Pecuniary Effects of Public Housing Demolitions: Evidence from Chicago*

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Abstract

This paper studies the effects of 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 can impose significant adverse consequences on its neighbors.

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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, Johnson and Zeckhauser (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, Diamond and Hainmueller, 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 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

[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.

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.

2 Background and Data

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 in-

[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).

cluded 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 national 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, Katz, Cunningham, Brown, Gustafson and Turner, 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.

[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

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 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 B.1 lists all developments and the relationship of grants, award years and demolition dates.

[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

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 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]

$$\ln \mathbf{P}_{ht} = \rho_{c(h),t} + \alpha_m + \gamma' \mathbf{X}_{ht} + u_{ht} \tag{1}$$

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

[7] Appendix C.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.

[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.

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 C.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 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 C.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.

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.

[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.

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.

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. Ia 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. Ib 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.

[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).

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. III 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$\times$1" and "Neighbor$\times$2". The former 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.

[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.

Summary statistics

Table I 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 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.

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:

$$\begin{aligned} \min_{W_i(\lambda)\in\mathbb{R}} \quad & \|X_i - X_0 W_i\| + \lambda \sum_{j=1}^{n_0} \|X_i - X_j\| W_{i,j} \\ \text{s.t.} \quad & \sum_{j=1}^{n_0} W_{i,j} = 1 \\ & 0 \leq W_{i,j} \leq 1 \ \ \forall \text{ i,j} \end{aligned} \tag{2}$$

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. (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

[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 D provides more details on the choice of this matrix.

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 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

[16] Appendix D provides a more detailed explanation of the penalized synthetic control methodology, including how to estimate λ.

treated units in the treatment group, then:

$$\tau_t = \frac{1}{\sum_{i=1}^{n_1} H_i^{1990}} \sum_{i=1}^{n_1} H_i^{1990} \times \tau_{it} \tag{3}$$

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 D provides the details.

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.

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. II plots the path of price effects by treatment group and the first columns of each group in Table II 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 B.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. III highlights untreated tracts by the sum of weights with which they contribute to the synthetic control of Neighbor$\times$1 tracts, with darker blue colors indicating a higher contribution.[19] Finally, tracts in the

[17]See Appendix E 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$\times$1 tracts of this development are geographically close.

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 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 B.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* to farther away areas in the

[20] A comparison between Fig. III 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$\times$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.

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.

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 III 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$\times 1$ (15%), and Neighbor$\times 2$ (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.

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 II, 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 B.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 B.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$\times$2"), I use house sales in the third ring of tracts as the control group ("Neighbor$\times$3"), which gives a flavor of a more traditional spatial DID design but avoiding issues arising from overlapping rings. Panel (a) plots the coefficients for a specification in which the treatment period is 1994 for all treated tracts, the year when the first demolition is announced. This exercise gives a sense of how long-run house prices evolved in calendar time in each of the treatment groups relative to slightly farther away areas. Panel (b) uses years relative to the announcement of the first demolition in the house sale's tract instead of calendar years. Both approaches lead to very similar results to synthetic controls: house price increases of around 35%, 20% and 0% in the Demolition, Neighbor$\times$1 and Neighbor$\times$2 tracts, respectively.

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.

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. IV 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 ΔP^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 ($\Delta P^A = 0$). 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 (ΔH^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]

$$\Delta \ln P^S = \frac{\Delta \ln H^S}{\varepsilon^s} \tag{4}$$

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 ($\Delta \ln H^S$), and (iii) an estimate of the housing supply elasticity in the market (ε^s). Given that these items are either difficult to define precisely or imperfectly observed, Section 5.3 estimates the public supply effect using alternative definitions of these items.

[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.

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 III 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 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 III 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 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).

[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.

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. V 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).

5.3 Estimating the Public Supply Effect

I present a range of estimates for the public supply effect in Eq. (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

[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 C.2.

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 IV 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]

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

[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 (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.

of displaced tenants. The upper bound, ΔH^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 IV 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 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 IV 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

[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 $\Delta \ln H^S$ 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.

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$\times 1$ tracts, the second also includes Neighbor$\times 2$ 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 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 IV).[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.

[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.

[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.

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.

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 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.

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

[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 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.

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.

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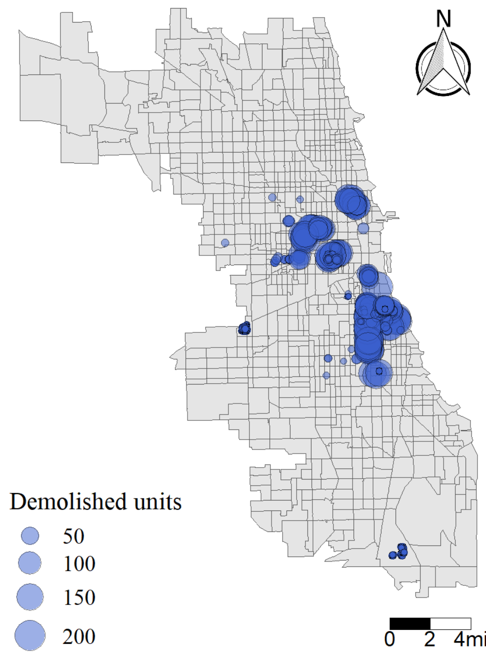
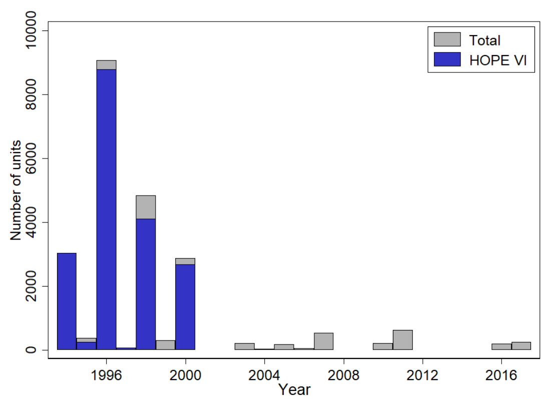
Figures

Figure I: Public housing demolitions: location and timing



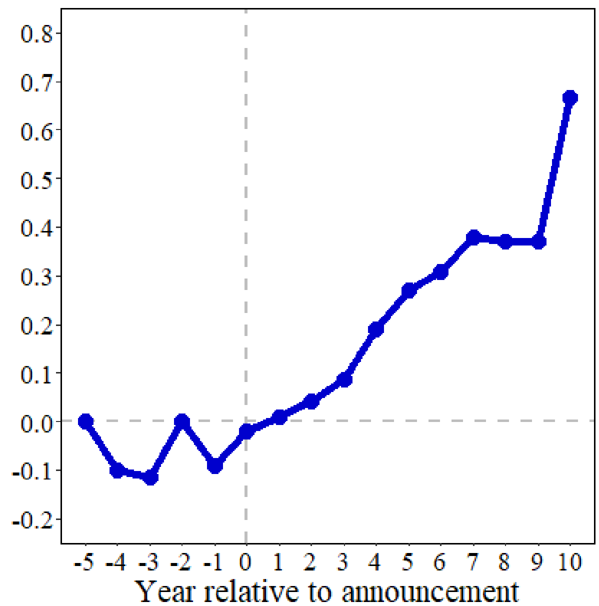
(a) Demolished addresses (1995-2018)



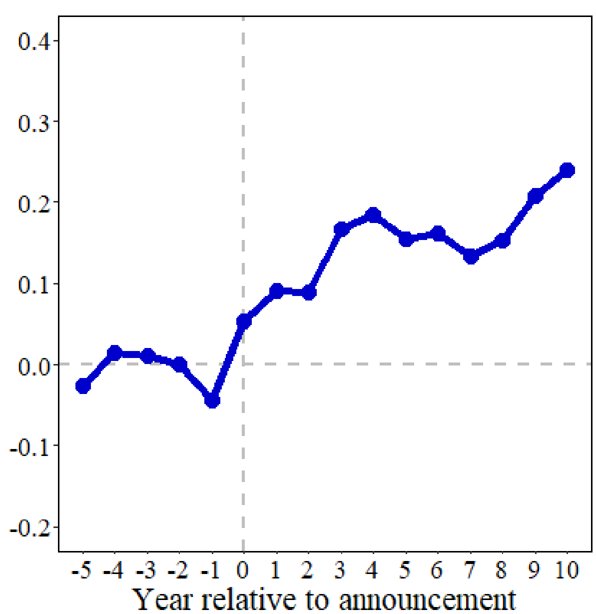
(b) Units announced for demolition by year

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.

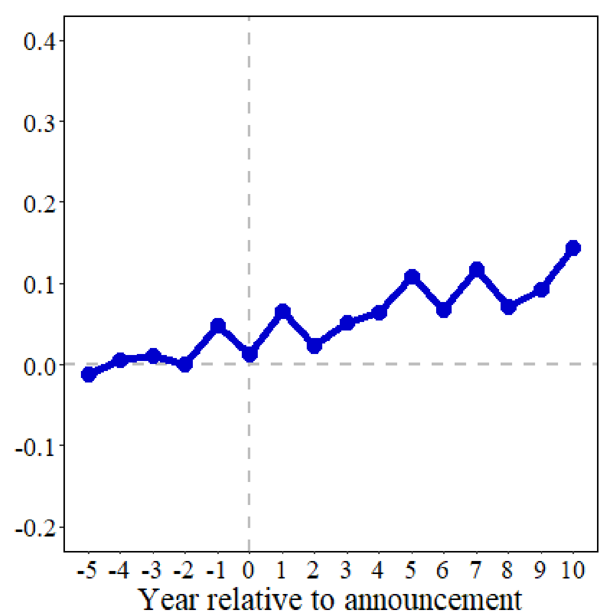
Figure II: Effects of demolitions on the house price index, ρ_{ct}



(a) Demolition



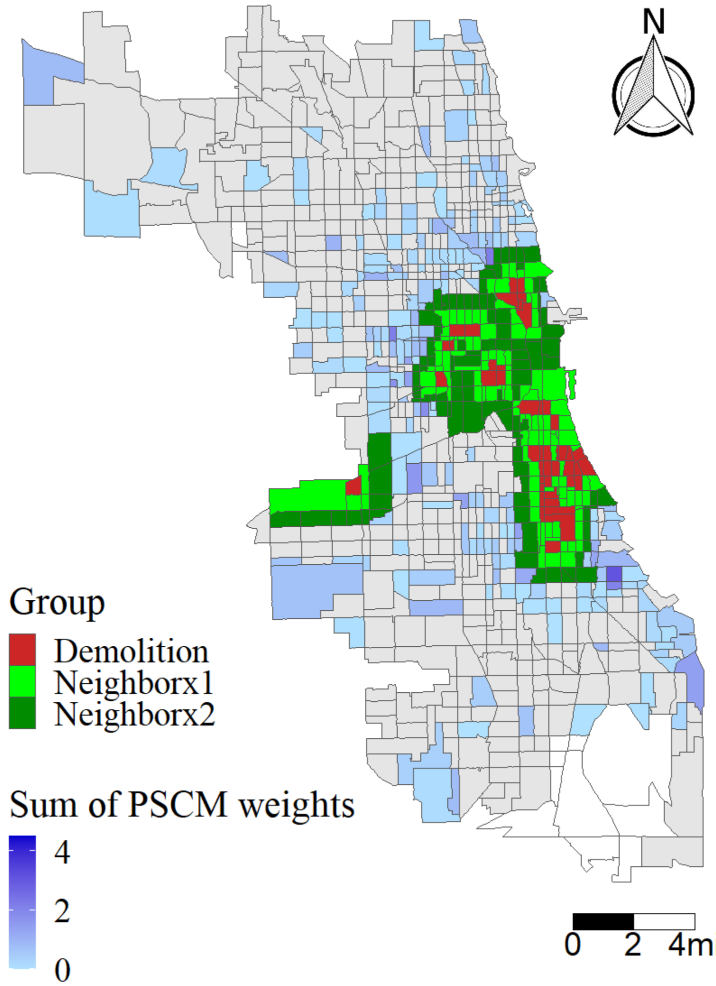
(b) Neighbor$\times$1



(c) Neighbor$\times$2

Note: The graph plots the evolution over time of τ_t in Eq. (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 III: Treated tracts and contributors to synthetic controls for Neighbor$\times$1 tracts



Note: This figure illustrates the contribution of each 1990 census tract to the creation of synthetic controls for the Neighbor$\times$1 treatment group. In particular, it reports the sum of weights with which each tract i contributes to each of the synthetic controls of that treatment group ($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$\times$1 (light green) and Neighbor$\times$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 IV: Private housing market before and after the demolitions

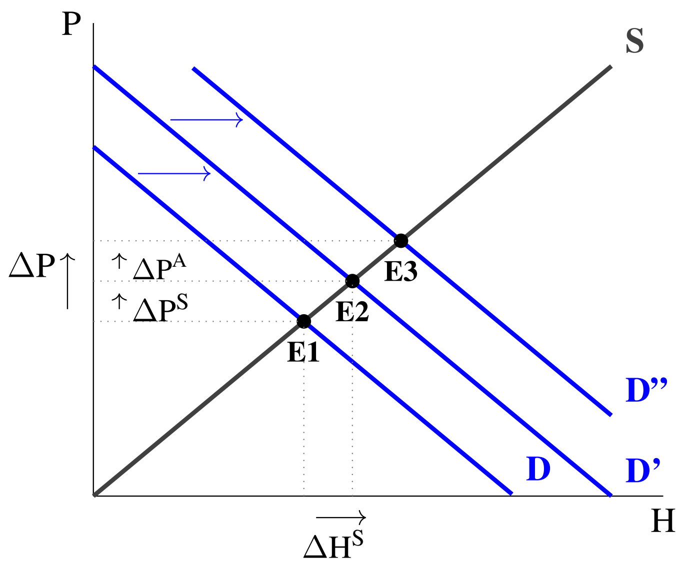
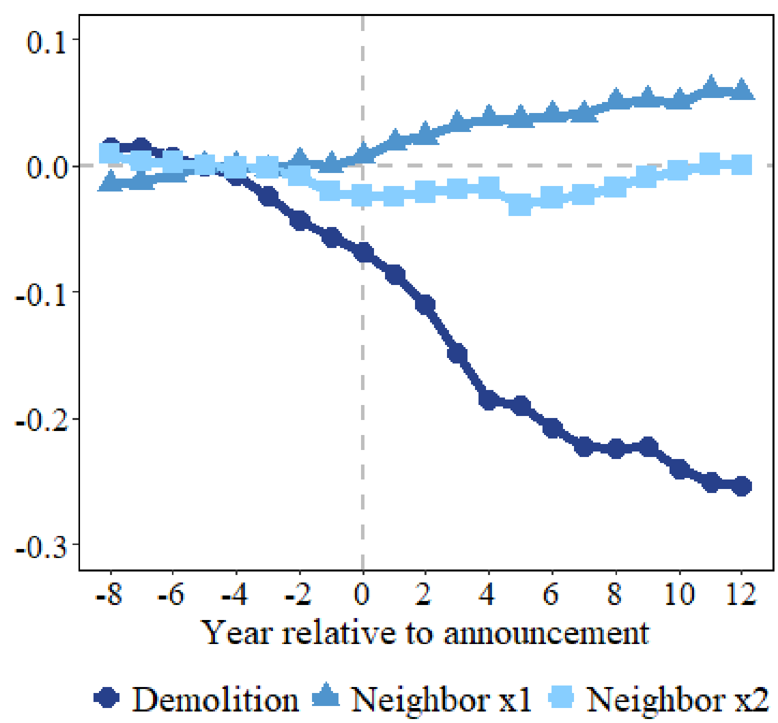


Figure V: PSCM: Effects of demolitions on (Infutor) population count



Note: The graph plots the evolution over time of τ_t in Eq. (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 I: Descriptive statistics by treatment group

	Demolition		**Neighbor**$\times 1$		**Neighbor**$\times 2$		**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 II: Price effects and permutation p-values by period

	Demolition		Neighbor $\times 1$		Neighbor $\times 2$		Neighbor $\times 3$
	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. (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 III: Effects on long-run census tract characteristics (using 1990 as baseline)

	2000				2010			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rent	Units	Income	Black	Rent	Units	Income	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. (3). I use the outcome variable in 1990, in addition to the census tract characteristics mentioned in Section 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 IV: Estimates of the implied public supply effect by housing market

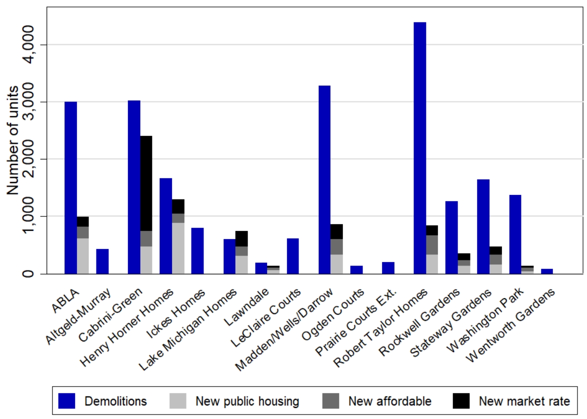
	Price effect $\Delta \ln P$	**Public supply effect** $\Delta \ln P^S$				$\frac{\Delta \ln P^S}{\Delta \ln P}$ **Pct (%)**	
	Period: 5-10 y.	$\varepsilon^{\text{Tract}}$ ΔH^L	$\varepsilon^{\text{Tract}}$ ΔH^U	$\varepsilon^{\text{Saiz}}$ ΔH^L	$\varepsilon^{\text{Saiz}}$ ΔH^U	Min	Max
Proximity-based							
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 $\Delta \ln \mathrm{H}^S$ 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 $\Delta \ln \mathrm{H}^S$ in Eq. (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).

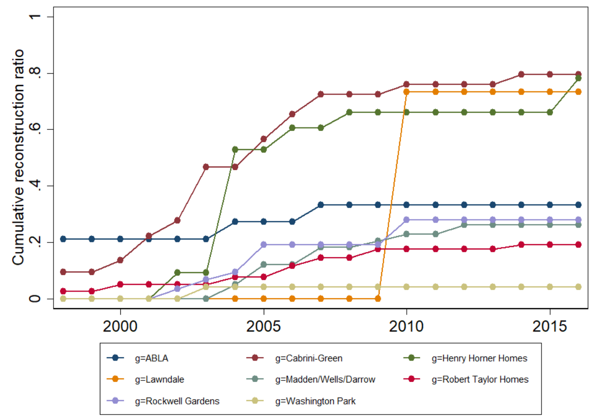
Online Appendix

A Figures

Figure A.1: New construction by public housing development



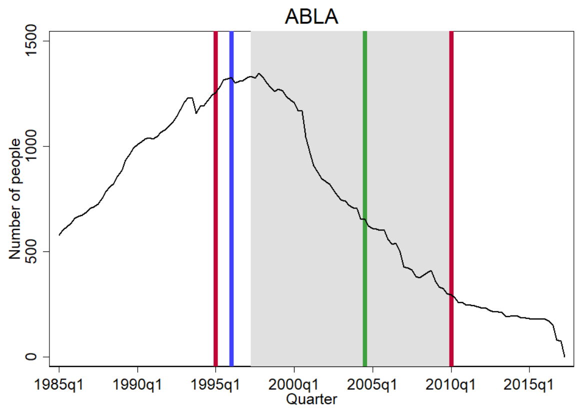
(a) By unit type



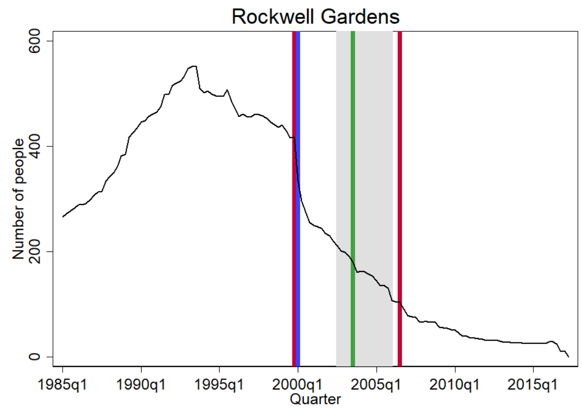
(b) Timing of reconstruction (completion)

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.

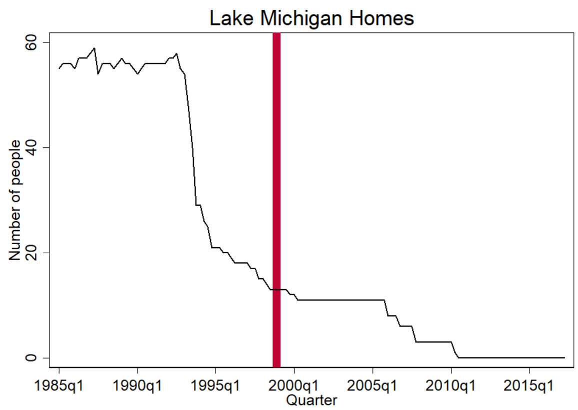
Figure A.2: Evolution of Infutor population by development



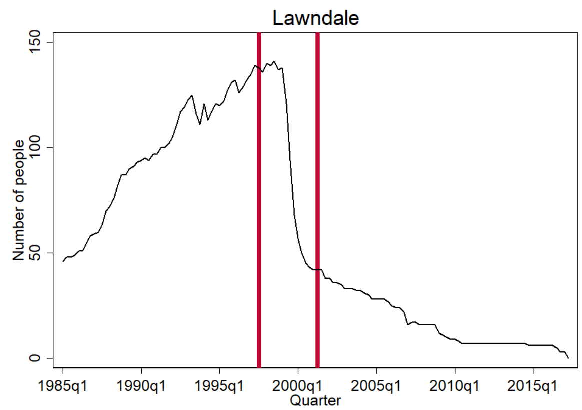
(a) ABLA Homes



(b) Rockwell Gardens



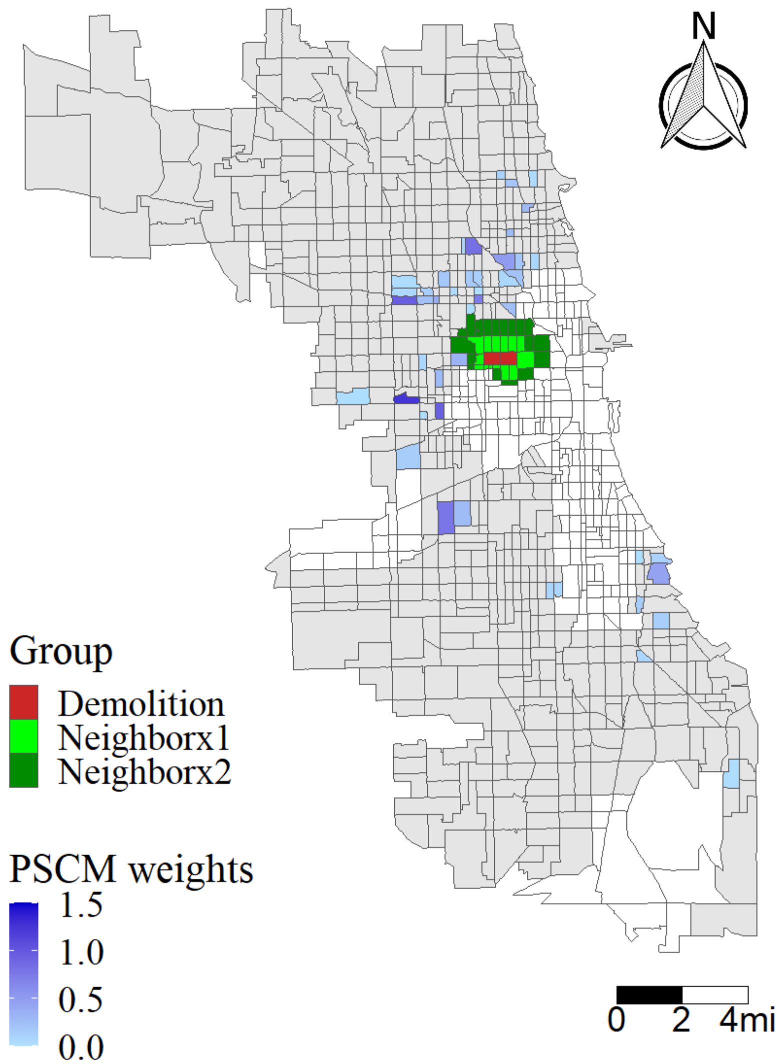
(c) Lake Michigan Homes



(d) Lawndale

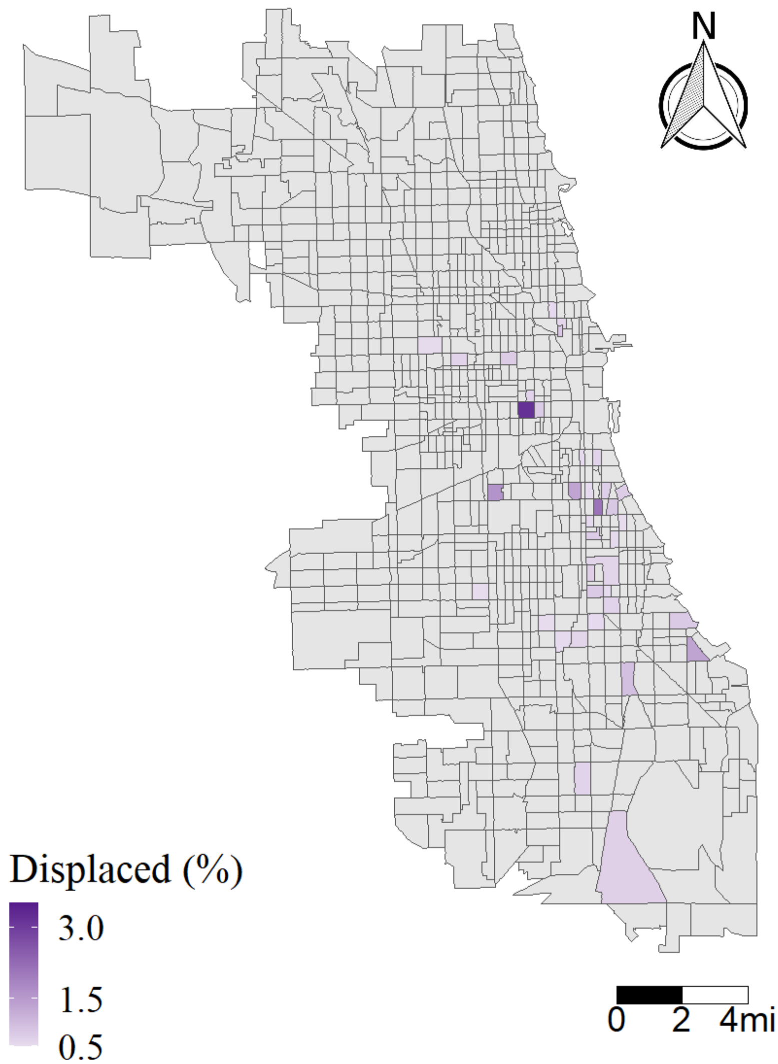
Note: Red lines indicate start and end dates of demolition as reported by the Chicago Housing Authority, while blue and green lines denote the grant award date and the start date of new construction, respectively, as in HOPE VI administrative data. The shaded area is the period in which tenants were 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



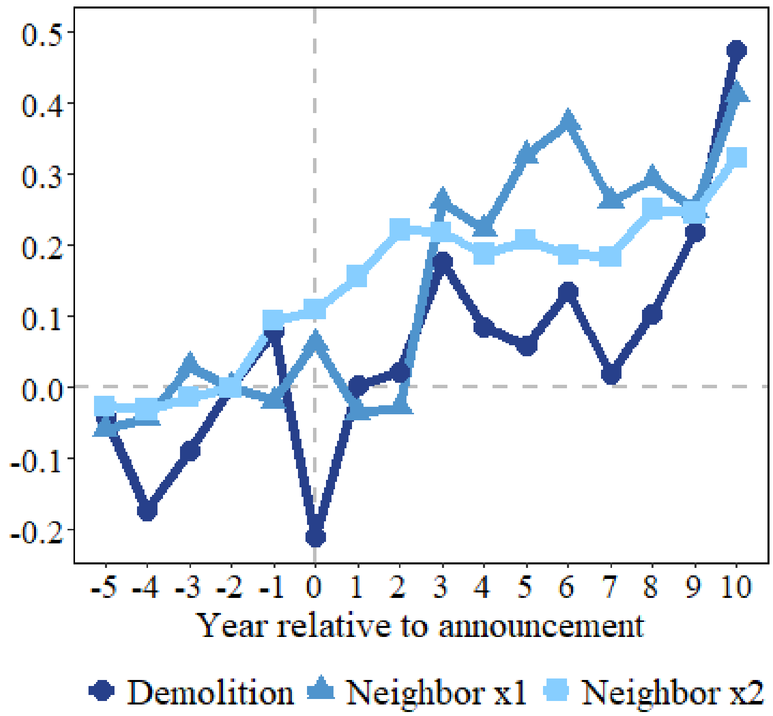
Note: This figure reproduces the map in Fig. III but for the Neighbor×1 treatment group corresponding only to the Henry Horner Homes.

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



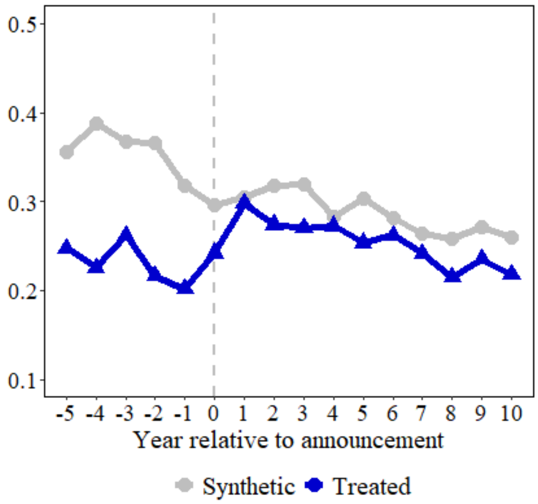
Note: This figure plots the percentage of total displaced tenants migrating to each census tract as observed in the displacement dataset described in Section 2.2.

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

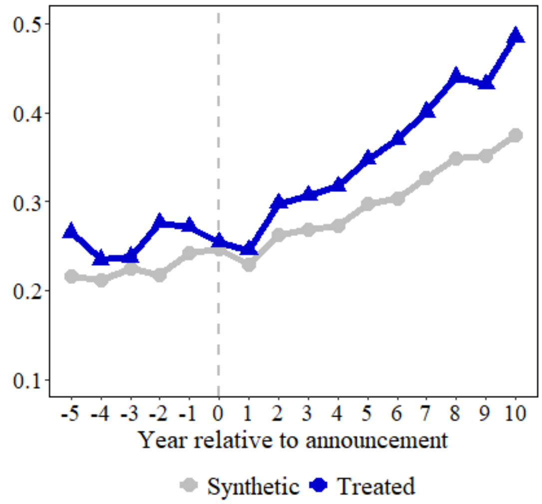


Note: The graph plots the evolution over time of τ_t in Eq. (3) by treatment group using the inverse hyperbolic sine (asinh) of the number of sales as an outcome variable. For this plot, penalized synthetic control methods (PSCM) are used on the "Full sample". The x-axis indicates the year relative to the first demolition announcement.

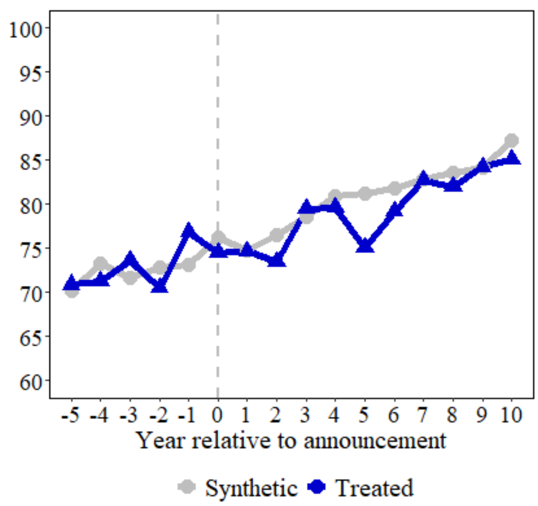
Figure A.6: Evolution of house characteristics for Neighbor×1 tracts



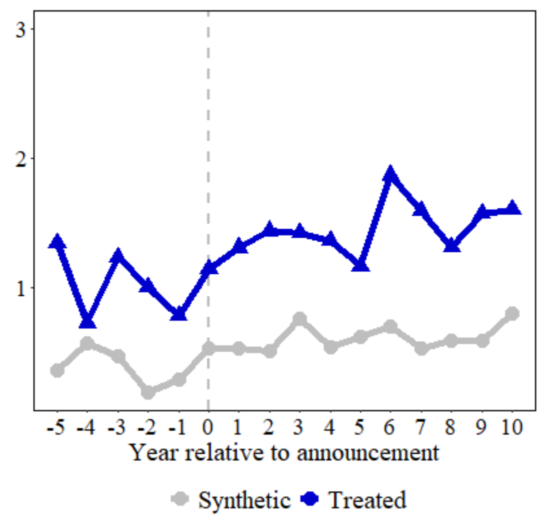
(a) SFR - share of sales



(b) Condo - share of sales



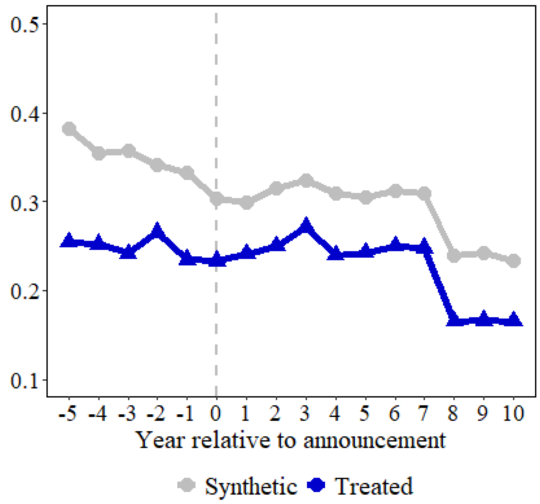
(c) Building age



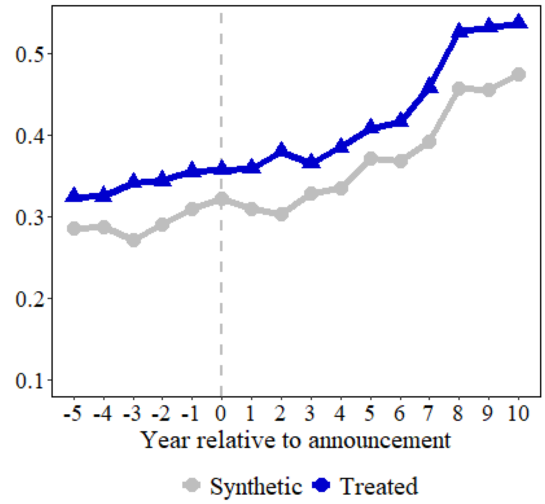
(d) Number of bathrooms (SFR)

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.

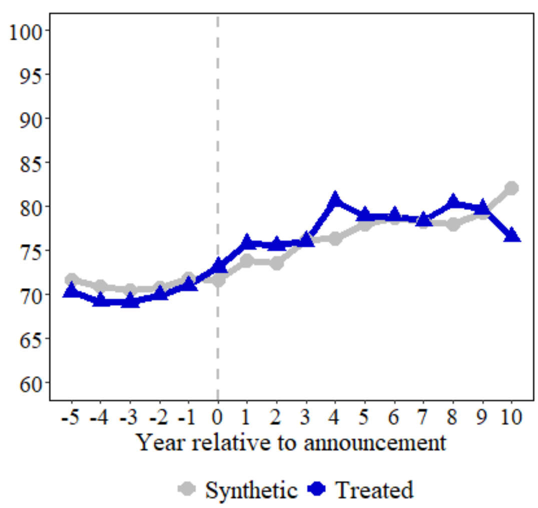
Figure A.7: Evolution of house characteristics for Neighbor×2 tracts



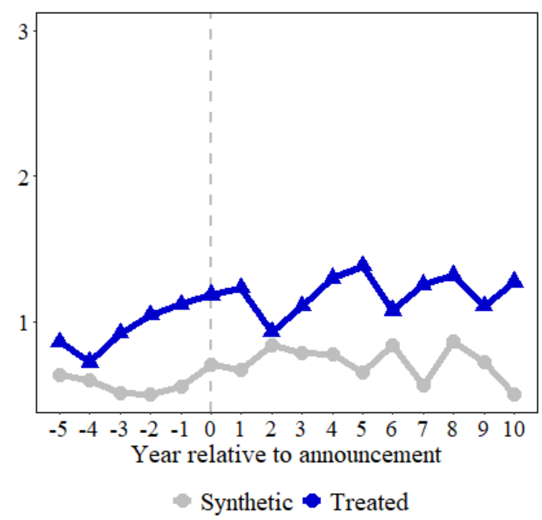
(a) SFR - share of sales



(b) Condo - share of sales



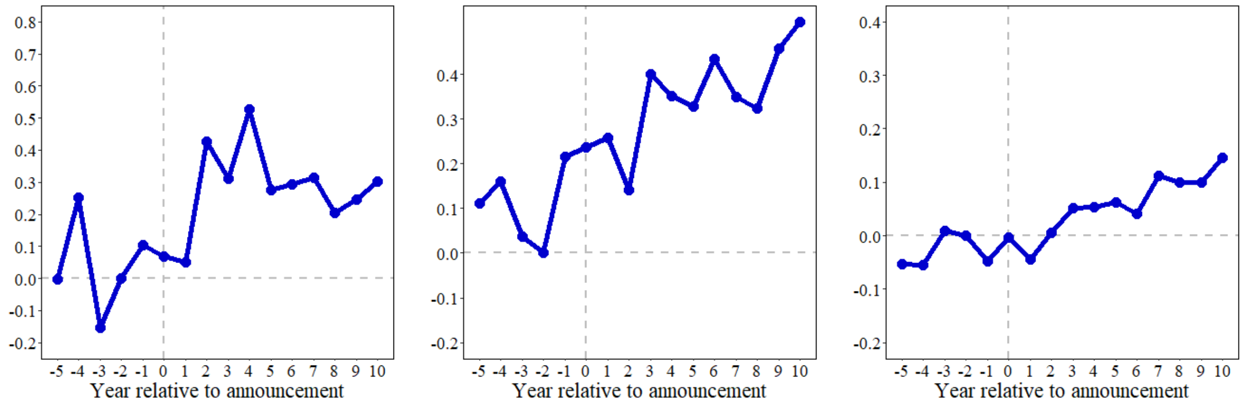
(c) Building age



(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.

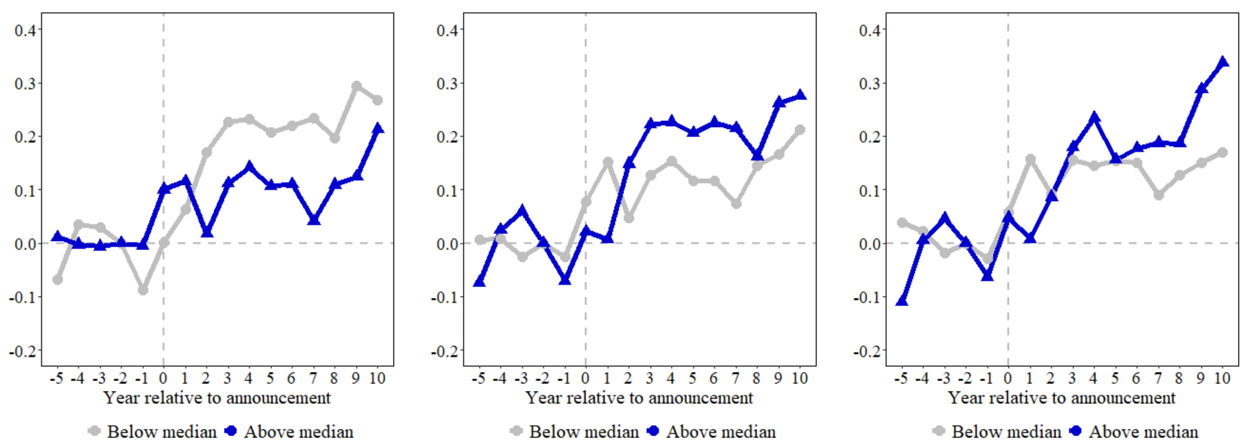
Figure A.8: Effects of demolitions on the house price index for single family houses, ρ_{ct}



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

Note: The graph plots the evolution over time of τ_t in Eq. (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".

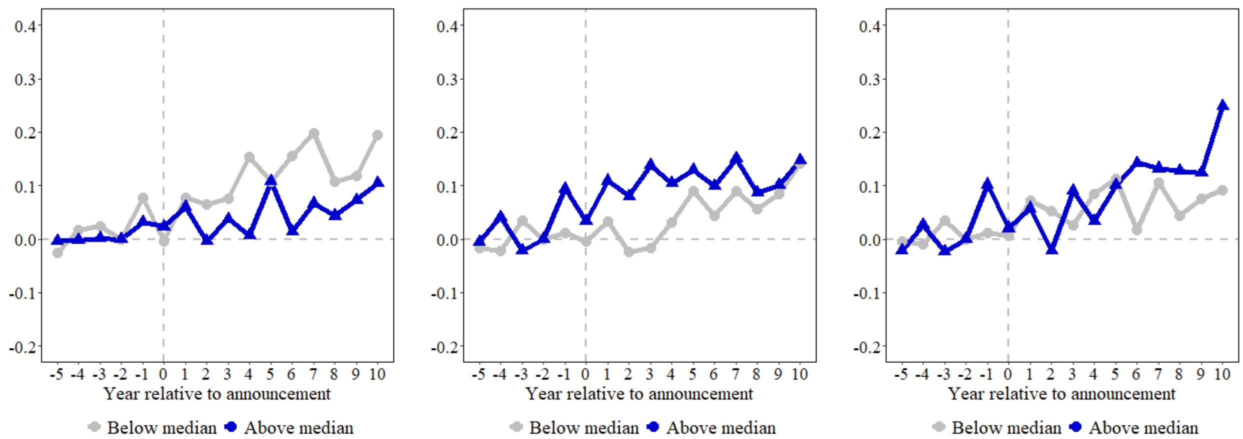
Figure A.9: Heterogeneity of price effects by baseline variables for Neighbor×1



(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. (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".

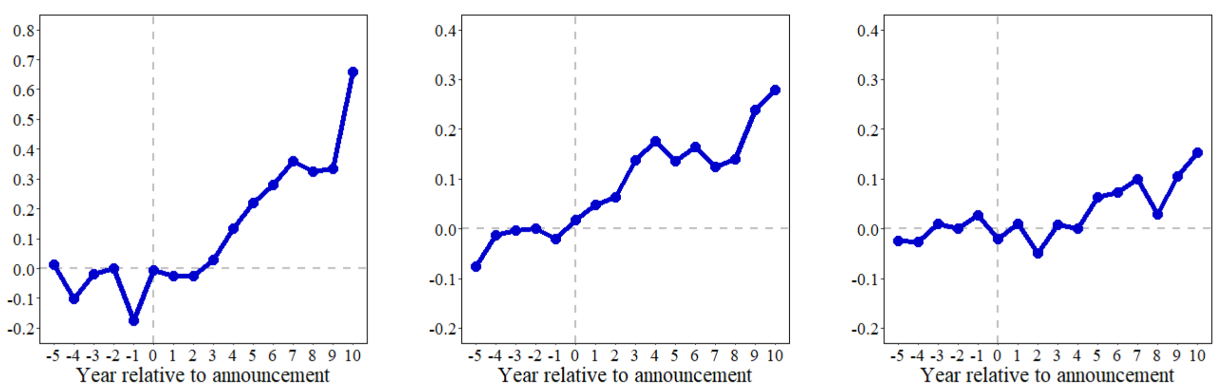
Figure A.10: Heterogeneity of price effects by baseline variables for Neighbor$\times$2



(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. (3) for Neighbor$\times$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".

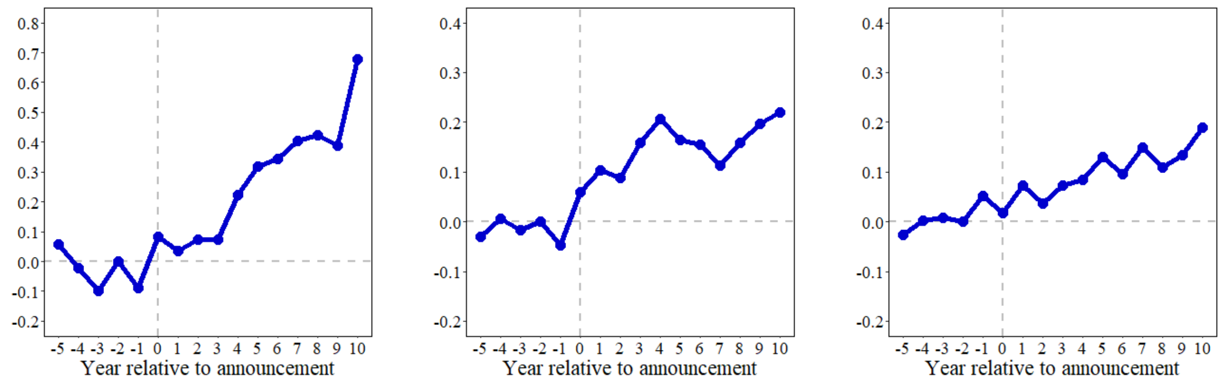
Figure A.11: Non-reconstructed sample: Effects of demolitions on the house price index, ρ_{ct}



(a) Demolition **(b)** Neighbor$\times$1 **(c)** Neighbor$\times$2

Note: The graph plots the evolution over time of τ_t in Eq. (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.

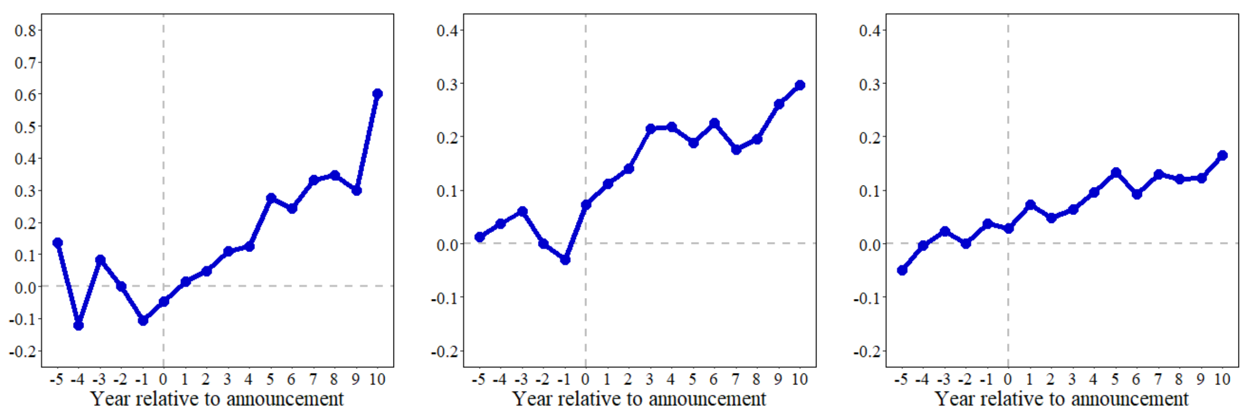
Figure A.12: Restricted sample: Effects of demolitions on the house price index, ρ_{ct}



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

Note: The graph plots the evolution over time of τ_t in Eq. (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".

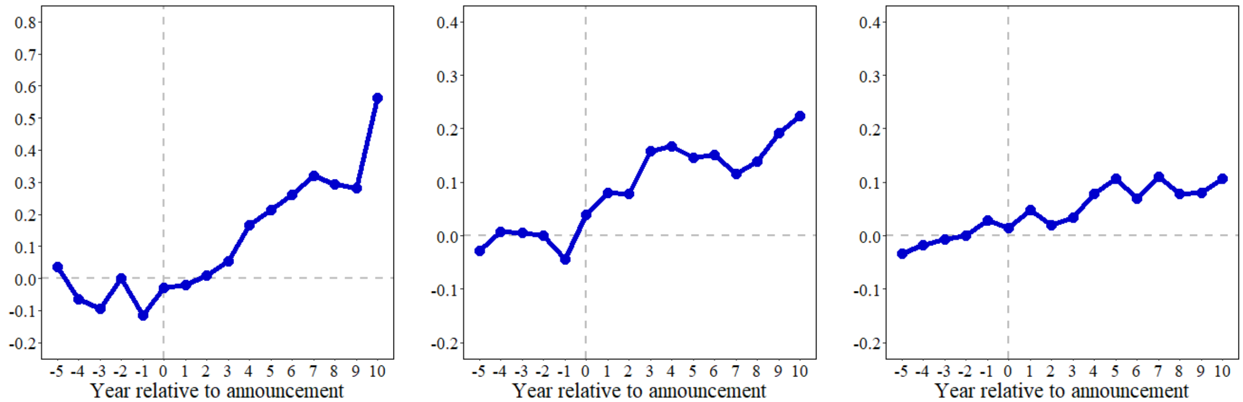
Figure A.13: Matching on average pre-trends: Effects of demolitions on the house price index, ρ_{ct}



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

Note: The graph plots the evolution over time of τ_t in Eq. (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.

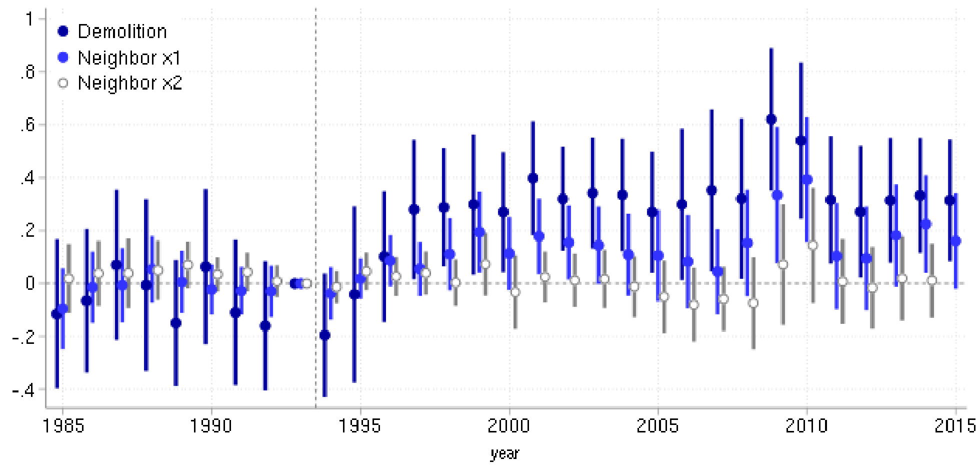
Figure A.14: SCM: Effects of demolitions on the house price index, ρ_{ct}



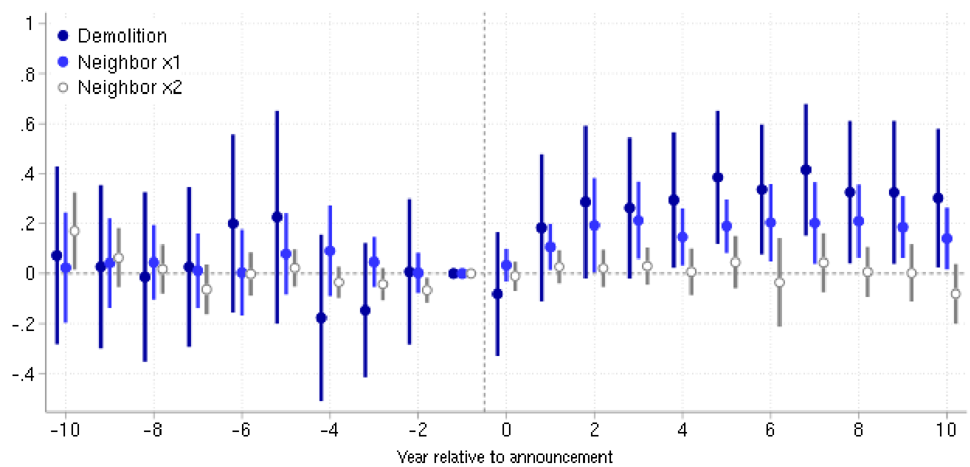
(a) Demolition **(b)** Neighbor$\times$1 **(c)** Neighbor$\times$2

Note: The graph plots the evolution over time of τ_t in Eq. (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:

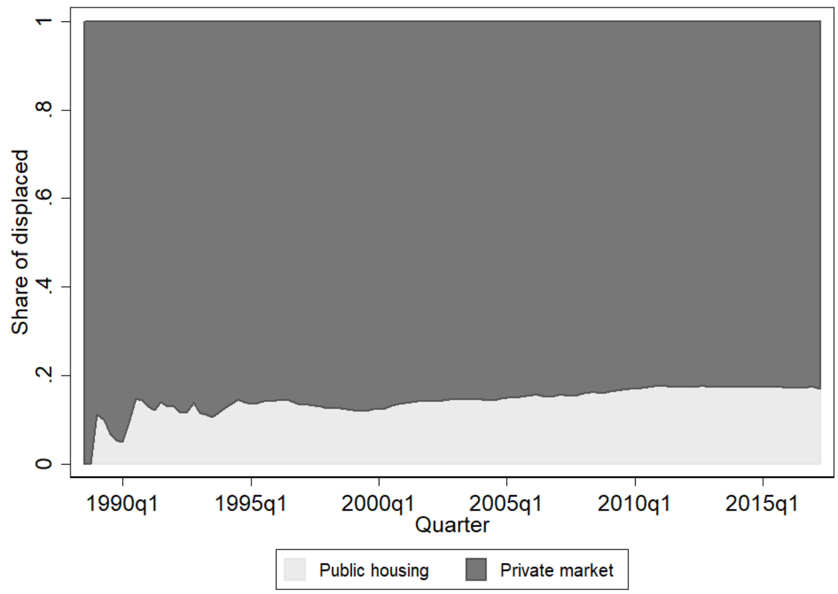
$$\ln \mathrm{P}_{ht} = \mu_{c(h)} + \sum_{t,g} \beta_{t,g}\ 1(\text{Sale year}_h = t) \times \mathbb{1}(G_{c(h)} = g) + \gamma' \mathbf{X}_{ht} + u_{ht}$$

where Sale year$_h$ is the year when house h is sold and $g \in G_{c(h)}$ = {Demolition, Neighbor $\times$ 1, 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). Panel (b) reports the coefficients $\theta_{\tau,g}$ of the regression:

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

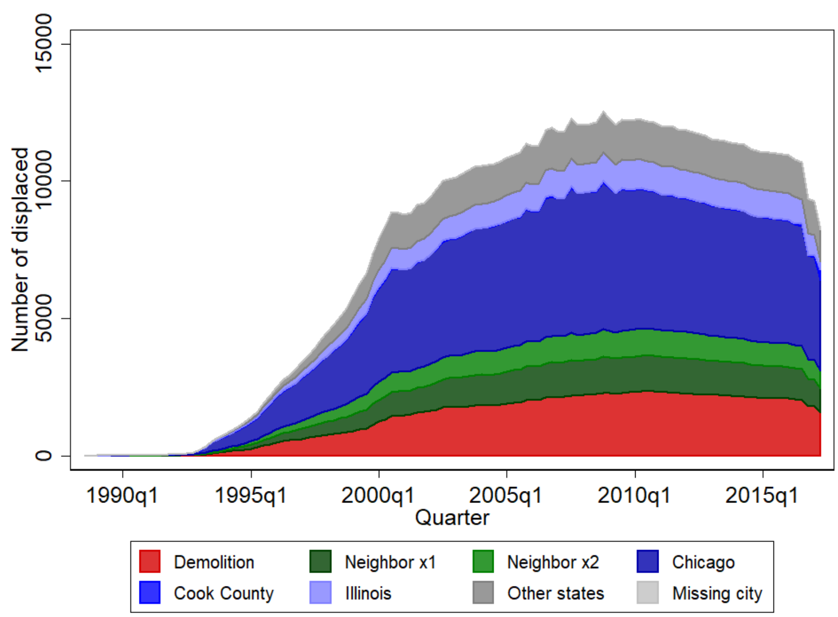
where $D_{c(h),t}$ is a dummy variable for each year τ relative to the announcement of the first demolition in census tract $c(h)$. Note that, in this specification, we also include calendar year FE, ω_t. In both regressions, house sales in Neighbor$\times$3 tracts are the omitted group and I cluster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

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



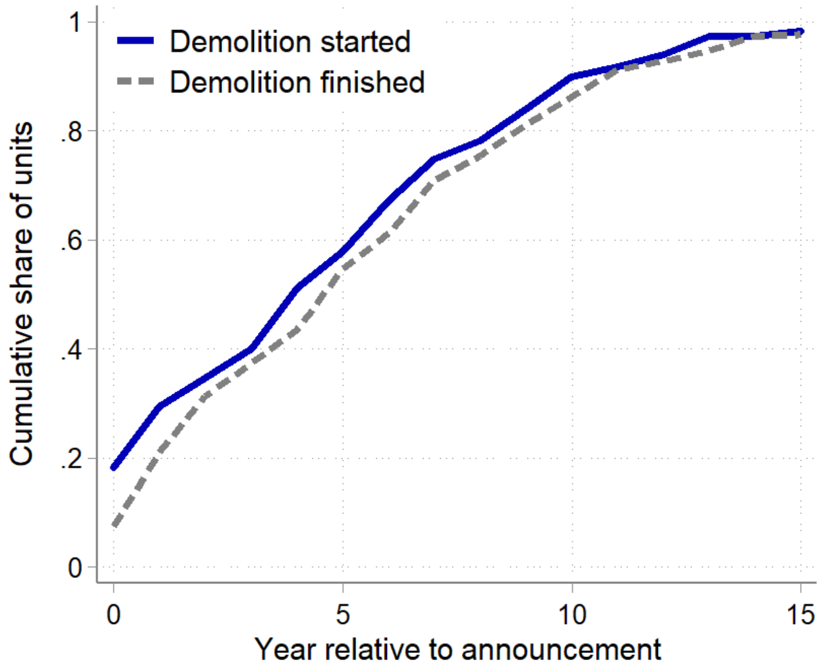
Note: This graph plots the share of displaced tenants as identified in the *displacement dataset* (introduced in Section 2.2) by housing type destination over time. I identify private housing by exclusion, i.e. if it does not correspond to a public housing address.

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



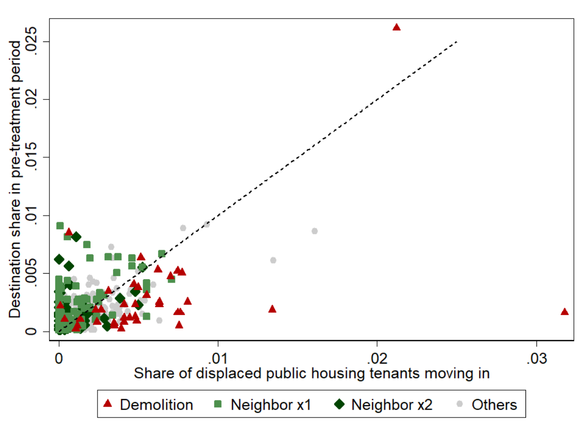
Note: This graph plots the number of displaced tenants as identified in the *displacement dataset* (introduced in Section 2.2) by destination over time.

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

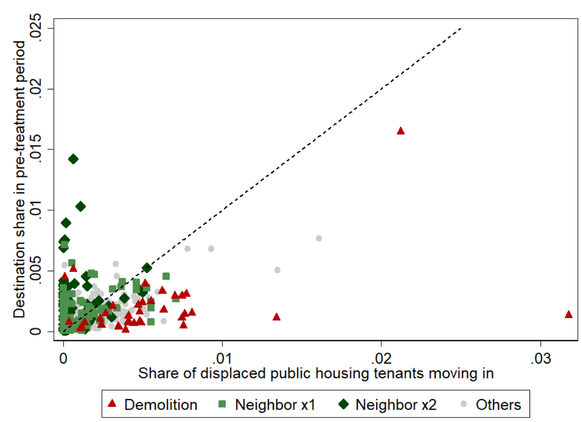


Note: This graph plots the cumulative share of demolished units by start and completion date for every year relative to the announcement of the first demolition in the same tract as reported by the Chicago Housing authority.

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 $\times 1$ (left) or Demolition, Neighbor $\times 1$ and Neighbor $\times 2$ (right) tracts. The x-axis is the share of displaced households in the *displacement dataset* (introduced in Section 2.2) moving into the tract after the demolitions.

B Tables

Table B.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 B.2: Price effects on Neighbor$\times$3 tracts by period

	Demolition	Neighbor $\times 1$	Neighbor $\times 2$	Neighbor $\times 3$
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. (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 B.3: Characteristics of treated and synthetic controls

	Demolition		**Neighbor**$\times 1$		**Neighbor**$\times 2$	
	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 B.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. (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 B.5: Non-reconstructed sample: Price effects and p-values by period

	Demolition		Neighbor $\times 1$		Neighbor $\times 2$	
	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. (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 B.6: SCM: Price effects and p-values by period

	Demolition		Neighbor $\times 1$		Neighbor $\times 2$	
	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. (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 B.7: Effects on (log) Infutor population and p-values by period

	Demolition		Neighbor $\times 1$		Neighbor $\times 2$	
	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. (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").

C Data appendix

C.1 House price dataset

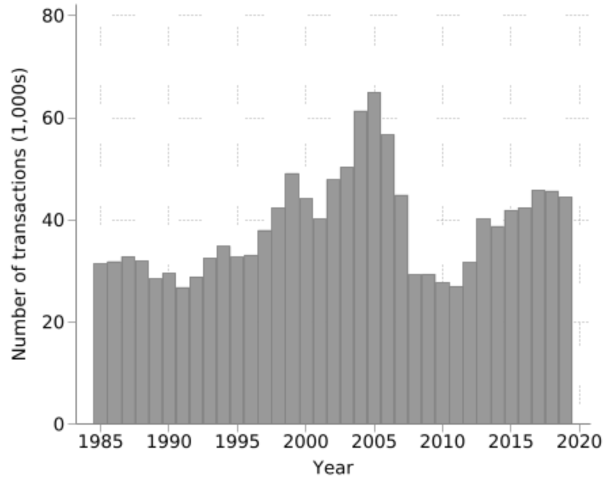
I use house price data from two different sources.

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. C.1 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.

Figure C.1: Histogram of house sales in Corelogic by year

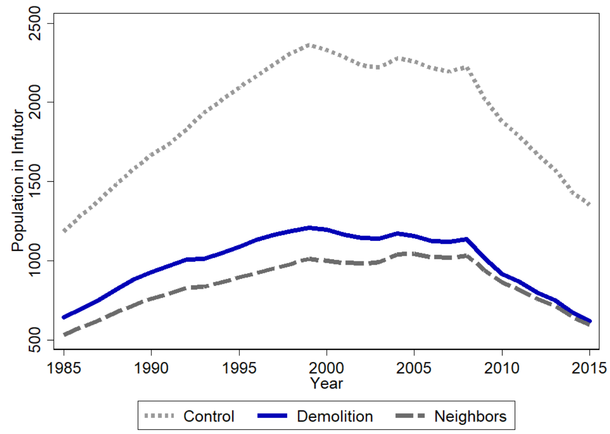


Note: This histogram shows the number of transactions (in thousands) in the Corelogic dataset by year for the city of Chicago.

C.2 Infutor dataset

This section examines how Infutor's coverage evolves for different groups of tracts. Fig. C.2 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 incomplete for earlier years in the sample, and grows until reaching full coverage in the early 2000s.

Figure C.2: Evolution of Infutor population by census tract group



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

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.

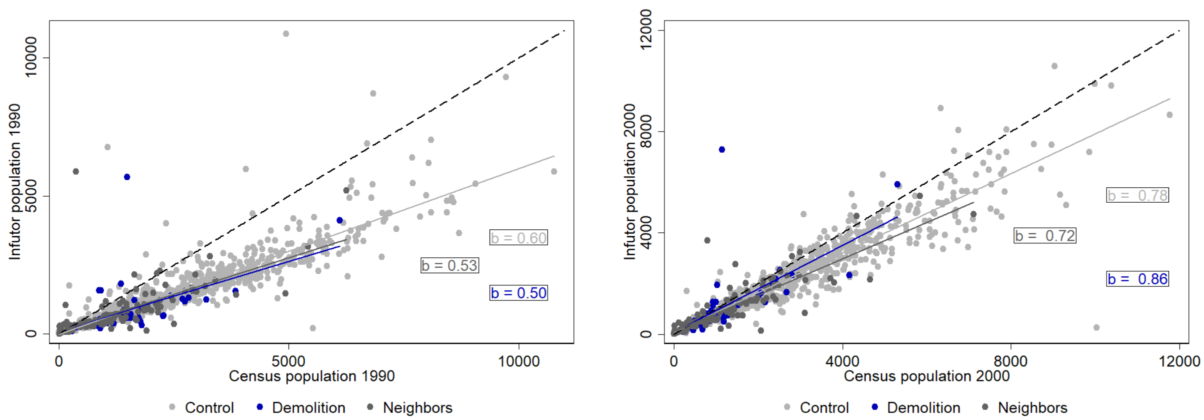
To explore this issue, I compare the Infutor population to the census adult population count in 1990 and 2000 at the census tract level[34]. Figure C.3 shows a scatter plot of this comparison for 3 groups of tracts: Demolition, Neighbor$\times 1$ and Control tracts. The first group is defined as census 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 the same rate. In particular, the growth rate of coverage in the Control, Demolition and Neighbor$\times 1$ groups are 30%, 75% and 35%, respectively. An immediate implication is that, when I measure population

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

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$\times 1$ 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.

Figure C.3: 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.

C.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 Authority sent the notice-to-proceed for demolition.

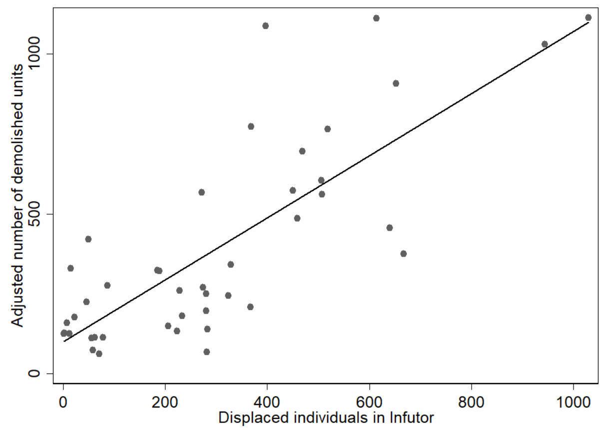
   The notice-to-proceed notifies tenants that the building is going to be torn down and must be issued at least 90 days before the demolition. Note that I also include individuals who left the building up to 1 year after the notice-to-proceed because Infutor may capture changes in addresses with a lag.

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 C.4 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 C.4: 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.

D Penalized synthetic control methods (PSCM)

D.1 Notation

Let $i \in \{1, ..., N\}$ and $t \in \mathcal{T}$ denote each unit and time period. Throughout this section, I follow **?** 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.

D.2 Methodology

Definition. This paper defines the penalized synthetic control method with many treated units 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:

$$\begin{aligned}
\min_{W_i \in \mathbb{R}} \quad & \|X_i - X_0 W_i\| + \lambda \sum_{\mathbf{j=n_1+1}}^{\mathbf{N}} \|\mathbf{X_i} - \mathbf{X_j}\| \mathbf{W_{i,j}} \qquad (5) \\
\text{s.t.} \quad & 1'_{n_0} W_i = 1 \\
& 0_{n_0} \leq W_i \leq 1_{n_0}
\end{aligned}$$

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. (5). In PSCM, this term measures the pairwise 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,

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

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:

$$\begin{aligned} W_i(\lambda^*, V_i) \in \arg\min_{W_i \in \mathbb{R}} \ & (X_i - X_0 W_i)' V_i (X_i - X_0 W_i) \\ & + \lambda^* \sum_{\mathbf{j}=\mathbf{n_1}+\mathbf{1}}^{\mathbf{N}} (\mathbf{X_i} - \mathbf{X_j})' \mathbf{V_i} (\mathbf{X_i} - \mathbf{X_j}) \mathbf{W_{i,j}} \\ \text{s.t.} \quad & 1'_{n_0} W_i = 1 \\ & 0_{n_0} \leq W_i \leq 1_{n_0} \end{aligned} \tag{6}$$

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[35].

There are 835 census tracts, 689 of which are never treated. When running the algorithm for

[35] 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.

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.

D.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 **?** to test

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:

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

E 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. C.5a 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[36].

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. C.5b 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.

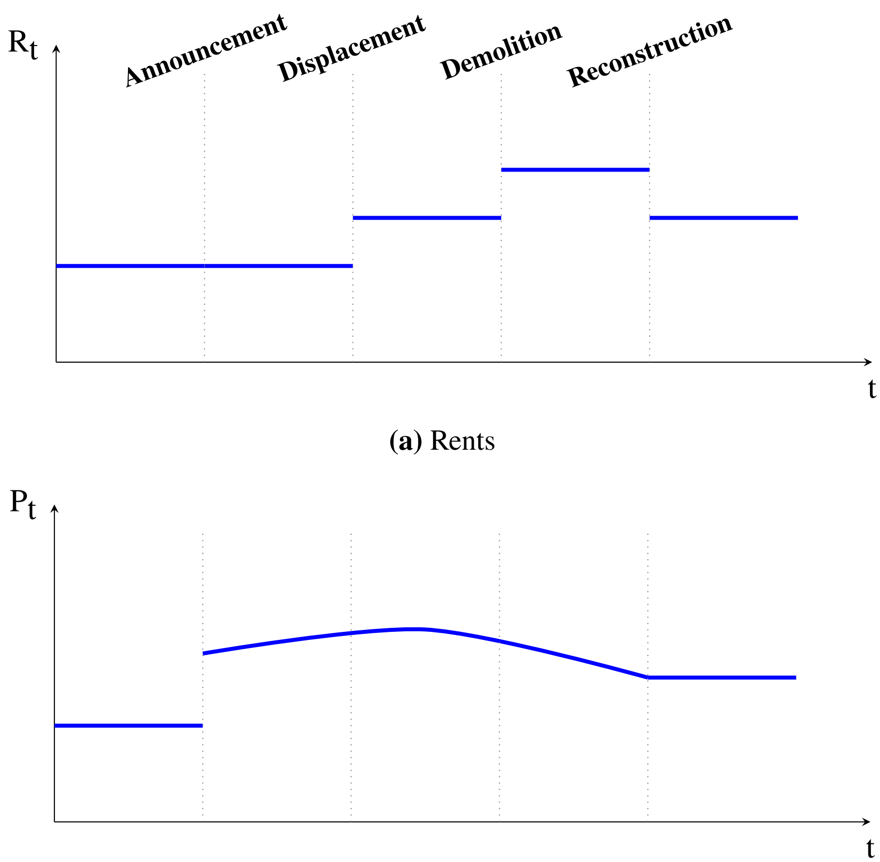
However, there are several reasons why I might not observe the path of price effects in Fig. C.5b. First, different stages of the demolition and reconstruction of a public housing development can overlap in practice[37]. Second, information on the plans for a certain public housing development might 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[38]. 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.

[36]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.

[37]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

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

Figure C.5: Expected path of price effects after demolitions



(a) Rents

(b) House prices