did it fall out of favor.

uates and young professionals. If the costs of converting owner-occupied housing to rental housing are high, the supply of rental units cannot adjust as much. This issue can be exacerbated by the low share of privately rented units near regenerations (Table 2.1). Finally, an alternative explanation is that landlords upgrade rental units to cater to higher-income households coming to the area after the regeneration –Section 2.4.3 examines this question in more detail.[34]

Despite the mixed results at different distances, the long-run aggregate effect of estate regenerations on house prices is slightly negative. The number of units exposed to large house price increases within 100m is presumably much lower than that of units experiencing mild decreases within 300-600m. Appendix Table F.1 weights the price effect by the number of private units at each distance.[35] We find that, on aggregate, price decreases in the broader area more than offset nearby price increases: mean prices went down by 0.7 to 1%. Such percentage change is equivalent to a loss of £360-570 millions in 2001 housing stock value –around £1,430-2,250 per unit. An immediate implication is that supply effects in the broader neighborhood compensate regeneration-induced house price increases in the immediate surroundings of the estate.

We also document substantial heterogeneity across estate regenerations. Appendix Fig. F.6 estimates Eq. (2.2) separately for each regeneration and reports the long-run estimates (4-6 years after permission) for the first three 200m rings. About 25% of regenerations show large increases in house prices and rents in the first ring, while estimates are more evenly distributed and closer to zero in the next two rings. We interpret this as evidence that the trade-off between demand and supply can manifest very differently across regenerations. For instance, demand effects must be very strong for the few regenerations with large housing price increases in the first ring, while the magnitude of supply effects may have lower variance across the estates. Section 2.6 further explores how results vary by the change in a neighborhood's socioeconomic

[34] Data on whether sold units correspond to owner-occupied or rental units would be useful to assess this differential impact, yet it is not available.

[35] For this computation, we weight the long-run point estimates of the 100m-ring DID specification ($\theta_{1,r}$ in Eq. (2.2)) by the number of housing units in each of these rings in 2001.

composition that is induced by regenerations, which can explain an important part of the observed heterogeneity.[36]

Although prior research mainly focuses on housing prices, estate regenerations can also generate endogenous responses in the quantity and quality of transacted housing stock. Examining such responses is important for two reasons. One is that quantity and quality effects also carry information on the effects of regenerations on the surrounding neighborhood. For instance, increases in sales suggest changes in neighborhood composition, while quality changes hint at the characteristics of incoming households. The second reason is that such responses raise the concern that our price estimates are not only capturing the value of living close to a regeneration but also endogenous quality changes. The next two sections further explore these issues.

2.4.2 Effects on Quantities: Sales, Rental Listings and New Construction

We start by documenting that estate regenerations significantly increased the supply of new homeownership and rental units. To show this, we estimate Eq. (2.1) using the inverse hyperbolic sine of the number of sales and rental listings per block as the outcome variable.[37] Fig. 2-4 shows the results by whether the unit is a new build –Table 2.2 also reports the estimates for the pooled DID in Eq. (2.2). 17% and 10% of housing units within 800m of the estates are categorized as new in the sales and listings samples, respectively. First, we focus on new units in regenerated buildings to examine the magnitude of the shock induced by the regeneration and the time

[36] To show this, Fig. F.7 regresses the estimates in Fig. F.6 on several building and neighborhood characteristics. This exercise reveals two main patterns. First, regenerations in high-income areas led to lower price effects within 200-600m, suggesting that the supply effect is stronger in these areas. Second, house prices and rents are sensitive to changes in socioeconomic composition in the new buildings. An increase in 1pp of market-rate units in the new building relative to all housing units within 800m increase rents by about 3% within 200m, but a decrease in 1pp of public housing units using the same measure increases house prices and rents by about 7-10% within the same distance.

[37] The inverse hyperbolic sine function (asinh) is defined as $\text{asinh(a)} = \ln(a + \sqrt{1 + a^2})$. This function preserves the interpretation of the logarithm while accounting for the cases in which the value of the variable is zero.

when these units become available (panels (a) and (c)). Unsurprisingly, the number of sales and rental listings within 200m jumps by about 20% and 30%, respectively, from four to six years after the permission is approved, picking up new construction in regenerated buildings. Reassuringly, there is no anticipation and no significant effects in any of the other rings.

Regenerations also significantly increased turnover in the market for old units. Panels (b) and (d) reproduce the analysis above for old housing. Sales of old units increase steadily up to about 20% in the first ring, an effect that persists in the second ring (8%). We interpret this increase as a sign that the area might be becoming more attractive for higher-income households and potentially lead to displacement: sales of (now more expensive) old units suggest a replacement of incumbent households by presumably richer families. In contrast, the number of rental listings of old units in the first ring temporarily decreases in number (5%) around the permission year, which indicates that disruptions caused by construction temporarily affected the rental market. After three years, however, the number of listings only slightly increases.[38] Taken together, these findings suggest that there was not a significant reallocation between the sales and the rental market, i.e., landlords did not put their units up for sale in response to regenerations and vice versa.

Lastly, regenerations temporarily led to more market-rate construction nearby. Table 2.2 estimates Eq. (2.2) for the inverse hyperbolic sine of the number of newly constructed units approved by tenure (public or market-rate). Column 5 shows that regenerations attracted more market-rate units within 200m in the short-run (up to 6%). Such increase supports the idea that regenerations make the area more appealing for high-income households, which are the likely occupants of these units.

2.4.3 Effects on Quality

The quality of existing housing stock can also change in response to estate regenerations. For instance, landlords may anticipate that regenerated areas will attract

[38]Since we distinguish new and old units in the rental market by whether the listing advertises the unit as new, this is an upper bound –the plot for old units might be including some new units.

high-income households and improve the quality of their units to cater to this group and charge higher rents. We investigate this question in the sample of rental listings, where we can leverage more information on housing quality from listings' descriptions.

We find that nearby landlords are more likely to upgrade rental units and advertise characteristics that appeal to high-income households after regenerations. Fig. 2-5 estimates Eq. (2.2) for dummy variables indicating the presence of several unit and building characteristics in a listing's description. We exclude listings that are advertised as new builds: we focus on changes in advertisement patterns for units already available for rent before regeneration. First, nearby rental units are up to 5 percentage points (about 40% of the baseline) more likely to be refurbished after regeneration –this category includes keywords such as "refurbished", "renovated" or "rehabilitated". These types of investment can improve the quality of the unit in a way that appeals to the high-end of the rental market, luring more high-income individuals into the neighborhood. Interestingly, this effect persists as far as 400m away from regenerated sites, which is consistent with the very significant rent increases in the range between 200 and 400m. It may also explain the differences with the negative price effect in the sales market for this distance range since units up for sale may not be upgraded. Moreover, listings are more likely to advertise several other features. While some results are not statistically significant, they are all suggestive of quality improvements. Within 100m, they are 7.5 p.p. (70% of baseline) more likely to advertise luxury units –although we cannot reject that part of it is capturing units in the new building.[39] More broadly, nearby listings are more likely to mention in-unit washing machines, communal gardens, gyms and concierges.

Reassuringly, the pattern of estimated price effects does not change when we control for these endogenous changes in housing quality. The specification for housing prices in Section 2.4.1 already controls for a wide range of unit and block characteristics, which can account for changes in the composition of the transacted housing stock. Appendix Fig F.8 reproduces the event study and pooled DID specifications

[39]The sample in Fig 2-5 only includes units that are not explicitly advertised as new builds in the listing's description. Our text analysis method would include new units in regenerated buildings that are not advertised as newly constructed.

for rents also controlling for the unit and building characteristics in Fig. 2-5. These quality-adjusted estimates yield almost identical findings, suggesting that endogenous responses to housing quality are not likely driving our price estimates.

2.4.4 Robustness of the Results

The results hold under several robustness checks. First, we obtain similar estimates using an alternative comparison group that leverages plausibly exogenous variation in the timing of regenerations. Second, a specification accounting for the exposure of housing units to multiple regenerations throughout the sample period yields remarkably similar results. Lastly, our findings are robust to only including old units and dropping observations associated to multiple regenerations.

2.4.4.1 An Alternative Comparison Group: Using Variation in Timing

Following the literature (Aliprantis and Hartley, 2015; Asquith et al., 2021), we develop an alternative difference-in-differences strategy that builds the comparison group using *variation in the timing* of regenerations. This strategy compares the outcomes of housing units near regenerations taking place earlier in the period to those experiencing nearby regenerations later in the future. The idea is that units very close to different public housing estates should be similar. For instance, we can compare the evolution of house prices within 200m of a 2004 regeneration to that of house prices within 200m of a 2018 regeneration between 1998 and 2017.

We view this strategy as complementary to our main specification. The proximity-based method in Section 2.3.3 assumes that there are no spillovers in the outer ring in order to interpret the gap between the inner and the outer ring as the full treatment effect. A comparison of the estimates of that approach to those of this timing-based method helps us assess the validity of that assumption. However, the timing-based approach may be less well-suited to study effects in areas farther away from regenerated sites. The reason is that, although units immediately surrounding public housing estates in different areas of the city should be similar, it is less plausible that units farther away from the estates are comparable across regenerated sites.[40]

The identifying assumption is that the timing of estate regenerations is as good as random, e.g., LAs are not targeting estates in the most profitable areas first, which has been argued in the literature (Li, 2019; Mense, 2020; Pennington, 2021). The plausibility of this assumption depends on a number of factors, some of which are observable (e.g., building and neighborhood characteristics) and some that we cannot observe, such as differential availability of funds over time, negotiations with developers, consultation with tenants, etc. Consistent with the assumption, regeneration seems uncorrelated with several characteristics. Appendix Fig. F.9 regresses the permission year on building and neighborhoood characteristics; none of them is statistically significant.

To implement this strategy, we run a stacked event study design (Cengiz et al., 2019; Deshpande and Li, 2019; Fadlon and Nielsen, 2019) for each of the four treated rings in the main specification (0-200, 200-400, 400-600, 600-800m).[41] We construct the sample as follows. First, we keep observations in the relevant ring to any regenerated estate e. For each estate, we create a separate dataset d. In each dataset d, estates that experience the current regeneration have $\text{Treated}_{ed} = 1$, and estates that are regenerated more than two years later serve as the comparison group, which we further restrict in two ways. First, since regeneration decisions take place at the LA level, we exclude to-be regenerated estates in the same LA as the treated estate to rule out anticipation effects.[42] Second, we only include to-be regenerated estates in the same broad London area, defined as being either in Inner or Outer London.[43]

[40]For instance, the timing-based method probably performs well comparing units within 200m of the estates but poorly when studying units within 600-800m. In this last case, the proximity-based method is likely to perform better since it compares units that are only slightly farther away from each other that belong to the same neighborhood –e.g., compare units within 600-800m to those within 800-1,000m of the same estate.

[41]The stacked methodology is robust to heterogeneous treatment effects, under which traditional event studies perform poorly (Callaway and Sant'Anna, 2020; Sun and Abraham, 2020; Baker et al., 2021; Borusyak et al., 2021).

[42]E.g., regenerating an estate can be a signal of how likely other estates in that LA are to be regenerated in the future.

[43]This restriction partially addresses the concern that this specification cannot control for neighborhood time patterns by accounting for different time patterns in the center and the outskirts of the city.

Then, we create an event year τ variable with respect to the permission year of the treated estate in dataset d. Finally, we append all datasets and keep sales/listings within 6 years of permission.

We estimate the following equation separately for each ring:

$$\mathrm{Y}_{hetd} = \quad \omega_{td} + \phi_{e,g(h),d} + \sum_{\tau=-6}^{6} \beta_\tau \ \mathbb{1}(t - E_d = \tau) \times \text{Treated}_{ed} + \gamma' \mathbf{X_{ht}} + \epsilon_{hetd} \qquad (2.3)$$

where β_τ is the effect of interest, i.e., evolution of the outcomes for units near a current regeneration compared to those that experience a regeneration in the future. We include calendar year-dataset FE (ω_{td}) and estate-census tract-dataset FE ($\phi_{e,g(h),d}$) to control for time patterns and baseline characteristics, as well as the same controls $\mathbf{X_{ht}}$ as in Eq. (2.1). To be consistent with the proximity-based specification, we weight each dataset-year-treated estate equally and, within it, we also weight each estate-block equally. Standard errors are clustered at the dataset level to account for the fact that estates appear in the comparison group for multiple datasets.

The timing-based method yields very similar results to our main specification (Appendix Fig. F.10). We find that house prices go up to 5% within 200m and decrease by 3% within 400-600m –there is also a temporary decrease of house prices within 200m around permission consistent with Fig. 2-2. Note that this strategy, however, perfoorms poorly for rings farther away from the estate: the 600-800m ring shows a pre-trend of decreasing house prices before regeneration. This fact warns against comparing units in far away rings (>600m) across estates, likely because neighborhoods around regenerated buildings are no longer similar at those distances. Regarding rents, they also increase by 5% within 200-400m, although the results do not hold for units within 200m, which show an unstable pre-trend under this method. Estimated effects on the number of sales and listings, as well as changes in housing quality, are also very close to our main specification (Appendix Figs. F.11 and F.12). Overall, the similarity of timing-based estimates supports the assumption of no spillover effects to the outermost ring in the proximity-based method.

2.4.4.2 Robustness to Treatment Intensity

As discussed in Chapter 1, a concern with our main DID specification is that it does not account for the fact that some units appear in different rings for different estate regenerations and, thus, are contaminated by another treatment –i.e., it treats each regeneration as a separate event. To address this, we lay out an empirical specification that estimates the effect of an additional regeneration at a given distance of a census block conditional on other regenerations taking place in that block's neighborhood.

We regress the long-run change in a block's house price level on the number of regenerations taking place in all 100m rings around that block up to 1.2km. To do this, we follow Baum-Snow and Han (2020) and Blanco (2021) and create a quality-adjusted house price index ρ_{it} for each block i and period t, where $t = \{1998\text{-}2002, 2015\text{-}2019\}$.[44] Note that the first period ends before the first regeneration is approved and the second period starts three years after the last regeneration in our balanced sample is approved. Next, we compute the number of estate regeneratons in 2004-2012 within each 100m ring of every block in London and run the following regression:

$$\Delta\rho_i = \alpha_{l(i)} + \sum_{r(i)\in R} \beta_r \text{Regenerated estates}_{r(i)} + \rho_i^{98-02} + \omega \mathrm{X}_i + \varepsilon_i \tag{2.4}$$

where $\Delta\rho_i = \rho_i^{15-19} - \rho_i^{98-02}$. $r(i)$ denotes 100m rings up to 1.2 km of block i and $\alpha_{l(i)}$ are local authority FE. As control variables X_i, we include baseline census block density, number of households, share of public housing units, share of owners and the baseline house price index.[45]

Accounting for the intensity of treatment yields remarkably similar results to the

[44] This house price index is the result of running a regression of log house prices on unit characteristics in the sample that includes the years in each of the two periods:

$$\ln(P_{ht}) = \alpha + \tilde{\rho}_{i(h)t} + \gamma X_{ht} + u_{ht}$$

where X_{ht} includes all of the control variables we used in the analysis above, except for block characteristics. $\tilde{\rho}_{i(h)t}$ are block-by-period FE. Then, we generate the house price index as $\rho_{it} = \alpha + \tilde{\rho}_{i(h)t}$.

[45] We adjust standard errors for spatial autocorrelation following Conley (1999).

DID strategy –results do not seem to be driven by overlapping rings. Panel (a) of Appendix Fig. F.13 shows the results for Eq. (2.4). Blocks experiencing one regeneration within 100m experience house appreciations of up to 10%, 4 percentage points higher than in our difference-in-differences estimates. Likewise, we observe negative price effects of up to 2% slightly farther away (around 500m away from the estate) and effects go back to zero beyond 600m. Note that the interpretation of β_r is slightly different in this case: it measures the effect of being exposed to an additional regeneration at a given ring. As a robustness check for this specification, panel (b) runs a placebo test using the change in the house price index between 1998-2000 and 2001-2003, both in the pre-period: all coefficients go to zero and become statistically insignificant. We do not replicate this result for rents because data is only available starting in 2006: the pre-treatment period is very short for some regenerations, which may not have enough listings and generate a noisy index.

Moreover, dropping sales and listings that show up for multiple regenerations in our main DID specification does not affect the results. Fig. F.14 estimates Eq. (2.2) with 200m rings for house prices and rents using three different samples: including only the house sale occurring closest to the regenerated building, i.e., most intensely treated; only the earliest house sale; or dropping all duplicated observations. Coefficients are remarkably similar to our main results for all samples.

2.4.4.3 Robustness to Sample Selection and Others

The estimated price effects also hold under additional robustness checks. First, our findings are not sensitive to the set of permission years included in the sample. Appendix Fig. F.15 shows that the estimates for house prices hold when using the same sample than for rents (permissions approved in 2007-2012). Fig. F.16 reveals that rent effects also hold for a panel of regenerations balanced between event years -3 and 6, i.e., with permission years in 2009-2012.

Second, we run the main regressions only using the sample of old units. Since new builds can substantially change the housing stock quality, old units are better suited to estimate the price effects that are mainly due to neighborhood changes. Figs. F.17

and F.18 show that the results hold almost equally for the two main event study designs and the pooled DID specification, respectively.

Lastly, the analysis is robust to using different control variables. Fig. F.19 runs the pooled DID in Eq. (2.2) for house prices and rents using 200m rings with different subsets of control variables. Only the house price effect for house sales within 200m of an estate regeneration is slightly affected when the average square footage of units in the postcode is included: it goes from 2% to 4%. The reason is that units sold in the immediately surrounding area are smaller after the regeneration, which can be partly driven by the new market-rate construction induced by it (Fig. F.20).

2.5 Mechanisms: The Role of Demand Effects

The spatial pattern of price effects suggests that demand effects are concentrated very close to regenerated sites. We present supportive evidence for this hypothesis: we show that regenerations led to an inflow of high-income households, an increase in positive local amenities (e.g., cafés, restaurants) and a reduction in negative local amenities (e.g., crime).

2.5.1 Effects on the Neighborhood's Socioeconomic Composition

Estate regenerations substantially change the neighborhood composition by bringing in higher-income households. To show this, we estimate a version of Eq. (2.1) at the block group level using the number of (primary school-age) children per block group that are eligible/not eligible for school subsidized lunches as an outcome.[46] Panel (a) of Fig. 2-6 shows that there is no long-run change in the number of children with subsidized lunch, which is consistent with the fact that public housing units were preserved on average. Instead, panel (b) shows that, six years after the regeneration's

[46] The sample is constructed analogously as in Section 2.4, i.e., we link each block group to all regenerations within 1km based on its population centroid. Although the sample of regenerations is the same as in that section (regenerations with permissions in 2004-2012), the sample is unbalanced because data on children are available only for 2002-2016.

announcement, the number of children without subsidized lunch living near regenerated sites increases by up to 10 children (about 12.5% of the baseline average of 80 unsubsidized children). These results likely underestimate the compositional change because we cannot measure differences in socioeconomic status for residents without school children or at different points in the distribution than the school lunch cutoff.[47]

The growing number of richer households likely puts further upward pressure on prices, given that prior literature indicates that households are willing to pay to live near higher-income and more educated neighbors (Bayer et al., 2007; Guerrieri et al., 2013; Diamond, 2016). In our context, households also seem to place a significant discount on units near public housing. We find that a 1 p.p. increase in the public housing share in a newly regenerated estate is associated with a 0.58% price reduction for new market-rate sales in the same block –although there is no significant effect for rents (Appendix Fig. F.21). In addition, high-income households relocating to the new buildings will demand better schools (Hastings et al., 2010). In fact, Neri (2020) shows that student performance increases in primary schools near regenerations after completion. In contexts where school admission runs by distance such as England, housing units near good schools benefit from a substantial price premium (Black, 1999; Fack and Grenet, 2010; Gibbons et al., 2013; Battistin and Neri, 2017).

2.5.2 Effects on Neighborhood Amenities

We find that rental listings within 100m are more likely to advertise their units as being close to local amenities, cafés, restaurants and parks. The first row of Fig. 2-5 estimates Eq. (2.2) for dummies indicating the presence of these amenities in a listing's description. In the case of cafés, restaurants and parks, these effects are also sizeable slightly farther away from regenerated sites. Note that we exclude listings that are advertised as new builds: we focus on changes in advertisement patterns for

[47] Using subsidized lunch eligibility of children as a proxy for socioeconomic status of the block group assumes that incoming households after the regeneration have the same number of primary school-age children on average than previous residents. However, as discussed in Section 2.4.4, new units are usually smaller in the sales market, suggesting smaller household sizes of new neighbors. In line with this, revitalization might also attract more young professionals to the area, who tend to have less children than households living in the old public housing estates.

units already available for rent before regeneration.

Listings are more likely to include these words right after the announcement of a regeneration, which is consistent with two alternative explanations. On the one hand, businesses might anticipate the revitalization of these neighborhoods and open an establishment before regenerations are completed. This explanation implies that regenerations actually attract new businesses that potentially cater high-income households such as cafés.[48] On the other hand, landlords may anticipate that the regeneration process will bring high-income households that otherwise would have not rented in the area. Thus, landlords may decide to tailor the listings' description to these newcomers by reporting these amenities more frequently. This last explanation does not necessarily mean that new businesses and green spaces actually open as a result of regenerations.[49]

2.5.3 Effects on Crime

Estate regenerations also significantly decrease the number of crimes in the immediate surroundings. Again, we run the proximity specification on the inverse hyperbolic sine of the total number of crimes in a block group.[50] Note that, in this case, the sample of regenerations does not coincide with that section, since data is only available for the period 2008-2018. Fig. 2-7 shows the results for the full sample of regenerations being approved in that period and for the subset of estates with a size of the existing building that is above the median. Regenerations decreased crime by around 5% within 200m, 12% for large regenerations. These numbers are close to crime decreases after public housing demolitions in the US –8.8% decrease within 400m (Aliprantis and Hartley, 2015; Sandler, 2016).

[48] Previous literature uses cafés and restaurants as proxies for neighborhood change (Couture and Handbury, 2017; Glaeser et al., 2018; Li, 2019; Singh, 2020). In particular, cafés and restaurants increase the attractiveness of a neighborhood to young professionals and college graduates, and drive up house prices and rents.

[49] In the future, we plan to test this last hypothesis by contrasting these results with the effects of regenerations on the issue of actual business licenses. We are in the process of applying for these data.

[50] The sample is constructed analogously as in Section 2.4, i.e., we link each block groups to all regenerations within 1km based on its population centroid.

Using estimates from the literature that relate crime to house price changes, we estimate that only one-third of the house price increases within 200m of a regeneration can be explained by observed crime reductions. Our back-of-the-envelope calculation is based on Gibbons (2004), that estimates that a 10% increase (at the sample mean) of criminal damage crimes per km^2 pushes down property prices in Inner London by 1.5%. This estimate is especially convenient because Gibbons (2004) considers the number of crimes within 250m of a property as their independent variable - whereas we use 200m bins in our main specification. Appendix Table F.2 reports the effects on both total crime and criminal damage using Gibbons' methodology.[51] The estimated 8.84% decrease at the sample mean in criminal damage crimes implies a 1.33% increase in house prices (the sample mean is 1.21 per km^2 –in 100s– and criminal damages decreased by 0.107 –column 7), a number that goes up to 1.71% when considering the largest estates.

2.6 Heterogeneity: Regenerations as a Shock to Neighborhood Socioeconomic Composition

Urban renewal programs such as public housing regenerations usually raise concerns about increased housing unaffordability for nearby low-income households. In our context, new market-rate units can be a shock to the neighborhood socioeconomic composition by bringing in relatively higher-income households. Given that prior research indicates that households are willing to pay to live near higher-income neigh-

[51]Since Gibbons (2004) measures crime as deviations from a locally weighted average of crimes, we follow that paper and redefine our variable as the difference between the number of crimes in a block group (in 100s per km^2) and a locally weighted average of the number of crimes in all other block groups within 2km. Our locally weighted average of variable x_i in block group i, $\hat{m}(x_i|d_{i,-j})$ is constructed as follows:

$$\hat{m}(x_i|d_{i,-j}) = \left\{\sum_{j\neq i} x_j \phi(d_{ij}h_i^{-1})\right\}\left\{\sum_{j\neq i} \phi(d_{ij}h_i^{-1})\right\}^{-1}$$

where d_{ij} is the distance between block group i and j, h_i is the standard deviation of d_{ij} for block group i. We compute this variable for every block group i-year combination.

bors,[52] such shock may trigger (or intensify) an upward trend on housing prices near regenerated sites. We provide suggestive evidence for this hypothesis by studying heterogeneity in price effects along two dimensions: baseline neighborhood socioeconomic composition and the magnitude of market-rate construction.

2.6.1 Heterogeneity by Baseline Socioeconomic Composition

Low-income neighborhoods are especially affected by regeneration-induced housing price increases. These neighborhoods have the highest potential for composition changes: holding market-rate construction (i.e., incoming higher-income households) constant, low-income neighborhoods experience a larger shock to socioeconomic composition than high-income neighborhoods.

To explore this idea, we study heterogeneity by two neighborhood characteristics: mean household income and house price levels in 2001. For each regeneration, we compute the mean of these two variables among all census block groups within 800m of the estate, which includes all treated rings in our main specification.[53] Next, we estimate a version of Eq. (2.2) that interacts relative time and 100m-ring indicators with a dummy variable indicating whether the value of the heterogeneity variable is above or below the median in the regenerations sample (Z_e):

$$\begin{aligned}\mathrm{Y}_{het} = \quad & \alpha_{et} + \kappa_{e,r(h,e),g(h)} + \gamma' \mathbf{X_{ht}} \\ & + \sum_{z\in\{0,1\}} \sum_{r\in R} (\theta^z_{0,r} \mathrm{Post}^{0\text{-}3}_{et} + \theta^z_{1,r} \mathrm{Post}^{4\text{-}6}_{et}) \times \mathbb{1}(r(h,e) = r, Z_e = z) + \epsilon_{het} \quad (2.5)\end{aligned}$$

We find that rent increases are especially concentrated in low-income areas. Fig. 2-8 illustrates the results for the two variables, plotting the price effects by distance for two subsamples: regenerations that are above vs below the median. While regenerations in higher-income neighborhoods and areas with higher baseline house prices experience larger rent increases within 100m of regenerated sites, the opposite

[52] E.g., Bayer et al. (2007); Guerrieri et al. (2013); Diamond (2016).

[53] We construct house price levels for each block group as in Section 2.4.4.2 –the same regression is run for year 2001.

is true for units within 100 and 600m: poorer areas bear most of the rent increases. These findings suggest that richer neighbors value more changes in amenities, which are plausibly very concentrated near the new buildings, but such improvements in amenities make housing less affordable in the broader area for low-income households in poorer neighborhoods. In the case of house prices, no clear pattern arises.

The results are consistent with the supply effect being stronger for high-end units. The reason is that new market-rate units in regenerated estates are a closer substitute to the high-end of the rental market than to low-end units. Since household income and baseline house prices are proxies for housing quality, our results support this vision. The findings contrast with Asquith et al. (2021), who estimate that new market-rate buildings reduce nearby rents in low-income areas. A potential reason is that Asquith et al. (2021) use Zillow data, which overrepresent high-end units. In contrast, Rightmove seems more representative of London's rental market (Appendix B.3.1). Our results support the findings in Damiano and Frenier (2020), who find that new-market rate housing decreases nearby rents for high-end units but increases them for low-end units.[54]

Finally, we find suggestive evidence that nearby landlords respond to regenerations by increasing the quality of rental units only in low-income areas. Appendix Fig. F.22 shows that the significant effect on refurbishments in Section 2.4.3 exclusively takes place in low-income areas. As discussed in that section, landlords cater more to high-income households by upgrading. Quality upgrades are most profitable in low-income areas, since potential gains from rent increases are higher.

[54] We cannot reproduce the analysis in Damiano and Frenier (2020), which divides the sample in low, middle and high-end units –referred to as "housing submarkets". That paper leverages a panel of rental apartment buildings, which they can divide into housing submarkets using baseline rent levels within a zipcode. In contrast, Rightmove data does not perform well tracking listings belonging to the same unit over time.

2.6.2 Heterogeneity by the Magnitude of Market-Rate Construction

Market-rate construction is an alternative proxy for changes in neighborhood socioeconomic composition. Given two regenerations in identical neighborhoods, the regeneration building relatively more market-rate units should increase the neighborhood's socioeconomic status by more.

However, the effect on prices of building more market-rate housing is ambiguous. On the one hand, more incoming high-income households alter the income mix of a neighborhood and may in turn bring more amenities (Diamond, 2016). Because the neighborhood is more attractive, housing demand shifts outwards, putting an upward pressure on prices. On the other hand, a large expansion of market-rate supply puts more downward pressure on prices: keeping demand constant, the willingness to pay of the marginal incoming household is (weakly) smaller. Whether net prices go up or down with more extreme levels of market-rate construction gives us a sense of how much socioeconomic composition matters –if prices increase by more, the second mechanism is crucial to explain the results.

To study this, we define the variable "market shock" as the change in market-rate units in the regenerated estate over the total number of housing units within 800m, our last treated ring:

$$\text{Market shock}_e = \frac{\Delta\text{Market units}_e}{(\text{Housing units} \leq 800\text{m})_e}$$

We analogously define "public shock" and "total shock" as the change in public housing and total units, respectively. Then, we estimate Eq. (2.5) for each of the three "shocks" separately, where Z_e is a dummy variable that splits the sample of regenerations in two based on whether they are above or below the median value of the shocks.

Regenerations with more market-rate construction consistently show larger house price and rent increases (Fig. 2-9). In the case of house prices, regenerated areas below the median of the market shock experience price decreases within 200-500m of an estate. In a similar fashion, rent increases within that range are exclusively concentrated in areas with market-rate construction above the median –areas below it do not experience significant changes in rents. In the case of rents, we find suggestive evidence that the result is not driven by developers building higher-quality units in regenerations with larger market shocks. Appendix Table F.3 regresses some unit characteristics on the market shock variable and none of them are statistically significant at the 5% level. Meanwhile, no clear pattern arises when examining price effects by the size of the public or total housing shocks.

These findings suggest that the mixed-income component of housing is key to explain observed effects on local housing prices. For moderate rates of market-rate construction, nearby house prices can decrease and rent levels can be maintained – supply effects weakly dominate demand effects in the broader area. However, large market-rate shocks are more likely to significantly change the neighborhood socioeconomic composition and potentially gentrify it. Such idea is consistent with the hypothesis that enough high-income households arriving to a low-income area are needed in order to change the trajectory of a neighborhood. Note that demand effects always dominate supply effects within 100m in our context. A likely explanation is that the new buildings are usually replacing distressed public housing estates and, thus, benefits from building improvements are exceptionally large for immediately surrounding housing units.

Lastly, a concern for the results above is that we observe positive price effects associated to larger market shocks because developers decide to partner up with LAs to supply more market-rate units in more profitable areas. To explore this, Fig. F.23 shows the coefficients of a multivariate regression of the market shock on building and neighborhood characteristics. Larger market shocks are not predicted by any neighborhood characteristics. Usually, the market-shock is bigger in larger existing estates and tracks the total shock in the nearby area. These results alleviate the concern that regenerations with high market shocks are in selected neighborhoods.

2.7 Cost Effectiveness of Public Housing Regenerations

The results indicate that mixed-income regenerations revitalize affected neighborhoods by improving local amenities and increasing income diversity, even after preserving the amount of public housing. However, regenerations are a costly investment. In this section, we compare the appreciation in nearby housing values due to an additional regenerated public housing unit to the associated costs for the public sector –Appendix B.3.3 provides the details. We focus on the "place-based" aspect of the policy: we exclude the benefits and costs for households in regenerated buildings.

The cost effectiveness analysis is especially challenging in our context. Regarding benefits, an ideal estimate of society's willingness to pay for the policy would be captured by the shift in housing demand after regenerations. However, our estimated price effects do not have a direct welfare interpretation because they conflate demand and supply responses.[55] Hence, we take increases in nearby housing prices as a lower bound for benefits. Regarding costs, we would ideally gather data on the costs of each redevelopment project that are borne by the public sector, but such data is not available. Furthermore, the cost for the public sector may widely vary from estate to estate, since they are the result of a negotiation process between LAs/HAs and developers.[56] More generally, the planning system in London –and the UK– is not based on zoning, i.e. there is no automatic right to build according to some local zoning rules. All planning decisions are discretionary and taken on a case-by-case basis by LAs. In the case of regenerations, this system may be used to relax the budget constraint of LAs, e.g., by allowing developers to build more market-rate housing if they bear a higher share of the cost of new public housing units. In the extreme case, regenerations can come at a zero cost for LAs –and, hence, always pay off. Below, we focus on the case in which the public sector pays a positive amount of

[55] For this reason, we cannot use the marginal value of public funds to measure the cost effectiveness of public housing regenerations (Hendren and Sprung-Keyser, 2020; Finkelstein and Hendren, 2020).

[56] Housing Committe Members, *"Knock it Down or Do it Up? The Challenge of Estate Regeneration"*, Greater London Authority, February 2015.

the cost.

For the benefits of regeneration, we compute a range of quantities that are likely an underestimate of WTP. First, we estimate that each regenerated unit leads to an increase in the aggregate value of house prices within 100m of £3,650. Second, we estimate that this number adds up to £39,650 when considering rental price increases within 400m. Lastly, we also compute the net present discounted value (NDVP) of changes in long-run earnings of children exposed to regenerations. This concept is just one of many factors that cause the outward shift in the demand curve –but that we can approximate. We translate increases in test scores of incumbent children induced by regenerations as estimated in Neri (2020) to increases in future earnings: each regenerated unit leads to an associated benefit of £21,730 for this concept.[57]

To put these numbers in context, we compute the NPDV of the net costs of regeneration, which include the mechanical costs of the demolition, reconstruction and relocation of households while the development is under construction, minus any fiscal revenues accruing to the government's budget. We approximate the first two types of costs using estimates from research reports. Since the financing of regeneration programs varies from site to site, we consider two scenarios for reconstruction costs: either LAs pay a flat subsidy for each regenerated public housing unit or they pay their full cost. For the latter, we consider a lower and an upper bound given by alternative costs estimates. For relocation, we consider the mean rental price of relocating a household within 800m of an estate in the four years leading to the completion of the project. Regarding fiscal revenues, we subtract tax savings from the council tax (analogous to a property tax) and the stamp duty land tax (a sales tax on house sales) of new market-rate units. In total, we estimate that the regeneration of an additional public housing unit ranges from £147,525 to £430,765.

Thus, estimated housing value appreciations are very low relative to regeneration costs (see Table F.4). While house price increases within 100m account for at most

[57]Neri (2020) estimates that regenerations increased test scores by 0.091 standard deviations for incumbent children living within approximately 1km of regenerated sites. We closely follow the computation in Hendren and Sprung-Keyser (2020), and convert them into future earnings using the estimate in Kline and Walters (2016).

2.5% of regeneration costs, rent increases within 400m represent between 9 and 27% of regeneration costs. Finally, increases in children's future earnings can account for 5 to 15% of the costs.

2.8 Conclusion

This paper estimates the impact of regenerating old public housing developments into mixed-income communities. Over a six-year period, we estimate that regenerations raised house prices and rents in the vicinity of the new building, and decreased house prices slightly farther away. This spatial pattern of net price effects is consistent with strong demand effects very close to the new development and supply effects that domine farther away in the sales market.

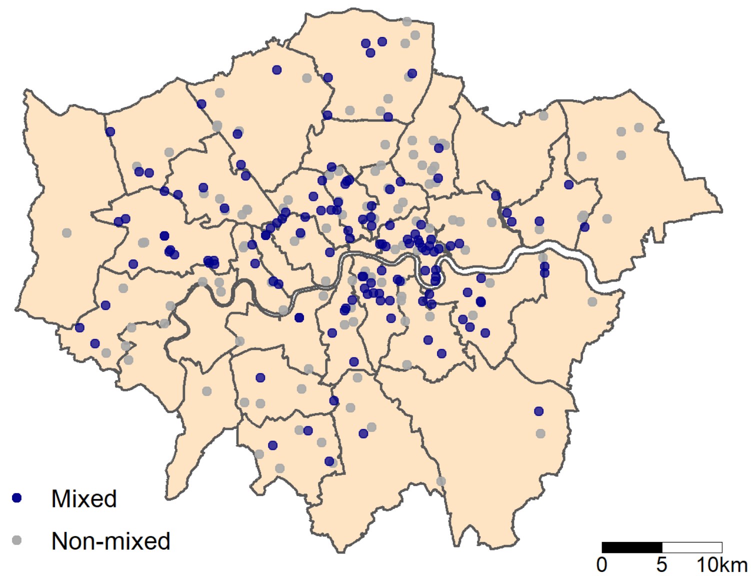
Our findings highlight that mixed-income developments have the potential of preventing the negative effects of public housing even when preserving the amount of existing public housing. Our results provide guidance for future place-based policies that aim to revitalize deprived neighborhoods, which can be particularly relevant in contexts characterized by the lack of mobility of low-income households (Bergman et al., 2019).

However, the improvement in local amenities and new-market rate construction can have unintended consequences. When we explore heterogeneity, we find suggestive evidence that low-income households are most affected by increased unaffordability. First, rent increases are concentrated in low-income and lower-priced areas at baseline. Second, large rates of new market-rate housing in the new building are associated with larger price increases: more incoming high-income households have the potential to change the trajectory of the neighborhood and gentrify it. Future research should study how low-income households can reap the benefits of urban renewal while not suffering the negative consequences from rising housing prices, e.g. displacement and financial stress. A potential solution proposed by Diamond et al. (2019) is for the government to provide insurance against rent increases.

Finally, this paper argues that policymakers need to consider the differential impacts of placed-based policies on sales and rental markets. Using unique micro-data on both sale and rental prices, we show that regenerations have contrasting price effects in the broader area: house prices go down while rents go up. While we provide some potential reasons for these differences, future research should further investigate the mechanisms behind this result.

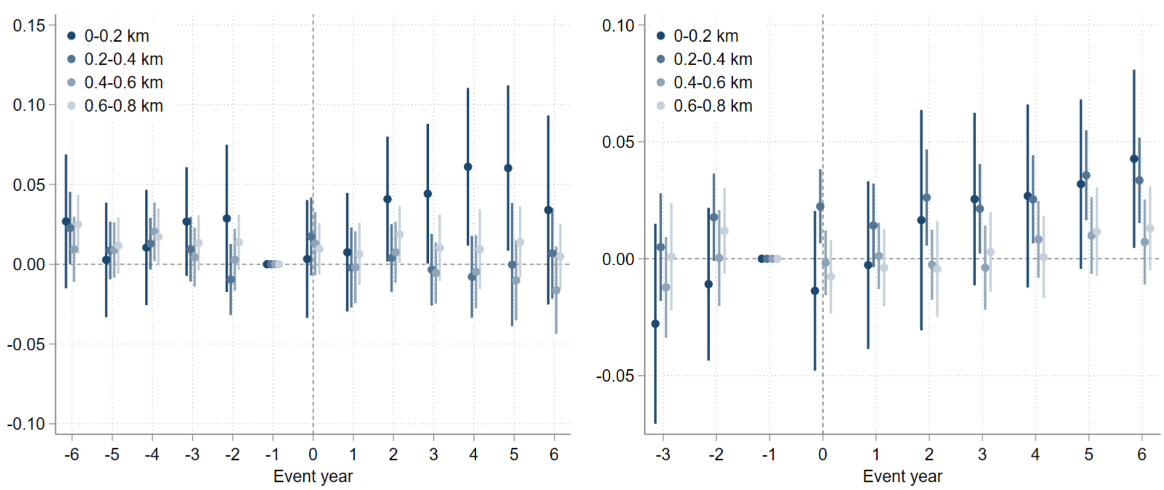
Figures

Figure 2-1: Location of regenerations by income type



Note: The black lines delimit each local authority. Blue dots correspond to mixed-income regenerations, gray dots refer to estates regenerated as public housing only ("non-mixed").

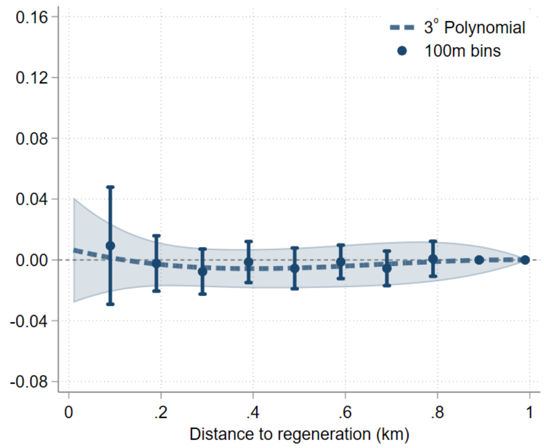
Figure 2-2: Effects of estate regenerations on house prices and rents



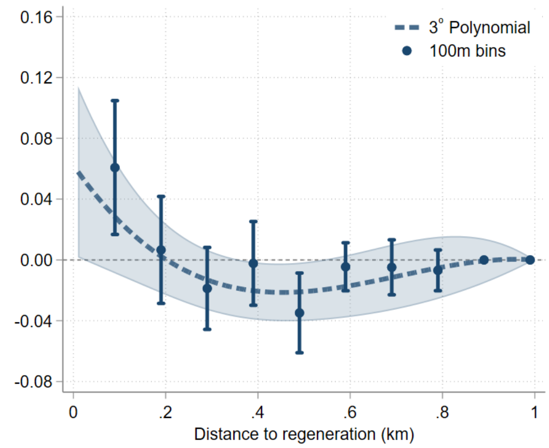
(a) House prices **(b)** Rents

Note: The plots report coefficients $\beta_{\tau,r}$ in Eq. (2.1) for each concentric 200m ring. The omitted category is housing units within 0.8-1km of the regeneration. Panel (a) uses the balanced sample of estate regenerations with a permission approval in 2004-2012; panel (b) uses those with a permission approval in 2007-2012.

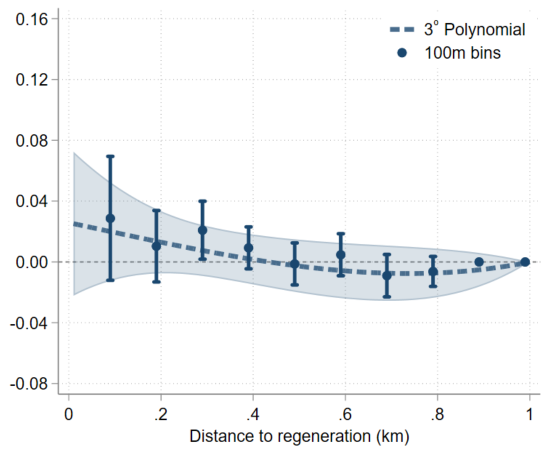
Figure 2-3: Effects on house prices and rents with a continuous definition of distance



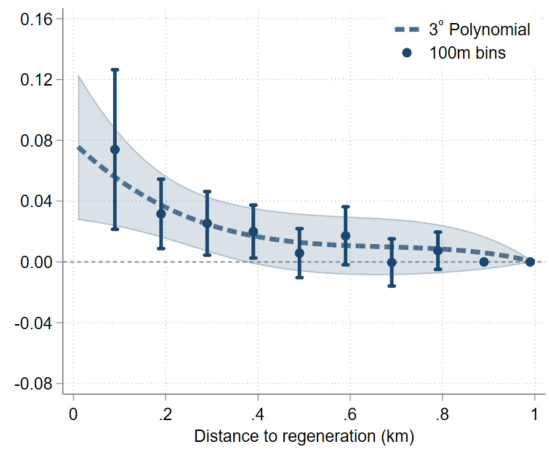
(a) House prices: 0-3 years



(b) House prices: 4-6 years



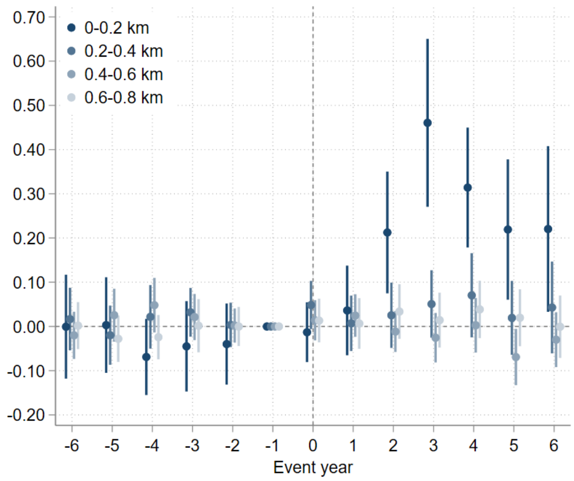
(c) Rents: 0-3 years



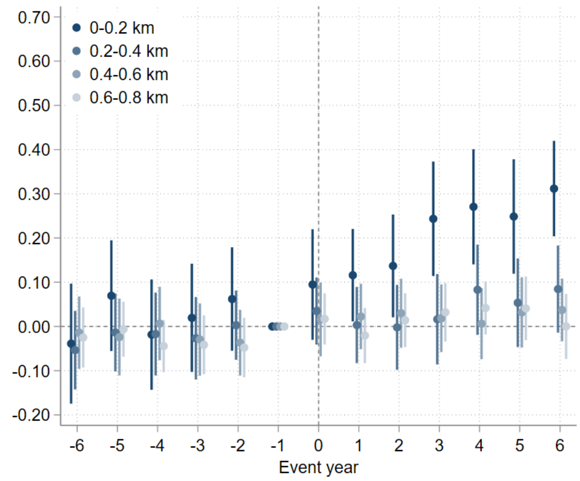
(d) Rents: 4-6 years

Note: The figure reports point estimates and 95% confidence intervals for coefficients $\theta_{0,r}$ (left panels) and $\theta_{1,r}$ (right panels) in Eq. (2.2) using 100m rings. The dotted line runs that same regression but using a 3rd order degree polynomial of the distance from each house sale to the regeneration site instead of rings. The shaded area indicates the corresponding 95% confidence interval. Panels (a) and (b) use the balanced sample of estate regenerations with a permission approval in 2004-2012; panels (c) and (d) use those with a permission approval in 2007-2012.

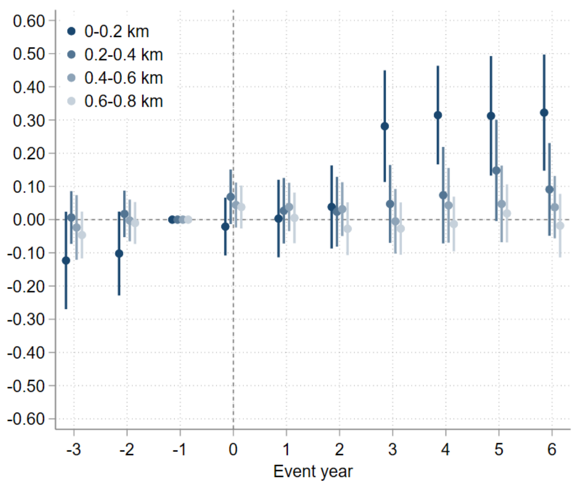
Figure 2-4: Effects of estate regenerations on house sales and rental listings



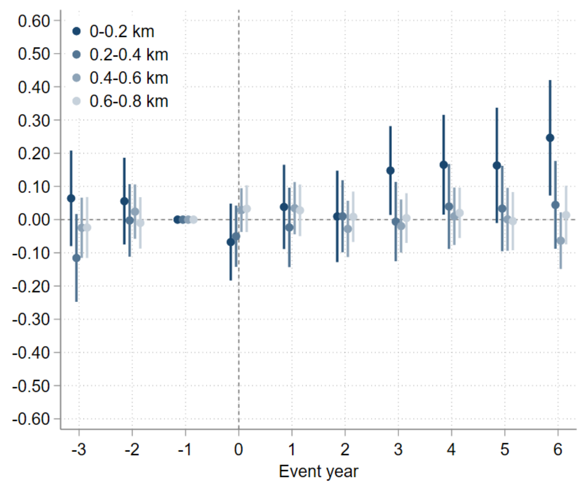
(a) Sales of new houses



(b) Sales of old houses



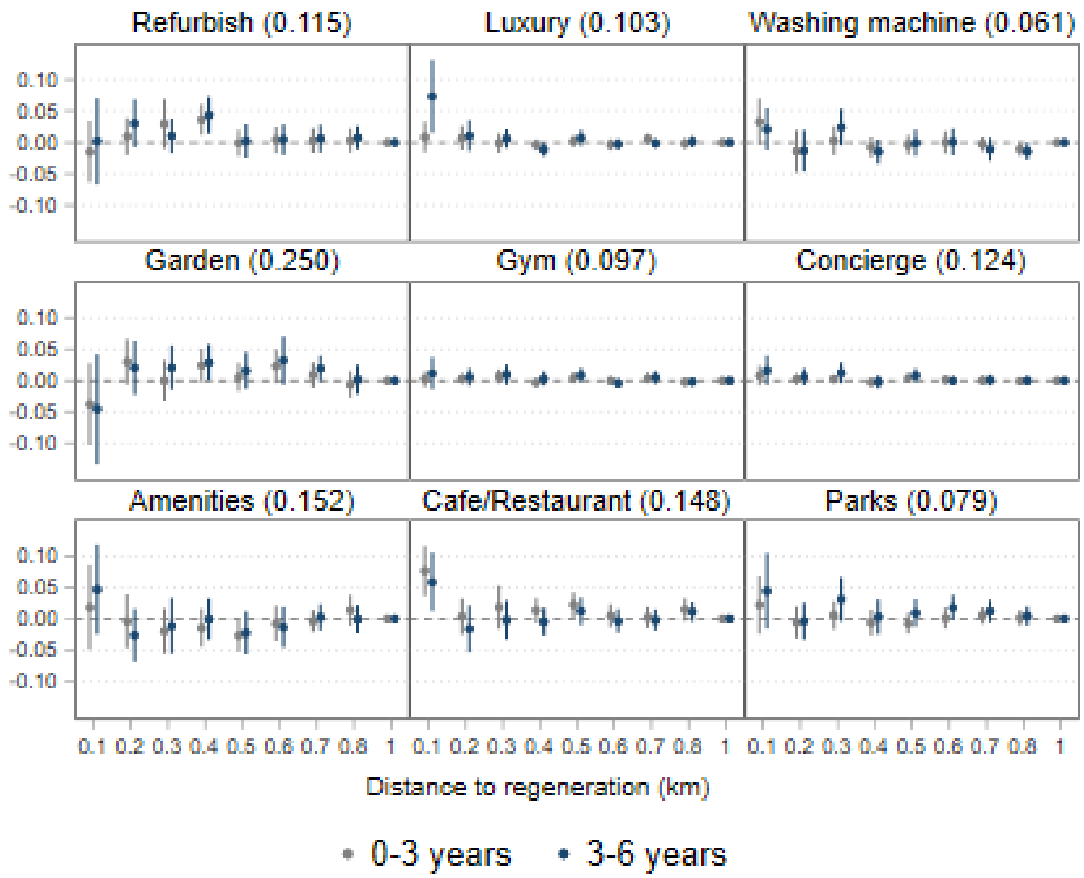
(c) Rental listings of new houses



(d) Rental listings of old houses

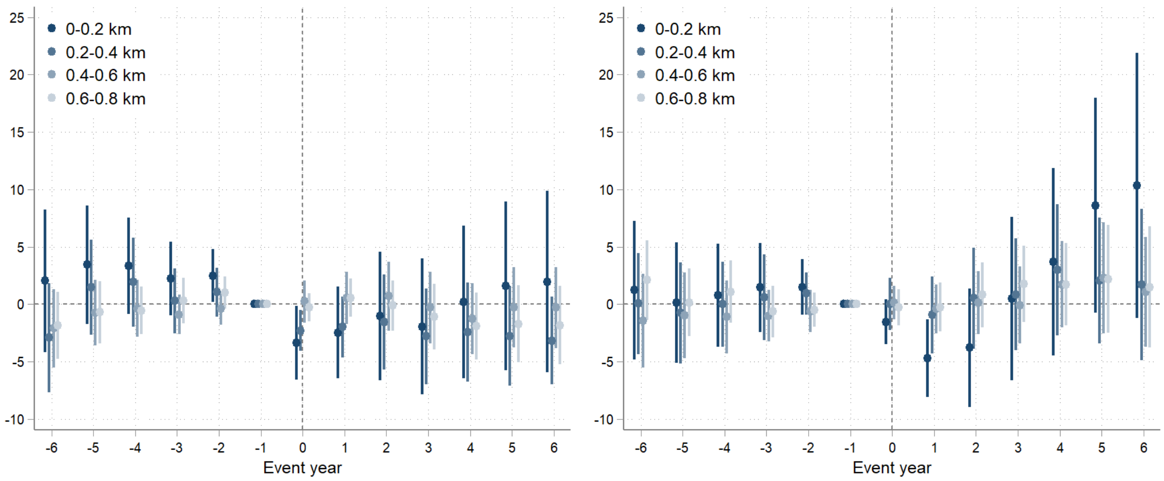
Note: The plots report coefficients $\beta_{\tau,r}$ in Eq. (2.1). For rental listings, we distinguish between "new" and "old" using text analysis on the description of the rental listing. Panels (a) and (b) use the balanced sample of estate regenerations with a permission approval in 2004-2012; panels (c) and (d) use those with a permission approval in 2007-2012.

Figure 2-5: Effects on rental listings' descriptions



Note: Coefficients and related 95% confidence intervals are obtained by estimating Eq. (2.2) on the sample of rental listings using 100m rings. The plots use the balanced sample of estate regenerations with a permission approval in 2007-2012. Numbers in parenthesis report the pre-treatment period average of the variable for listings within 800m of regenerations.

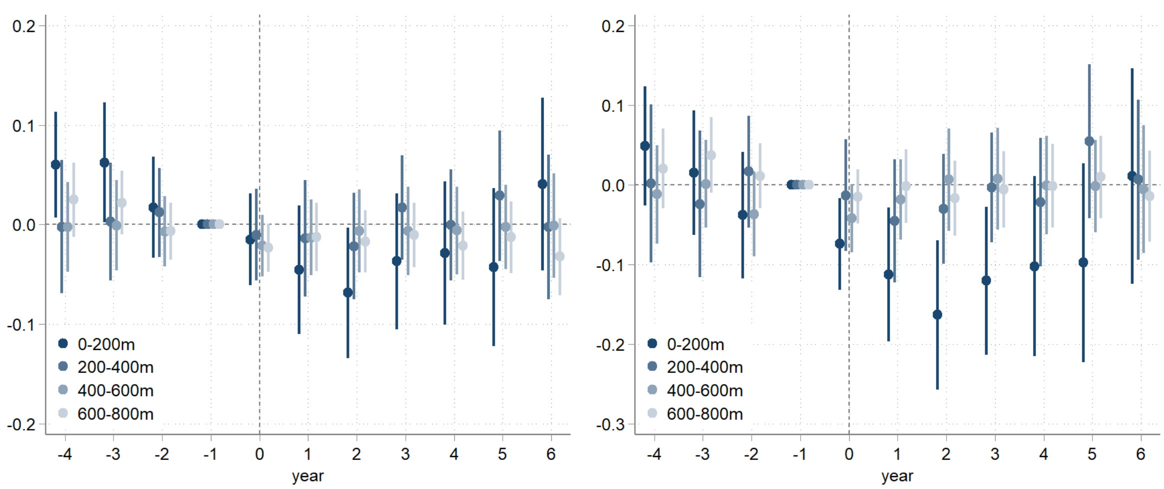
Figure 2-6: Effects on the number of kids eligible/not eligible for subsidized lunch



(a) With subsidized lunch **(b)** Without subsidized lunch

Note: The plots report coefficients $\beta_{\tau,r}$ and 95% confidence intervals in a block group version of Eq. (2.1). The plots use the balanced sample of estate regenerations with a permission approval in 2004-2012, and the sample period contains years from 2002 to 2016 –the sample is balanced between event years -2 and 4.

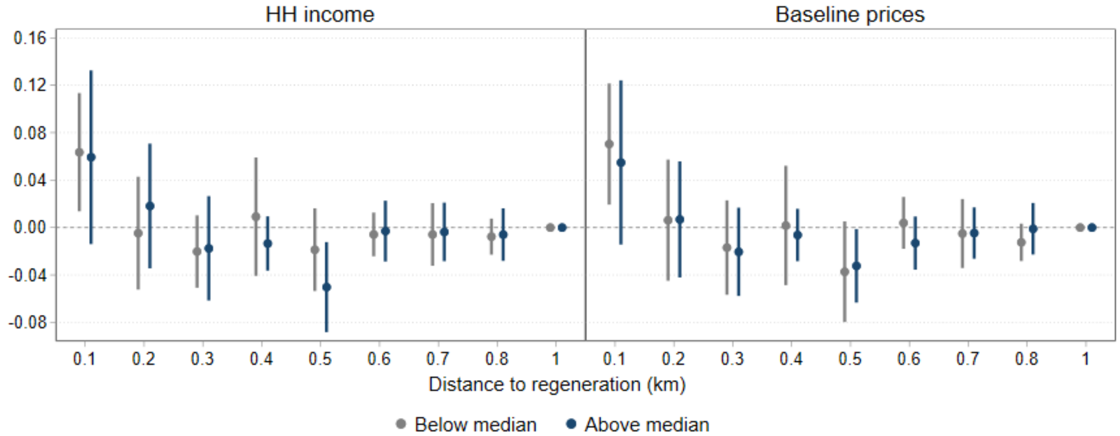
Figure 2-7: Effects on the total number of crimes



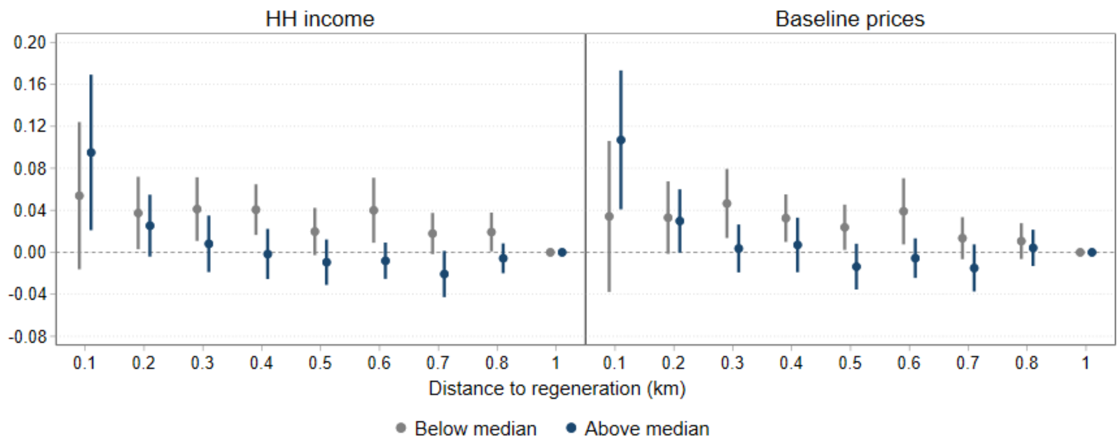
(a) All estate regenerations in 2009-2018 **(b)** Large estate regenerations in 2009-2018

Note: This figure shows the evolution of the inverse hyperbolic sine of total crimes in a census block group around the permission year of a regeneration. Coefficients and related 95% confidence intervals are obtained by estimating a block group version of equation (2.1). The sample includes all regenerations with a permission between 2009 and 2018. "Large estates" are those with a number of existing public housing units above the median of this sample.

Figure 2-8: Heterogeneity by neighborhood characteristics



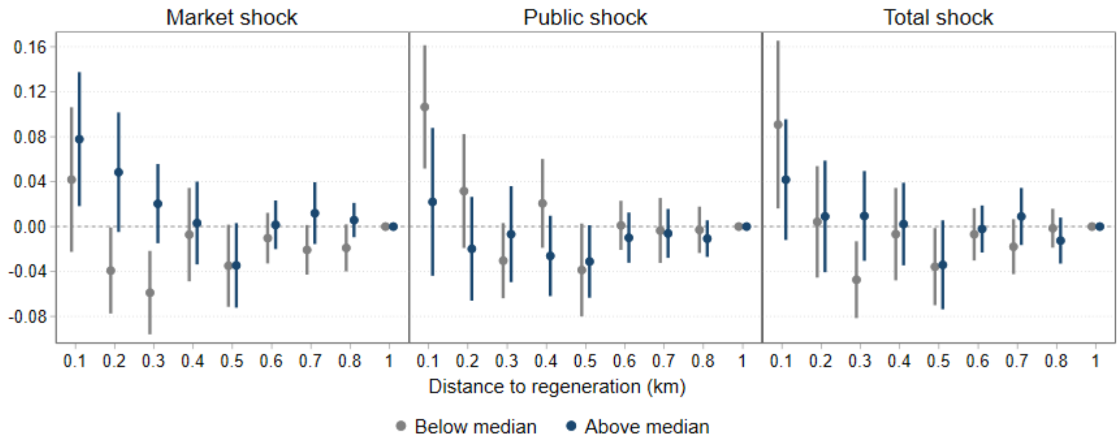
(a) House prices



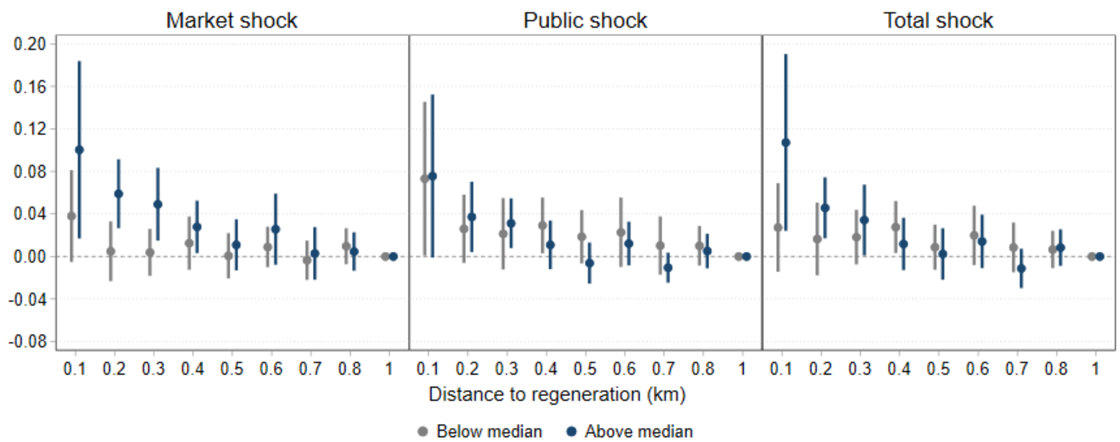
(b) Rents

Note: The plots report point estimates and 95% confidence intervals for coefficients $\theta^0_{1,r}$ (gray) and $\theta^1_{1,r}$ (blue) in Eq. (2.2) using 100m rings. Panel (a) uses the logarithm of house prices as an outcome on the balanced sample of estate regenerations with a permission approval in 2004-2012; panel (b) uses the logarithm of rents on regenerations with a permission approval in 2007-2012. Left plots use mean household income within 800m of the estate as the heterogeneity variable, right plots use the baseline house price index within 800m as constructed in Section 2.4.4.2.

Figure 2-9: Heterogeneity by the magnitude of the supply shock



(a) House prices



(b) Rents

Note: The plots report point estimates and 95% confidence intervals for coefficients $\theta^0_{1,r}$ (gray) and $\theta^1_{1,r}$ (blue) in Eq. (2.2) using 100m rings. Panel (a) uses the logarithm of house prices as an outcome on the balanced sample of estate regenerations with a permission approval in 2004-2012; panel (b) uses the logarithm of rents on regenerations with a permission approval in 2007-2012. The graphs use the market, public and total shock, respectively, as the heterogeneity variable.

Tables

Table 2.1: Summary statistics of public housing regenerations

	(1) London	(2) Full sample	(3) Balanced
Panel A: Building characteristics			
Total units before		248	246
Public housing units before		206	194
Total units after		457	431
Public housing units after		197	208
Panel B: Δ Market-rate units/total units within X			
$\leq$ 200m		0.41	0.32
$\leq$ 400m		0.11	0.11
$\leq$ 600m		0.05	0.05
$\leq$ 800m		0.03	0.03
$\leq$ 1,000m		0.02	0.02
Panel C: Neighborhood chars. (2001)			
Density (per ha)	108	151	136
High education	0.24	0.21	0.20
Unemployment	0.07	0.10	0.10
Public housing units	0.26	0.48	0.49
Owner-occupied units	0.55	0.35	0.35
Privately rented units	0.15	0.14	0.13
House price index	11.66	11.67	11.63
Household income	35,548	33,328	32,318
Census blocks/Estates	24,115	135	70

Note: Data in Panels A and B were obtained from the London Development Database; data in Panel C come from 2001 census data. Panel B is the average of the ratio between the change in market-rate units induced by the regeneration and the total number of housing units within several distances of regenerations. Neighborhood variables in Panel C are computed as the average of census blocks within 800m of the census block of reference weighted by population – consistent with our empirical strategy. The house price index (constructed as in Section 2.4.4.2) and household income use census block groups. The first column includes all blocks in London. Column 2 uses blocks for the full sample of estate regenerations, while column 3 uses a balanced sample of regenerations approved between 2004 and 2012.

Table 2.2: Effects of regenerations on sales, listings and new construction

	ihs(house sales)		ihs(rental listings)		ihs(new construction)		prob(new construction)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New	Old	New	Old	Public	Market	Public	Market
Panel A: 0-3 years								
0-200m	0.150***	0.103***	0.101**	-0.037	0.056*	0.063**	0.014*	0.016
	(0.037)	(0.035)	(0.045)	(0.038)	(0.031)	(0.030)	(0.008)	(0.011)
200-400m	0.023	0.025	0.036	-0.001	0.012	0.015	0.002	-0.001
	(0.022)	(0.022)	(0.036)	(0.038)	(0.015)	(0.022)	(0.004)	(0.008)
400-600m	-0.014	0.038*	0.034	0.009	0.005	0.015	0.002	0.005
	(0.014)	(0.019)	(0.031)	(0.029)	(0.013)	(0.016)	(0.003)	(0.007)
600-800m	0.023	0.038**	0.022	0.036	0.012	0.016	0.003	0.002
	(0.018)	(0.015)	(0.026)	(0.026)	(0.013)	(0.019)	(0.003)	(0.007)
Panel B: 4-6 years								
0-200m	0.193***	0.185***	0.278***	0.058	0.016	0.032	-0.002	-0.003
	(0.053)	(0.036)	(0.066)	(0.059)	(0.027)	(0.046)	(0.006)	(0.015)
200-400m	0.030	0.086***	0.099*	0.051	0.000	-0.005	-0.003	-0.002
	(0.035)	(0.026)	(0.059)	(0.047)	(0.016)	(0.026)	(0.005)	(0.011)
400-600m	-0.040*	0.040*	0.053	-0.010	-0.003	0.022	-0.003	0.006
	(0.022)	(0.020)	(0.047)	(0.032)	(0.016)	(0.024)	(0.004)	(0.010)
600-800m	0.023	0.052**	0.022	0.026	0.004	0.009	-0.000	0.000
	(0.021)	(0.022)	(0.037)	(0.035)	(0.014)	(0.020)	(0.004)	(0.008)
N	84,762	84,762	64,602	64,602	83,549	83,549	83,549	83,549
R-squared	0.29	0.64	0.58	0.80	0.15	0.25	0.14	0.25

Note: The table reports estimates of coefficients $\theta_{0,r}$ (Panel A) and $\theta_{1,r}$ (Panel B) in Eq. (2.2) using 200m rings for four dependent variables. Columns 1-2 use the inverse hyperbolic sine (ihs) of the number of house sales per year by new build status. Columns 3-4 use the ihs of the number of rental listings by status. Columns 5-6 use the ihs of the number of new units approved for construction by tenure type (public housing or market-rate), while columns 7-8 use the probability of any new construction by tenure type. Standard errors in parenthesis (clustered at the estate level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

From Public Housing to Subsidized Private Housing: The Distributional Consequences of Housing Assistance Programs

joint with Juliette Fournier[1]

> "In policy circles, [housing] vouchers were known as a 'public private partnership'. In real estate circles, they were known as a 'win'."
>
> *Evicted*, Matthew Desmond (2016)

3.1 Introduction

The past few decades saw a dramatic change in the provision of housing assistance to low-income families in the United States. The federal government gradually shifted

[1] This chapter uses data provided by Zillow through the Zillow Transaction and Assessment Dataset (ZTRAX) –more information on accessing the data can be found at http://www.zillow.com/ztrax. The results and opinions are those of the author and do not reflect the position of Zillow Group.

away from constructing public housing towards subsidizing private housing. An emblematic measure of this transition is the HOPE VI program, initiated in 1993, which led to the demolition of hundreds of public housing developments. Concurrently, two other housing programs expanded considerably (see Figure 3-1). First, tenant-based rental assistance through the Housing Choice Voucher Program, formerly known as Section 8. Second, project-based rental assistance, mainly through the Low Income Housing Tax Credits (LIHTC), a program that subsidies the supply of new low-income housing. These two programs leave a larger role to private developers and property owners, who may capture a substantial share of the benefits intended to disadvantaged households (Desmond, 2016). The academic literature has so far largely ignored the distributive implications of the decline of this housing policy shift.

This paper builds a framework to characterize theoretically and quantitatively the redistributive implications of housing assistance. Using a spatial model, we argue that public housing may in theory improve redistribution efficiency when income taxation does not dissociate wage and rental incomes. We then estimate the key parameters of our model by leveraging public housing demolitions as an exogenous shock, in combination with other key policy changes that quasi-exogenously shift housing market outcomes and neighborhoods' socioeconomic composition.

The main idea behind the model is that housing assistance programs feature a trade-off between indirect pecuniary redistribution and direct amenity effects. On the one hand, public housing increases the stock of housing units, which drives local rents down. However, it also amplifies the spatial concentration of poverty, lowering local amenities for recipients. On the other hand, voucher and LIHTC programs improve local amenities by encouraging mized-income housing, while pushing private landowners' rents up.

Focusing on seven major metropolitan areas,[2] we establish two facts that underpin this trade-off. First, for each of these cities, the demolition of public housing has not been compensated by the construction of new housing units, whether subsidized or not. Therefore, we expect that this reduction in housing supply benefited local

[2]Namely, Atlanta, Baltimore, Chicago, Memphis, Newark, Pittsburgh and Washington, DC.

landowners through higher rents. Second, the demographic composition of census tracts affected by demolitions changed drastically. In particular, the education level and per capita income increased more than twice as fast as in unaffected areas. This result suggests that areas near demolitions may have benefited from lower spatial concentration of poverty through enhanced amenities.

To quantify the distributional consequences of housing assistance programs, we develop a quantitative urban model with redistributive policies and endogenous amenities. The main departure from the existing literature is the introduction of two types of tax instruments, non-linear income taxation and housing assistance, that are used by the government to redistribute across households. We assume that the set of taxes that can be imposed on the workers are limited to be a function of their total income, but not their type, following Mirrlees (1971). This restriction creates a trade-off between redistribution and productive efficiency. As in Naito (1999), housing assistance programs complement non-linear income taxation by affecting the pre-taxation income distribution—specifically, the distribution of land rents.

We then estimate the key parameters of our model. The main goal of our estimation is to disentangle amenity effects from housing demand and supply elasticities, so we can accurately capture the aforementioned trade-off of housing assistance programs. First, we estimate the housing supply elasticity by leveraging the HOPE VI program as a shock to private housing demand. On the demand side, we use two instruments to jointly estimate both the housing demand elasticity and the parameter capturing amenity spillovers. The first instrument relies on LIHTC eligibility rules. LIHTC provides larger subsidies in low-income neighborhoods using a cutoff rule. Such rule provides exogenous variation both in new housing supply and neighborhood composition changes. The second instrument is distance to rapid transit lines, which also affects neighborhood composition. Our estimates for the three key parameters are close to previous estimates in the literature.

Our paper combines tools from public and urban economics to study housing policy. First, we propose a novel interpretation of the redistributive virtues of in-kind transfers which builds on insights from the optimal taxation literature. Early theoretical work argued that in-kind transfers could improve redistribution absent lump-sum transfers. Nichols and Zeckhauser (1982) point at targeting efficiency, while Coate et al. (1994) are the first to explore pecuniary effects as a rationale for in-kind transfers. We provide a general equilibrium framework to think about those so-called pecuniary effects. Our main point is that public ownership of some fixed factor –land in the case of housing assistance– is the underlying reason why in-kind transfers can enhance redistribution. The formal argument resembles Naito (1999)'s seminal paper and recent work by Costinot and Werning (2018): Assuming that the set of taxes that can be imposed on the workers are limited to be a function of their total income, but not their type, housing assistance programs complement non-linear income taxation by affecting the pre-taxation income distribution—specifically, the distribution of land rents.

Second, this paper adds to a growing empirical literature demonstrating both the indirect pecuniary effects and the direct amenity spin-offs of housing assistance programs. Blanco (2021) suggests that public housing demolitions in Chicago induced significant price increases in local housing markets. Blanco and Neri (2021) also point to large local house price increases and amenity improvements from regenerating public housing into mixed-income housing. Susin (2002) and Collinson et al. (2019) show that rents for low-income households increased in areas with more housing vouchers, and Susin (2002) argues that the overall rent increase is considerably larger than the subsidy to housing voucher beneficiaries. Diamond and McQuade (2019) find that new LIHTC buildings increase prices in low-income neighborhoods and decrease them in high-income areas. Diamond and McQuade (2019), as well as Davis et al. (2019a) and Davis et al. (2019b), estimate dynamic discrete choice models of location choice that include households that value endogenous characteristics such as demographic composition and median income in the neighborhood to rationalize households' location decisions in response to changes in housing policies.

Lastly, we borrow techniques from the quantitative urban literature to quantify the aforementioned trade-off between pecuniary effects and amenity spin-offs of housing assistance programs. This body of work highlights the role of endogenous amenities in explaining households' location choice within a city (Ahlfeldt et al., 2015; Couture et al., 2019; Tsivanidis, 2019). Recently, Gaubert et al. (2021) introduced optimal taxation into a quantitative spatial model to rationalize place-based redistribution. Our work introduces a second tax instrument, housing assistance, that is used to redistribute across households through pecuniary effects, but also through endogenously-determined local amenities.

3.2 Background and Data

We give a brief overview of the history of the three main housing assistance programs –public housing, housing vouchers and LIHTC– and their relevant characteristics.[3] A combination of administrative datasets and real estate transactions from the Zillow database allows to investigate the impact of the major changes in the mix of housing assistance programs at the census tract level since the early 1990s. We conclude this section with descriptive facts mirroring the trade-off between pecuniary effects and amenity spin-offs.

3.2.1 Background

3.2.1.1 Historical Overview: From Public Housing to Subsidized Private Housing

Although public housing used to be the main housing assistance program until the early 1990s, tenant-based vouchers and privately-owned subsidized housing, mainly in the form of LIHTC, experienced an enormous increase in the past few decades.

Public housing was introduced in the late 1930s as a solution to housing affordability, but it proved unsustainable by the late 1980s. The Housing Act 1937 aimed at providing affordable housing for low-income people by constructing high-rise buildings that were to be managed by public housing authorities (PHAs). The initial intention was for the federal government to pay for the construction of public housing devel-

[3] For a more comprehensive description of housing assistance programs, see Collinson et al. (2019) and Vale and Freemark (2012).

opment and for PHAs to be in charge of their maintenance through rent revenues. However, PHAs failed to upkeep the public housing developments and, by the end of the 1980s, most of the high-rise buildings were in poor physical conditions and concentrated high levels of poverty and crime.

As a result, Congress approved the HOPE VI program in 1993, which demolished around 8% of the nation's public housing stock. Under this program, PHAs could apply for funds to demolish, revitalize or rehabilitate public housing developments that were considered to be "severely distressed". This resulted in 100,000 demolished units, 11,000 rehabilitated units and approximately 90,000 new projected units. Out of the latter, 13% are market rate units and the remaining 87% are affordable housing units, which might include public housing, LIHTC or other subsidized housing. An immediate result of the HOPE VI program was the displacement of many families living in public housing. Around half of them were relocated to other public housing, while the other half were relocated to housing vouchers.

By the time HOPE VI was approved, housing vouchers had become the largest housing assistance program by number of beneficiaries, and the Low-Income Housing Tax Credit (LIHTC) program had been introduced in 1986 to boost the production of mixed-income multifamily housing. Figure 3-1 illustrates the dramatic shift in the mix of housing assistance programs over the past few decades. As programs like HOPE VI downsized public housing, there are now twice as many beneficiaries from either tenant-based vouchers or the LIHTC program as from public housing.

3.2.1.2 Program Characteristics: Concentrated Poverty versus Mixed-Income Communities

We now summarize some relevant characteristics of each of the three main housing assistance program.

Eligibility and the level of subsidized rents is determined similarly for each housing assistance program. Eligibility is based on the total annual gross income and the family size. PHAs tier income limits defined as a percentage of the Area Median Income (AMI), and reserve some units for the poorest households. The rent level is fixed at 30% of the monthly adjusted income of the family, the rest being covered by PHAs up to a rent ceiling (known as *Fair Market Rent*).

The main argument in favor of vouchers and LIHTC over public housing is that the former programs aim to give the opportunity to beneficiaries to live in better neighborhoods without concentrating low-income individuals in high-rise buildings. Voucher recipients are free to choose any housing that meets the requirements of the program and are not limited to units located in subsidized housing projects. At least 20 percent of the tenants in LIHTC projects must earn less than 50 percent of the area median gross income (AMGI) or, alternatively, at least 40 percent of them must earn less than 60 percent of AMGI. The other units tend to be occupied by middle-income households.

As a compensation for welcoming low-income households and curbing rents, private owners participating in subsidized-housing programs receive tax credits. To boost the production of mixed-income multifamily housing, the LIHTC program allocates federal tax credits based on population to the states, which in turn award these credits to developers of qualified projects. Developers can sell these credits to investors to raise equity capital for their projects and reduce the amount of capital they would otherwise have to borrow. Hence, investors receive a dollar-for-dollar credit against their federal tax liability for a period of 10 years as long as the qualified project complies with the program guidelines.[4] In exchange for the credit benefits, developers must not only welcome a substantial share of low-income households, but also restrict rents, including utility allowance, in low-income units to 30 percent of the relevant income limit for a minimum affordability period of 30 years.

3.2.2 Data

We bring together various datasets to obtain a comprehensive picture of the evolution of housing assistance programs, house prices and socio-demographic characteristics at the 2010 census tract level for the period 1990-2010.

[4] A more detailed description can be found in Diamond and McQuade (2019).

Housing assistance programs. The first dataset compiles information from several sources to cover all public housing buildings active at any point between 1995 and 2018. We use data coming from 1996 HUD-951 forms,[5] which contains a snapshot of public housing building addresses, units and geographical coordinates for developments in that year, complemented with a similar dataset for the year 2018.

Administrative data on the HOPE VI program are issued by the Department of Housing and Urban development (HUD). The data report the magnitude and timing of the demolitions, as well as new construction for developments linked to a HOPE VI *revitalization grant* –those involving some reconstruction. For *demolition grants*, we use public data containing the award year and the number of demolished units at the project level. For the city of Chicago, we also include the list of non-HOPE VI demolished public housing units provided by the Chicago Housing Authority.

Lastly, data on the LIHTC and tenant-based vouchers come from two additional sources. First, the public LIHTC database which contains address-level information on LIHTC-financed projects for the period 1987-2019. Second, the Picture of Subsidized Households includes the number of households per program and census tract during the period 1993-2019, with some discontinuity over the time period.

House prices. Zillow's Ztrax data on real estate transactions includes information regarding all real estate transactions in the main U.S. metropolitan areas starting in the early 1990s, as well as property characteristics recorded from local tax assessor's data. For each house sale, the transaction dataset contains a transaction id, address, sale date, sale price, mortgage information, foreclosure status and other information collected by the local tax assessor. We merge this with other property characteristics that Zillow acquired from local assessors' offices.

We clean the data to include only residential arms-length transactions and eliminate outliers. For the former, we restrict the sample to property sales with a residential use and drop intra-family transactions. For the latter, we eliminate outliers by

[5]These are forms that public housing authorities (PHAs) were required to report to the Department of Housing and Urban development (HUD) containing information on all of their public housing buildings. This dataset is publicly available in the HUD website.

excluding transactions in the top percentile of the yearly price distribution.

Our main outcome of interest is constructed as a quality-adjusted house price index at the census tract level, following Baum-Snow and Han (2020) and Blanco (2021). The house price index ρ_{ict} is obtained from the regression:

$$\ln \mathrm{P}_{hict} = \rho_{ict} + \alpha_m + \gamma' \mathbf{X}_{hict} + u_{hict} \tag{3.1}$$

The left-hand side is the logarithm of the sale price of property h in census tract i in county c in year t. α_m are month-of-sale fixed effects that capture seasonality in sale prices, whereas $\mathbf{X}_{hict}$ is a vector of property characteristics, including building type, building age dummies, lot size, lot size squared, number of stories, number of bedrooms and roof cover type.[6] We define the house price index as the census tract-county-year fixed effects in the regression above, ρ_{ict}.

Other data sources. Two additional sources complete our data. First, local data on demographic, socioeconomic and housing characteristics at the census tract level come from the decennial census for years 1990, 2000, 2010, downloaded from National Historical Geographic Information System (NHGIS). Second, we exploit a shapefile of the rail transit network in the United States in the years 1980, 1990, 2000 and 2004 from the National Transportation Atlas Database to construct an instrument for neighborhood composition changes based on subway station openings.[7]

3.2.3 Descriptive Evidence

We document the shift from public housing to subsidized private housing and how it affected the most exposed areas in a group of seven cities.[8] We uncover three

[6]Since some property characteristics are missing from some transactions, we generate dummy variables for missing values for each property characteristic except building type (which is never missing) and re-code missing values as zeros. In the regression, we include a term interacting each characteristic's missing dummy variable with building type to flexibly account for heterogeneity in that characteristic across property types when data is missing.

[7]We gratefully acknowledge Nathaniel Baum-Snow for sharing these data, used in Baum-Snow et al. (2005).

[8]Namely, Atlanta, Baltimore, Chicago, Memphis, Newark, Pittsburgh and Washington, DC.

main facts: First, the decline in public housing was far from offset by new construction, whether subsidized or not; second, the local demographic composition changed dramatically following public housing demolitions; third, demolition exposure is associated with price hikes.

The HOPE VI program led to a sharp reduction in public housing units, driving down housing supply. While the program financed the demolition of approximately 48,000 units in these cities, only around 6,600 units (14%) were rebuilt as public housing. The bulk of the new construction relied on other types of affordable (private) housing, of which almost 13,000 units were built. Figure 3-2 plots these numbers by city. Although this pattern repeats across cities, Chicago is a notable outlier, accounting for almost half of demolished units in the sample and with only 7% of them being regenerated as public housing.

Table 3.1 shows that neighborhoods exposed to public housing demolitions experienced substantial demographic changes, pointing at plausible local amenity effects. Between 1990 and 2010, tracts with demolitions increased their education levels and per capita income by more than twice the average remaining tract.[9]

Finally, we show that exposure to public housing demolitions is associated with local housing price hikes, which suggests a joint effect of reduced housing supply and improved amenities. To proceed, we regress the change in the house price index between the early 1990s and 2010 in census tract i on a demolition exposure index, defined as as follows:

$$\text{Demolition exposure}_i = \frac{1}{H_i^{1990}} \sum_j \frac{1}{\exp d_{ij}} \times \text{ Demolished units}_i \tag{3.2}$$

where H_i^{1990} is the baseline number of housing units in the tract and d_{ij} is the distance between census tracts i and j. That is, the index captures the number of demolished units weighted by their distance to census tract i. After controlling for several baseline characteristics, the relationship between house price changes and demolition exposure

[9]Tach and Emory (2017) provide a detailed analysis of how the demographic composition of these neighborhoods changed after the implementation of the HOPE VI program.

is positive and very significant (Figure 3-3).

To provide suggestive evidence of a supply channel, Table 3.2 reproduces the regression above but also interacts the demolition exposure index with the median household income tercile of the census tract (within each city). The table shows that, for a given level of demolition exposure, house prices increased by more in low-income tracts –consistent with the findings in Chapter 1's Appendix Fig. A-9. This fact, which is robust to including several control variables, suggests that housing in tracts that competed directly with public housing suffered higher increases due to a reduction in public supply. Conversely, richer tracts also experiencing higher house prices may be explained by richer households valuing more amenity changes.

3.3 A Quantitative Model with Income Taxation and Housing Assistance

We develop a quantitative urban model to analyze the distributional consequences of housing assistance programs. The main departure from the existing literature is the introduction of two types of tax instruments, non-linear income taxation and housing assistance, that are used by the government to redistribute across households. We assume that the set of taxes that can be imposed on the workers are limited to be a function of their total income, but not their type, following Mirrlees (1971). This restriction creates a trade-off between redistribution and productive efficiency. In the spirit of Naito (1999), housing assistance programs complement non-linear income taxation by affecting the pre-taxation income distribution—specifically, the distribution of land rents.

3.3.1 Environment

The starting point of this model is an urban framework where the city is assumed to be a collection of neighborhoods distant from each other. This city is populated by heterogeneous workers who supply labor elastically and own land. Redistribution is achieved through two distinct policies: non-linear income taxation and means-tested housing assistance.

3.3.1.1 Setup

The city is comprised of $\mathcal{I}$ locations or neighborhoods, that differ in their fundamental levels of amenity and productivity, their land supply and their distances to other locations. Three types of agents step in: workers, producers and developers.

Households, indexed by Θ, are heterogeneous in skill, preferences over locations, land ownership and family characteristics. A type-Θ individual chooses a location i in which to live and a location j in which to work. He derives utility from the consumption of tradable goods and residential floorspace, but incurs a disutility from supplying labor. Poor households spend a relatively higher share of their revenues on residential floorspace.

Good production and floorspace development occur in the various neighborhoods of the city. Both sectors are perfectly competitive. Producers assemble labor supplied by the different skill groups, commercial floorspace and intermediate goods into tradable goods. Developers use labor, intermediate goods and an additional fixed factor, land, to develop residential and commercial floorspace which is rented to workers and producers respectively.

Government redistributes across households with two policy instruments: non-linear income taxation and means-tested housing assistance. We assume that these policies are restricted to be a function of their total income, but not their type, following Mirrlees (1971). In particular, the government can't disentangle between labor supply and skill, and does not observe the composition of income between labor wages and land rents. As a result, a redistribution-efficiency trade-off arises. As in Naito (1999), housing assistance programs complement non-linear income taxation by affecting the pre-taxation income distribution. The government implements three distinct programs: public housing, subsidized private housing supply, and tenant-based vouchers. In equilibrium, housing assistance programs redistribute from landlords towards low-skilled households through two channels. First, directly by providing subsidized housing to the poorest workers. Second, indirectly by distorting equilibrium prices on the private housing market.

3.3.1.2 Preferences

Workers have weakly separable preferences between consumption and labor supply. They derive utility from consuming tradable good, c, and residential floorspace, h, and local amenities, a_i, but experience disutility from supplying labor, n. The utility of worker Θ is given by:

$$U(\Theta) = u(V(\Theta), n(\Theta), a_i; \Theta), \tag{3.3}$$

$$V(\Theta) = v(c(\Theta), h(\Theta)), \tag{3.4}$$

where $V(\Theta)$ is the sub-utility that worker Θ derives from consumption of tradable goods $c(\Theta)$ and housing $h(\Theta)$, and $n(\Theta)$ is his labor supply. We assume that the both utility functions $u(\cdot;\Theta)$ and $v(\cdot)$ are quasi-concave and strictly increasing.

Workers are distinguished by their multi-dimensional type $\Theta = (\theta, \varepsilon, \omega, \xi)$ with distribution F. The parameter θ indexes the household's skill, ε is a vector of idiosyncratic preference shocks for living in each location $i \in \mathcal{I}$, ω captures land ownership and ξ family characteristics, e.g., family size, that affect government redistributive preferences.

3.3.1.3 Technology

Tradable good production and floorspace development take place in each neighborhood.

Good production. Producers assemble tradable goods in every neighborhood j. They use labor inputs, $N_j^Y = \left(n_j^Y(\theta)\right)$, intermediate goods, M_j^Y, and commercial floorspace, H_j^Y. Their production technology Y_j is neighborhood-specific:

$$Y_j = Y_j\left(N_j^Y, M_j^Y, H_j^Y\right) \tag{3.5}$$

It exhibits constant returns to scale.

Floorspace development. Developers provide residential and commercial floorspace to households and producers in all neighborhoods. The presence of a fixed factor, land, induces decreasing returns to scale. Land ownership is split between private households and the government. Floorspace supply in sector $s \in \{P, G\}$ is given by:

$$H_{j,s} = H_{j,s}\left(N^H_{j,s}, M^H_{j,s}\right), \tag{3.6}$$

with $N^H_{j,s} = \left(N^H_{j,s}(\theta)\right)$ labor inputs and $M^H_{j,s}$ are intermediate goods, for $j \in \mathcal{I}$.

Amenity and productivity spillovers. Amenities in neighborhood i depend on the local distribution of types:

$$a_i = \bar{a}_i a(\mathbb{E}_{|i}\left[\theta\right]), \tag{3.7}$$

where $\bar{a}_i$ is the fundamental level of amenities in location i, a is some function of average skill of workers living in i, $\mathbb{E}_{|i}\left[\theta\right]$.

Similarly, we assume that productivity in neighborhood j is a function of the local distribution of workers:

$$y_j = \bar{y}_j y(\mathbb{E}_{|j}\left[\theta\right]), \tag{3.8}$$

where $\bar{y}_j$ is the fundamental level of productivity in location j, y is some function of the average skill of workers employed in j, $\mathbb{E}_{|j}\left[\theta\right]$.

3.3.1.4 Taxation and Public Policy

To redistribute across workers, the government implements two types of policy instruments: non-linear income taxation and means-tested housing assistance.

Income taxation. The government levies a non-linear income tax over total income. A worker of type Θ will retain:

$$x(\Theta) - T\big(x(\Theta)\big) \tag{3.9}$$

where $x(\Theta)$ is total income, which is comprised of labor income, as well as rental incomes from capital and land:

$$x(\Theta) = w(\theta)n(\Theta) + \sum_j \omega_j \Pi_j \tag{3.10}$$

Here, $w(\theta)$ denotes worker's wage, $n(\Theta)$ his labor supply, ω_j the shares of land used in location j floorspace development that he owns, and Π_j the total land rents in location j.

By assumption, both lump-sum transfers and factor-specific linear taxes are ruled out, so that neither the Second Welfare Theorem nor linear taxation results à la Diamond and Mirrlees (1971a,b) apply.

Housing assistance. Government provides housing assistance through three different means-tested programs: public housing supply (P), subsidized private housing (S), and vouchers (V). The first one, public housing, is a rent subsidy that is only available to workers renting residential floorspace developed on government-owned land. The other two, subsidized private housing and vouchers, are rent subsidies which are only available to households renting housing on the private market. The three different programs are modeled as *ad valorem* subsidies. Finally, we assume that subsidies from different programs can't be combined, so that a household only receives the most advantageous subsidy he is entitled to.

The total rent subsidy received for program $\pi \in \{P, S, V\}$, $\tau^{\pi}_{i,s}(x_{ij}(\Theta), \xi)$, may depend workers' income, $x_{ij}(\Theta)$, some observable characteristics such as family status, household size or age, ξ, neighborhood of residence, i, and sector $s \in \{P, G\}$. Specifically, we assume that $\tau^{P}_{i,P} \equiv 0$ and $\tau^{S}_{i,G} = \tau^{V}_{i,G} \equiv 0$ so that subsidized private housing and vouchers are only available to tenants on the private market. Voucher subsidies do not depend on residence, so $\tau_{i,G}^{V} \equiv \tau_{G}^{V}$. When a household is not eligible for a program, we simply write that he does not receive any subsidy, that is, $\tau_{i,s}^{\pi}(x_{ij}(\Theta), \xi) = 0$.

3.3.1.5 Closing the Model

Having described the model's primitives, we specify ownership and market structure in order to close the model. Ownership of fixed factors is split between the government and private households. Wages and prices are determined competitively. The city is assumed to be closed.

Ownership. Each type-Θ worker owns a share ω_j of privately-owned land used to produce floorspace in location j. Shares add up to 1 so:

$$\int \omega_j dF(\Theta) = 1, \quad j \in \mathcal{I} \tag{3.11}$$

Land used to produce public housing is entirely owned by the government.

Market structure. Production, labor and housing markets are perfectly competitive. All agents are price-takers.

Closed city. The mass of workers of each type Θ is equal to $f(\Theta)$.

3.3.2 Equilibrium

This subsection lays out the equilibrium behaviors of the different agents in order to define an equilibrium in this model.

3.3.2.1 Workers

Workers choose their residence, workplace, labor supply and consumption of goods and housing to maximize their utility.

Good and housing demands. Having chosen a residence i, a workplace j and a labor supply level $n_{ij}(\Theta)$, a worker chooses optimally his consumption of tradable goods, c, and of residential floorspace, h, given the local price index of consumption goods, P_i, and the local rent he has to pay, $R_i(\Theta)$, defined as:

$$R_{ij}(\Theta) \equiv \min\left\{ \Big(1 - \tau^{\pi}_{i,s}(x_{ij}(\Theta), \xi)\Big) R^R_{i,s} \;\middle|\; s \in \{P, G\}, \pi \in \{P, S, V\} \right\}, \tag{3.12}$$

where $R_{i,s}$ is the prevailing rent in the housing market of sector $s \in \{P, G\}$. The worker's budget constraint is thus given by:

$$P_i c + R_{ij}(\Theta) h \leq x_{ij}(\Theta) - T\big(x_{ij}(\Theta)\big) \tag{3.13}$$

Conditional on residence i and income $x_{ij}(\Theta)$, workers pick their good and housing consumptions, $c_{ij}(\Theta)$ and $h_{ij}(\Theta)$, by solving:

$$V_{ij}(\theta) \equiv \max_{c,\, h} \; v(c, h), \quad \text{subject to (3.13)} \tag{3.14}$$

Labor supply. Conditional on their residence, i, and workplace, j, workers choose their labor supply, $n_{ij}(\Theta)$, to maximize their utility:

$$U_{ij}(\Theta) \equiv \max_{n_{ij}(\Theta)} \; U(V_{ij}(\Theta), n_{ij}(\Theta); \Theta) \tag{3.15}$$

Choice of residence and workplace. Workers pick their residence $i(\Theta)$ and workplace $j(\Theta)$ to maximize their utility given :

$$\max_{i,j \in \mathcal{I}} \; U_{ij}(\Theta) \tag{3.16}$$

3.3.2.2 Producers

Producers assemble labor inputs, $N^Y_j = \big(N^Y_j(\theta)\big)$, intermediate goods, M^Y_j, and commercial floorspace, H^Y_j into a quantity Y_j of goods, taking prices on input and output markets as given. To produce a quantity Y_j of goods, they solve the following cost minimization problem:

$$\min_{N_j^Y, M_j^Y, H_j^Y} \int w_j(\theta) N_j^Y(\theta) d\theta + P_j M_j^Y + R_i^C H_j^Y, \tag{3.17}$$

subject to:

$$Y_j\big(N_j^Y, M_j^Y, H_j^Y\big) \geq Y_j \tag{3.18}$$

3.3.2.3 Developers

Developers of both sectors $s \in \{P, G\}$ use labor inputs $N_{j,s}^H = \big(N_{j,s}^H(\theta)\big)$ and intermediate goods $M_{j,s}^H$ to produce floorspace, and sell it to workers and producers. They take prices of inputs and outputs as given.

Input demands. Developers minimize the cost of inputs:

$$\min_{N_{j,s}^H, M_{j,s}^H} \int w_{j,s}(\theta) N_{j,s}^H(\theta) d\theta + P_j M_{j,s}^H, \tag{3.19}$$

subject to:

$$H_{j,s}\big(N_{j,s}^H, M_{j,s}^H\big) \geq H_{j,s} \tag{3.20}$$

Floorspace use allocation. Floorspace built on privately-owned land may be used for either residential or commercial floorspace. Developers choose the fraction λ_i if floorspace allocated to residential use to maximize profits. They allocate floorspace to its most profitable use, so that:

$$\begin{cases} \lambda_i \in (0,1) & \Rightarrow \quad R_{i,P}^R = R_{i,P}^C, \\ R_{i,P}^R > R_{i,P}^Y & \Rightarrow \quad \lambda_i = 1, \\ R_{i,P}^W < R_{i,P}^Y & \Rightarrow \quad \lambda_i = 0. \end{cases} \tag{3.21}$$

3.3.2.4 Definition of a Decentralized Equilibrium

Having characterized the equilibrium behavior of each agent, we define the decentralized equilibrium of this model. We describe the equilibrium conditions on the labor, good and housing markets, before giving the formal definition of a decentralized equilibrium.

Labor market equilibrium. Labor is used by both producers and developers. In equilibrium, the total labor inputs each skill θ used by producers and developers in each workplace j has to be equal to sum over i of labor supplied in j by i residents:

$$\sum_i n_{ij}(\theta)L_{ij}(\theta) = N_j^Y(\theta) + N_{j,P}^H(\theta) + N_{j,G}^H(\theta) \tag{3.22}$$

Good market equilibrium. Goods produced in each location j are used for consumption by workers and as intermediates by producers and developers. Geography is captured by iceberg trade frictions $\chi_{jl} \geq 1$. That is, producers in location j must ship $\chi_{jl}Q_{jl}$ units to location l for Q_{jl} units to arrive. The feasibility constraint for tradable goods implies:

$$Y_j \geq \sum_l \chi_{jl}Q_{jl}, \tag{3.23}$$

where Y_j is the production in location j and Q_{jl} is the sum of goods used by workers, producers and developers in location l. Tradable goods are differentiated by origin and aggregated through a homothetic and concave aggregator Q. Feasibility constraint for traded good good imposes:

$$Q(Q_{1i}, \ldots, Q_{Ii}) = M_i^Y + M_i^H + \sum_j \int c_{ij}(\theta)L_{ij}(\theta)d\theta, \tag{3.24}$$

for each location i. This flexible functional form covers in particular perfect substitution as in Rosen (1979) and Roback (1982)'s seminal model and constant elasticity of substitution (CES) à la Armington (1969), as in standard economic geography models.

Floorspace market equilibrium. Both private and public floorspace markets are in equilibrium. We also assume that developers cannot price discriminate across workers, so that the residential rent $R_{i,s}^R$ is the same for all units of a same sector.

Floorspace produced by developers is divided between residential and commercial floorspace in the private sector, so that:

$$H_{i,P}\big(N_{i,P}^{H}, M_{i,P}^{H}\big) = H_i^Y + \int h_{ij,P}(\Theta) L_{ij,P}^{R}(\Theta) d\Theta \tag{3.25}$$

Floorspace built on public land is reserved for residential use, so:

$$H_{i,G}\big(N_{i,G}^{H}, M_{i,G}^{H}\big) = \int h_{ij,G}(\theta) L_{ij,G}^{R}(\Theta) d\Theta \tag{3.26}$$

Definition of a decentralized equilibrium. Before defining a decentralized equilibrium, we introduce the convenient definition of an allocation.

Definition 1 (Allocation) *An allocation, $\mathcal{A}$, is the specification of a partition of workers, $(i(\Theta), j(\Theta))_{\Theta}$, associated per capita consumptions of tradable goods and housing, $(c_{ij}(\Theta), h_{ij}(\Theta))_{\Theta,i,j\in\mathcal{I}}$, labor inputs used in the production and development sectors, $(N_j^Y(\Theta))_{\Theta,j\in\mathcal{I}}$ and $(N_{j,s}^H(\Theta))_{\Theta,j\in\mathcal{I},s\in\{P,G\}}$, intermediate goods used in the production and development sectors, $(M_j^Y)_{j\in\mathcal{I}}$ and $(M_{j,s}^H)_{j\in\mathcal{I},s\in\{P,G\}}$, floorspace used in the production of tradable goods, $(H_j^Y)_{j\in\mathcal{I}}$, goods produced and floorspace developed, $(Y_j)_{j\in\mathcal{I}}$ and $(H_{j,s})_{j\in\mathcal{I},s\in\{P,G\}}$.*

Having determined the equilibrium behavior of each agent individually, we now summarize the above conditions to define a decentralized equilibrium.

Definition 2 (Decentralized Equilibrium) *A decentralized equilibrium is an allocation $\mathcal{A}$ such that:*

(i) Workers consume tradable goods and housing to maximize their utility subject to their budget constraint, conditions (3.14), (3.13), *and choose their residence optimally, condition* (3.16)*;*

(ii) Producers choose labor inputs and intermediate goods optimally, conditions (3.17) *and* (3.18)*;*

(iii) *Developers choose labor inputs and intermediate goods optimally, conditions* (3.19) *and* (3.20)*, and allocate optimally floorspace between residential and commercial use,* (3.21)*;*

(iv) *Goods are aggregated optimally, condition* (3.24)*;*

(v) *Labor, good and housing markets clear, conditions* (3.22)*,* (3.23)*,* (3.25) *and* (3.26)*.*

3.4 Model Estimation: Leveraging Public Housing Demolitions

We map the key parameters of the model developed in Section 3.3 to reduced-form counterparts of structural identities and estimate them by leveraging public housing demolitions as a quasi-experimental shock.

3.4.1 Quantitative Implementation

This subsection exposes the preliminary steps necessary to take the model developed in Section 3.3 to the data. We specify the functional forms and describe the estimation procedure.

3.4.1.1 Functional Forms

We start by defining the three key parameters of the model which will be estimated in Section 3.4.2.2: ζ, the elasticity of floorspace supply, σ, that captures the residential mobility of workers, and μ^A, which captures the strength of residential spillovers. We then specify the utility and production which are all Cobb-Douglas and will be calibrated in Section 3.4.2.1.

Key parameters. We assume that the floorspace production function is:

$$H_{j,s} = \bar{h}_{j,s}(M^H_{j,s})^{\frac{\zeta}{1+\zeta}}, \tag{3.27}$$

with $\bar{h}_{j,s}$ the fundamental floorspace supply and ζ the floorspace supply elasticity.

Idiosyncratic residence draws, $(\varepsilon_{g,i})$ follow a Fréchet distribution with dispersion parameter σ and are independent of the other components of Θ. This parameter captures the strength of idiosyncratic preferences for locations and is inversely proportional to residential mobility.

The amenity spillovers are modelled as follows:

$$a(\Theta_i^R) = (\bar{\theta}_i)^{\mu^A}, \tag{3.28}$$

with $\bar{\theta}_i$ the mean skill level in neighborhood i and μ^A the strength of residential spillovers.

Utility, production and matching functions. We assume that workers have Stone-Geary preferences over tradable goods and housing:

$$u(c,h) = c^{\gamma}(h - \bar{h})^{1-\gamma}, \tag{3.29}$$

with $\gamma \in (0,1)$.

Producers use a Cobb-Douglas technology given by:

$$Y_j = y_j(\bar{N}_j^Y)^{\alpha^Y}(M_j^Y)^{\beta^Y}, \tag{3.30}$$

where composite labor $\bar{N}_j^Y = \int_\theta n_{g,j}(\theta) L_j^E(\theta) d\theta$. The labor share is α^Y.

Finally, the skill draws θ follow a Pareto distribution with shape parameter ρ:

$$G(\theta) = \frac{1}{\theta^\rho}, \quad \theta \geq 1, \tag{3.31}$$

while the ω_j's are uniform over $[0,1]$ and χ is non-random.

3.4.1.2 Model Inversion

Fundamental location characteristics such as productivities, amenities and housing supplies cannot be directly observed in the data. While the presence of local amenity and productivity spillovers allows for the possibility of multiple equilibria, we are able to recover unique values of intrinsic components of productivities, amenities, and housing supplies that rationalize the observed data as a model equilibrium.

This inversion process follows closely the steps outlined in Ahlfeldt et al. (2015) and Tsivanidis (2019). We combine those observed data with the model structure to solve for the endogenous variables and back out the unobservable amenities, productivities and housing supplies.

Proposition 1 (Model Inversion)

1. *Given data on residence by type, $(L_i^R(\Theta))$, total employment by workplace, $(\bar{L}_j^E)$, in addition to model parameters, there exists a unique vector of labor input prices, $(w_j(\theta))$ that rationalizes the observed data as an equilibrium of the model.*

2. *Given model parameters, data on residence by type, $(L_i^R(\Theta))$ and rent levels $(R_{i,s})$, and a vectors of labor input prices, $(w_j(\theta))$, there exist unique vectors of unobservable amenities $(a_i(\Theta))$ (to scale), productivities (y_j) and housing supplies $(h_{i,s})$ that rationalize the observed data as an equilibrium of the model.*

3.4.2 Estimation

We now implement the method exposed in Section 3.4.1 to estimate the model. We start with calibrated parameters, before switching to the estimation of the key structural parameters.

3.4.2.1 Calibrated Parameters

Parameters $\{\gamma, \rho, \alpha^Y, \beta^Y\}$ are calibrated either directly from the data or to existing values from the literature. We set the share of housing expenditure for workers to the commonly used value of $1 - \gamma = 0.3$ for the United States. The shape of the Pareto distribution of skill draws θ is estimated from wage data using a Hill's estimator, and is approximately equal to 2. The shares of labor and equipment correspond to their estimates in Greenwood et al. (1997), renormalized to exclude structures which are absent from the model.

3.4.2.2 Estimation of Key Structural Parameters

The key parameters of our model are the housing supply and demand elasticities, and the local amenity spillovers parameter. These parameters will allow us to disentangle the effects of the housing policy shift: pecuniary effects from reduced public housing supply and increased demand for private housing, and amenity improvements from poverty deconcentration.

Housing supply elasticity ζ. From Eq. (3.27), it follows that:

$$\Delta \ln \mathrm{H}_i = \alpha^s + \zeta \Delta \ln \mathrm{P}_i + u_i^s \tag{3.32}$$

We estimate the housing supply elasticity ζ using changes in the housing stock ($\Delta \ln \mathrm{H}_i$) and the house price index ($\ln \mathrm{P}_i$) in Eq. (3.1) between the early 1990s and 2010 at the census tract i level. Crucially, we need an instrument for the house price index that isolates variation in housing demand.

Conveniently, public housing demolitions via the HOPE VI program can be interpreted as a large demand shock for private housing developers. The reduction in public housing supply increased the residual demand for nearby private housing, which led to substantial house price increases. Hence, we use the demolition exposure index in Eq. (3.2) as an instrument for house prices. We estimate Eq. (3.32) using a two-stage least squares approach on our sample of seven cities.

Table 3.3 shows the results. Columns (1) reports the OLS estimate of ζ, which is slightly positive at 0.08. Columns (2) and (3) instrument for house prices only controlling for county fixed effects, for which the first stage is very strong and significant. In this specification, we find that $\zeta \simeq 0.51$. When we also control for baseline census tract characteristics in columns (4) and (5), we find that the first stage becomes even stronger and that the supply elasticity estimate slightly decreases to $\zeta \simeq 0.44$. This estimate implies that a 1% increase in house prices leads to a 0.44% increase in housing supply. As expected, the OLS estimate is biased downwards.

The estimated housing supply elasticity is within the range of estimates in the literature. For the sample of seven cities in this paper, Saiz (2010) estimated that the metropolitan area level housing supply elasticity ranged from 0.81 to 2.55. Note that we estimate housing supply elasticities at the census tract level, since we are also interested in studying how the distribution of housing policies affects housing supply across neighborhoods within the same metropolitan area. As a result, we should expect housing supply elasticities to be smaller –i.e., supply is necessarily more constrained in smaller geographical units. More recently, Baum-Snow and Han (2020) estimate housing supply elasticites at the census tract level for US metropolitan areas using labor demand shocks to commuting destinations as an instrument. Their work implies an average housing supply elasticity between 0.15 and 0.23 for the seven metropolitan areas considered in this paper. Reassuringly, our estimate is close to this range.

Residential mobility σ and local amenity spillovers μ^A. Following Tsivanidis (2019), it can be shown that the housing demand elasticity (a function of the residential mobility parameter, $\varepsilon^d = (1-\gamma)\sigma$) and the local amenity spillovers parameter can be estimated from the following housing demand equation:

$$\Delta \ln \mathrm{H}_i = \alpha^d + \varepsilon^d \Delta \ln \mathrm{P}_i + \mu_A \Delta \ln \mathrm{Low\ Skill}_i + u_i^d \tag{3.33}$$

where $\varepsilon^s = (1-\gamma)\sigma$ is the housing demand elasticity. Again, we can estimate the equation above using changes in the housing stock ($\Delta \ln \mathrm{H}_i$), the distribution of workers (share of workers with a high school degree or less, $\Delta \ln \mathrm{Low\ Skill}_i$), and the house price index ($\ln \mathrm{P}_i$).

We use two different instruments that affect both the supply of housing and the local skill composition quasi-exogenously. Our first instrument relies on LIHTC eligibility rules: Qualified Census Tracts (QCTs) receive larger subsidies for the construction of low-income units. We exploit the fact that HUD defines QCTs for the LIHTC programs with a discontinuous rule. Specifically, census tracts must fulfill on of these two requirements: (1) tract median income is below 60% of the area median income, or (2) poverty rate is above 25%. We follow Davis et al. (2019a), and combine these two rules into a single index of QCT eligibility E_i, where $E_i = \max\{\text{poverty rate}_i - 0.25, 0.6 - \text{median income index}_i\}$, where the median income index is the ratio of the median income in the tract over the area median income. Our instrument is defined as $1_{E_i>0} + E_i + 1_{E_i>0}E_i$. In our context, we think that this instrument affects both house prices (Diamond and McQuade, 2019) and neighborhood composition by shifting housing supply.

Our second instrument is distance to rapid transit lines. Baum-Snow and Kahn (2000) estimate the impact of the distance to new rail transit lines (mostly built in the 1980s) on their usage and housing values. They document that decreasing distance to transit from 3 to 1km away increases rents by \$19 per month and housing values by around \$5,000. Kahn (2007) shows that increased access to rapid transit lines increases gentrification, which affects neighborhood composition. We use changes in the distance (in km) to a rapid transit line from 1980 to 2004 as an instrument —we include the period before 1990 because it is where most of the variation takes place.

Table 3.4 reports the results for the OLS and 2SLS specifications. Regarding the housing demand elasticity, we find an estimate of about -0.25 using the instrumental variables approach –the OLS estimate of 0.08 is substantially upward biased. This implies $\sigma \simeq 0.83$. The elasticity estimate is similar but slightly smaller than other estimates in the literature. Mayo (1981) provides a comprehensive review of different studies that find housing demand elasticities below one (in absolute value). Some close estimates are Polinsky and Ellwood (1979) and Hanushek and Quigley (1980), who found experimental estimates of -0.64 and -0.45 in Pittsburgh and Phoenix, respectively, at the metropolitan area level. More recently, Albouy et al. (2016) found a demand elasticity of -2/3 using cross-sectional 2000 decennial census data.

Regarding preferences for local amenities, we find that $\mu^A \simeq -0.35$. That is, increasing the percentage of workers with a high-school degree or less by 1% decrases the housing stock in a census tract by about -0.35%. Note that our estimate is smaller than in previous research. For example, Tsivanidis (2019) finds an elasticity between 1.23 and 1.91 of the population share with more than a high-school degree with respect to low and high-skilled residents in an area, respectively, in his study of the introduction of a bus rapid line in Colombia.

3.5 Conclusion

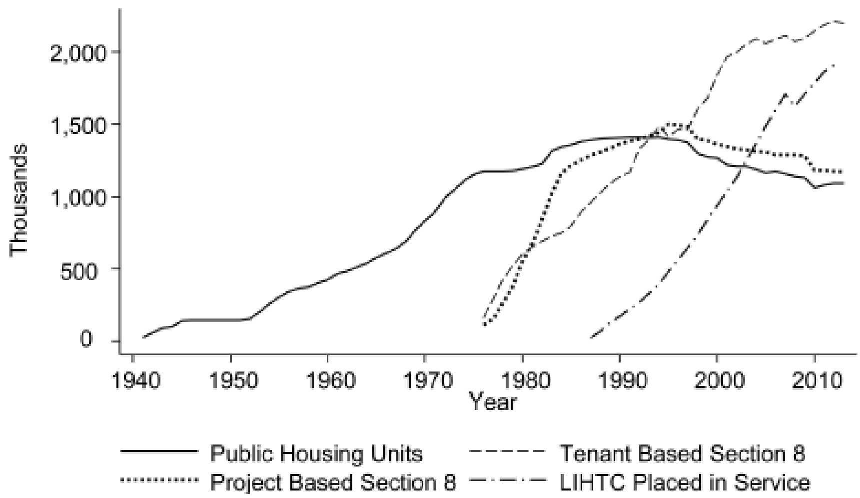
In this paper, we developed an urban quantitative model to assess the distributional implications of the recent U.S. housing policy shift from public housing to subsidized private housing programs.

Our model captures the trade-off in housing assistance programs between indirect pecuniary redistribution –by affecting general equilibrium prices– and direct amenity effects –by concentrating or deconcentrating poverty. In the model, public housing assistance programs complement non-linear income taxation to redistribute across households. In this framework, public housing may improve redistributional efficiency, but at the expense of lower local amenities for low-income households. We then estimated the structural parameters of our model leveraging demolition of public housing, among other quasi-exogenous policy changes, and found estimates that are close to prior literature.

In future work, we intend to apply this model to assess the incidence of the shift from public housing to subsidized private housing and to benchmark our results against an optimally-designed housing assistance program.

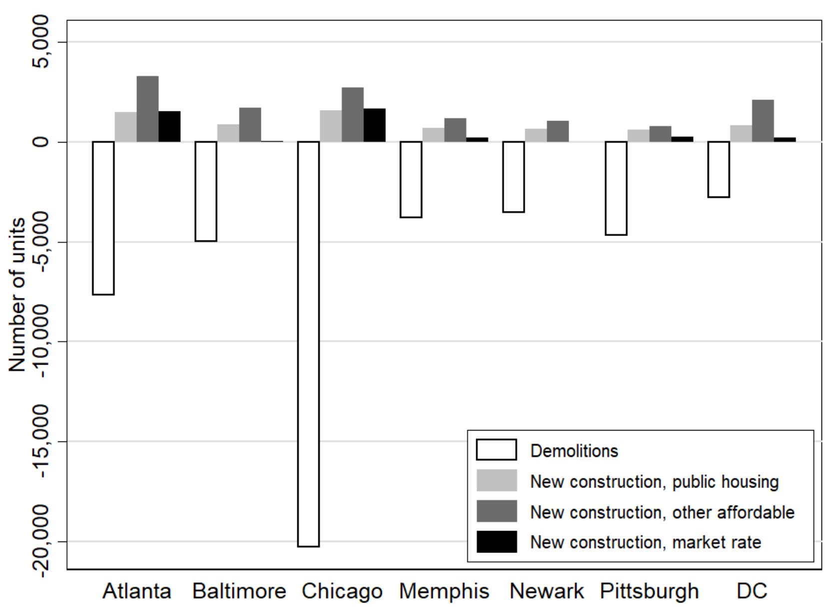
Figures

Figure 3-1: Number of beneficiaries by housing assistance program



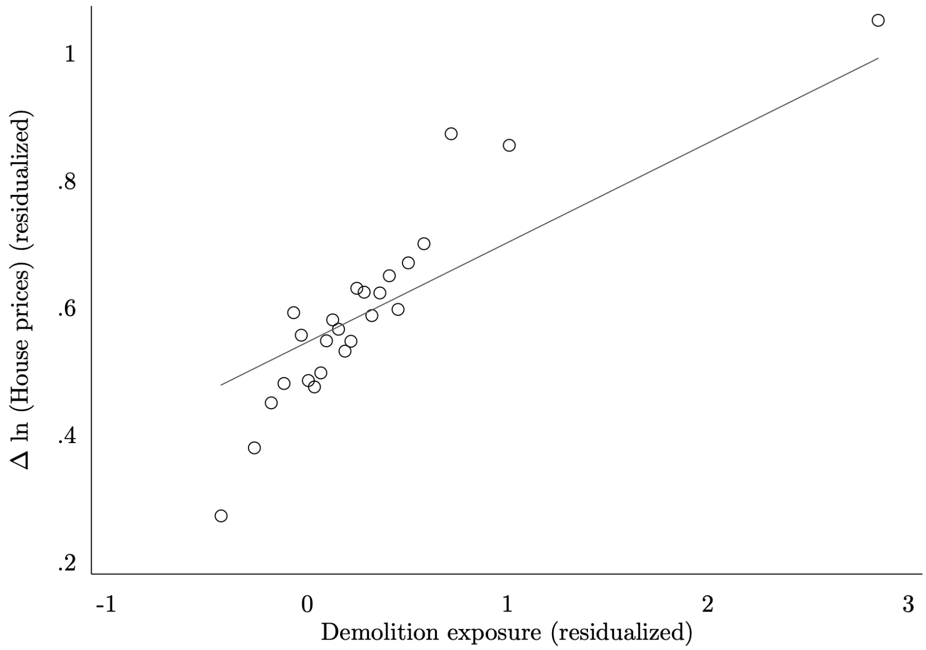
Source: Collinson et al. (2019)

Figure 3-2: Demolition and reconstruction under HOPE VI



Notes: The barplot shows the number of units demolished and constructed between 1995 and 2011 as reported in HOPE VI data. The category "New construction, public housing" is a conservative estimate: it also includes new construction labeled as a mix between public housing and other affordable housing. The category "New construction, other affordable" includes both LIHTC units and units generally labeled as affordable.

Figure 3-3: Increased exposure to demolitions raises house prices



Notes: The figure is a binned scatter plot of the increase in (the logarithm of) house prices between 1990 and 2010 on the demolition exposure index, after residualizing them for several baseline characteristics in 1990 (education levels, income per capita, black share and the number of housing units in 1990), the change in the share of housing units owned by the public sector, and county fixed effects.

Tables

Table 3.1: Exposure to demolitions correlates with compositional changes

	Pre-demo: 1990		**Post-demo: 2010**		**Change (%)**	
	Other	Demo	Other	Demo	Other	Demo
Population	3,577	3,047	3,744	2,137	5	-30
Black share	0.30	0.83	0.35	0.73	15.28	-12.10
Educ: $\geq$ bachelor	0.23	0.08	0.33	0.23	44.33	193.26
Per capita income	16,176	6,503	30,859	20,244	91	211
Housing units	1,496	1,323	1,667	1,116	11	-16
Demolished units	0	423	0	423		
Observations	2826	123	2826	123	1	1

Notes: This table reports the mean of several census tract characteristics for census tracts affected by demolitions ("Demo") and remaining tracts in the included counties ("Other"). The included counties belong to the cities of Atlanta, Baltimore, Chicago, Memphis, Newark, Pittsburgh and Washington, DC.

Table 3.2: Exposure to demolitions raises house prices more in low-income areas

	(1)	(2)	(3)
Demolition exposure	0.074***	0.073***	0.086***
	(0.019)	(0.020)	(0.018)
Demolition exposure × Low Income	0.117**	0.120***	0.131***
	(0.036)	(0.030)	(0.030)
Demolition exposure × High Income	0.041	0.040	0.050**
	(0.025)	(0.026)	(0.018)
Baseline prices	No	Yes	Yes
Baseline census chars.	No	No	Yes
County FE	Yes	Yes	Yes
N	2837	2837	2837

Notes: All columns include fixed effects for low and high income census tracts. Baseline census characteristics contain education levels, black shares and the number of housing units in 1990. The included counties belong to the cities of Atlanta, Baltimore, Chicago, Memphis, Newark, Pittsburgh and Washington, DC. Standard errors in parenthesis, clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.3: Estimation of housing supply elasticity ζ

	(1)	(2)	(3)	(4)	(5)
	OLS	1st stage	2nd stage	1st stage	2nd stage
Δ ln P (ε^s)	0.08***		0.51***		0.44***
	(0.01)		(0.09)		(0.06)
Demo exposure		0.11***		0.16***	
		(0.01)		(0.01)	
Observations	2,833	2,833	2,833	2,833	2,833
R-squared	0.17	0.27		0.37	0.03
County FE	Y	Y	Y	Y	Y
Controls	Y	N	N	Y	Y
F-stat		93.49		189.7	

Notes: This tables estimates Eq. (3.32). Control variables include the following census tract characteristics in 1990: share of low-educated households, income per capita, black share, number of housing units, and public housing share of units. The included counties belong to the cities of Atlanta, Baltimore, Chicago, Memphis, Newark, Pittsburgh and Washington, DC. Standard errors in parenthesis, clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3.4: Estimation of residential mobility σ and local amenity spillovers μ^A

	(1)	(2)
	OLS	IV
Δ ln P (ε^s)	0.08*	-0.25***
	(0.04)	(0.05)
Δ ln(Low educ)	-0.06***	-0.35***
	(0.02)	(0.13)
Observations	2,815	2,207
R-squared	0.12	
County FE	Y	Y
Controls	N	Y

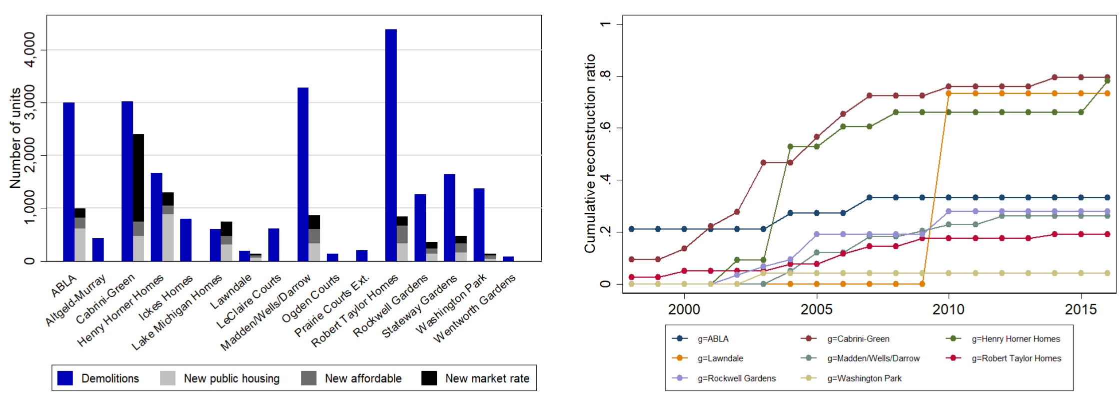
Notes: This tables estimates Eq. (3.33). Control variables include the following census tract characteristics in 1990: share of low-educated households, income per capita, black share, number of housing units, and distance to rapid transit lines in 1980. The counties included in the IV specification belong to the cities of Atlanta, Chicago, Newark, Pittsburgh and Washington, DC. Baltimore and Memphis were excluded due to data unavailability. Standard errors in parenthesis, clustered at the county level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Appendix A

Chapter 1 Appendices

A.1 Figures

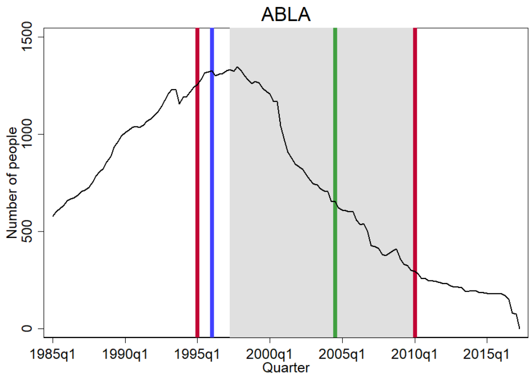
Figure A-1: New construction by public housing development



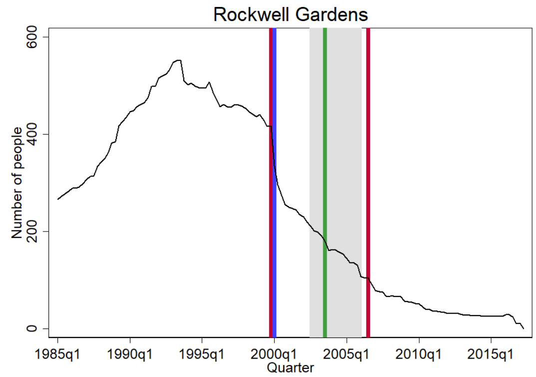
(a) By unit type **(b)** Timing of reconstruction (completion)

Note: In panel (a), red bars show the number of units demolished. Light gray, dark gray and black bars show the total number of newly constructed units by type (public housing, other affordable housing and market rate housing, respectively) as reported by the Chicago Housing Authority. In panel (b), I show the cumulative ratio of reconstructed units over total demolished units as reported by CHA. In a small number of cases where the year of completion was missing, I assigned the first year where new units were fully available in that development as reported by HOPE VI.

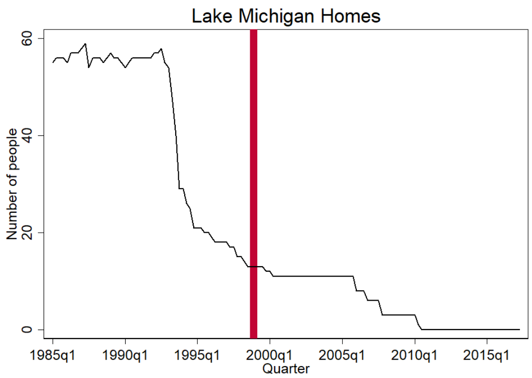
Figure A-2: Evolution of Infutor population by development



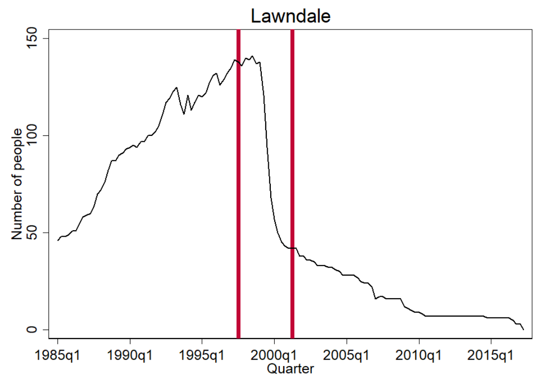
(a) ABLA Homes



(b) Rockwell Gardens



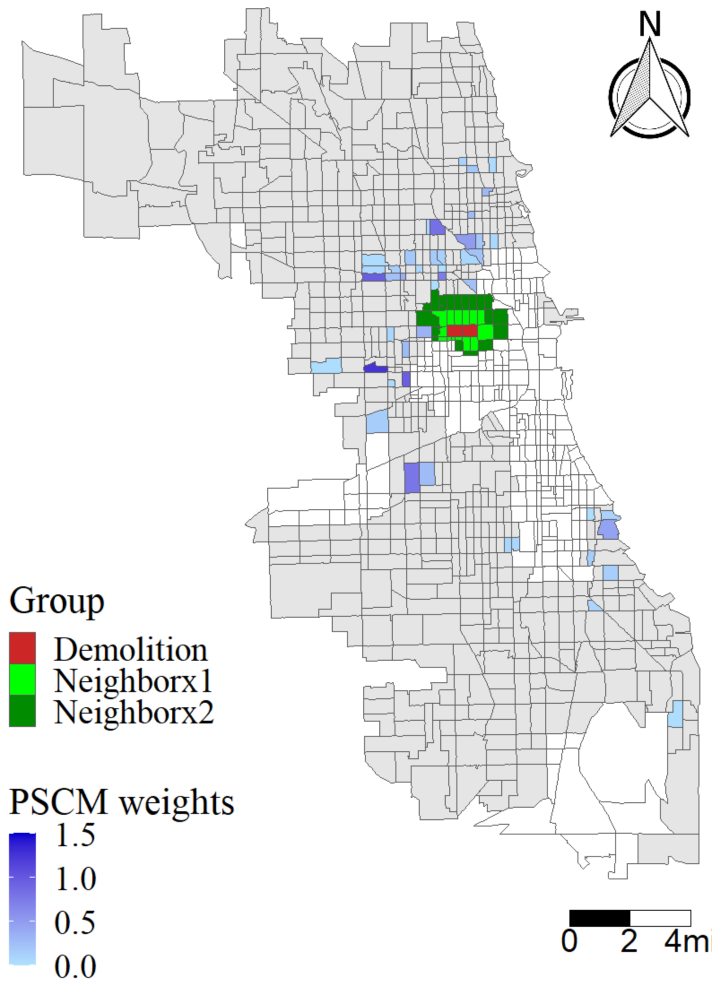
(c) Lake Michigan Homes



(d) Lawndale

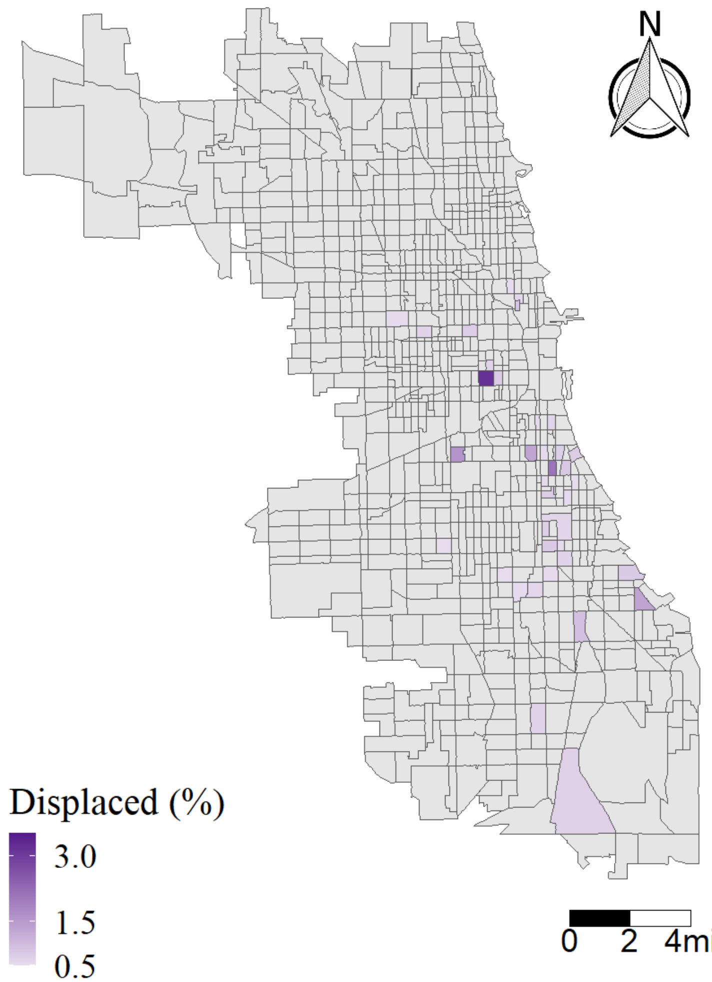
Note: Red lines indicate start and end dates of demolition as reported by the Chicago Housing Authority, while blue and green lines denote the grant award date and the start date of new construction, respectively, as in HOPE VI administrative data. The shaded area is the period in which tenants were relocated according to HOPE VI. Notice that, in all graphs, the total number of tenants is increasing at the beginning of the period. This does not mean that more tenants are moving into the demolished public housing developments, but it is due to the fact that coverage in Infutor is incomplete in earlier years and it increases up until the 2000s, when it reaches its full coverage

Figure A-3: Henry Horner Homes: treated and synthetic controls for Neighbor×1 group



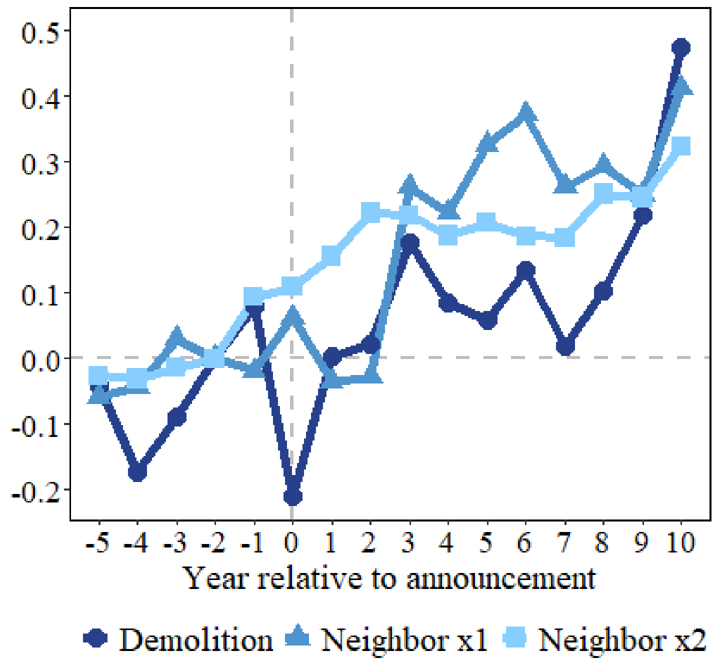
Note: This figure reproduces the map in Fig. 1-3 but for the Neighbor×1 treatment group corresponding only to the Henry Horner Homes.

Figure A-4: Census tracts by share of displaced tenants



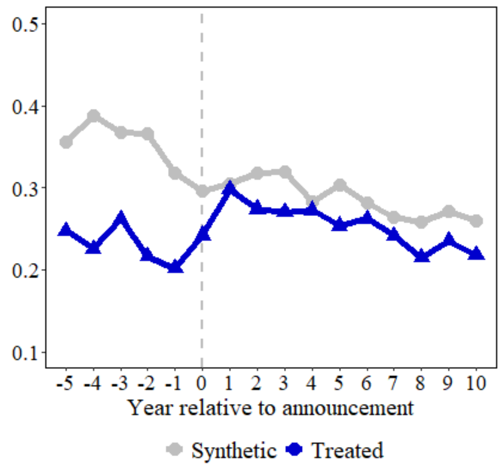
Note: This figure plots the percentage of total displaced tenants migrating to each census tract as observed in the displacement dataset described in Section 1.2.2.

Figure A-5: Effects of demolitions on (asinh) number of sales

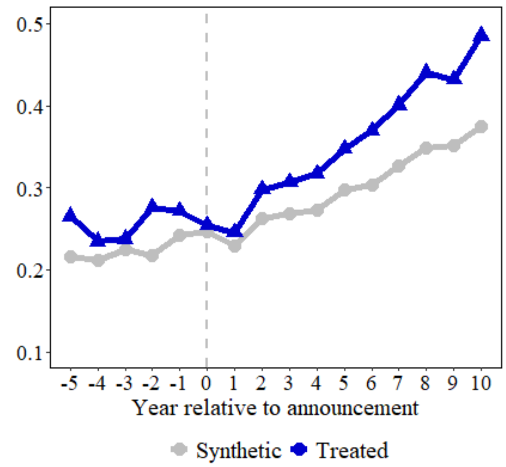


Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the inverse hyperbolic sine (asinh) of the number of sales as an outcome variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Full sample". The x-axis indicates the year relative to the first demolition announcement.

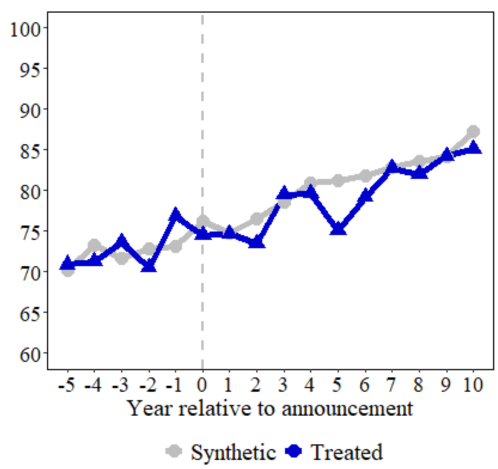
Figure A-6: Evolution of house characteristics for Neighbor×1 tracts



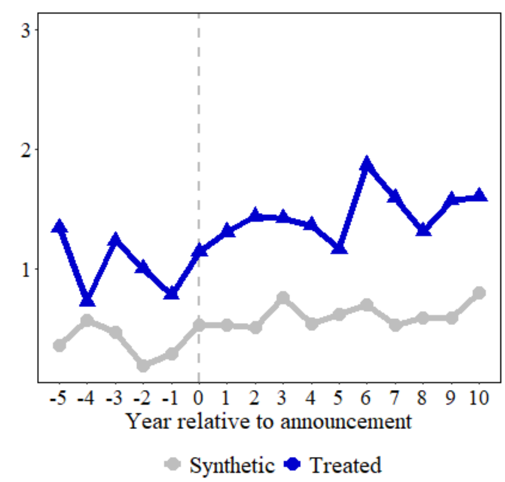
(a) SFR - share of sales



(b) Condo - share of sales



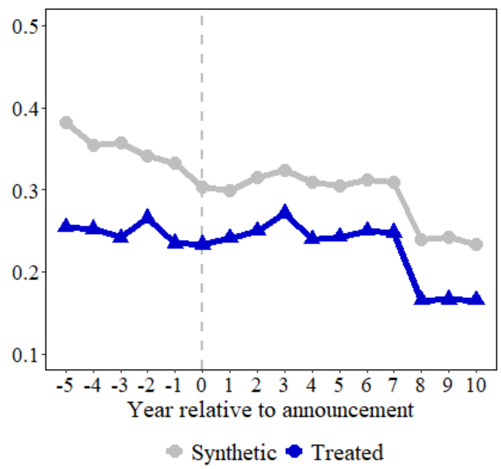
(c) Building age



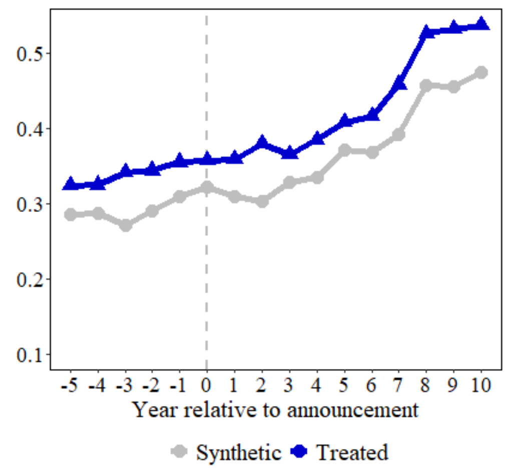
(d) Number of bathrooms (SFR)

Note: Each panel is a binscatter plot of the corresponding house sale characteristic for the "Analysis sample" of the Neighbor×1 group, by year relative to the announcement of the demolitions. I weight each treated tract and their synthetic control by the number of private housing units in the treated tract in 1990.

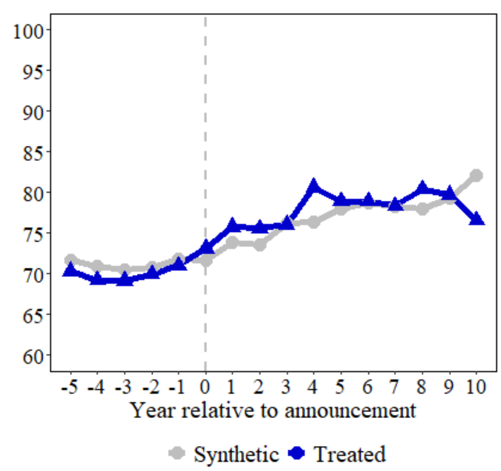
Figure A-7: Evolution of house characteristics for Neighbor×2 tracts



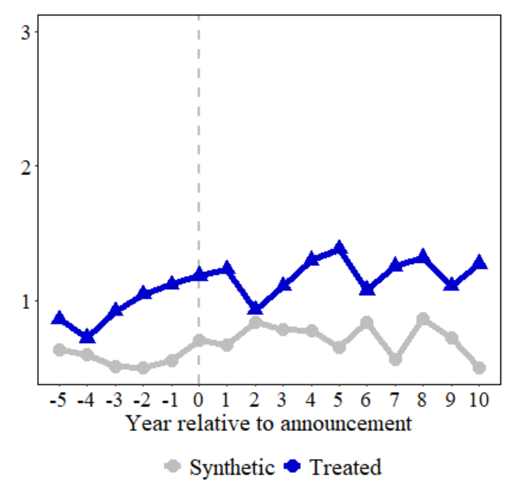
(a) SFR - share of sales



(b) Condo - share of sales



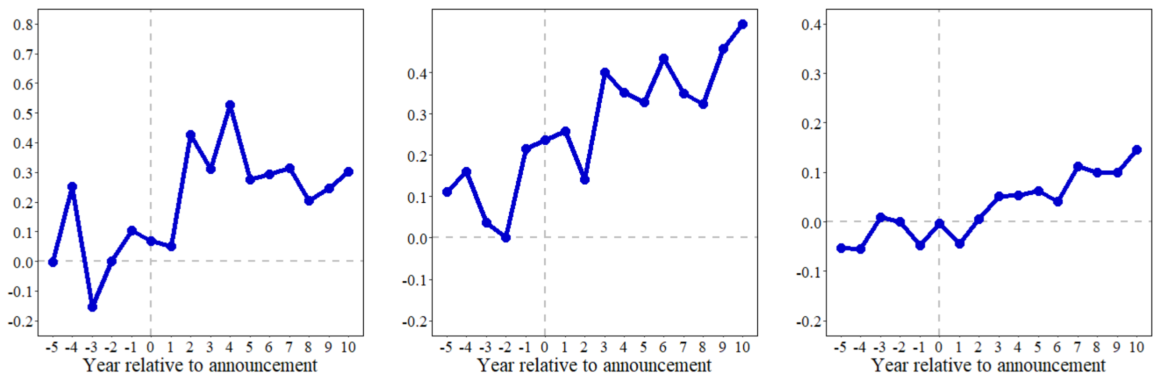
(c) Building age



(d) Number of bathrooms (SFR)

Note: Each panel is a binscatter plot of the corresponding house sale characteristic for the "Analysis sample" of the Neighbor×2 group, by year relative to the announcement of the demolitions. I weight each treated tract and their synthetic control by the number of private housing units in the treated tract in 1990.

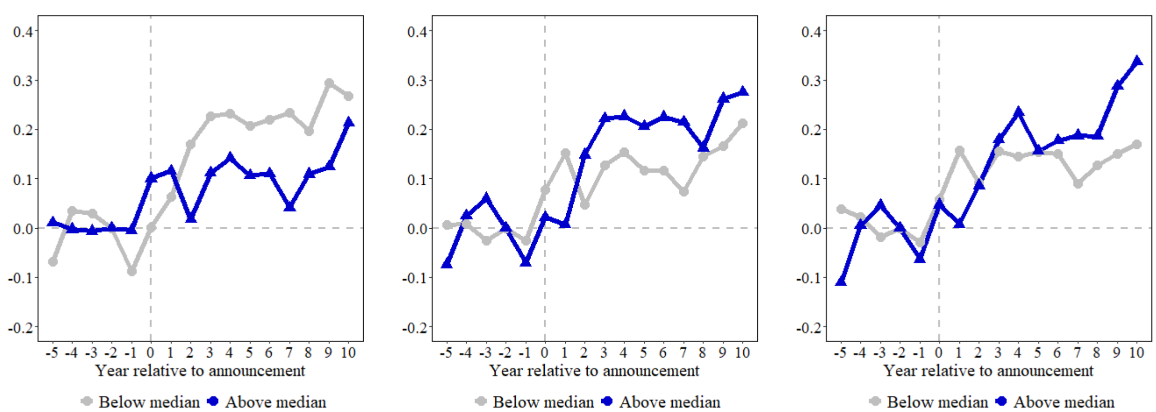
Figure A-8: Effects of demolitions on the house price index for single family houses, ρ_{ct}



(a) Demolition **(b)** Neighbor×1 **(c)** Neighbor×2

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the house price index ρ_{ct} constructed by using only single family residence sales as an outcome variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Analysis sample".

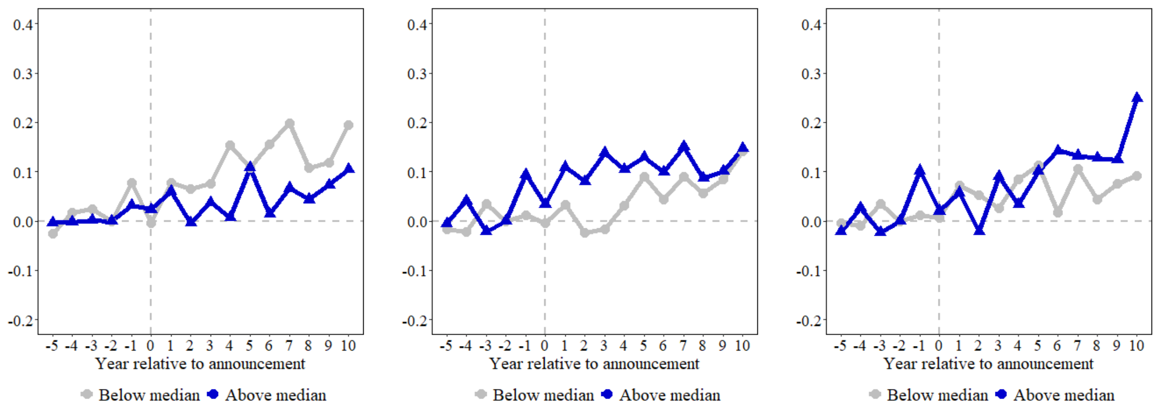
Figure A-9: Heterogeneity of price effects by baseline variables for Neighbor×1



(a) Median HH income 1990 **(b)** Black share 1990 **(c)** Distance to CBD

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) for Neighbor×1 tracts by heterogeneity group using the house price index ρ_{ct} as an outcome variable. For each variable, we divide treated tracts into those who are above vs below the median of the corresponding variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Analysis sample".

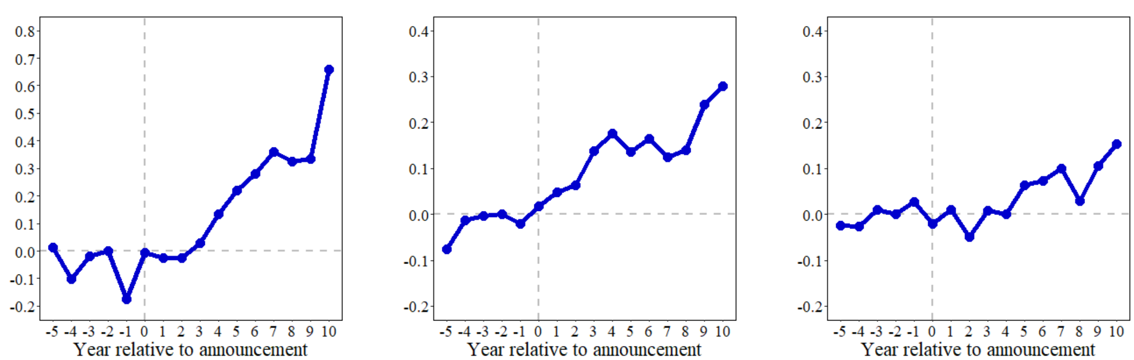
Figure A-10: Heterogeneity of price effects by baseline variables for Neighbor×2



(a) Median HH income 1990 **(b)** Black share 1990 **(c)** Distance to CBD

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) for Neighbor×2 tracts by heterogeneity group using the house price index ρ_{ct} as an outcome variable. For each variable, we divide treated tracts into those who are above vs below the median of the corresponding variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Analysis sample".

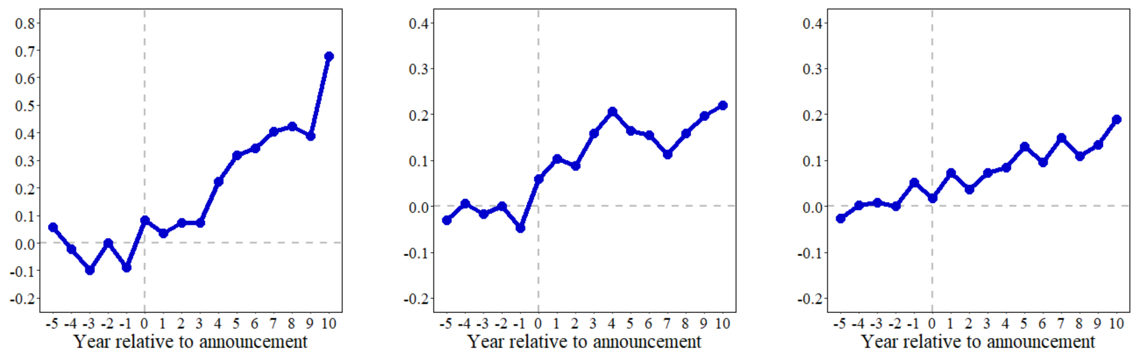
Figure A-11: Non-reconstructed sample: Effects of demolitions on the house price index, ρ_{ct}



(a) Demolition **(b)** Neighbor×1 **(c)** Neighbor×2

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the house price index ρ_{ct} as an outcome variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Analysis sample" excluding Cabrini-Green, Henry Horner Homes and Lake Michigan Homes.

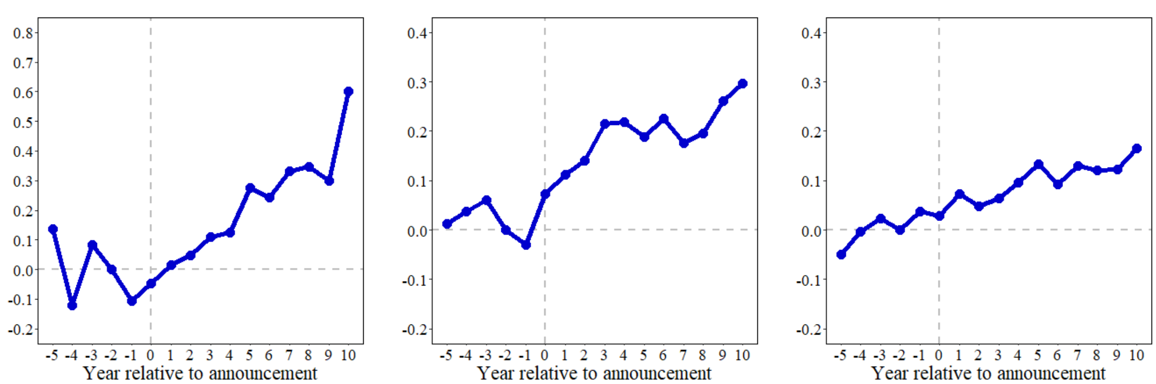
Figure A-12: Restricted sample: Effects of demolitions on the house price index, ρ_{ct}



(a) Demolition **(b)** Neighbor×1 **(c)** Neighbor×2

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the house price index ρ_{ct} as an outcome variable. Penalized synthetic control methods (PSCM) are used on the "Restricted sample".

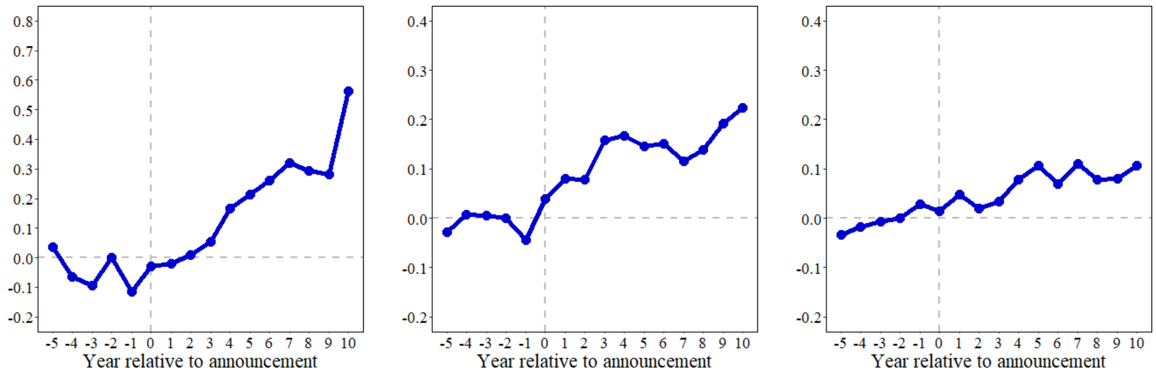
Figure A-13: Matching on average pre-trends: Effects of demolitions on the house price index, ρ_{ct}



(a) Demolition **(b)** Neighbor×1 **(c)** Neighbor×2

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the house price index ρ_{ct} as the outcome. Penalized synthetic control methods (PSCM) are used on the "Analysis sample" using the *average* house price index in years -5 to -2 and 1990 tract characteristics to compute the optimal weights for the synthetic control.

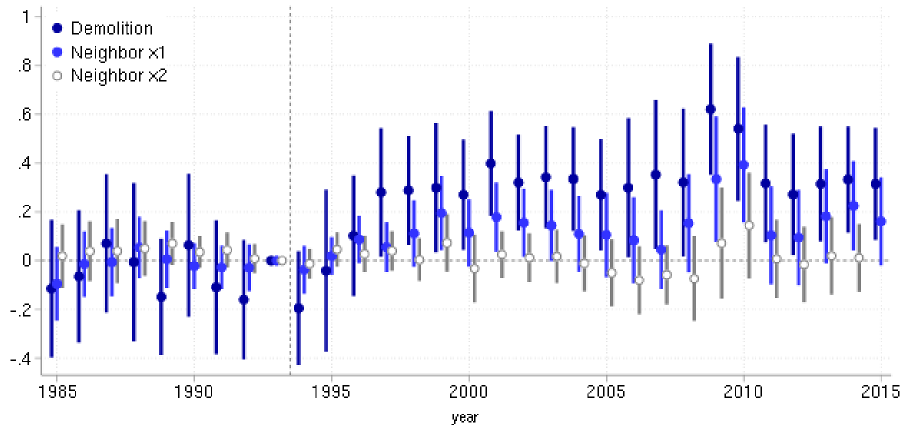
Figure A-14: SCM: Effects of demolitions on the house price index, ρ_{ct}



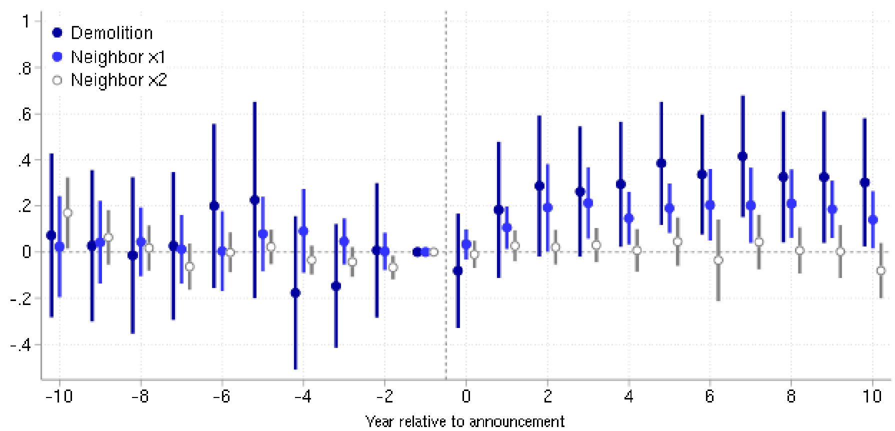
(a) Demolition **(b)** Neighbor×1 **(c)** Neighbor×2

Note: The graph plots the evolution over time of τ_t in Eq. (1.3) by treatment group using the house price index ρ_{ct} as an outcome variable. Traditional synthetic control methods (SCM) are used on the "Analysis sample", i.e. $\lambda = 0$.

Figure A-15: Event studies: Effects of demolitions on the house price index, ρ_{ct}



(a) Calendar year specification



(b) Relative year specification

Note: Both panels plot results for an event study specification at the house h and year t level ($c(h)$ denotes the census tract of house h). Panel (a) reports the coefficients $\beta_{t,g}$ of the following regression:

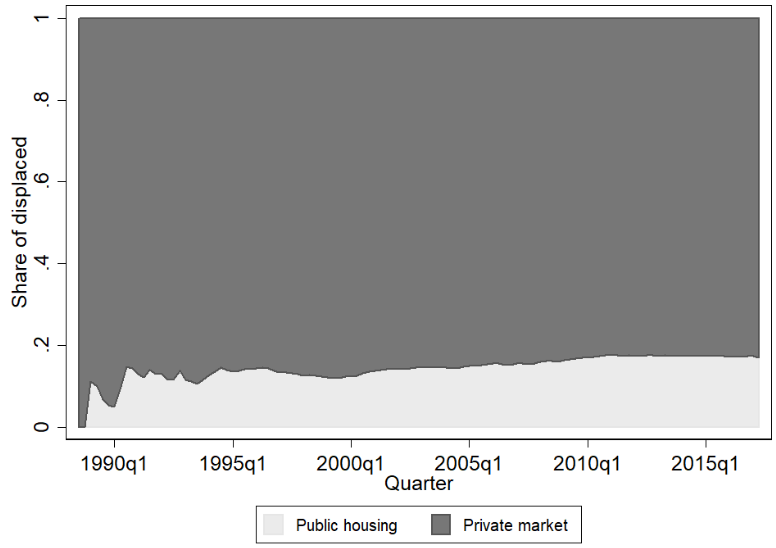
$$\ln \mathrm{P}_{ht} = \mu_{c(h)} + \sum_{t,g} \beta_{t,g} \; 1(\text{Sale year}_h = t) \times \mathbb{1}(G_{c(h)} = g) + \gamma' \mathbf{X}_{ht} + u_{ht}$$

where Sale year$_h$ is the year when house h is sold and $g \in G_{c(h)} = \{\text{Demolition}, \text{Neighbor} \times 1, \text{Neighbor} \times 2\}$ indicates the census tract treatment group. $\mu_{c(h)}$ are census tract FE and $\mathbf{X}_{ht}$ includes the same control variables as in Eq. (1.1). Panel (b) reports the coefficients $\theta_{\tau,g}$ of the regression:

$$\ln \mathrm{P}_{ht} = \mu_{c(h)} + \omega_t + \sum_{\tau,g} \theta_{\tau,g} \; \mathbb{1}(D_{c(h),t} = \tau) \times \mathbb{1}(G_{c(h)} = g) + \gamma' \mathbf{X}_{ht} + u_{ht}$$

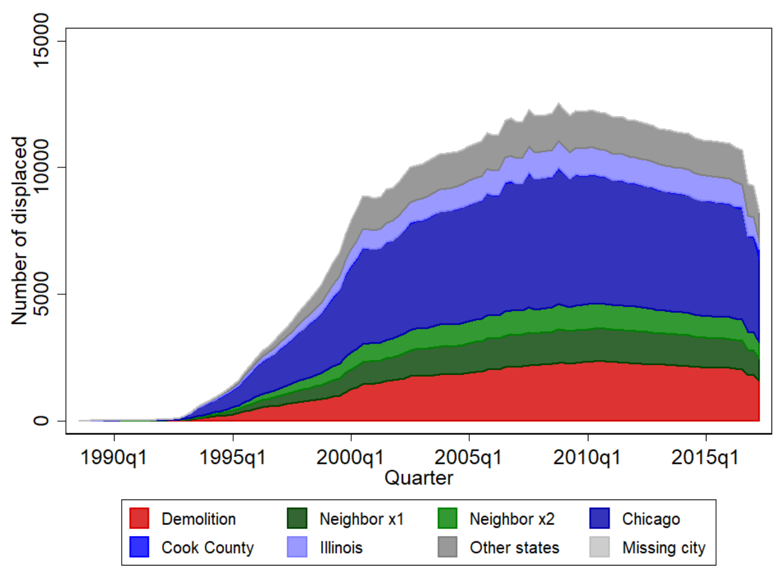
where $D_{c(h),t}$ is a dummy variable for each year τ relative to the announcement of the first demolition in census tract $c(h)$. Note that, in this specification, we also include calendar year FE, ω_t. In both regressions, house sales in Neighbor×3 tracts are the omitted group and I cluster standard errors at the census tract level.

Figure A-16: Share of displaced tenants by housing type in Infutor



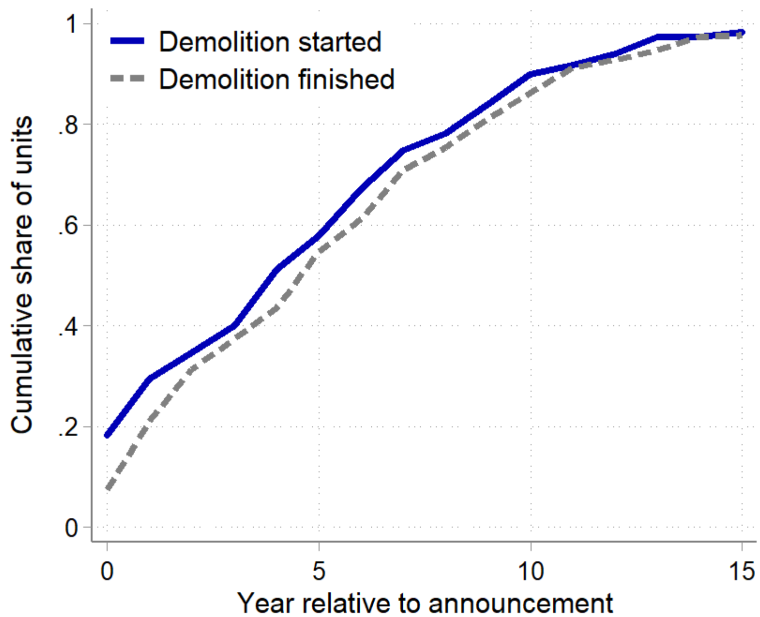
Note: This graph plots the share of displaced tenants as identified in the *displacement dataset* (introduced in Section 1.2.2) by housing type destination over time. I identify private housing by exclusion, i.e. if it does not correspond to a public housing address.

Figure A-17: Cumulative number of displaced tenants by destination in Infutor



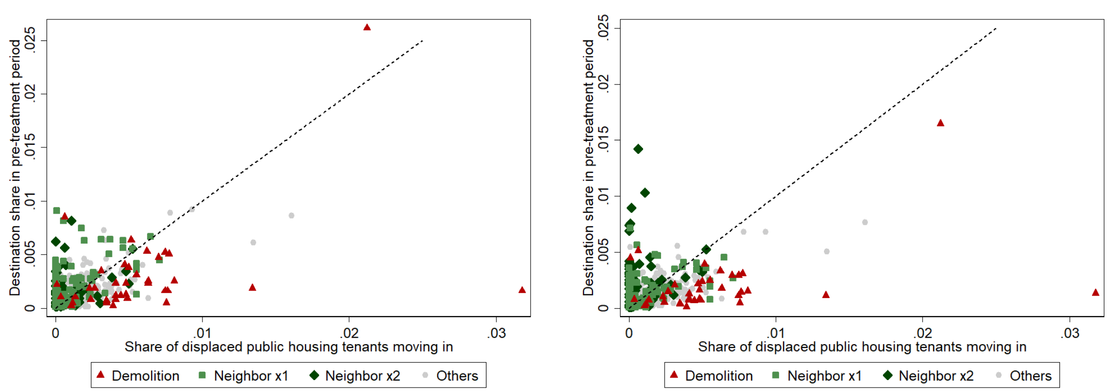
Note: This graph plots the number of displaced tenants as identified in the *displacement dataset* (introduced in Section 1.2.2) by destination over time.

Figure A-18: Cumulative share of demolished units by relative year



Note: This graph plots the cumulative share of demolished units by start and completion date for every year relative to the announcement of the first demolition in the same tract as reported by the Chicago Housing authority.

Figure A-19: Destination shares vs share of displaced public housing tenants moving in, by migration-based housing market definition



(a) D + N1 (b) D + N1 + N2

Note: Every dot represents a census tract and the color represents the treatment group. The y-axis represents the share of movers in the pre-treatment period coming from Demolition and Neighbor ×1 (left) or Demolition, Neighbor ×1 and Neighbor ×2 (right) tracts. The x-axis is the share of displaced households in the *displacement dataset* (introduced in Section 1.2.2) moving into the tract after the demolitions.

A.2 Tables

Table A.1: HOPE VI vs CHA demolition dates

		Award year		HOPE VI		CHA	
Development	Units	Rev	Demo	Start	End	Start	End
ABLA (Brooks/Brooks Ext.)	836	1996	1998	1997q4	2001q3	1995q1	2001q3
ABLA (Abbott/Addams)	2162	1998	2001	1999q4	2010q1	1995q1	2010q1
Altgeld-Murray	426					2016q3	2018q3
Cabrini-Green	3023	1994	2000	2007q3	2008q2	1995q3	2011q2
Henry Horner Homes	1665	1996	2000	2002q2	2009q1	1996q2	2008q2
Ickes Homes	804					2000q3	2011q3
Lake Michigan Homes	607					1998q4	1999q1
Lawndale	187		2000			2001q1	2001q2
LeClaire Courts	616					2011q1	2011q3
Madden/Wells/Darrow	3287	2000	1998	2001q1	2006q2	1995q3	2011q3
Maplewood Courts	132					2005q2	2005q3
Ogden Courts	136					2005q4	2006q2
Prairie Courts Ext.	203					2003q2	2003q3
Robert Taylor Homes	4389	1996	2000	1998q4	1999q4	1997q3	2007q2
Rockwell Gardens	1134	2001	2000	2003q4	2008q2	1999q4	2006q3
Stateway Gardens	1644	2008	2000			2000q4	2007q3
Washington Park	1374		1998			1995q3	2008q3
Wentworth Gardens	78					2005q2	2006q3

Note: The first column shows the number of units demolished by development between 1995 and 2018 as reported by the Chicago Housing Authority (CHA). The second and third columns show the year when a HOPE VI grant was awarded (if any), where "Rev" stands for "Revitalization" grant and "Demo only" indicates that the grant was awarded only for demolition purposes. The fourth and fifth columns report the actual quarters of start and end of demolitions as reported in HOPE VI data, while the last two columns show the same information as reported by the CHA.

Table A.2: Price effects on Neighbor×3 tracts by period

	Demolition	Neighbor ×1	Neighbor ×2	Neighbor ×3
Yrs. -5 to -3				
Price change	-0.04	-0.00	0.00	-0.00
p-value	0.008	0.991	0.916	0.804
Yr. -1				
Price change	-0.11	-0.04	0.05	-0.03
p-value	0.052	0.063	0.047	0.173
Yr. 0				
Price change	0.02	0.05	0.01	0.00
p-value	0.644	0.029	0.586	0.989
Yrs. 1 to 5				
Price change	0.07	0.14	0.06	-0.02
p-value	0.004	0.001	0.002	0.311
Yrs. 6 to 10				
Price change	0.34	0.18	0.10	0.05
p-value	0.001	0.001	0.001	0.015
λ	0.01	0.01	0.03	0.01
Number of tracts	21	86	100	90

Note: The table reports the ATET on house prices in different periods by treatment group using PSCM. Instead of reporting τ_t as described in Eq. (1.3), we compute: $\tau_p = (1/\sum_{i=1}^{n_1} H_i^{1990})\ \sum_{i=1}^{n_1}\ H_i^{1990} \times \tau_{ip}$ where p denotes both a period and the set of years included in that period, so $\tau_{ip} = (1/|p|) \sum_{t \in p} \tau_{it}$. Every column uses the "Analysis sample" of treated tracts.

Table A.3: Characteristics of treated and synthetic controls

	Demolition		**Neighbor**×1		**Neighbor**×2	
	Treated	Synthetic	Treated	Synthetic	Treated	Synthetic
Panel A: Matching variables						
Population density, per km^2 (1,000s)	7.14	7.17	7.81	7.99	9.79	9.43
Black (%)	0.80	0.77	0.45	0.43	0.24	0.22
Education: no diploma (%)	0.52	0.50	0.38	0.38	0.34	0.35
Median household income (%)	11,071	12,399	22,753	23,570	30,162	29,808
Below poverty line (%)	0.50	0.47	0.27	0.26	0.20	0.20
House price index (-5 to -2)	10.45	10.39	10.89	10.84	11.10	11.10
Panel B: Census characteristics 1990						
Population	2,977	2,295	2,916	3,422	3,975	3,817
Housing units	1,358	851	1,543	1,543	2,038	1,802
Female (%)	0.55	0.53	0.52	0.52	0.51	0.52
Population under 18 (%)	0.29	0.32	0.21	0.25	0.20	0.22
Population over 65 (%)	0.15	0.09	0.13	0.10	0.11	0.12
Occupancy rate	0.81	0.88	0.85	0.89	0.89	0.90
Renter households (%)	0.68	0.67	0.63	0.60	0.57	0.53
Median rent	230	301	389	377	448	430
Distance to CBD (mi)	4.23	5.61	3.75	6.26	4.10	6.04
Panel C: House sales in 1994						
Sale price	90,910	74,131	132,087	116,745	129,408	126,247
Number of sales	9	14	30	37	57	58
Lot size sq. ft.	5.10	3.54	3.57	3.92	3.55	3.50
Condo (%)	0.07	0.04	0.22	0.22	0.34	0.30
Single-family (%)	0.23	0.38	0.20	0.37	0.25	0.37
Multifamily/Apartment (%)	0.32	0.55	0.38	0.36	0.33	0.32
Year built	1918	1912	1919	1897	1926	1926
Number of tracts	21	21	86	86	100	100

Note: This table reports the characteristics of treated tracts and their synthetic control by treatment group. More specifically, I pick the synthetic controls that result from running PSCM on the house price index for the "Analysis" sample. I weight each treated tract and their synthetic control by the number of private housing units in the treated tract in 1990.

Table A.4: Effects on (asinh) number of sales and p-values by period

	Demolition		Neighbor $\times 1$		Neighbor $\times 2$	
	Full	Restricted	Full	Restricted	Full	Restricted
Yrs. -5 to -3						
Price change	-0.10	-0.28	-0.02	-0.06	-0.02	-0.05
p-value	0.003	0.001	0.222	0.013	0.222	0.026
Yr. -1						
Price change	0.08	-0.16	-0.02	0.02	0.09	0.10
p-value	0.132	0.027	0.586	0.635	0.012	0.008
Yr. 0						
Price change	-0.21	0.01	0.06	0.08	0.11	0.10
p-value	0.001	0.850	0.097	0.072	0.001	0.006
Yrs. 1 to 5						
Price change	0.07	0.02	0.15	0.11	0.20	0.15
p-value	0.190	0.793	0.002	0.019	0.001	0.001
Yrs. 6 to 10						
Price change	0.19	0.01	0.32	0.21	0.24	0.14
p-value	0.013	0.871	0.001	0.002	0.001	0.006
λ	0.01	0.01	0.01	0.01	0.00	0.00
Number of tracts	43	20	105	71	103	93

Note: The table reports the ATET on the inverse hyperbolic sine (asinh) of the number of sales in different periods by treatment group using PSCM. Instead of reporting τ_t as described in Eq. (1.3), I compute: $\tau_p = (1/\sum_{i=1}^{n_1} H_i^{1990}) \sum_{i=1}^{n_1} H_i^{1990} \times \tau_{ip}$ where p denotes both a period and the set of years included in that period, so $\tau_{ip} = (1/|p|) \sum_{t \in p} \tau_{it}$.
The first column of each treatment group uses the "Full sample" of treated tracts, while the second restricts the sample to those with an average of at least 4 sales per year in the pre-treatment period ("Restricted sample").

Table A.5: Non-reconstructed sample: Price effects and p-values by period

	Demolition		Neighbor ×1		Neighbor ×2	
	Analysis	Restricted	Analysis	Restricted	Analysis	Restricted
Yrs. -5 to -3						
Price change	-0.00	0.02	-0.03	-0.06	-0.01	-0.02
p-value	0.161	0.362	0.025	0.002	0.352	0.166
Yr. -1						
Price change	-0.20	-0.14	-0.02	-0.05	0.03	0.03
p-value	0.005	0.018	0.418	0.082	0.367	0.402
Yr. 0						
Price change	0.04	0.12	0.02	0.00	-0.02	-0.03
p-value	0.921	0.042	0.538	0.919	0.508	0.332
Yrs. 1 to 5						
Price change	0.01	0.09	0.11	0.09	0.01	0.00
p-value	0.124	0.053	0.001	0.001	0.752	0.865
Yrs. 6 to 10						
Price change	0.31	0.41	0.19	0.15	0.09	0.08
p-value	0.001	0.001	0.001	0.001	0.001	0.006
λ	0.01	0.01	0.01	0.01	0.03	0.03
Number of tracts	16	17	52	64	63	67

Note: The table reports the ATET on house prices in different periods by treatment group using PSCM. Instead of reporting τ_t as described in Eq. (1.3), I compute: $\tau_p = (1/\sum_{i=1}^{n_1} H_i^{1990}) \sum_{i=1}^{n_1} H_i^{1990} \times \tau_{ip}$ where p denotes both a period and the set of years included in that period, so $\tau_{ip} = (1/|p|) \sum_{t \in p} \tau_{it}$.
All columns exclude Cabrini-Green, Henry Horner Homes and Lake Michigan Homes. The first column of each treatment group uses the "Analysis sample" of treated tracts, while the second restricts the sample to those with an average of at least 4 sales per year in the pre-treatment period ("Restricted sample").

Table A.6: SCM: Price effects and p-values by period

	Demolition		Neighbor ×1		Neighbor ×2	
	Analysis	Restricted	Analysis	Restricted	Analysis	Restricted
Yrs. -5 to -3						
Price change	-0.04	-0.02	-0.01	-0.02	-0.02	-0.02
p-value	0.047	0.384	0.538	0.132	0.046	0.018
Yr. -1						
Price change	-0.11	-0.10	-0.04	-0.04	0.03	0.03
p-value	0.022	0.033	0.062	0.068	0.225	0.323
Yr. 0						
Price change	-0.03	0.07	0.04	0.04	0.01	0.01
p-value	0.519	0.141	0.108	0.114	0.574	0.621
Yrs. 1 to 5						
Price change	0.08	0.13	0.13	0.13	0.05	0.05
p-value	0.047	0.001	0.001	0.001	0.013	0.013
Yrs. 6 to 10						
Price change	0.34	0.43	0.16	0.16	0.09	0.09
p-value	0.001	0.001	0.001	0.001	0.002	0.003
λ	0.00	0.00	0.00	0.00	0.00	0.00
Number of tracts	21	20	86	69	100	94

Note: The table reports the ATET on house prices in different periods by treatment group using traditional synthetic control methods (SCM). Instead of reporting τ_t as described in Eq. (1.3), we compute: $\tau_p = (1/\sum_{i=1}^{n_1} H_i^{1990})\ \sum_{i=1}^{n_1}\ H_i^{1990} \times \tau_{ip}$ where p denotes both a period and the set of years included in that period, so $\tau_{ip} = (1/|p|)\sum_{t\in p}\tau_{it}$.
The first column of each treatment group uses the "Analysis sample" of treated tracts, while the second restricts the sample to those with an average of at least 4 sales per year in the pre-treatment period ("Restricted sample").

Table A.7: Effects on (log) Infutor population and p-values by period

	Demolition		Neighbor ×1		Neighbor ×2	
	PSCM	SCM	PSCM	SCM	PSCM	SCM
Yrs. -10 to -6						
Price change	0.05	0.06	-0.02	-0.02	0.01	0.01
p-value	0.001	0.001	0.001	0.001	0.001	0.003
Yrs. -4 to 0						
Price change	0.00	0.00	-0.00	-0.00	-0.00	-0.00
p-value	0.001	0.001	0.790	0.585	0.010	0.013
Yrs. 1 to 5						
Price change	-0.12	-0.12	0.03	0.03	-0.01	-0.01
p-value	0.001	0.001	0.025	0.018	0.075	0.093
Yrs. 6 to 10						
Price change	-0.18	-0.19	0.05	0.05	-0.01	-0.00
p-value	0.001	0.001	0.050	0.053	0.370	0.442
Yrs. 11 to 15						
Price change	-0.22	-0.23	0.06	0.06	0.01	0.01
p-value	0.001	0.001	0.068	0.060	0.952	0.956
λ	0.00	0.00	0.00	0.00	0.00	0.00
Number of tracts	43	43	105	105	103	103

Note: The table reports the ATET on the log census tract population count in Infutor in different periods by treatment group using PSCM. Instead of reporting τ_t as described in Eq. (1.3), we compute: $\tau_p = (1/\sum_{i=1}^{n_1} H_i^{1990})\ \sum_{i=1}^{n_1}\ H_i^{1990} \times \tau_{ip}$ where p denotes both a period and the set of years included in that period, so $\tau_{ip} = (1/|p|)\sum_{t\in p}\tau_{it}$.
The first column of each treatment group uses the "Full sample" of treated tracts, while the second restricts the sample to those with an average of at least 4 sales per year in the pre-treatment period ("Restricted sample").

A.3 Data Appendix

A.3.1 House Price Dataset

I use house price data from two different sources.

1. Transaction data on residential transactions in Cook County, IL, from 1985 to 2018 was obtained from Corelogic, a company that collects detailed public records from county assessor and register of deeds officers. It contains the main variables related to the sale and location of the property, including the parcel number. Fig. A-20 shows that the coverage of the dataset is consistent from 1985 to 2019.

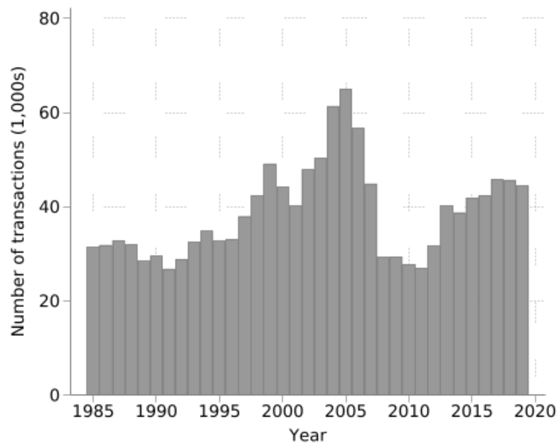
2. Property assessment data come from Zillow Ztrax data, collected from county assessor officers. The data contain information on property characteristics for every parcel in Cook County, IL, from 2000 to 2017. I use data from years 2000, 2005, 2010 and 2017.

I merge these two datasets based on the parcel number as follows. I merge transactions in the Corelogic dataset occurring in 2000 or before with Zillow assessment data in 2000; transactions between 2001 and 2005 with assessment data in 2005; transactions between 2006 and 2010 with assessment data in 2010, and transactions taking place later than 2010 with assessment data in 2017. For transactions whose parcel was not matched in this initial merge, I merge them with the next closer assessment year data. The intention is to reflect the property's characteristics as close as possible to the sale date.

A.3.2 Infutor Dataset

This section examines how Infutor's coverage evolves for different groups of tracts. Fig. A-21 plots the evolution of census tract population in Infutor by group of tracts: Demolition (with 50 or more units demolished), Neighbors (tracts adjacent to Demolition tracts) and Control (all remaining tracts). The plot shows how coverage is

Figure A-20: Histogram of house sales in Corelogic by year



Note: This histogram shows the number of transactions (in thousands) in the Corelogic dataset by year for the city of Chicago.

incomplete for earlier years in the sample, and grows until reaching full coverage in the early 2000s.

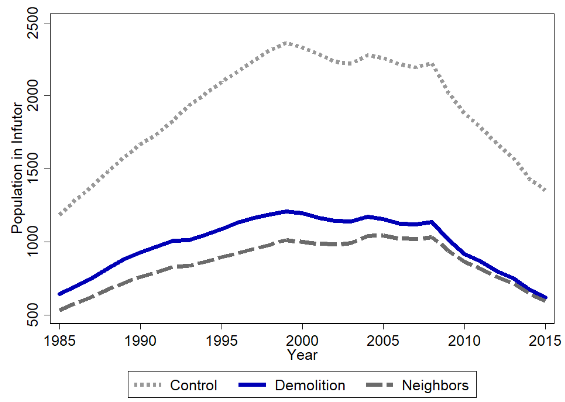
One concern is that, if the growth rate of coverage is unequal between tracts affected and not affected by demolitions, the unaffected tracts cannot serve as a valid control group when I look at demographic changes.

To explore this issue, I compare the Infutor population to the census adult population count in 1990 and 2000 at the census tract level.[1] Figure A-22 shows a scatter plot of this comparison for 3 groups of tracts: Demolition, Neighbor×1 and Control tracts. The first group is defined as census tracts with 50 or more demolished public housing units, the second group refers to census tracts adjacent to Demolition tracts and Control tracts include all remaining census tracts within the city of Chicago. The plots illustrate how coverage improves in 2000 (the slope of the linear fit of each group becomes closer to 1) even though Demolition and Neighbor tracts seem to have lower coverage on average.

However, Infutor coverage within the three groups of tracts grows at approximately

[1]Note that Infutor only covers the adult population. Hence, I compare it to the population count over 18 years of age

Figure A-21: Evolution of Infutor population by census tract group



Note: This graph uses raw population counts by census tract in Infutor data.

the same rate. In particular, the growth rate of coverage in the Control, Demolition and Neighbor×1 groups are 30%, 75% and 35%, respectively. An immediate implication is that, when I measure population *changes* using Infutor data, the unequal coverage *level* across census tract groups is not a big threat for neighboring tracts because coverage is growing at a similar rate across the Neighbor×1 and the Control groups. For Demolition tracts, the fact that they are growing at a faster rate than the control group implies that I overestimate population increases and underestimate population decreases.

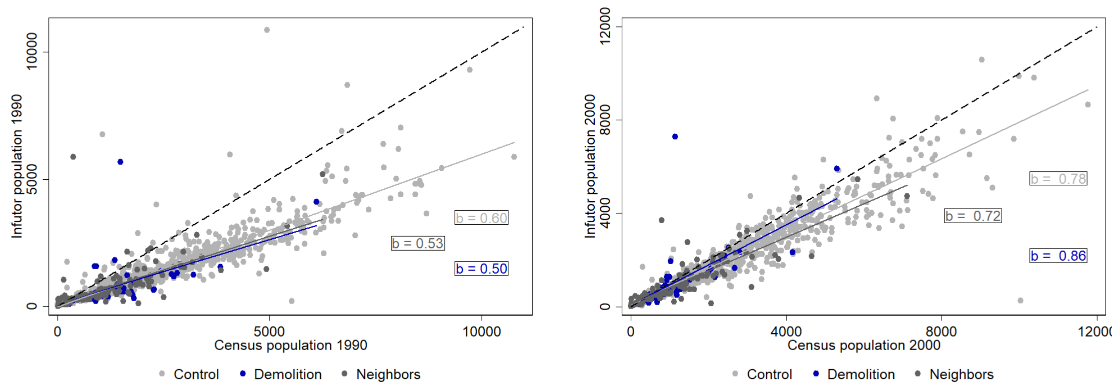
A.3.3 Displacement Dataset

I construct a sample of tenants that were displaced by the demolitions. For each displaced individual, the dataset contains living spells information on both the last address at a demolished site and the stream of future addresses. In order to build this dataset, I followed the steps below:

1. Restrict the Infutor dataset to individuals who lived at a demolished address in Infutor.

2. *Definition of "displaced tenants"*. I restrict the dataset to people who left a demolished address between 7 years before and 1 year after the Chicago Housing

Figure A-22: Comparison of Infutor and census population by year



(a) 1990 **(b)** 2000

Note: Panel (a) shows a scatter plot of census tract adult population in 1990 against the population count by census tract in Infutor for that year, by group. For each group, the plot reports the coefficient of the linear fit regression. Panel (b) does the same for 2000. In both cases, the black dotted line is the 45° line.

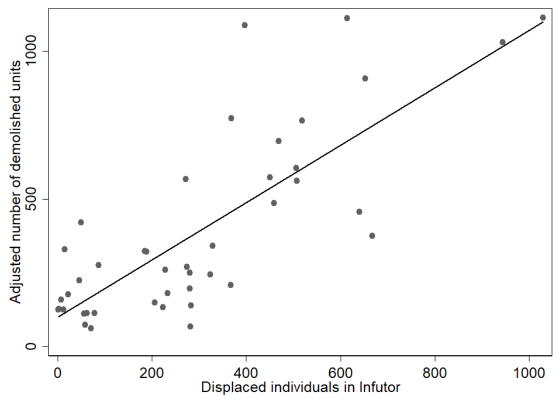
Authority sent the notice-to-proceed for demolition.
The notice-to-proceed notifies tenants that the building will be torn down and must be issued at least 90 days before demolition. I include individuals who left the building up to 1 year after the notice-to-proceed because Infutor may capture addresss changes with a lag.

3. Restrict the dataset to the last address at a demolished site and all future addresses.

4. Caveat. Setting the time frame to be 7 years previous to the notice to proceed date might not be including displaced tenants –some buildings were already closing due to poor conditions, in this case the move was spurred by future demolition.

The resulting dataset contains 13,917 displaced individuals. Figure A-23 plots the number of demolished units (adjusted by the occupancy rate at the block group level in 1990) against the number of displaced individuals that I observe in Infutor for each demolished tract. On average, the number of demolished units is a approximately equal to the number of displaced tenants that I see in Infutor (slope is near 1). However, this does not mean that I perfectly observe all displaced tenants for two reasons. First, I compare demolished *units* to displaced *individuals*. Since there may be several adults living in each public housing units, I might not be able to follow all displaced households. Second, the construction of the displacement dataset captures moves going back 7 years before the start of the demolition, which might include individuals that were not moving out because of displacement but other reasons. This would lead us to overestimate the number of displaced individuals.

Figure A-23: Comparison of demolished units and displaced tenants in Infutor



Note: This graph shows a scatterplot at the census tract level of the number of active public housing units that experienced demolition against the number of public housing tenants that I observe as being displaced in my dataset. The former is defined as the number of units demolished adjusted by the occupancy rate of the census block group of the building in 1990. This accounts for the fact that some of the demolished units were already closed in the pre-treatment period.

A.4 Penalized Synthetic Control Methods (PSCM)

A.4.1 Notation

Let $i \in \{1, ..., N\}$ and $t \in \mathcal{T}$ denote each unit and time period. Throughout this section, I follow Abadie and L'Hour (2021) and let the first n_1 units correspond to treated units and the last n_0 to be in the donor pool, so that $n_1 + n_0 = N$.

In addition, Y_{it} is the outcome of interest and X_{it} is a vector of covariates of dimension $k \times 1$. Consequently, I define Y and X as vectors of dimensions $1 \times N$ and $k \times N$. When I refer to the donor pool, I define Y_0 and X_0 as having dimensions $1 \times n_0$ and $k \times n_0$, respectively.

A.4.2 Methodology

Definition. The penalized synthetic control method with many treated units is defined as follows:

1. For each treated unit $i = 1, ..., n_1$ compute the n_0-vector of weights $W_i^*(\lambda) = (W_{i,n_1+1}^*(\lambda), ..., W_{i,N}^*(\lambda))$ that solves the following problem:

$$\begin{aligned} \min_{W_i \in \mathbb{R}} \quad & \|X_i - X_0 W_i\| + \lambda \sum_{\mathbf{j=n_1+1}}^{\mathbf{N}} \|\mathbf{X_i} - \mathbf{X_j}\| \mathbf{W_{i,j}} \\ \text{s.t.} \quad & 1'_{n_0} W_i = 1 \\ & 0_{n_0} \leq W_i \leq 1_{n_0} \end{aligned} \tag{A.1}$$

   where $W_i^*(\lambda)$ is the vector of weights given to the each unit in the donor pool in the synthetic control unit corresponding to treated unit i and the operator $\|A\|$ denotes some distance measure. In practice, I choose the operation $\|A\|$ to be a weighted quadratic distance:

$$\|X_i - X_0 W_i\| = (X_i - X_0 W_i)' V_i (X_i - X_0 W_i)$$

   where V_i is a $k \times k$ diagonal matrix that assigns importance weights to the different components of the covariates vector.

Note that the main difference with the traditional synthetic control method (SCM) is the second term in Eq. (A.1). In PSCM, this term measures the pair-wise matching discrepancies in order to reduce worst-case interpolation biases. Parameter λ governs the trade-off between component-wise and aggregate fit: as $\lambda \to \infty$, the estimator becomes the one-match nearest-neighbor matching with replacement estimator; as $\lambda \to 0$, it becomes the classic synthetic control. The idea is that the additional term in the minimization problem chooses the weights so that the tracts with positive weight look the closest to the treated unit among all possible weight combinations.

2. Estimate the average treatment effect on the treated (ATET) for each period, denoted by τ_t, using the mean difference between the realized outcome and the synthetic outcome for the treated, weighted by some variable ω_i:

$$\hat{\tau}_t(\lambda) = \frac{1}{\sum_{i=1}^{n_1} \omega_i} \sum_{i=1}^{n_1} \omega_i \left[Y_{it} - Y_{0t} W_i^*(\lambda) \right]$$

In this paper, when the outcome variable is the house price index or the number of sales, ω_i is equal to the number of private housing units in tract i, while for population counts I weight them by the total number of housing units in the tract in 1990.

Note: Before aggregating, I normalize both the treated and the synthetic control series with respect to $t = -2$ ($t = -5$ when population is the outcome variable) by taking the difference $Y_{it} - Y_{i,-2}$. Since the outcomes are in logarithms, this normalization provides a convenient interpretation. For instance, the difference in the house price index between the treated and the synthetic series at time t can be interpreted as the percentage difference in prices at t with respect to their value in $t = -2$.

Selection of λ. I select λ by using a leave-one-out cross-validation procedure that minimizes the mean squared prediction error for the control units in the post-intervention period. The procedure is as follows:

1. For each control unit $i = n_1 + 1, ..., n$ and post-intervention period, $t = t^* + 1, ..., T$, compute

   $$\hat{\tau}_{it}(\lambda) = Y_{it} - Y_{-i,t} W^*_{-i}(\lambda)$$

   where $Y_{-i,t}$ is a vector of post-treatment outcomes in period t for all control units except for i and, similarly, W_{-i} is a vector of weights for all control units except for i.

   Note that, in order to compute the optimal weights $W^*_{-i}(\lambda)$ in this sample, each unit in the control group needs to be assigned to a treatment period. In our context, I choose to randomly draw a value from the real treatment period distribution. E.g. when computing λ for the demolition group analysis, I randomly assign each unit in the control group to a treatment period in its distribution given by the 23 census tracts in that group.

2. Choose λ to minimize a measure of error, such as the mean squared prediction error for the individual outcomes,

   $$\lambda^* \;\in\; \arg\min_{\lambda} \frac{1}{n_0(T - t^*)} \sum_{i=n_1+1}^{n} \sum_{t=t^*+1}^{T} \left(\hat{\tau}_{it}(\lambda)\right)^2$$

Selection of V_i. I follow Abadie and Gardeazabal (2003) in defining the matrix V_i that assigns importance weights to the different predictors. For each unit, the procedure is the following:

1. For a given λ and matrix V_i, I compute:

$$\begin{aligned} W_i(\lambda^*, V_i) \in \arg\min_{W_i \in \mathbb{R}} \;& (X_i - X_0 W_i)' V_i (X_i - X_0 W_i) \\ & + \lambda^* \sum_{\mathbf{j}=\mathbf{n_1}+\mathbf{1}}^{\mathbf{N}} (\mathbf{X_i} - \mathbf{X_j})' \mathbf{V_i} (\mathbf{X_i} - \mathbf{X_j}) \mathbf{W_{i,j}} \qquad \text{(A.2)} \\ \text{s.t.} \quad & 1'_{n_0} W_i = 1 \\ & 0_{n_0} \leq W_i \leq 1_{n_0} \end{aligned}$$

2. Select V_i^* that minimizes the mean square error of the difference between the outcome variable of the treated and the synthetic control. That is, I choose the V_i with the highest predictive power.

$$\min_{V_i} \quad (Y_i - Y_0 W_i^*(\lambda^*, V_i))'(Y_i - Y_0 W_i^*(\lambda^*, V_i))$$

Restricting the control group. While implementing SCM for each treated tract, I reduced the number of census tracts in the control group in order to reduce the computational burden. For every unit in the treatment group, I drop census tracts in the control group with characteristics that are very far from the treated according to some distance measure.[2]

There are 835 census tracts, 689 of which are never treated. When running the algorithm for every treated unit, I drop this number to 50. I follow these steps:

1. For every treated unit i and control units j, I compute:

$$M_{i,j} = \frac{1}{m}(X_i - X_j)'(X_i - X_j)$$

   where X_i and X_j are $m \times 1$ vectors containing the 1990 census characteristics that I use as predictors in terms of standard deviations. $M_{i,j}$ can be thought of as a measure of proximity in characteristics between tracts i and j

2. Select all tracts i such that $M_{i,j} \leq \bar{M}_{i,j}^{50}$, where $\bar{M}_{i,j}^{50}$ is the 50 lowest $M_{i,j}$ value.

A.4.3 Inference: Permutation Test

I use permutation methods to provide a test statistic that indicates whether the results are statistically significant. In particular, I test for the significance of the aggregate effects of each treated group, by using a simple method suggested by Abadie and L'Hour (2021).

[2] This approach is not likely to affect the results. The reason is that I am only reducing the number of units that contribute to the optimal weight vector by removing control units that are very different from the unit of interest and, thus, were not likely to show up with a positive weight in the synthetic control anyway.

In essence, I compute the following test statistic for every post-treatment period t:

$$\hat{S}_t = \frac{\sum_{i=1}^{n_1} \hat{\tau}_{it}(\lambda)}{\sum_{i=1}^{n_1} \hat{\tau}_{i,-2}(\lambda)}$$

That is, I take the ratio of the treatment effect of each period t and the treatment effect two periods before the treatment period. Before describing the method, let us introduce some notation. Let $D^{obs} = \{D_1, ..., D_N\}$ denote the observed treatment assignment. Then, $\hat{S}_t(D^{obs})$ is the value of the test statistic in the sample, while $\hat{S}_t(D)$ is the corresponding value when treatment values are reassigned as in D. The procedure is as follows:

1. Compute the treatment effect estimate in the original sample, $\hat{S}_t(D^{obs})$.

2. At each iteration, $b = 1, ..., B$, permute at random the components of D^{obs} to obtain $\hat{S}_t(D^{(b)})$.

3. Calculate p-values as the frequency across iterations of values $\hat{S}_t(D^{(b)})$ more extreme than $\hat{S}_t(D^{obs})$. For the two-sided test:

$$p\text{-value}_t = \frac{1}{B+1}\left(1 + \sum_{b=1}^{B} \mathbb{1}\left\{|\hat{S}_t(D^{(b)})| \geq |\hat{S}_t(D^{obs})|\right\}\right)$$

A.5 Rational Expectation Models and House Prices

Using an asset-market approach, I give intuition on the expected path of price effects after the demolitions. In this paper, I focus on house prices, as opposed to rents. Under rational expectations, house (asset) prices reflect the present discounted value of expected future rents (spot prices) (Poterba, 1984; Sinai and Waldfogel, 2005). Hence, buyers and sellers in the housing market incorporate any changes in future rents into house prices when information first arrives. At the end of the section, I highlight some cases in which this might not hold.

Although rents are an interesting outcome *per se*, this paper focuses on the effects of demolitions on house prices due to the unavailability of yearly data on rents at a small geographical level. The distinction between rents and house prices is important in our context. Under rational expectations, the former reflects the equilibrium price of the flow of housing services at a given point in time, and the latter is defined as the present discounted value of the future expected path of that flow. It immediately follows that, when there is a shock to the housing market, rents jump when the shift in either the supply or demand of housing is realized, while house prices jump right after information about such shocks is revealed.

To recover the expected path of house prices effects after the demolitions, it is useful to first understand changes in expected future rents. Fig. F.1a plots the change in rents after a number of demolition-related events. In this paper, I think of a demolition as a four-stage process including its announcement, the displacement of public housing tenants, the structural demolition of the building and site reconstruction (if any). Consistent with the reasoning above, rents do not react to announcements of future demolitions. However, rents experience a discrete jump both at the time of displacement and structural demolitions. The former is an outward shift of the private housing demand, caused by both an outflow of households from public housing into housing vouchers and potential changes in the neighborhood composition as a result of the relocation of very low-income individuals. The latter removes a negative physical externality, given the poor conditions of some of these buildings. Lastly, rents might experience a sudden drop after reconstruction due to the outward shift in housing supply.[3]

The expected path of house price effects incorporates these changes in future rents when information is revealed, i.e. when the plans for displacement, demolition and reconstruction are announced. Fig. F.1b plots the evolution of house prices, which is equivalent to the present discounted value of the flow of rents described above. Conditional on the announcement comprising all stages of the demolition process, house prices should jump immediately after the announcement and continue to rise due to the higher PDV of the early stages of the demolition process, until it goes back to its new permanent level.

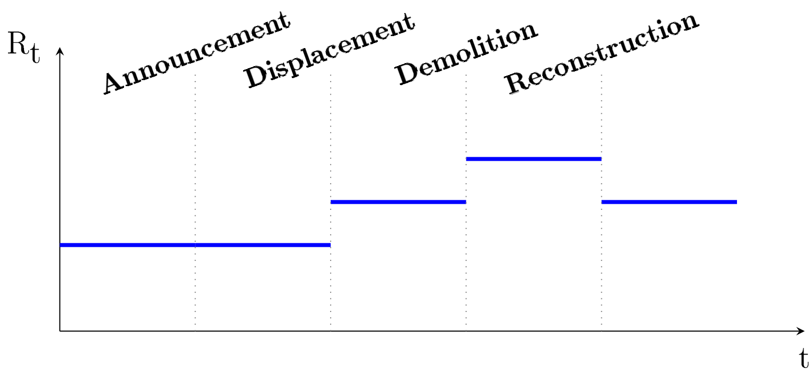
However, there are several reasons why I might not observe the path of price effects in Fig. F.1b. First, different stages of the demolition and reconstruction of a public housing development can overlap in practice.[4] Second, information on the plans for a certain public housing development might be updated after the initial announcement. A good example of this is the fact that some developments received more than one HOPE VI grant for different stages of the demolition process.[5] Finally, buyers and sellers may not trust initial plans or associate high levels of uncertainty to them, which would imply a failure of the rational expectation model above.

[3]Although this is unclear. Reconstruction might further raise prices if few new units are constructed and they are either seen as a large positive physical externality for nearby houses or bring higher-income households into the neighborhood.

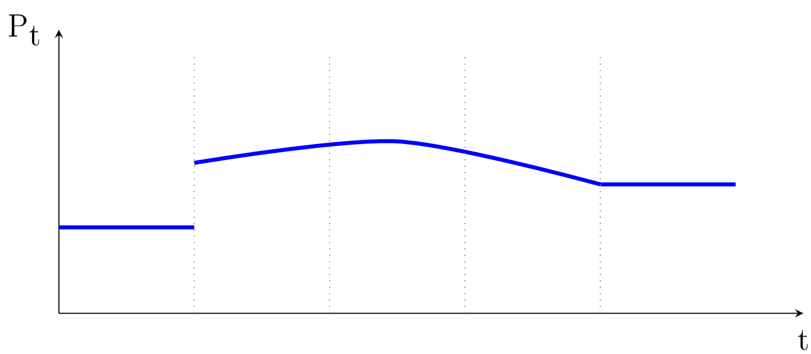
[4]An extreme example of this is given by the last Cabrini-Green high-rise to be knocked down. While its demolition was announced in 1995, resident opposition delayed actual demolition until 2011, when other parts of the development had already been reconstructed. Source: https://www.chicagotribune.com/news/ct-bn-xpm-2011-03-30-29364731-story.html

[5]For instance, Stateway Gardens was awarded one grant to demolish the projects in 2000 and another to revitalize the area in 2008.

Figure F.1: Expected path of price effects after demolitions



(a) Rents



(b) House prices

Appendix B

Chapter 2 Appendices

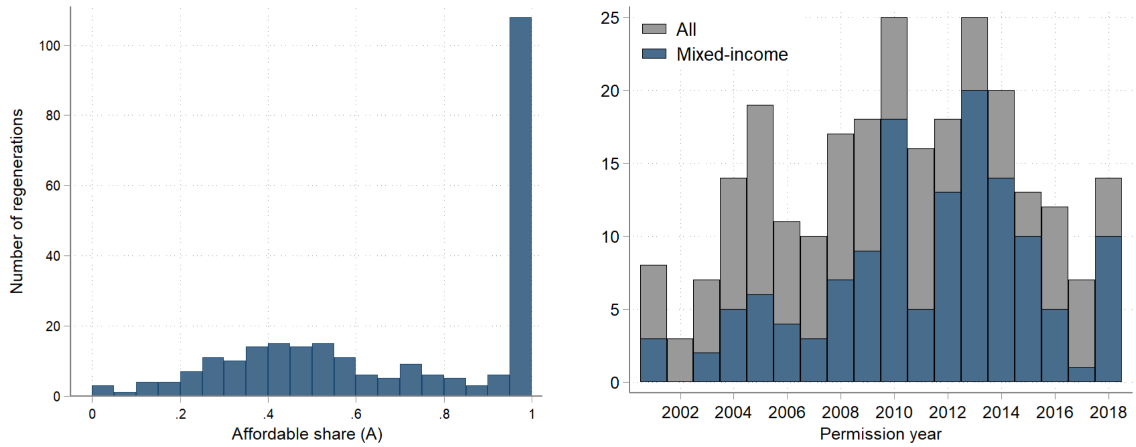
B.1 Figures

Figure F.1: The regeneration of the Meredith Tower in West London

(a) Existing building **(b)** New building

Note: This figure shows an example of a regeneration program carried out in West London. Panel A shows the building slated for demolition; Panel B shows a digital rendering of the new building constructed on site.

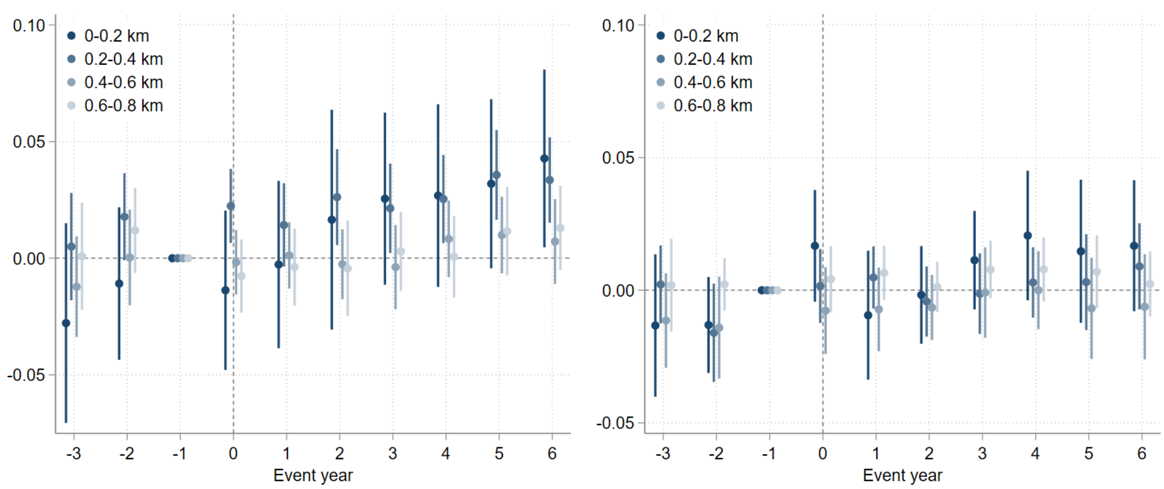
Figure F.2: Histograms of public housing share in the new building and timing



(a) Share of public housing - new building **(b)** Estate regenerations by year

Note: The plots use the sample of estates regenerated between 2001 and 2018.

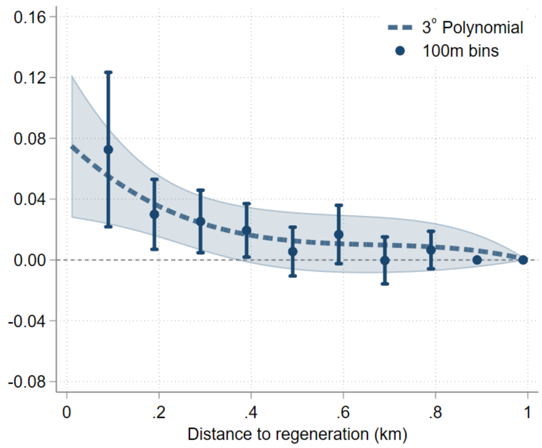
Figure F.3: Effects on rents by choice of treatment period



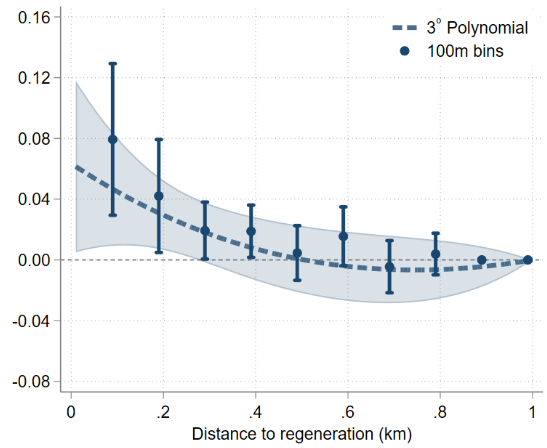
(a) Around permission year **(b)** Around completion year

Note: The plots report coefficients $\beta_{\tau,r}$ in Eq. (2.1). Both plots use the sample of estate regenerations in 2007-2012.

Figure F.4: Long-run effects of estate regenerations on rents



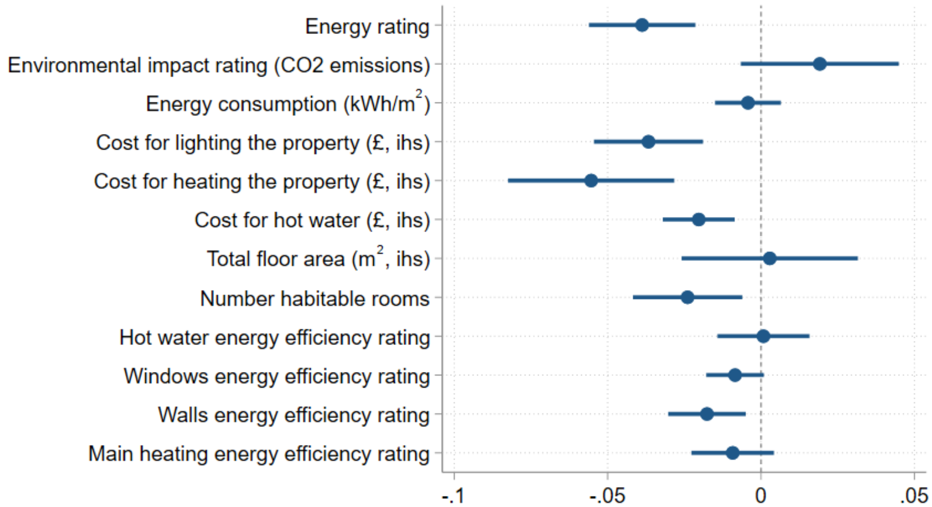
(a) Event years 4-6



(b) Event years 7-9

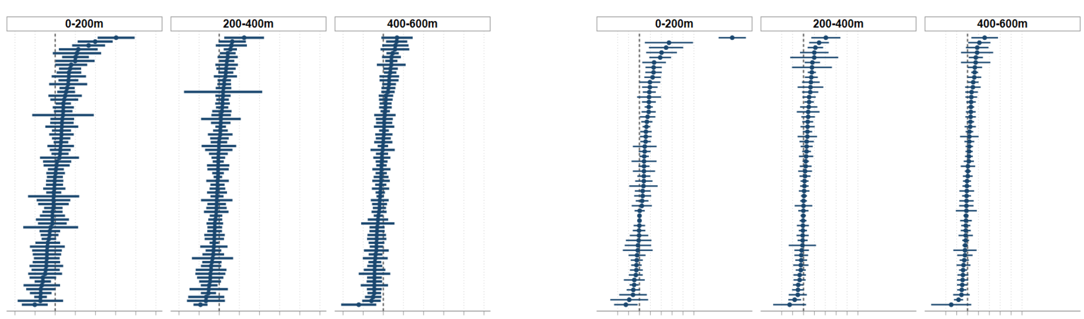
Note: The plots reproduce Fig. 2-3, by estimating Eq. (2.2) adding an interaction with the dummy variable $\text{Post}_{et}^{7\text{-}9}$ in the summatory, which indicates transactiones taking place in years 7 to 9 after permission.

Figure F.5: EPC differences between rental and owner-occupied units at baseline



Note: The plot regresses a dummy variable indicating whether a property is rented (vs. owner-occupied) on several unit characteristics as reported in Energy Performance Certificates (EPC). "ihs" indicates that we use the inverse hyperbolic sine transformation of the variable. Energy ratings go from A to G, and we transform it into integers going from 7 to 1 –higher numbers denote higher energy efficiency. Energy efficiency ratings in the last four rows are also reported in five categories ("Very good", "Good", "Average", "Poor", "Very poor") that we transform in a similar way. For the regression, we standardize all variables by subtracting their mean and dividing by their standard deviation. The sample includes all owner-occupied and rental units that were issued an EPC three to one years before the corresponding permission approval for regeneration and were located within 800m of a regeneration approved between 2009 and 2012. The regression includes estate fixed effects and standard errors are clustered at the estate level.

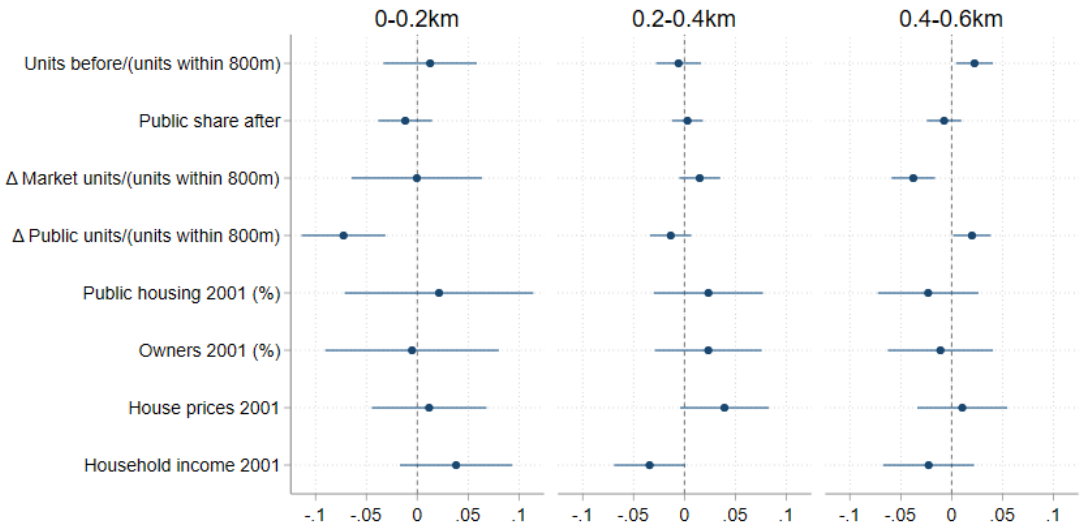
Figure F.6: Price effects by regeneration and ring, 4-6 years after permission



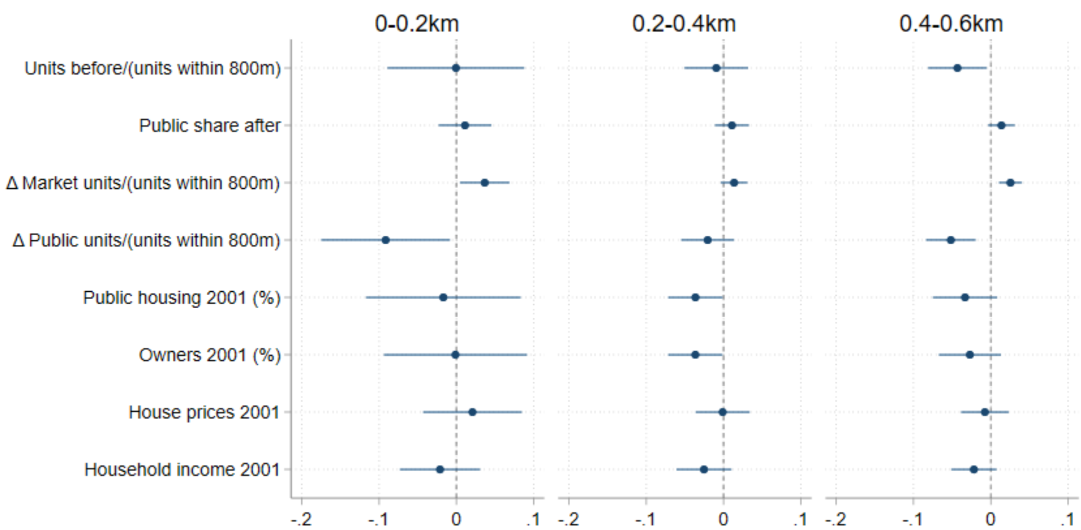
(a) House prices **(b)** Rents

Note: The figure reports point estimates and 95% confidence intervals for coefficient $\theta_{1,r}$ in Eq. (2.2) using 200m rings and ran separately for each regeneration. Estimates are sorted from lowest to highest for each separate bin.

Figure F.7: Correlation of regeneration-specific price effects with observables



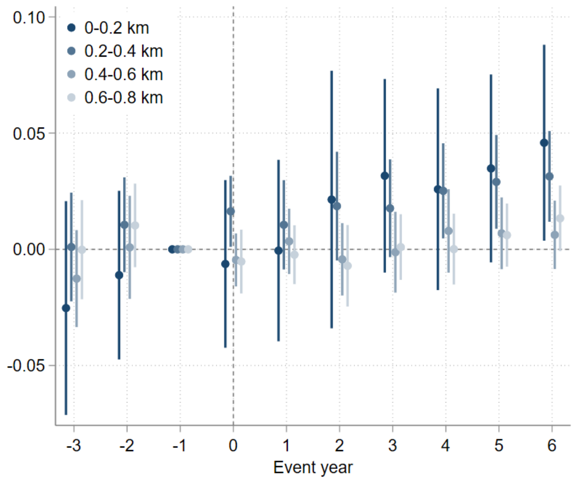
(a) House prices (comparison: 0.8-1km ring)



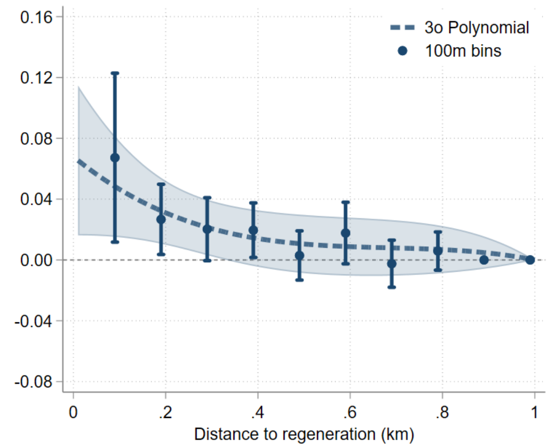
(b) Rents (comparison: 0.8-1km ring)

Note: This plot regresses the coefficients for each ring in Fig. F.6 on building and neighborhood characteristics.

Figure F.8: Quality-adjusted effects of estate regenerations on rents



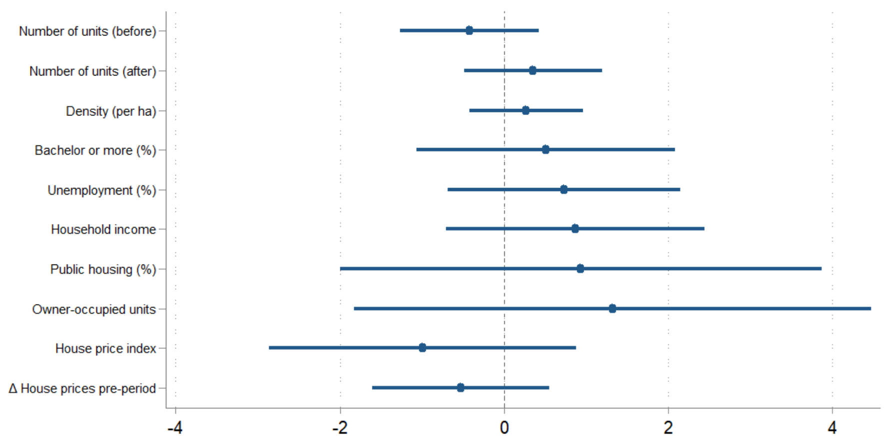
(a) Event study



(b) Pooled DID (years 4-6)

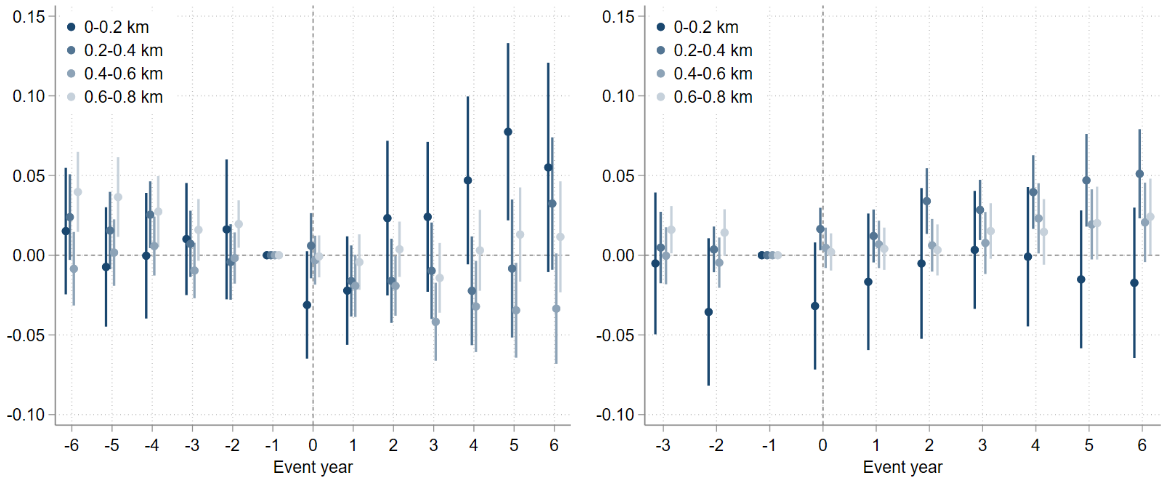
Note: Panel (a) and panel (b) reproduce Fig. 2-2 (panel (b)) and 2-3 (panel (d)), respectively, including as controls indicators for the presence of the keywords "refurbish", "luxury", "washing machine", "gym", "garden" and "concierge" in a listing's description. Both panels use the sample of estate regenerations with a permission approval in 2007-2012.

Figure F.9: Random timing of estate regenerations



Note: The plot shows the results from a regression of the announcement year on a series of regeneration (first two variables) and neighborhood characteristics in 2001 using the sample of regenerations with a permission approval in 2004-2018. Regeneration characteristics are measured either for the building slated for demolition (before) or for the new development (after). Neighborhood characteristics are constructed as the average of the variable of census block groups within 800 of the estate. The house price index refers is a proxy for baseline house prices in 2001 and is constructed as detailed in Section 2.4.4.2. The change in house prices in the pre-period is a proxy for rising prices and gentrification, and is constructed analogously for periods 1998-2000 and 2001-2003 –the change is defined as the difference between these two periods. All variables used as regressors are standardized.

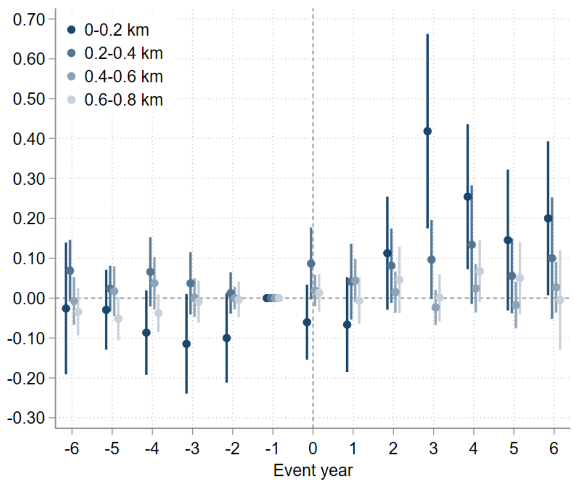
Figure F.10: Effects of estate regenerations on house prices and rents (using timing variation)



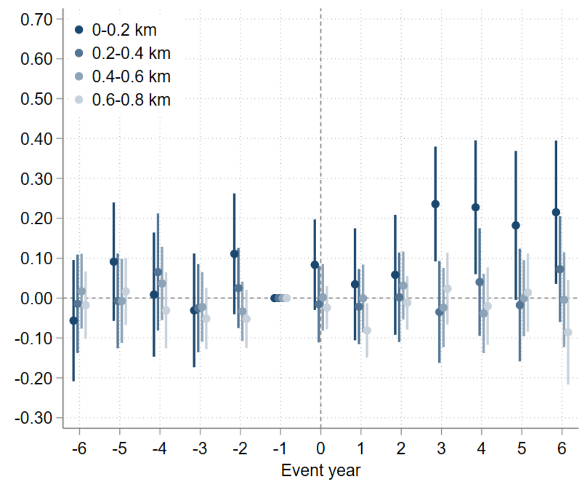
(a) House prices **(b)** Rents

Note: The plots report coefficients β_τ in Eq. (2.3) for each concentric 200m ring. The omitted category is housing units within the same distance ring of regenerations approved more than two years later. Panel (a) uses the sample of estate regenerations with a permission approval in 2004-2012; panel (b) uses those with a permission approval in 2007-2012.

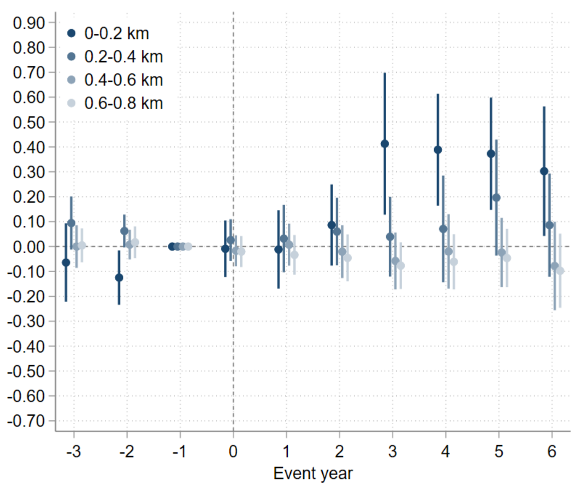
Figure F.11: Effects of estate regenerations on sales and listings (using timing variation)



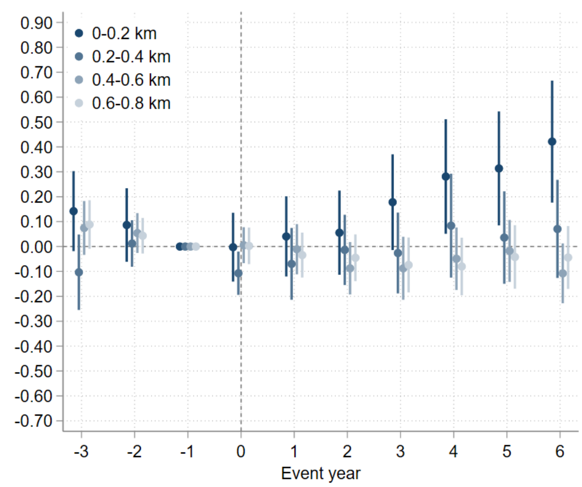
(a) Sales of new houses



(b) Sales of old houses



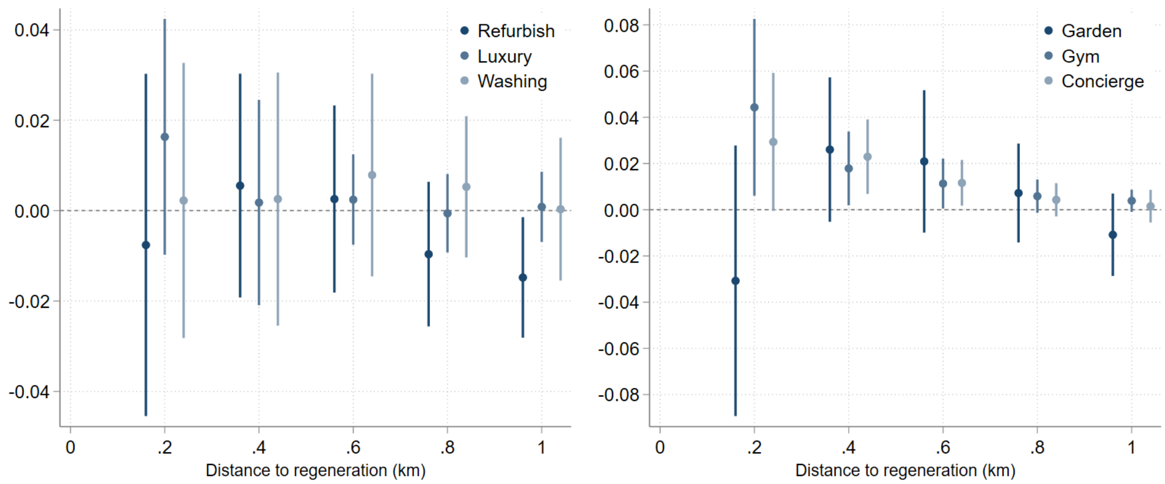
(c) Rental listings of new houses



(d) Rental listings of old houses

Note: The plots report coefficients $\beta_{\tau,r}$ in Eq. (2.3). For rental listings, we distinguish between "new" and "old" using text analysis on the description of the rental listing. Panels (a) and (b) use the balanced sample of estate regenerations with a permission approval in 2004-2012; panels (c) and (d) use those with a permission approval in 2007-2012.

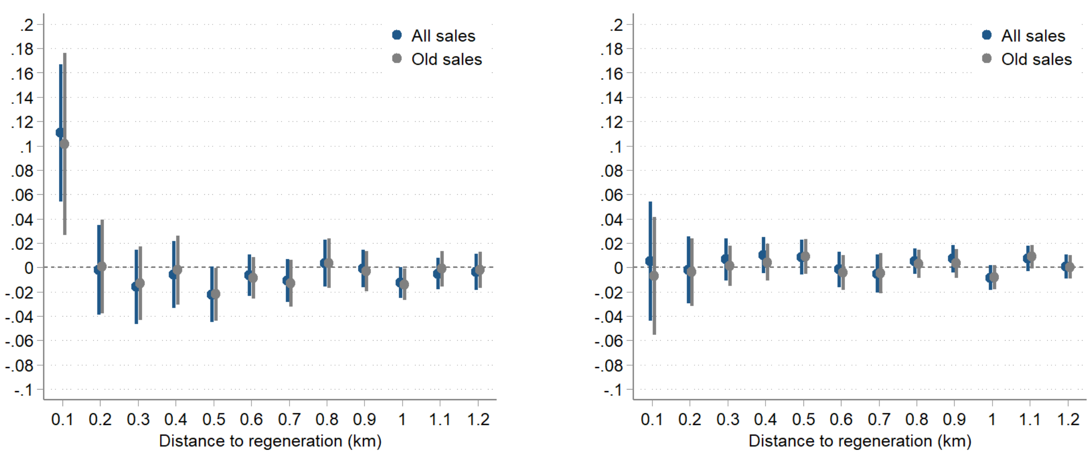
Figure F.12: Effects on listings' descriptions 4-6 years after permission (using timing variation)



(a) Unit characteristics **(b)** Building characteristics

Note: Coefficients and related 95% confidence intervals are obtained by estimating a version of Eq. (2.3) that collapses event years into three periods (-3 to -1, 0 to 3 and 4 to 6) on the sample of rental listings using 200m rings. This plot shows coefficients for the 4-6 event year period. The plots use the balanced sample of estate regenerations with a permission approval in 2007-2012.

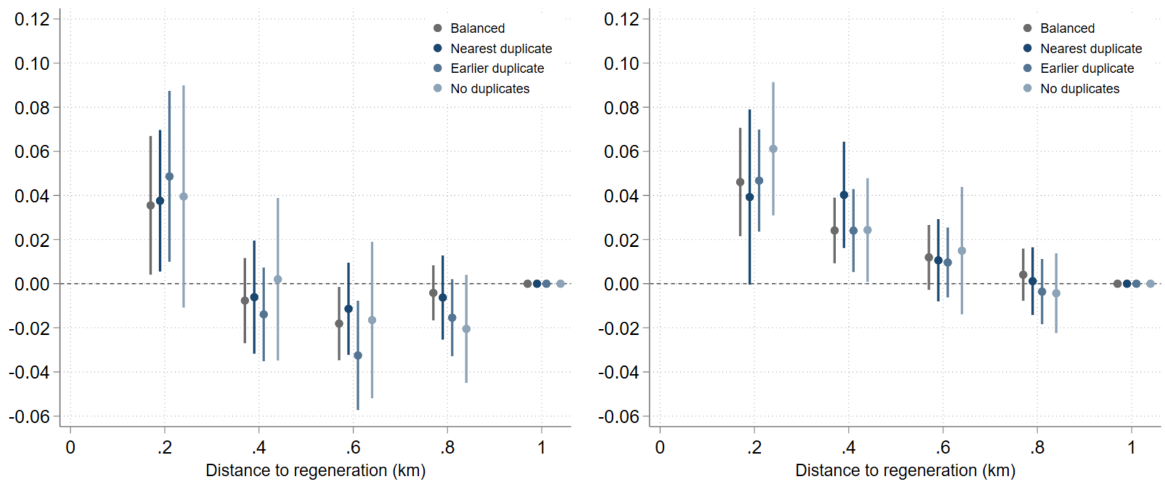
Figure F.13: Price effects accounting for treatment intensity and a placebo test



(a) Price effects: from 98-02 to 15-19 **(b)** Placebo test: from 98-00 to 01-03

Note: The plots report coefficients β_r in Eq. (2.4). Blue estimates use all residential sales to create the house price index ρ_{it}, gray estimates only use sales of old houses. Standard errors are adjusted for spatial autocorrelation following Conley (1999).

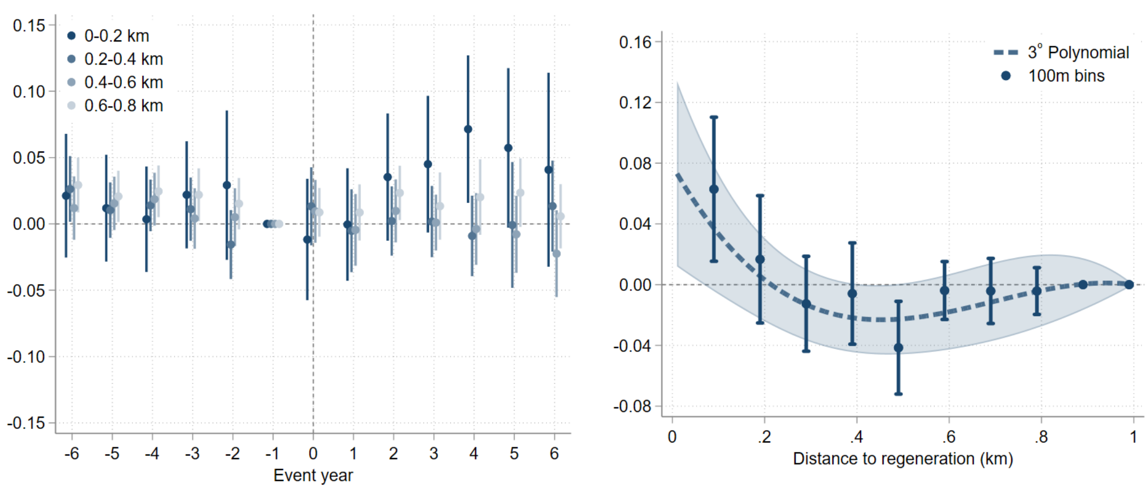
Figure F.14: Price effects in years 4-6 by 200m rings for samples without duplicates



(a) House prices **(b)** Rents

Note: The figure reports point estimates and 95% confidence intervals for coefficients $\theta_{1,r}$ in Eq. (2.2) using 200m rings. The "balanced sample" includes all housing units within 1km of an estate and corresponds to the main sample we use. "Nearest duplicate" and "Earliest duplicate" includes units that are duplicated across estates only for the estate the unit is closest to and for the estate that is regenerated earlier, respectively. "No duplicates" removes all units that are within 1km of more than one estate.

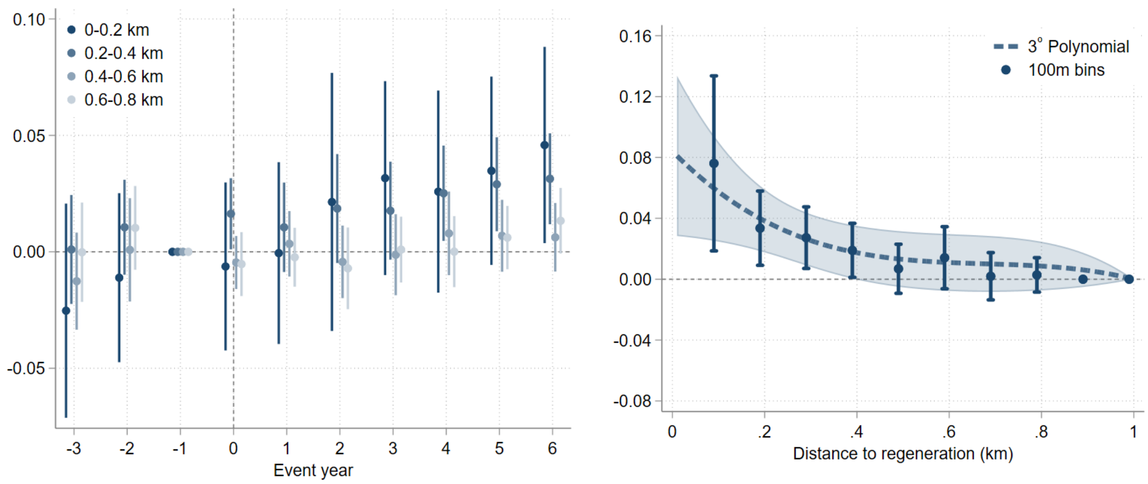
Figure F.15: Effects of estate regenerations on house prices for regenerations approved in 2007-2012



(a) Event study **(b)** Pooled DID (years 4-6)

Note: Panel (a) and panel (b) reproduce Fig. 2-2 (panel (a)) and 2-3 (panel (b)), respectively, using the sample of estate regenerations with a permission approval in 2007-2012.

Figure F.16: Effects of estate regenerations on rents for a balanced sample (2009-2012)

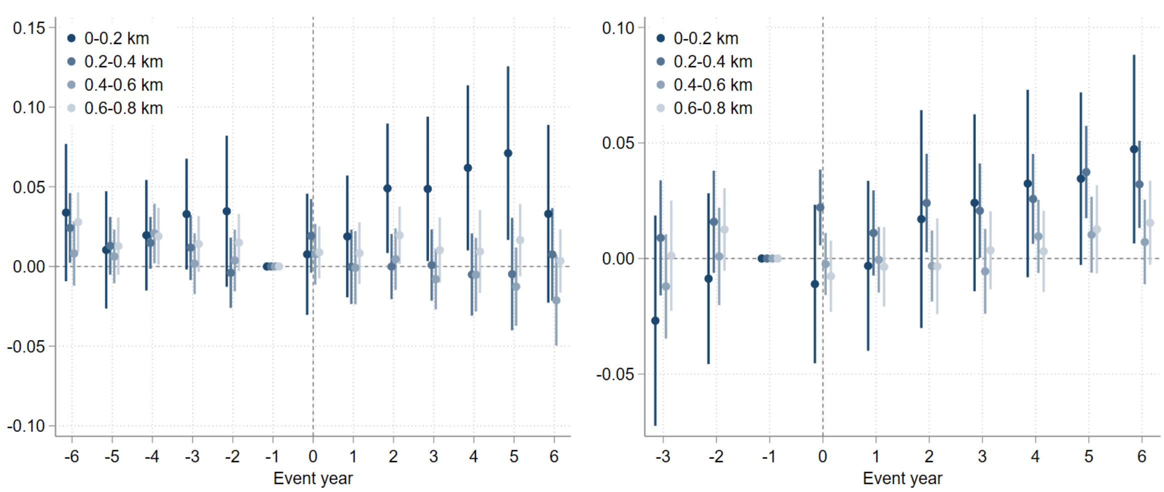


(a) Event study

(b) Pooled DID (years 4-6)

Note: Panel (a) and panel (b) reproduce Fig. 2-2 (panel (b)) and 2-3 (panel (d)), respectively, using the sample of estate regenerations with a permission approval in 2009-2012.

Figure F.17: Effects of estate regenerations on house prices and rents of old units

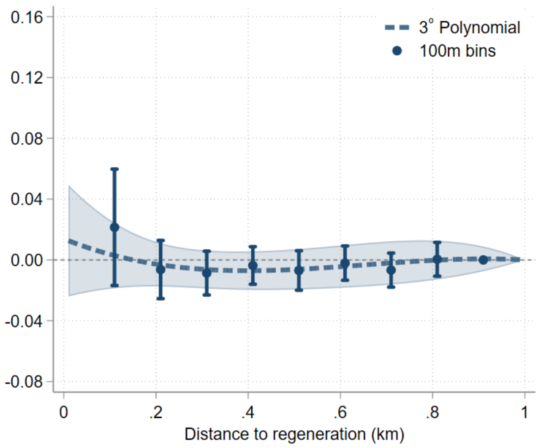


(a) House prices

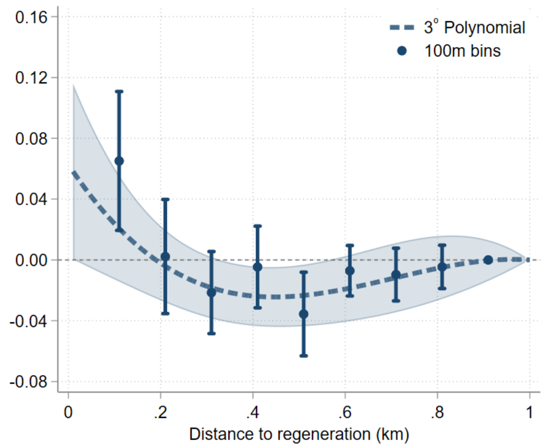
(b) Rents

Note: This figure reproduces Fig. 2-2 only using the sample of old units.

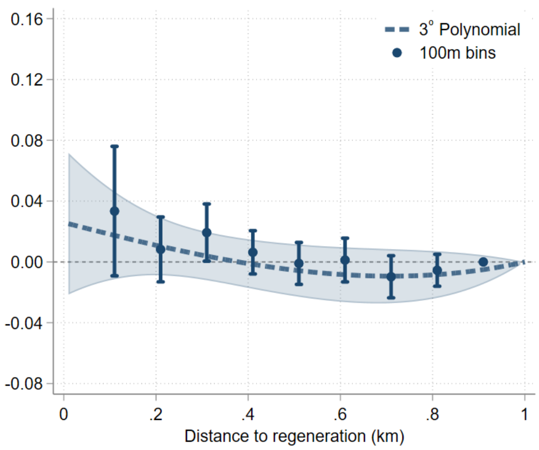
Figure F.18: Price effects for old houses with a continuous definition of distance for old units



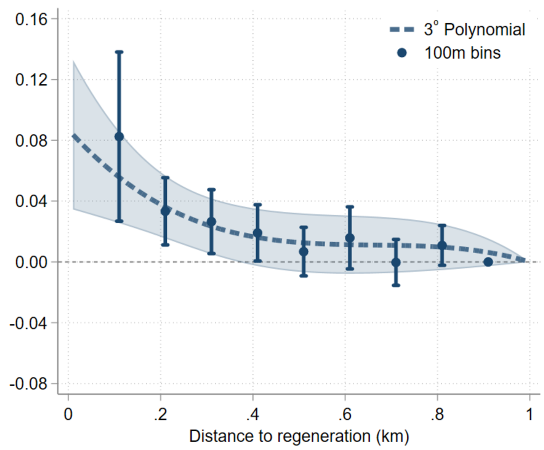
(a) House prices: 0-3 years



(b) House prices: 4-6 years



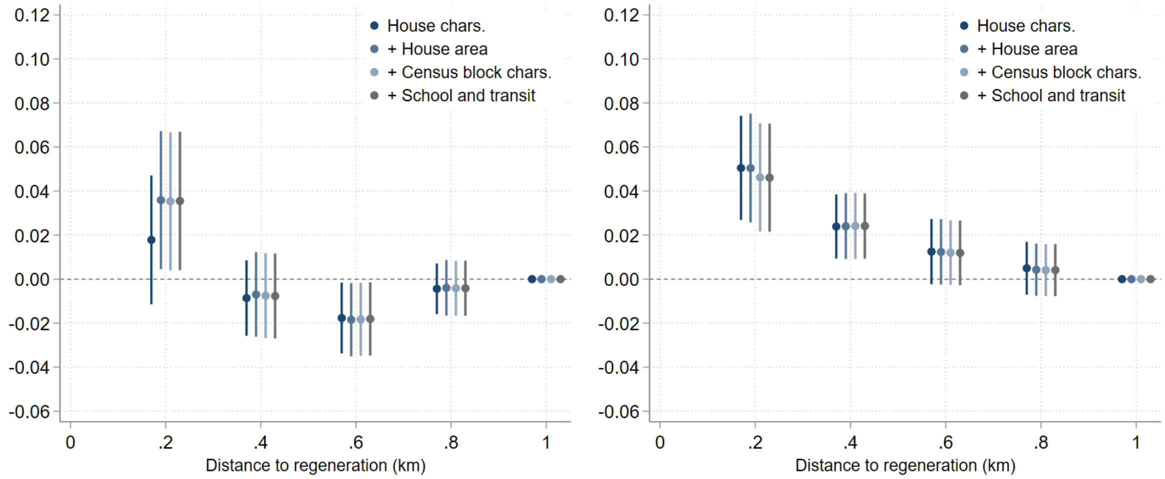
(c) Rents: 0-3 years



(d) Rents: 4-6 years

Note: This figure reproduces Fig. 2-3 only using the sample of old houses.

Figure F.19: Price effects in years 4-6 by 200m rings with several controls

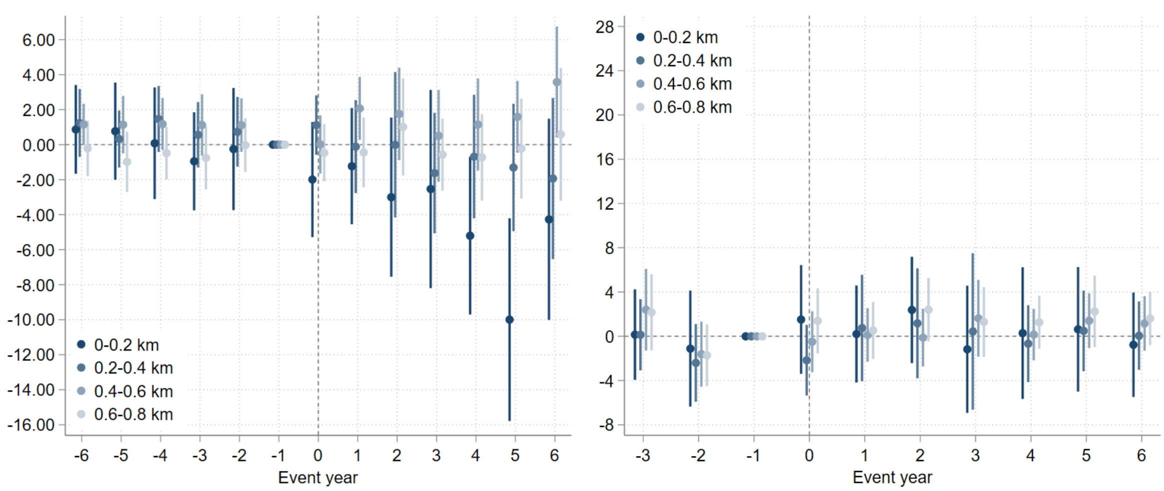


(a) House prices

(b) Rents

Note: The figure reports point estimates and 95% confidence intervals for coefficients $\theta_{1,r}$ in Eq. (2.2) using 200m rings. We include the control variables cumulatively, i.e. the gray estimates include all controls.

Figure F.20: Effects of estate regenerations on house area of house sales/listings, in sq ft

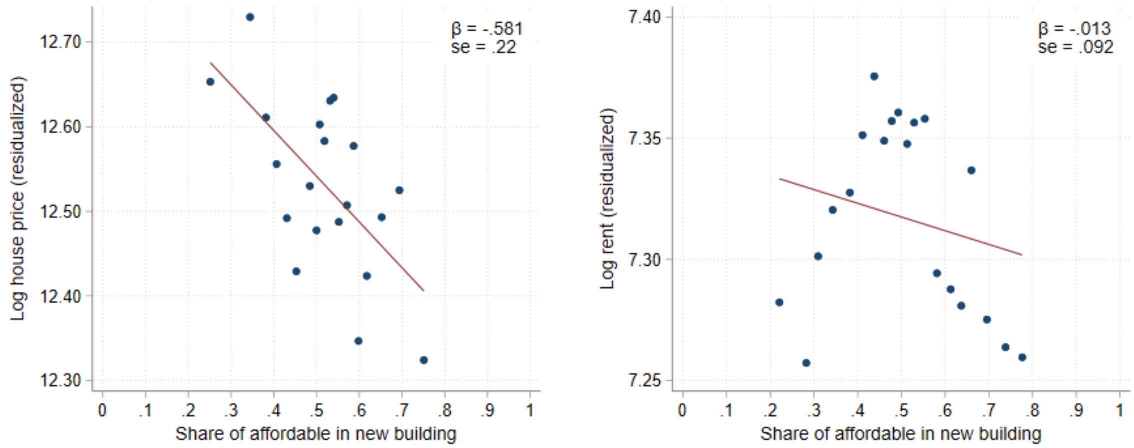


(a) House sales

(b) Rental listings

Note: The plots report coefficients $\beta_{\tau,r}$ and 95% confidence intervals of Eq. (2.1) using the average square footage of housing units in the period 2008-2018 of the postcode associated to each sale/listing. The plots use the residential sales and listings datasets –i.e. same dataset as Fig. 2-2.

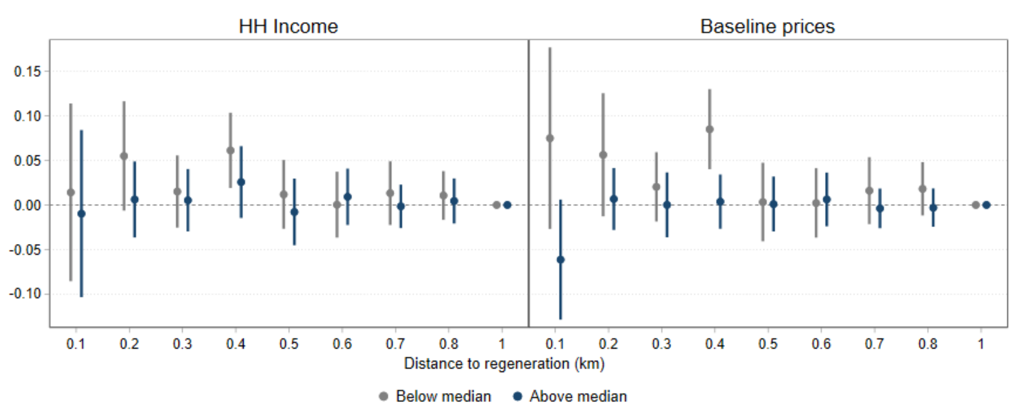
Figure F.21: Price elasticity to the share of public housing



(a) House prices **(b)** Rents

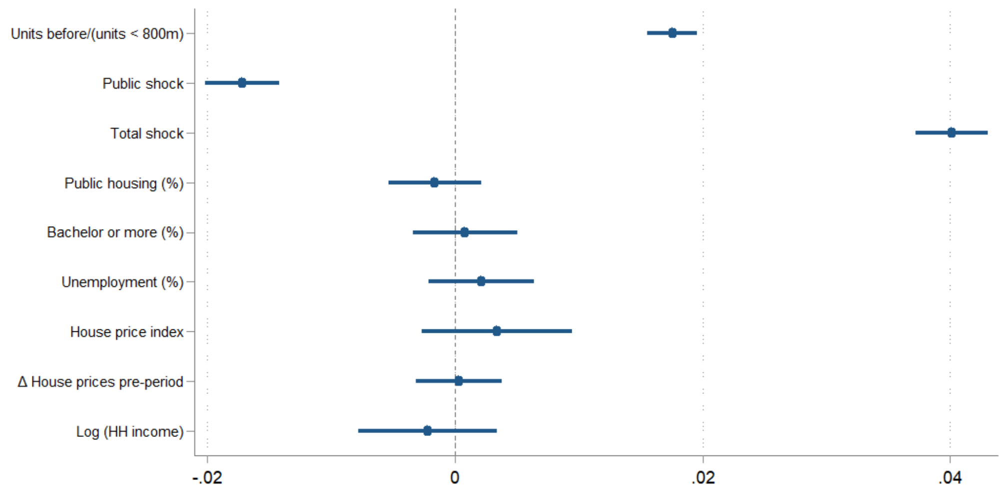
Note: The plots are binned scatterplots of the logarithm of house prices/rents of market-rate units in regenerated blocks against the share of public housing units of the regeneration for sales/listings taking place after permission approval. We residualize both variables using the controls $\mathbf{X_{ht}}$ in Eq. (2.1), as well as year FE, a dummy for Inner London, the number of units in the estate before and after, and neighborhood household income and baseline house prices. The vertical line is the result of a linear regression, with the coefficient and standard error reported in the top right.

Figure F.22: Effect on refurbishments 4-6 years after regeneration by neighborhood type



Note: The plots reproduce the analysis of panel (b) of Fig. 2-8 using a dummy indicating whether the rental unit was recently refurbished as the dependent variable.

Figure F.23: Correlation of the market shock with building and neighborhood characteristics



Note: This figure presents the estimated coefficients and 95% confidence intervals of a multivariate regression of the "market shock" variable on several building (units before, affordable shock, total shock) and neighborhood (the remaining variables) characteristics. The sample contains regenerations in the balanced sample, i.e. with a permission between 2004 and 2012. All variables used as regressors are standardized.

B.2 Tables

Table F.1: Back-of-the-envelope calculation: overall house price changes

	200m distance bins			100m distance bins		
	(1)	(2)	(3)	(4)	(5)	(6)
	Total (M)	Per unit	Pct. (%)	Total (M)	Per unit	Pct. (%)
All sales estimates	-356.5	-1,434.3	-0.7	-480.8	-1,920.0	-0.9
Old sales estimates	-448.9	-1,791.8	-0.8	-566.7	-2,250.5	-1.0

Notes: For the computation, the table uses average raw house prices at the census block level in the period 2000-2002, and the number of private housing units in 2001 times the average raw house prices as the housing stock value measure. Aggregate price changes are calculated in 2000-2002 millions of pounds, price changes per unit in pounds.

Table F.2: Effects of estate regenerations on crime (à la Gibbons (2004))

	Total crimes				**Criminal damage**			
	Mixed		Non-mixed		Mixed		Non-mixed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full	Large	Full	Large	Full	Large	Full	Large
0-200m	-0.453*	-0.648	-0.127	-0.015	-0.107***	-0.139**	-0.007	0.007
	(0.260)	(0.398)	(0.176)	(0.287)	(0.035)	(0.057)	(0.032)	(0.046)
200-400m	-0.043	-0.006	0.109	-0.132	-0.023	-0.041	0.006	-0.027
	(0.226)	(0.326)	(0.162)	(0.225)	(0.030)	(0.048)	(0.033)	(0.047)
400-600m	-0.196	0.145	0.185	0.312	-0.023	-0.014	-0.010	-0.023
	(0.174)	(0.192)	(0.188)	(0.302)	(0.021)	(0.025)	(0.025)	(0.035)
600-800m	-0.258*	-0.116	0.210	0.299	-0.031	0.002	0.010	0.045
	(0.145)	(0.195)	(0.196)	(0.317)	(0.020)	(0.025)	(0.027)	(0.038)
N	18,076	9,337	7,930	4,363	18,076	9,337	7,930	4,363
R-squared	0.92	0.91	0.92	0.93	0.65	0.65	0.64	0.67

Note: This table estimates Eq. (2.2) using 200m rings and collapsing the entire post-treatment period into a unique "Post" dummy. Columns 1-4 report the results for the total number of crimes, Columns 5-8 do it only for criminal damage crimes. Within each dependent variable, we report results separately for mixed-income and non-mixed regenerations (mixed-income are the main sample throughout this paper). "Full" columns show estimates for the entire sample, "Large" columns do it for regenerations with a number of existing units above the median of the sample. Panel data goes from year 2008 to 2018 and includes regenerations with a permission approval between 2009 and 2018. Standard errors in parenthesis (clustered at the LA level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F.3: Quality differences of new market-rate units on-site by market shock

	Unit chars.			Building chars.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Luxury	Modern	Washing	Pool	Gym	Garden	Concierge
Market shock > p50	0.050	-0.043	-0.002	0.000	0.015	-0.003	0.079*
	(0.043)	(0.072)	(0.044)	(0.007)	(0.027)	(0.053)	(0.041)
House chars.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Census chars.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School chars.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Distance to tube	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline nhood chars.	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	2,972	2,972	2,972	2,972	2,972	2,972	2,972
R-squared	0.18	0.09	0.12	0.20	0.35	0.23	0.21
Y mean	0.17	0.51	0.14	0.01	0.06	0.33	0.10

Note: This table regresses dummy variables indicating the presence of the corresponding keyword in the listing description of market-rate units in regenerated blocks on the market shock dummy Z_e for listings taking place after completion. As control variables, we include the controls $\mathbf{X_{ht}}$ in Eq. (2.1), as well as year FE, a dummy for Inner London, the number of units in the estate before and after, and neighborhood household income and baseline house prices. Standard errors in parenthesis (clustered at the LA level). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table F.4: Cost effectiveness calculations of public housing regenerations

Benefits	
House price increases	£3,647
Rent increases	£39,645
Children's future earnings	£21,729
Costs	
+ Demolition costs	£83,513
+ New construction costs	?
+ Relocation costs	£47,580
– Council tax savings	£41,511
– Stamp duty land tax savings	£4,816
Total costs (new construction costs in parenthesis)	
If LA pays subsidy per units	£147,525
	(£62,759)
If LA pays full new construction (lower bound)	£312,936
	(£228,170)
If LA pays full new construction (upper bound)	£430,765
	(£345,999)

Note: The quantities in this table are expressed in 2001 pounds per regenerated public housing unit. Appendix B.3.3 provides the details of this calculation.

B.3 Appendix

B.3.1 Data Appendix

B.3.1.1 House Prices and Rents: Coverage and Representativeness

The coverage of residential sales and rental listings in the data is comprehensive for our sample period. Fig. F.24 shows a histogram of the fraction of sales and listings per year. The plot shows a decrease in the number of sales around 2007 due to the Great Recession, while the number of listings is slightly increasing due to the increased popularity of online advertisements.

We limit the sample of residential sales and rental listings in several ways. In both cases, we drop sales and listings that are in the top and bottom 0.5% distribution of prices to decrease sensitivity to outliers. For rental listings, we make three further sample restrictions:

1. *Drop listings with more than 5 bedrooms.* The objective is making our results less sensitive to outliers and presumably very high-end properties.

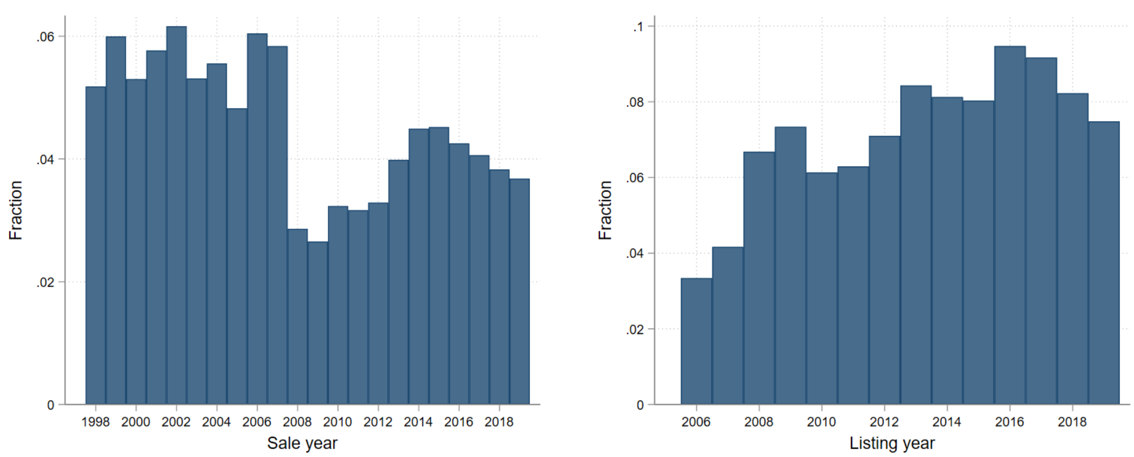
2. *Drop listings with extreme values.* For every postcode-number of bedrooms combination, we drop listings priced more than 3 times the mean rent. These instances are likely to be reporting errors.

3. *Drop listings reflecting bedroom prices.* For each postcode-number of bedrooms combination, we drop listings with a rental price that is less or equal 1.25 times the mean rent divided by the number of bedrooms. We only do this for listings with 2 or more bedrooms. This restriction is intended to eliminate listings referring to a single room within a unit.

We find that the sample of rental listings is representative of private rents in London. To show this, we compare rents in the Rightmove dataset with official estimates of average private rents at the LA level from the Valuation Office Agency (VOA). Fig F.25 compares the 25th, 50th, 75th percentiles and the mean LA private rents for the first and third quarters of years 2011-2016. Rightmove rents are 10% higher than official estimates across all reported statistics.

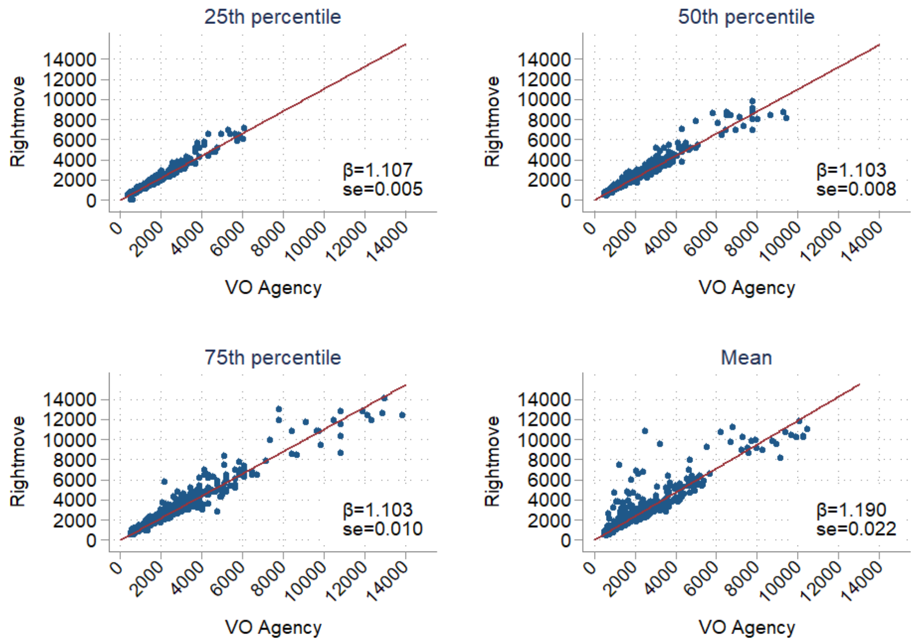
The difference between Rightmove rents and official estimates is at least partially driven by the fact that Rightmove mostly reports asking rents as opposed to agreed rents (76% and 24%, respectively). To explore this, Table F.5 regresses the logarithm of the rent on a dummy variable indicating whether the rent is the agreed price (asking price is the omitted category). We first run this regression without any controls, then we add LA-year FE and, lastly, we add a bunch of unit and neighborhood characteristics. The table shows that agreed rents are on average 5-10% lower than asking rents, close to the difference between Rightmove rents and official estimates. This result suggests that Rightmove rents are a good representation of private rents in London and are not disproportionately skewed to the high-end of the distribution.

Figure F.24: Histograms of residential sales and rental listings



(a) Residential sales **(b)** Rental listings

Figure F.25: Comparison of LA rents in Rightmove to rents in Valuation Office Agency (VOA)



Note: Scatter plot of several statistics from Rightmove and VOA at the LA level. Fitted lines are the result of a linear regression that does not include a constant.

Table F.5: Difference between asking and agreed rents (asking is omitted)

	(1)	(2)	(3)
Agreed rent	-0.103***	-0.066***	-0.047***
	(0.030)	(0.006)	(0.004)
House chars.	No	No	Yes
Census chars.	No	No	Yes
School chars.	No	No	Yes
Distance to tube	No	No	Yes
LA × year FE	No	Yes	Yes
N	4,826,481	4,826,481	4,817,825
Adjusted R-squared	0.01	0.38	0.70

Notes: The table shows the results from regressing the logarithm of the rental price on the rental price type (asking or agreed). The control variables that we use are equivalent to those used in Eq. (2.1). Standard errors are clustered at the LA level. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.3.1.2 Rental Listings' Description

To obtain a comprehensive picture of how rental housing characteristics evolve around regeneration, we scraped listings' descriptions using the website link in the dataset (95% have accessible links). Fig. F.26 is an example of a Rightmove rental listing in London. It usually provides the price, location, pictures and some key features. At the bottom of the listing, there is usually a description that provides more details of the advertised unit.

In many cases, agents describe not only properties of the units (bedrooms, new unit, bathrooms, etc), but also properties of the building and the neighborhood (amenities, cafés, trendy shops, vibrant). Using this, we created several variables related to these three categories. Table F.6 provides the relation of keywords to several of these variables: when any of the keywords are present in the description, the variable takes value 1; otherwise, it takes value 0.

Figure F.26: Example of rental listing's description

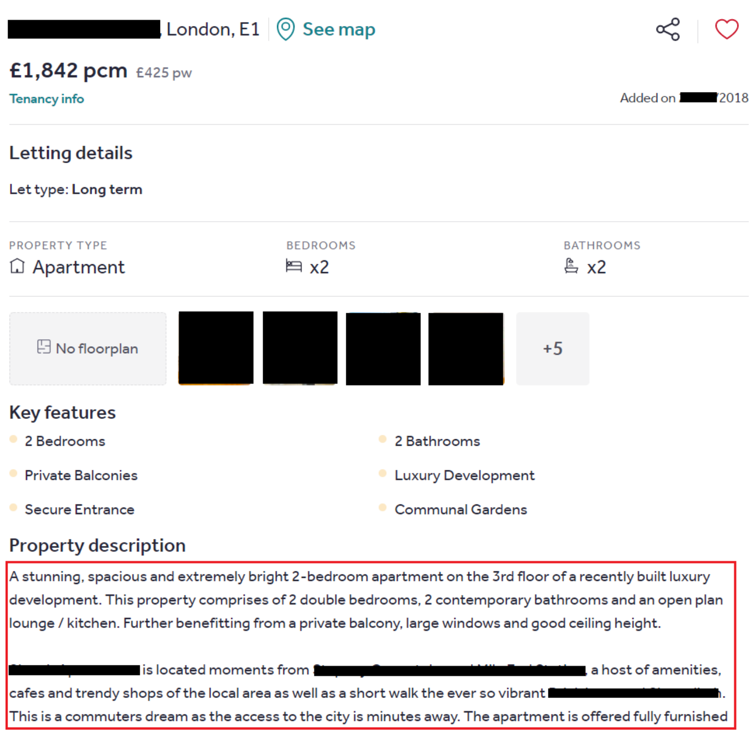


Table F.6: Relation of keywords in listings' descriptions

Variable	Keywords
Panel A: Unit characteristics	
New	brand new, new build, new construct, new develop
Refurbished	refurbish, renovat, rehabilitat, reform, upgrad
Luxury	luxur, deluxe
Washing machine	washing machine
Panel B: Building characteristics	
Garden	garden, courtyard, backyard, patio
Gym	gym, fitness
Concierge	concierge
Panel C: Neighborhood characteristics	
Amenities	amenities
Cafe	café, cafe, coffee
Restaurant	restaurant
Parks	park, green space

B.3.2 Main Results: "Public Housing Only" Regenerations

As we explain in Section 2.2.2, some public housing estates were regenerated by including only public housing units in the new building (henceforth "non-mixed"). However, Table F.7 shows that these estates are not directly comparable to the sample of mixed-income regenerations. First, non-mixed regenerations were much smaller in size (77 units versus 248). Since the distribution of units in the existing building does not overlap enough, we cannot use non-mixed regenerations as a counterfactual for mixed-income regenerations. Second, they were located in observably different neighborhoods of the city. Non-mixed regenerations were in less denser areas with less public housing, less renter households and much lower housing prices –presumably because they were more likely to be located in Outer London.

Despite this, we report the main results for non-mixed regenerations following our first empirical method, which uses housing units within 800-1,000m of the estate as a comparison group. In particular, Fig. F.27 estimates Eq. (2.2) for the logarithm of house prices and rents using 100m bins and a third-order degree polynomial to indicate distance to the estates. Table F.8 estimates the same equation for the inverse hyperbolic sine of the number of sales, rental listings and new construction.

The price effects of non-mixed regenerations are very different in the sales and rental markets. In the sales market, house prices do not react to the regeneration announcement –similarly to mixed-income regenerations–, but later drop by 8% within 100m of the estate. In contrast, rents rise by about 4% within right from the permission approval. Both markets are only affected in the immediately surrounding area (100m), which is consistent with the fact that these regenerations were smaller in size and, hence, had less potential to change the neighborhood. A potential explanation for the opposite effects in these markets is that unobservable quality aspects of sold units changes as a result of the regeneration, i.e. the lowest-quality units near the estates are sold.

Similarly, there are contrasting effects in the number of sales and rental listings. When examining the market for old units, the number of sales increases by 10% within

200m after the regeneration is approved while the number of rental listings decreases by a similar percentage. This result indicates that there might be some substitution between the sales and rental markets: landlords sell their units and buyers stay in the new apartments. In both markets, the effects on the quantity of sales and listings of new units are zero –although it decreases slighlty for rental listings in the long-run.

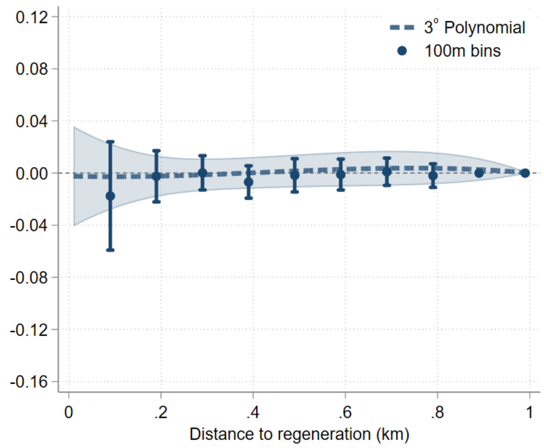
Finally, we find suggestive evidence that non-mixed regenerations are partially crowding out the construction of public housing in the short run. Columns 5 and 7 of Table F.8 show that the number of newly approved public housing units decreases by 5% within 200m for the first three years and that the probability of approving new public housing units decreases by 1.2 p.p..

Table F.7: Summary statistics of public housing regenerations

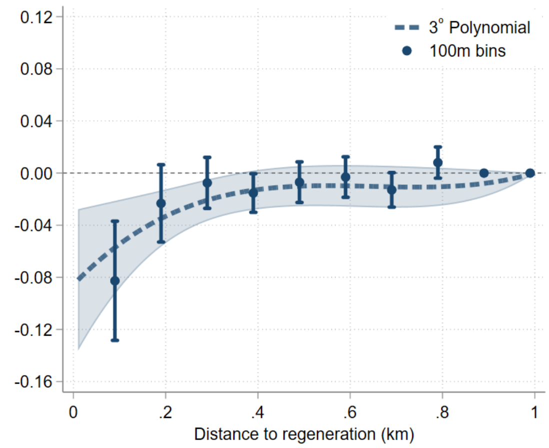
	London	Mixed-income		Non-mixed	
	(1)	(2)	(3)	(4)	(5)
	All blocks	Full	Balanced	Full	Balanced
Building characteristics					
Total units before		248	246	77	60
Public housing units before		206	194	75	60
Total units after		457	431	76	72
Public housing units after		197	208	73	70
Neighborhood chars. (2001)					
Density (per ha)	108	151	136	124	136
High education	0.24	0.21	0.20	0.19	0.20
Unemployment	0.07	0.10	0.10	0.09	0.09
Public housing units	0.26	0.48	0.49	0.39	0.38
Owner-occupied units	0.55	0.35	0.35	0.44	0.45
Privately rented units	0.15	0.14	0.13	0.13	0.14
House price index	11.66	11.67	11.63	11.54	11.56
Household income	35,548	33,328	32,318	31,709	31,915
Census blocks/Estates	24,115	135	70	122	78

Note: The table reports a subset of the same variable than in Table 2.1.

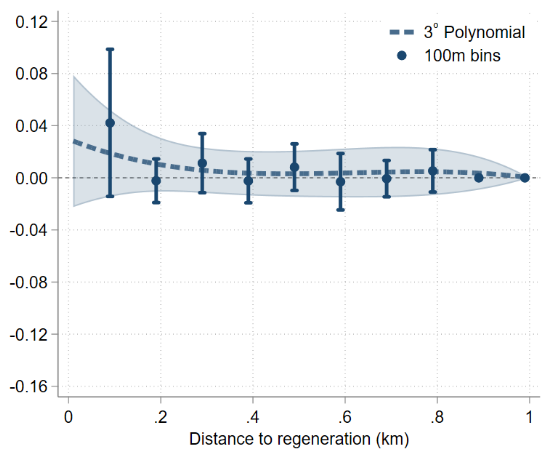
Figure F.27: Effects on house prices and rents with a continuous definition of distance



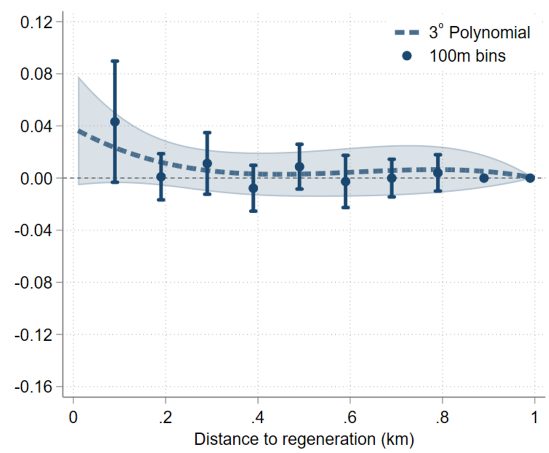
(a) House prices: 0-3 years



(b) House prices: 4-6 years



(c) Rents: 0-3 years



(d) Rents: 4-6 years

Note: The figure reports point estimates and 95% confidence intervals for coefficients $\theta_{0,r}$ (left panels) and $\theta_{1,r}$ (right panels) in Eq. (2.2) using 100m rings. The dotted line runs that same regression but using a 3rd order degree polynomial of the distance from each house sale to the regeneration site instead of rings. The shaded area indicates the corresponding 95% confidence interval.

Table F.8: Effects of regenerations on sales, listings and new construction

	ihs(house sales)		ihs(rental listings)		ihs(new construction)		prob(new construction)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	New	Old	New	Old	Public	Market	Public	Market
Panel A: 0-3 years								
0-200m	0.019	0.091***	-0.020	-0.131***	-0.052**	-0.015	-0.012**	0.002
	(0.025)	(0.031)	(0.031)	(0.038)	(0.020)	(0.025)	(0.005)	(0.012)
200-400m	0.007	0.045**	-0.016	-0.043	0.007	0.049***	0.001	0.026***
	(0.017)	(0.020)	(0.029)	(0.036)	(0.015)	(0.017)	(0.004)	(0.007)
400-600m	0.005	0.010	-0.020	-0.009	0.012	0.017	0.002	0.009
	(0.015)	(0.017)	(0.024)	(0.031)	(0.013)	(0.012)	(0.003)	(0.007)
600-800m	0.012	-0.007	0.011	-0.004	-0.006	0.004	-0.002	0.006
	(0.015)	(0.016)	(0.021)	(0.026)	(0.010)	(0.015)	(0.003)	(0.007)
Panel B: 4-6 years								
0-200m	-0.023	0.096***	-0.086**	-0.094*	-0.024	-0.009	-0.005	0.006
	(0.022)	(0.036)	(0.042)	(0.048)	(0.020)	(0.026)	(0.006)	(0.013)
200-400m	-0.001	0.029	0.016	-0.043	-0.001	0.011	0.001	0.009
	(0.018)	(0.026)	(0.031)	(0.036)	(0.017)	(0.025)	(0.005)	(0.009)
400-600m	-0.013	0.009	-0.041	-0.071**	0.015	0.015	0.002	0.003
	(0.015)	(0.022)	(0.030)	(0.033)	(0.015)	(0.019)	(0.004)	(0.008)
600-800m	0.006	0.018	0.007	-0.029	-0.006	0.006	-0.002	0.011
	(0.013)	(0.019)	(0.029)	(0.028)	(0.011)	(0.018)	(0.003)	(0.008)
N	86,784	86,784	58,269	58,269	84,155	84,155	84,155	84,155
R-squared	0.25	0.63	0.51	0.78	0.14	0.24	0.13	0.24

Note: The table reports estimates of coefficients $\theta_{0,r}$ (Panel A) and $\theta_{1,r}$ (Panel B) in Eq. (2.2) using 200m rings for different dependent variables. Columns 1-2 use the inverse hyperbolic sine (ihs) of the number of house sales per year by new build status. Similarly, columns 3-4 use the ihs of the number of rental listings by status. Columns 5-6 use the ihs of the number of new units approved for construction by tenure type (public housing or market-rate), while columns 7-8 use the probability of any new construction by tenure type. Standard errors in parenthesis. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.3.3 Cost Effectiveness of Regenerations: Details

In this section, we describe the steps to obtain the estimates in Table F.4, which computes the benefits and costs for regenerations with a permission approved between 2004 and 2012. All of our calculations are expressed in benefits/costs per regenerated public housing unit in the old building. Note that we deflate all estimates to 2001 prices using the Consumer Price Index of all items in the UK from FRED data.[1] Following Hendren and Sprung-Keyser (2020), we also use a discount rate of 0.03 when computing the net present discounted value (NPDV) of benefits and costs –we consider the NPDV at the time of permission approval and that regenerations take 4 years to complete.

B.3.3.1 Benefits

We consider the following concepts:

1. *House price increases.* We divide the aggregate effects on house prices within 100m as calculated in Table F.1 by the number of public housing units in the existing buildings.

2. *Rent increases.* We reproduce the calculation in Table F.1 for the NPDV of the change in all future rents within 400m (using 100m ring estimates). We use 2-bedroom rental listings in years 2006 to 2008 to construct baseline rents (deflated to 2001 prices). After converting monthly rents to annual rents, we multiply estimated rent increases within each 100m ring by the number of privately rented units in that ring, calculate the sum for the four rings and divide it by the number of regenerated public housing units.

3. *Children's future earnings.* We use the estimates in Neri (2020) to translate the effects of regenerations on nearby primary school-age children's test scores to future earnings. We closely follow the computation in Hendren and Sprung-Keyser (2020).
   First, we obtain the lifecycle earnings for the average person in London. We use Table 6.7a of the UK's Annual Survey of Hours and Earnings (ASHE), where the mean earnings are reported by age group (18-21, 22-29, 30-39, 40-49, 50-59, 60+). We obtain average earnings at every age by fitting a fourth-order

[1] https://fred.stlouisfed.org/series/GBRCPIALLMINMEI

polynomial to a dataset that assigns the mean earnings of each income group to the midpoint age in that group. In this exercise, we assume that individuals earn income only in ages 18-65.

Second, we compute the number of primary school-age children exposed to regenerations as those living within 1km of a regeneration in 2002. We assume that the number of children of ages between 5 and 11 within 1km is the same at the moment of completion of the regeneration process. For every age and completion year, we estimate the NPDV at the moment of permission of future earnings assuming a wage growth of 0.5% per year, as is assumed in Hendren and Sprung-Keyser (2020). Finally, we aggregate the total NPDV of future earnings of all children within 1km of a regeneration and divide it by the number of public housing regenerated units.

B.3.3.2 Costs

We include the following mechanical costs of regeneration:

1. *Demolition costs.* The cost of demolishing a public housing unit includes the structural building demolition cost, home loss and disturbance costs, and buying the remaining private units in the building (previously bought through the RTB scheme). We obtain the demolition cost estimates from Power (2008), which is around £17,500-35,000 per unit in 2006 –we take the upper bound. We place the value of home loss and disturbance at £8,900 in 2018 –from a research report for the regeneration of a specific estate, Aylesbury estate.[2] For buying RTB units, we estimate the average value of old units within 800m of any estate in 2001 and adjust it by the ratio of RTB units to public housing units in the old building.

2. *New construction costs.* Official estimates are not available, thus, we draw on research reports to estimate the construction costs for the government. Since the financing of new units varies from estate to estate, we consider two different scenarios. On the one hand, we consider that the government pays a flat fee of £100,000 per regenerated unit that stays at social rent (71%) and £38,000 for other rent levels (in 2018 pounds).[3] On the other hand, we consider that the

[2] "The Costs of Estate Regeneration: A Report by Architects for Social Housing", by Architects for Social Housing (ASH)

[3] Source: Mayor of London, *"Building Council Homes for Londoners"*, Funding Prospectus, May 2018. These quantities are grants that LA can obtain from the Greater London authority.

government pays for the full cost of new construction, which might range from £145,500 to £305,000 per unit (in 2016 and 2018 pounds, respectively).[4] We adjust these quantities by multiplying them by the ratio of public housing units in the new relative to the old building.

3. *Relocation costs.* We assume that the cost of relocating one family is equal to the rental price of a 2-bedroom apartment within 800m of a regeneration from the permission to the average completion year. To implement this, we compute the average rent of a 2-bedroom apartment of regenerations taking place between 2007 and 2012 –balanced sample for rental outcomes– and adjust it downwards by 7.5% if it is an asking rent –based on Table F.5. We weight the regeneration-specific average rents by the number of public housing units in the old building and take the NPDV in the permission year.

We subtract the following tax savings:

1. *Council tax savings.* We compute the NPDV of the future stream of new council tax revenues of market-rate units in the new building. The council tax is a lump-sum tax on property. Each property is assigned a council tax band depending on the value of the housing unit at 1991 prices –there are eight bands in total. First, we compute the mean council tax rate per band across LAs, weighted by the number of market-rate units in regenerated buildings in each LA. Second, we deflate to 1991 prices all sales of new units taking place in regenerated blocks from years 0 to 6 relative to permission. Third, we apply the corresponding mean council tax rate to each sale according to their council tax band. Fourth, we compute the aggregate NPDV of all future revenues. Finally, we express this number in terms of pounds per regenerating units. We first multiply it by the ratio of the change in total market-rate units in the building to the number of observed new unit sales in order to reflect all new market-rate units –rental units included. Then, we divide this number by the number of regenerated public housing units.

2. *Stamp duty land tax savings.* We compute the NPDV of the stamp duty land tax (analogous to a property tax), which is imposed on the purchase of land and properties with values over a certain threshold. To do this, we apply the tax to all sales of new units in regenerated blocks according to their value –we

[4]Sources: "Completing London's Streets", by Savills UK (Research Report to the Cabinet Office) and ASH

use the rates just before July 2020. We aggregate these quantities, compute the NPDV in the permission year, and divide it by the number of regenerated public housing units.

Appendix C

Chapter 3 Appendices

C.1 Data Appendix

To obtain a comprehensive picture of the number of public housing units demolished in every census tract, we match developments demolished under the HOPE VI program to their geolocated addresses in the 1996 HUD-951 form public file. For each city included in the sample, we follow these steps:

1. **Match developments in HOPE VI with 1996 HUD-951 form**. HOPE VI administrative data only provides the name and, in some cases, the HUD project number of the public housing development. For this reason, we use the 1996 HUD-951 form public file to associate them to geolocated addresses, which allows us to assign each demolished unit to a particular census tract.

    - For "revitalization" grants, HOPE VI administrative data provides the HUD project number of the development, which is also indicated in HUD-951 forms. Thus, we match on this number.
    - For "demolition only" grants, we only obtained a list of development names. We proceed as follows. First, we manually discard those developments already counted in a "revitalization" grant. Second, we use an algorithm that matches development names in the "demolitions only" grant list with similar development names in the 1996 HUD-951 form public file, within

the same city. To do this, we use the package "matchit" in Stata. Finally, we manually revise all of the matches.

- In the case of Chicago, we include the list of non-HOPE VI demolished public housing addresses, which was provided by the Chicago Housing Authority through a FOIA request.
- We merge the three datasets above to obtain the full list of demolished public housing addresses.

2. **Compute the number of demolished units per address**. Not all of the developments were fully demolished, thus, we use the following method to compute the number of demolished units per address:

   - First, count as demolished all units that appear in the 1996 HUD-951 form public file[1].
   - Second, we manually check the developments that were partially demolished and change the number of demolished units to reflect the actual number under HOPE VI.

[1]In this file, sometimes the total number of units in geolocated addresses for a development is less than the actual number of units. We solve this by assigning every geolocated address a proportional number of the missing geolocated units until obtaining the total number of units in the development.

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