

Building Segregation: The Long-Run Neighborhood Effects of American Public Housing *

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Abstract

This paper studies the long-term neighborhood effects of the American public housing program, one of the largest and most controversial American urban policies of the 20th century. I construct a new national dataset tracking the locations, completion dates, and characteristics of over 1 million public housing units built between 1935 and 1973, which I link to neighborhood-level data from 1930 to 2010. I first show that public housing projects were systematically targeted towards initially poorer, more populated neighborhoods with higher Black population shares, reflecting the program's slum clearance goals and racialized site selection politics. Using a stacked matched difference-in-differences approach, I estimate causal effects of public housing construction on neighborhood change by comparing treated neighborhoods to matched control areas within the same county based on pre-treatment characteristics that predict placement. Public housing neighborhoods experienced large, persistent increases in Black population and population shares and substantial declines in median incomes and rents. Geographic spillovers to nearby neighborhoods were limited: median incomes declined modestly, but demographic composition remained relatively stable on average. I find evidence consistent with neighborhood tipping dynamics: neighborhoods with initial Black shares in a plausible tipping range experienced substantial white population outflows in response to public housing construction. Linking to modern mobility data, I show that children from low-income families who grew up in public housing neighborhoods experienced significantly lower rates of upward mobility. These findings demonstrate that, despite intentions of slum clearance and neighborhood revitalization, mid-century public housing reinforced existing patterns of economic and racial segregation and reduced long-run economic opportunity, although effects were largely confined to project neighborhoods themselves.

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1 Introduction

The Congress hereby declares that the general welfare and security of the Nation and the health and living standards of its people require housing production and community development sufficient to remedy the housing shortage, eliminate substandard housing through the clearance of slums and blighted areas, and achieve as soon as feasible the goal of a decent home and suitable living environment for every American family.

— *Housing Act of 1949*

Between the 1930s and 1970s, the United States constructed approximately 1.4 million federally-funded public housing units in one of the most ambitious urban policy initiatives of the 20th century ([Schwartz 2021](#)). Designed to clear slums, address urban housing shortages, and provide affordable housing to low-income families, the American public housing program has, to many, become synonymous with policy failure. Prominent accounts in urban history and sociology have argued that the program created and entrenched racial and economic segregation (e.g., [R. Rothstein 2017](#); [Hirsch 1998](#); [Massey and Denton 2003](#)), and accelerated mid-century urban decline ([Jackson 1985](#)). Large, megablock projects such as Cabrini-Green, Pruitt-Igoe, and the Robert Taylor Homes became infamous, criticized for concentrating poverty, promoting crime, and destroying the urban fabric of neighborhoods ([Jacobs 1961](#); [Newman 1972](#)). Federal policy since the 1970s has reflected this negative view through a retreat from public housing: The government has shifted funding toward market-based alternatives like Housing Choice Vouchers and the Low Income Housing Tax Credit, while simultaneously dismantling the existing stock through the HOPE VI program, which has funded the demolition or removal of roughly 30% of the original federal public housing stock since the early 1990s ([Schwartz 2021](#)).

Yet the evidence for public housing's failure is mixed. Some scholars contend that the program has been unfairly maligned by its most notorious examples and argue its poor reputation partially reflects broader social and economic challenges faced by American cities in the post-war period ([Bauman 1994](#); [Bloom, Umbach, and Vale 2015](#); [Goetz 2013](#)). Indeed, despite widespread criticism of the program, approximately two-thirds of public housing residents reported being "satisfied" or "very satisfied" with their housing as late as 1999 ([Schwartz 2021](#)). Some causal evidence from economists suggests that living in public housing either has neutral or positive effects on individual outcomes ([Chaudhry n.d.](#); [Currie and Yelowitz 2000](#); [Jacob 2004](#); [Pollakowski et al. 2022](#)), which could indicate that public housing may not have been as detrimental as some narratives suggest. At the same time, disentangling the local effects of public housing from pre-existing neighborhood conditions remains challenging. Given

that projects were often sited in already-distressed areas as part of slum clearance efforts, did public housing cause neighborhood decline, or simply reflect the targeting of struggling areas? A complete understanding of the public housing program has high stakes as cities grapple with housing affordability: The question of what role public housing can and should play in addressing urban housing challenges remains highly relevant.

Resolving these debates requires systematic evidence on where public housing was built and how it shaped neighborhoods over the long run. Yet such an analysis has been hindered by the lack of a comprehensive public dataset linking projects to their construction dates and precise locations. This paper addresses this research gap by constructing a new dataset on American public housing. Combining previously digitized data, public data sources, and newly digitized materials, I obtain the locations, construction dates, and characteristics, such as project size and racial composition, of over 8,000 public housing projects containing over 1 million units nationwide. I then integrate this dataset with a consistent-area panel of Census tract-level data from 1930-2000 that I build using both existing Census tract data and geolocated full count Census data from 1930 and 1940, all concorded to consistent tract boundaries. I also merge these data with three other sources: Home Owners' Loan Corporation (HOLC) "redlining" maps from the late 1930s,¹ maps with the locations of Urban Renewal projects, and the locations of interstate highways. This new dataset enables me to examine several fundamental questions about the siting and long-run neighborhood consequences of the U.S. public housing program.

First, what neighborhood characteristics influenced public housing site selection? This is important for understanding the political economy of public housing and for informing my identification strategy for estimating the neighborhood effects of public housing construction. In particular, I am interested in whether public housing was explicitly targeted towards Black neighborhoods, as some historical accounts suggest (e.g., [Hirsch 1998](#); [R. Rothstein 2017](#)), or whether it was more broadly targeted towards poor and working-class neighborhoods. To answer this, I estimate linear probability models to explore how pre-existing neighborhood characteristics, including demographic, socioeconomic, housing, and institutional factors, predict the placement of public housing projects. I find that public housing projects were more likely to be built in neighborhoods that were initially poorer, more populated, and had higher shares of Black residents, consistent with the program's slum clearance goals and the racialized politics of housing policy in the mid-20th century. I also find that neighborhoods that were designated as "Hazardous" in the 1930s by the Home Owners' Loan Corporation (HOLC) were significantly

¹Recent research has shown that the HOLC maps themselves likely did not significantly influence lending decisions and should not be thought of as redlining maps ([Fishback et al. 2024](#)). However, these maps provide detailed information about neighborhood characteristics as of the late 1930s.

more likely to receive public housing projects, and that neighborhoods that received public housing were also more likely to be affected by urban renewal, another large mid-century urban policy that targeted predominantly Black neighborhoods ([LaVoice 2024](#)). Finally, I find that public housing projects built in poorer neighborhoods and neighborhoods with higher Black population shares tended to have higher shares of Black residents themselves, indicating that public housing reinforced rather than disrupted existing residential segregation patterns. I also show that these patterns hold in a specific instance of site selection in Philadelphia by digitizing a map of proposed-but-not-selected public housing sites from Bauman (1987): In particular, neighborhoods proposed but rejected for public housing were initially much whiter than those that ultimately received projects. These results are consistent with historical narratives and case study evidence about the politics of public housing site selection (e.g., [Meyerson and Banfield 1955](#); [Bauman 1994](#); [Hirsch 1998](#)).

Second, what were the short- and long-run effects of public housing construction on both the recipient and surrounding neighborhoods? To answer this question, I employ a stacked matched difference-in-differences strategy that explicitly compares each public housing neighborhood to a matched never-treated control neighborhood based on pre-treatment characteristics. For each neighborhood that received a public housing project, I use nearest-neighbor propensity score matching to identify a comparable control neighborhood in the same county based on the pre-treatment characteristics that I demonstrated were key determinants of site selection. I then treat each matched pair as a separate "sub-experiment" and stack these sub-experiments into a single analytic dataset. The specification includes matched-pair-by-year fixed effects, ensuring that each treated neighborhood is compared only to its matched control in each year, while controlling for time-varying shocks that affect both neighborhoods in the same pair. Identification stems from the fact that federal funding for public housing was limited, and local housing authorities could not build projects in every eligible neighborhood, creating variation in project placement across otherwise similar areas. Importantly, this stacked framework also avoids the econometric pitfalls that arise in staggered adoption settings when treatment effects are heterogeneous across cohorts and over time ([Wing, Freedman, and Hollingsworth 2024](#)). I extend this methodology to analyze the geographic spillovers of the projects by conducting a similar exercise for neighborhoods adjacent to public housing neighborhoods, thereby estimating the broader geographic effects of the projects.

In the public housing ("treated") neighborhoods, I find large increases in total population driven particularly by increases in the Black population, resulting in substantial increases in Black population shares. I also find large decreases in median rents, along with declines in

median incomes and various other measures of economic well-being, including lower labor force participation rates and higher unemployment rates. These results, in conjunction with the site-selection results, suggest that while public housing was targeted toward poorer, minority neighborhoods, the construction of the projects further accelerated demographic changes and socioeconomic decline in these neighborhoods over the long run.

In terms of geographic spillovers, I find that adjacent neighborhoods experienced decreases in median incomes but showed little evidence of significant changes in racial composition, population, or rents on average. This suggests that while public housing may have had some negative spillovers on surrounding neighborhoods, these effects were more muted than some historical narratives have suggested (e.g. [Jackson 1985](#)).

I also explore the heterogeneity of these effects along several dimensions. First, I explore heterogeneity based on the neighborhood's initial racial composition. I find that treated and nearby neighborhoods that initially had Black population shares within a potential "tipping range" (between 1 and 12%, based on work by [Card, Mas, and J. Rothstein 2008](#)) saw outflows of white residents. In contrast, neighborhoods that were initially more Black did not. This suggests that public housing construction triggered racial tipping dynamics in some neighborhoods. I also find that the long-run effects of public housing construction on neighborhoods were larger for projects built before 1960, and that the impact of the projects I detect was driven mainly by neighborhoods not treated by the urban renewal program.

Finally, I explore the implications of these neighborhood effects for economic opportunity. To do so, I link my dataset to data on the upward mobility of children born to low-income families born from 1978-1983 from Opportunity Atlas ([Chetty et al. 2018](#)). I find that low-income children growing up in neighborhoods that received public housing projects experienced significantly lower upward mobility than children in matched control neighborhoods, even after controlling for neighborhood characteristics. I also find small adverse effects on upward mobility in nearby neighborhoods, but these effects are fully accounted for by controlling for neighborhood income and demographics in 1970, with no additional effects from proximity to public housing itself. This pattern suggests that these modest spillovers occurred through earlier neighborhood changes, rather than through persistent or independent effects of public housing on later mobility outcomes in surrounding neighborhoods.

This paper contributes to several literatures in urban and public economics and economic history. First, it contributes to the literature on the local effects of affordable housing programs ([Baum-Snow and Marion 2009](#); [Diamond and McQuade 2019](#)) and revitalization policies ([Collins and Shester 2013](#); [LaVoice 2024](#); [Rossi-Hansberg, Sarte, and Owens 2010](#)) by

estimating the neighborhood effects of one of the largest urban policies in American history. More directly, recent literature has studied the effects of HOPE VI public housing demolitions on neighborhood ([Tach and Emory 2017](#); [Blanco and Neri 2025](#); [Aliprantis and Hartley 2015](#); [Sandler 2017](#); [Almagro, Chyn, and Stuart 2023](#)) and individual ([Haltiwanger et al. 2024](#); [Chyn 2018](#)) outcomes. This literature has focused on the demolitions of the most distressed public housing projects, and much of it has been focused on a single city (Chicago). There are several reasons to think these estimates of the effects of demolitions may not generalize to those of public housing construction. First, the projects demolished through the HOPE VI program are a selected set of the most distressed projects. They are thus not necessarily representative of the public housing program as a whole. Second, the effects of project construction likely depended on existing neighborhood conditions, which likely varied in ways that they did not for demolitions. New projects may have served as local amenities in some neighborhoods but disamenities in others. Finally, the effects in Chicago might not necessarily generalize to other cities.

My findings also complement earlier work from other social sciences on public housing and the concentration of poverty in central cities, which generally used data compiled from one or a small number of cities (e.g. [Carter, Schill, and Wachter 1998](#); [Massey and Kanaiaupuni 1993](#)). Relative to this older literature, I use a dataset covering a much broader set of cities, projects, and time periods, and apply modern econometric techniques to estimate the causal effects of public housing construction on neighborhoods.

My paper adds to the small literature estimating the effects of public housing construction. Shester ([2013](#)) uses digitized data from HUD that contains all federally funded public housing projects built until 1973, with locations at only the locality level (discussed in [Section 4](#)) to study the effect of public housing construction on a variety of county- and city-level outcomes. This paper finds that cities in the same state with more public housing construction experienced decreases in median property values, median family income, and population density. Shester, Allen, and Handy ([2019](#)) uses the same dataset to study the effects of public housing construction on the rise in single motherhood at the MSA level. I build on this work by merging precise locations to these data, allowing me to study neighborhood effects.

The most closely related contribution is that of Guennewig-Moenert ([2025](#)), who studies the effects of public housing construction in New York City, specifically on neighborhood racial composition and rents, along with welfare estimates based on a structural model. I compile data on public housing projects across the country, enabling me to characterize the effects of public housing in the United States more broadly and to speak to broader historical debates

about the program. Scholars have argued that the New York City Housing Authority (NYCHA) was in many ways exceptional in terms of its effectiveness in dealing with many of the factors that have plagued public housing authorities in the U.S. (Bloom 2008).² Our empirical approaches also differ: he uses variation in proximity to public housing to define the control group, while I use similarity in pre-treatment characteristics. Another related contribution is contemporaneous work by Harris (2025), who studies the short-run neighborhood effects in the first decades of the program, focusing on whether they initially reduced poverty concentration in recipient neighborhoods.

Finally, my paper contributes to the economic literature on the emergence and consequences of segregation in the United States. While prior research has documented migration responses to neighborhood demographic change (Shertzer and Walsh 2019; Boustan 2010), my paper addresses one way in which federal housing policy directly influenced neighborhood sorting. More broadly, this paper contributes to the literature and debates around what role public policy has played in shaping racial and economic segregation in American cities (Rothstein 2017; Trounstein 2018; Boustan 2013; T. D. Logan and Parman 2025).

The paper proceeds as follows. Section 2 describes the history of the public housing program. Section 4 describes data sources and construction. Section 5 analyzes the determinants of public housing site selection. Section 6 presents the empirical strategy and estimates of neighborhood effects. Section 9 concludes.

2 Historical Background: The U.S. Public Housing Program

Public housing in the United States emerged during the Great Depression as part of New Deal efforts to alleviate urban housing shortages and provide economic stimulus. The Public Works Administration built the first projects, producing about 20,000 units before court challenges over land acquisition limited its scope (Jackson 1985).

The program was greatly expanded and decentralized with the passage of the Housing Act of 1937, which encouraged the creation of local Public Housing Authorities (PHAs) to construct and manage public housing with federal funding. In particular, federal grants would cover the difference between the cost of operating the projects and the revenue PHAs would receive from tenants, while PHAs would be responsible for site selection and project operations. The Act's stated purpose was slum clearance and redevelopment rather than explicitly expand-

²For example, NYCHA was a relatively small participant in HOPE VI and did not demolish any of its high-rise projects through the program (Schwartz 2021).

ing housing supply, reflecting contemporary beliefs that deteriorated neighborhoods reduced nearby property values and caused poor health and behavior among residents ([Meyerson and Banfield 1955](#)). Over 170,000 housing units were built under the Housing Act of 1937, with nearly 90% built on former slum sites ([Schwartz 2021](#)).

World War II temporarily shifted focus to defense worker housing, but the post-war period brought renewed attention to urban conditions. The 1949 Housing Act authorized funding for an additional 810,000 units of public housing within a broader program of urban renewal ([Meyerson and Banfield 1955](#)). This legislation marked a significant shift in the program's scope and ambition, expanding public housing as a tool for slum clearance, a source of low-income housing, and a potential destination for those who the urban renewal program would displace.

The ambitious goals of the 1949 Act, however, quickly met political, fiscal, and social obstacles. Several developments in the 1950s and 1960s transformed public housing into a deeply controversial policy.

First, as described in Section 2.1, racial dynamics in site selection generated intense political conflict. Second, the tenant composition shifted dramatically over time. Early projects had housed working families and the "deserving poor," but by the 1960s, they increasingly concentrated the most disadvantaged households. The growth of suburban home ownership enabled working-class families to leave public housing, while authorities tightened income eligibility limits. Federal regulations also required PHAs to prioritize the neediest applicants, concentrating poverty within projects ([Schwartz 2021](#)). Third, design and construction problems became increasingly apparent. To avoid competing with private housing and to comply with mandated construction cost limits, projects were deliberately austere and inexpensive, making them prone to rapid deterioration. The increasingly poor tenant base, combined with inadequate funding, led to widespread disrepair and maintenance backlogs by the 1960s and 1970s ([Schwartz 2021](#)). High-profile disasters like St. Louis's Pruitt-Igoe Homes, built in 1954 and demolished in 1972, became national symbols of urban policy failure. Initially hailed as a model development, Pruitt-Igoe quickly descended into crime, vandalism, and abandonment as maintenance funding dried up and middle-class residents moved out ([Bristol 1991](#)). Other critics pointed to the design of the projects themselves, arguing that the modernist high-rise towers and superblocks created alienating environments that fostered social problems ([Jacobs 1961](#)).

In response to mounting criticism, federal housing policy shifted away from direct public housing provision. A 1971 report by the Nixon Administration wrote that "drab, monolithic

housing projects, largely segregated...still stand in our cities as prisons of the poor" (Orlebeke 2000). In 1973, President Nixon declared a moratorium on subsidies for traditional public housing. By the early 1990s, the public housing stock faced serious challenges: physical deterioration due to deferred maintenance, extreme concentration of poverty as working families moved out, and, in some cases, rampant crime and drug activity. These struggles laid the groundwork for the HOPE VI program, launched in 1992, which provided federal funding to demolish distressed projects or transform them into mixed-income developments.

2.1 Race, Segregation, and Public Housing

Race and segregation emerged as central and contentious issues throughout the program's history. From the outset, the Public Works Administration followed a so-called "neighborhood composition rule", formally segregating projects based on the demographics of the neighborhoods in which they were built. While ostensibly a neutral policy intended to maintain neighborhood "stability", in practice the rule entrenched segregation. Prominent accounts, such as R. Rothstein (2017), argue that public housing not only reinforced existing segregation, but actively created it by building segregated projects in previously integrated neighborhoods. A well-known example is Atlanta's Techwood Homes, constructed in 1936 as an all-white development, which displaced a previously integrated low-income neighborhood (R. Rothstein 2017). Many large public housing authorities continued to follow an explicit neighborhood-composition rule until racial segregation was banned in 1954. Yet even after this legal shift, projects remained highly segregated in practice (Bickford and Massey 1991).

Historical case studies suggest that issues around race and segregation also shaped site selection decisions, and contemporaneous debates among policy-makers about the role of public housing in promoting or fighting racial segregation reveal that officials were well aware of the program's potential to either exacerbate or ameliorate residential segregation patterns (Hirsch 2000). In Chicago, the Housing Authority initially planned to build projects throughout the city. Still, fierce opposition from white neighborhoods led to the concentration of projects in predominantly Black areas on the South and West Sides (Meyerson and Banfield 1955; Hirsch 1998). Similar battles played out in other major cities, with white residents and politicians successfully blocking projects in their neighborhoods. I explore a particular example of this dynamic in Philadelphia, comparing proposed-but-not-selected areas to public housing locations in Section 5.2. Building on this historical evidence, Section 5.1 tests whether these dynamics held systematically nationwide.

3 Potential Effects of Public Housing

In this section, I outline the main channels through which the construction of public housing projects might affect neighborhood outcomes.

First, public housing construction may mechanically alter the racial and socioeconomic composition of neighborhoods. Tenant selection policies, income limits, and subsidized rents determined who occupied these projects, thereby altering the demographic makeup of the areas where they were built.

Second, these compositional changes could trigger endogenous sorting by private households. If individuals have preferences for their neighbors' race or income, the arrival of public housing residents might prompt some existing residents to relocate. Historical work, such as Jackson (1985), argues that part of mid-century "white flight" from central cities was a response to the public housing program. However, empirical evidence on this point remains limited.

Third, the physical structure of public housing projects could create built-environment externalities. Replacing substandard or vacant structures with new housing could improve neighborhood quality and increase neighborhood desirability (Rossi-Hansberg, Sarte, and Owens 2010; Ellen et al. 2007). Conversely, large superblocks or architecturally incongruous high-rise towers might be viewed as local disamenities, and indeed, criticism of the mid-century public housing program often focused on the design of the projects (Jacobs 1961; Newman 1997).

Fourth, public housing construction may generate broader social and market externalities. A frequent concern is that concentrated poverty within large projects generated social disorder and elevated crime, with potential spillovers into surrounding neighborhoods (Sandler 2017). Conversely, visible public investment could signal neighborhood revitalization and spur private reinvestment, improving local conditions (Rossi-Hansberg, Sarte, and Owens 2010). The direction of these effects may vary across contexts. Modern evidence on the LIHTC program suggests that subsidized housing investments may be a positive amenity in distressed areas but may be perceived negatively in more affluent ones (Diamond and McQuade 2019).

I explore these mechanisms empirically in Section 7.

4 Data

4.1 Public Housing Data

A significant challenge in studying the history of public housing is the lack of a comprehensive dataset that includes project lists, construction dates, and precise project locations. Consequently, previous research on the neighborhood effects of public housing has been limited in scope or restricted to a small number of cities where researchers could obtain this information directly from housing authorities. The data limitations have also hindered broader historical analyses of the public housing program, and have been acknowledged by previous work ([Ellen et al. 2007](#); [Hunt 2018](#)). My paper addresses this gap by constructing what I believe to be the most complete dataset of mid-century public housing by combining information from a series of administrative datasets, along with previously digitized and newly digitized sources.

The first source I use is the *Consolidated Development Directory* (CDD), published by the US Department of Housing and Urban Development (HUD) in 1973 and digitized by [Shester \(2013\)](#). This data contains the universe of federally funded public housing projects that existed in 1973, along with various project characteristics (e.g., number of units) and, crucially, the year each project was completed. However, the CDD does not contain location information for the projects. Previous work using these data ([Shester 2013](#); [Shester, Allen, and Handy 2019](#)) has therefore been limited to studying aggregate city- and county-level outcomes. These data also contain a set of three project numbers, which I combine to form federal project codes. These project codes allow me to link the CDD data to other project-level datasets. As far as I know, this aspect of these data has not been previously exploited.

To obtain location information for the projects, I turn to a series of publicly available HUD administrative datasets. The primary of these is the Picture of Subsidized Households (PSH) datasets, which contain a list of federally funded housing projects, various project characteristics (e.g., number of units, demographics), and, from 1997 onward, their locations. I use the PSH datasets from 2000 and 1997, due to limitations in each. I also link to more recent National Data Geospatial Asset (2023) data. Finally, I link to the HUD File 951 dataset, which lists street addresses, latitudes, and longitudes for the stock of multifamily assisted housing projects that existed between 1986 and 1995 ([Kucheva 2013](#)). These datasets largely overlap in coverage, but each contains projects missing from the others. This linkage allows me to assign locations to over 90% of the projects in the CDD. I will refer to this as the geocoded CDD dataset.

I supplemented these data with hand-collected information from historical annual reports

of local public housing agencies, obtained from various libraries (or directly from the housing authority in the case of San Francisco), and from FOIA requests to public housing agencies. From this effort, I was able to obtain supplementary data from eight major cities: New York, Chicago, Boston, Los Angeles, Washington, DC, San Francisco, Atlanta, and Baltimore.³ For these cities, I collected the complete set of projects built up to 1973, including their construction dates and locations. I was able to geolocate these projects using the Google Maps API. For projects I was unable to geolocate successfully, I hand-identified locations based on street address and name.

There were two motivations for collecting these additional data. First, it allowed for a better understanding of the projects in the CDD for which I was unable to assign locations. Public housing demolitions due to HOPE VI, which began in 1993, as well as earlier demolitions, may have left some projects in the CDD missing from the data. Indeed, the incomplete matching between CDD and PSH data suggests that some projects may have ceased to exist when PSH data collection began. Moreover, these missing projects might not be randomly distributed, as demolitions targeted particularly blighted projects.

Second, the CDD-HUD data include only federally funded public housing projects. Little data exist on city- and state-funded public housing projects, but some cities may have funded some public housing construction outside of the federal program. New York City, in particular, has a notable city- and state-funded public housing program, and using only the data on federal projects in New York City misses a substantial number of housing projects and units.⁴ The other cities for which I collected data had either none or very few city or state-funded projects. Ultimately, I use my hand-collected data for housing projects in New York City, Chicago, Baltimore, and San Francisco, supplement the geocoded CDD for Boston and Washington, DC, and rely on the geocoded CDD data for all other cities.⁵ I found little evidence in the historical record that, by otherwise relying on data on federal projects, I am missing a notable stock of public housing in other cities.

The last step in constructing the public housing dataset is to obtain information on the populations and racial composition of the projects. These data allow me to distinguish between public housing residents and the rest of the neighborhood's population. For most cities, I obtain this information from the 1977 Picture of Subsidized Households dataset, which was

³I collected digitized reports from other cities, such as Cleveland and Cincinnati, but not all contained the precise project locations information.

⁴My digitized dataset of NYC housing projects contains 129,430 housing units, compared to 80,000 in the PSH-CDD data.

⁵For the other cities for which I hand-collected data, I find no additional projects in my digitized data versus the geocoded CDD data.

cleaned and shared with me by Yana Kucheva ([Kucheva 2013](#)). These data report the number of subsidized households by race in each project, but do not include direct population counts. In Appendix A, I describe how I convert these household counts to population counts. I match these data to the geocoded CDD dataset using the federal project codes. For New York City, I used population-by-race data from the early 1970s digitized and shared with me by Max Guennewig-Moenert ([Guennewig-Moenert 2025](#)). And for Chicago, I obtained population-by-race data from the 1973 digitized Annual Report of the Chicago Housing Authority. While it would be ideal to measure population by race at the time of construction, these data are not available in most cities. For the panel analysis, I assign these 1970s population estimates to the treatment year and all subsequent decades, assuming they remain constant over time. For pre-treatment years, the public housing population is set to zero. This approach allows me to estimate the private (non-public-housing) population in each decade by subtracting the estimated public-housing population from the total Census-reported population.

The result of this process is a dataset containing the construction dates, locations, and characteristics of over 8,000 projects and 1 million units of public housing built from 1935 until 1973.

4.2 Neighborhood Data

To study the neighborhood effects of public housing construction, I construct a panel dataset of neighborhood-level characteristics spanning 1930 to 2010. Building such a dataset presents several challenges. First, the number of cities with tract-level data is limited in 1940 and especially in 1930. Second, Census tract boundaries change over time, requiring the construction of a consistent panel. And third, income data was not collected in the 1930 Census, and median income was not reported in the tract-level Census tables in 1940.

I proceed as follows. First, I collect outcomes at the Census tract level from the 1930 to 2010 decennial censuses. These data include tract-level population counts by race (white and Black), several socioeconomic measures (median income, high school graduation rates, labor force participation, and unemployment rates), and median rents and home values. All census data and shapefiles were acquired from IPUMS NHGIS ([Manson et al. 2022](#)).

I construct a consistent panel of census tracts by concordancing all tracts to 1950 Census tract boundaries using an area-reweighting approach. I describe the tract harmonization in more detail in Appendix B. I chose 1950 as the base year for two reasons. First, to limit concerns about results being driven by public housing-driven changes in tract boundaries, I chose a year early in my analysis period, rather than at the end, as much of the literature does. Second,

publicly available 1940 New York City census tract shapefiles do not correspond to actual Census tracts, but to much larger health districts, making 1940 a less suitable base year. Given that New York City is a major city in my sample, I chose 1950 as the base year for the entire sample.

I supplement this tract-level data with data from the 1930 and 1940 full-count Census (Ruggles et al. 2022). To convert the full count data to the tract level, I first aggregate the individual-level data to the enumeration district (ED) level using the 1930 and 1940 enumeration district shapefiles from the Urban Transitions Project (J. R. Logan et al. 2024). I then use area reweighting to aggregate these data to 1950 Census tracts. Incorporating these full-count data serves two purposes. First, they allow me to expand my set of cities and neighborhoods for which tract-level data was unavailable in 1930 and 1940. Second, they allow me to include income information for 1930 and 1940, which is not available in the tract-level tables for those years. I proxy for income in 1940 using total wage income per household, and in 1930 using total machine-learning-adjusted occupation scores from Saavedra and Twinam (2020). I convert all monetary values to 2000-dollar values using the US CPI from Officer and Williamson (2025).

I also incorporate digitized data and shapefiles of the Home Owners' Loan Corporation (HOLC) "redlining" maps drawn in the late 1930s, made available by *Mapping Inequality* (Nelson and Winling 2023). These maps graded neighborhoods from "A" (best) to "D" (hazardous) to indicate perceived mortgage risk. Although many scholars have argued that these maps institutionalized redlining and curtailed investment in minority neighborhoods (Aaronson, Hartley, and Mazumder 2021), recent evidence complicates this view. Fishback et al. (2024) shows that the maps largely reflected existing patterns of racial and socioeconomic segregation rather than shaping new lending decisions. For my purposes, these maps serve as a historical snapshot of neighborhood conditions and perceived credit risk in the late 1930s and thus serve as a proxy for pre-existing discrimination, rather than a direct driver of exclusion. I overlay these maps onto the 1950 Census tract boundaries. Following Weiwu (2025), I classify a neighborhood as "redlined" if at least 80% of the area is designated as "hazardous" (HOLC grade D).

Finally, I incorporate data on the locations of U.S. urban renewal projects (1955-1966) from *Renewing Inequality* (Nelson and Ayers 2025). Urban renewal, established under Title I of the 1949 Housing Act, provided federal subsidies for cities to acquire and clear "blighted" neighborhoods. Urban renewal was closely intertwined with public housing: clearance projects often created sites for new developments or displaced families that public housing was intended to rehouse (von Hoffman 2000). Recent research finds that Black neighborhoods were two to

three times more likely to receive urban renewal projects than white neighborhoods, and that urban renewal areas experienced declines in housing and population density alongside rising rents and incomes (LaVoice 2024). To account for potentially confounding effects of urban renewal and to explore the relationship between the two programs, I overlay these maps onto the 1950 Census tract boundaries and classify a tract as an “urban renewal” tract if more than 5% of its area overlaps with an urban renewal project.

4.3 Defining Treatment and Spillover Neighborhoods

To study the neighborhood effects of public housing construction, I define a set of *treated* tracts that received public housing projects and a set of *nearby* tracts that may have experienced spillover effects from nearby projects. To ensure that a project represents a meaningful neighborhood intervention, I include only projects with at least 50 housing units.

Because the public housing projects are geocoded at the coordinate level, I use a geographic buffer approach to define treated tracts. Specifically, a tract is defined as treated if any portion of it intersects a 100-meter buffer around a public housing project. This definition captures cases in which a project straddles multiple tracts; when that occurs, I allocate its units and population evenly across the affected tracts.

Since the Census data are decennial, I assign treatment timing by decade. Following the literature, I use the project’s completion date as the treatment date (Asquith, Mast, and Reed 2023). A tract is considered treated in year t if its first qualifying project was completed between $t-9$ and t (e.g., tracts receiving projects between 1951 and 1960 are treated in 1960). This timing convention ensures that the treatment occurs after the pre-treatment observation but before the post-treatment observation in the panel.⁶ For tracts receiving multiple public housing projects over time, treatment timing is determined by the first project meeting the size thresholds. Subsequent projects in the same tract are not considered separate treatment events, since the tract has already been “treated” by public housing construction.

To examine spatial spillovers, I define a nearby tract as one that shares a border with a treated tract, is not itself treated, and lies within one kilometer of the nearest public housing project. This hybrid contiguity-and-distance definition identifies neighborhoods that are close enough to plausibly experience externalities from nearby developments while excluding tracts that are technically adjacent to project neighborhoods but geographically distant from

⁶If a project was completed in the Census year but after the Census enumeration date, the effects of the project would meaningfully be captured in the second, rather than first, post-treatment decade, and the $t = 0$ effects would be understated.

a project. I adopt a one-kilometer threshold based on prior literature showing the geographic extent of spatial spillovers from public housing interventions (Blanco and Neri 2025). Nearby tracts inherit the earliest treatment year among their treated neighbors. Figure 1 illustrates the treated and nearby classification for Chicago, showing treated tracts and their adjacent spillover areas.

4.4 Sample Selection

My empirical analysis focuses on projects built between 1941 and 1973, with neighborhood outcomes measured from 1930 to 2010. I restrict the sample in several ways to ensure a balanced panel of neighborhoods with sufficient pre-treatment data to conduct my difference-in-differences analysis.

Table 1 shows the impact of each filtering step on the sample composition. Starting from the original sample of 12,062 census tracts representing 38.1 million people in 1940 (approximately 29% of the U.S. population), I apply the following restrictions. First, I require that tracts exist in all years from 1930 to 2010, which drops about 25% of tracts. Second, I drop tracts for which I cannot calculate population by race, median income, median rent, labor force participation rates, and unemployment rates for all years, which removes an additional 5% of tracts. Third, I exclude tracts with public housing built before 1941 to preserve 1930 and 1940 as clean pre-treatment periods, which removes 109 treated tracts containing 109 projects. Fourth, I drop metropolitan areas with fewer than 30 total tracts to ensure sufficient within-city variation. This excludes three small metropolitan areas. Finally, I exclude population outliers: tracts with populations below the 5th percentile or above the 98th percentile in any year.

The resulting balanced sample includes 6,506 census tracts across 47 CBSAs, representing 27.5 million people in 1940 (approximately 21% of the U.S. population). This sample contains 814 public housing projects with 300,964 total units located in 822 treated tracts. Figure 2 shows the geographic distribution of CBSAs included in the analysis.

5 Site Selection

In this section, I study the placement of the public housing projects in my sample. Understanding these dynamics is essential for several reasons. First, the site-selection process for mid-century public housing was a significant criticism of the program. Historical case studies in several cities suggest that projects were often targeted to poorer and minority neighborhoods,

both because of the program’s slum clearance goals and because of local backlash in white neighborhoods against construction of projects (Meyerson and Banfield 1955; Bauman 1994; Sugrue 2005). Second, these site selection dynamics have had important legal implications: Lawsuits in several cities have alleged that public housing site selection was discriminatory and in violation of Civil Rights Law, most famously *Gautreaux v. Chicago Housing Authority* (1966), but also in cities like Dallas (*Walker v. HUD* 1985) and Baltimore (*Thompson v. HUD* 2005). Still, there has been little systematic evidence on the nationwide patterns of public housing site selection. Finally, understanding these site-selection dynamics is important for estimating the neighborhood effects of public housing and interpreting those effects.

To begin, in Section 5.1, I estimate which pre-existing neighborhood characteristics predict the eventual locations of public housing projects. I also test whether the placement of public housing projects was related to the locations of other mid-century urban policies, particularly urban renewal and the interstate highway system. Then, in Section 5.2, I zoom in on a particular case study of site selection by examining the locations of proposed-but-not-built public housing in Philadelphia in Section 5.2. This example illustrates the political dynamics around site selection decisions.

Finally, in Section 5.3, I examine whether the racial composition of the projects themselves varied systematically with the initial characteristics of the neighborhoods in which they were built.

5.1 Where were the projects built?

I estimate the relationship between pre-treatment neighborhood characteristics and the probability of receiving a public housing project within my balanced neighborhood sample. To do this, I estimate linear probability models predicting whether a census tract ever received a public housing project from 1941 to 1973 using neighborhood characteristics measured in 1940. In particular, I estimate:

$$\text{Treated}_{ic} = \gamma_c + \mathbf{X}'_{i,1940} \boldsymbol{\beta} + \epsilon_{ic} \quad (1)$$

where Treated_{ic} indicates whether tract i in county c received a public housing project from 1941-1973, $\mathbf{X}_{i,1940}$ represents a vector of 1940 neighborhood characteristics, and γ_c denotes county fixed effects. I include county fixed effects to control for differences in implementation by different local public housing authorities. I adjust standard errors for spatial correlation following Conley (1999), allowing for correlation of residuals across census tracts within a

2-kilometer radius.

I chose variables based on historical narratives of public housing site selection and the program's stated intentions. First, as described in Section 2, the public housing program was largely intended as a "slum clearance" program, so we might expect that neighborhoods with lower socioeconomic status and worse housing market conditions would be more likely to receive public housing. Second, historical case studies in multiple cities have documented the key role of race in determining where public housing was built (e.g. [Hirsch 1998](#); [Bauman 1994](#)), so I include the Black population share as a key predictor of public housing placement. I also include the HOLC redlining designation to capture long-term patterns of racial segregation, discrimination, and disinvestment.

The results from these linear probability models are shown in Columns 1-4 in Table 2. Column 1 shows a parsimonious model with several key neighborhood characteristics, while Column 2 shows a more saturated model including all neighborhood characteristics. Column 3 adds the share of housing units deemed in need of major repairs in 1940, which is missing for about 5% of tracts. Consistent with the historical narrative, I find that public housing projects tended to be targeted towards poorer, minority neighborhoods: Census tracts that were initially more populated, had higher Black population shares, lower median incomes and labor force participation rates, higher unemployment rates, had lower rents, and were designated as "hazardous" by the HOLC maps were more likely to receive public housing projects during this period. The share of housing units needing major repairs is also positively associated with public housing placement, although the estimate does not reach statistical significance in the fully saturated model. These findings reflect the slum clearance motivation of the public housing program, and are also consistent with historical accounts emphasizing the racial targeting of public housing siting: Even after controlling for local economic and housing market conditions, the Black population share remains a strong and significant predictor of public housing placement.

Column 4 additionally tests whether neighborhoods that ultimately received public housing were more likely to be affected by two other transformative mid-century urban policies: urban renewal and the interstate highway system. Evidence and historical narrative suggest that these programs may have affected many of the neighborhoods that were also targeted for public housing. The Urban Renewal program, in particular, was directly related to public housing in many cities, as public housing projects were used to house individuals displaced by urban renewal ([Bauman 1994](#); [Hirsch 1998](#)). Neighborhoods that received public housing were also much more likely to be affected by urban renewal, whereas proximity to an interstate highway

was not significantly related to public housing placement. This result further confirms the interplay between these two programs and motivates accounting for urban renewal in my empirical strategy in Section 6.⁷

Quantitatively, these effects are sizable. The baseline probability that a census tract in the sample received a public housing project between 1941 and 1973 is 12.6%. Based on the estimates in Column (4), the coefficient on the redlined indicator suggests that neighborhoods designated as redlined were 4.3 percentage points more likely to receive public housing in subsequent decades, representing a 33.8% increase relative to the baseline probability. A one standard deviation increase in Black population share increased public housing selection probability by 3.6 percentage points (28.4% increase), a one standard deviation increase in unemployment rate increased the probability by 4.4 percentage points (34.7% increase), and a one standard deviation decrease in median income increased the probability by 2.1 percentage points (16.6% increase). Finally, neighborhoods that were eventually targeted by urban renewal were 7.8 percentage points (73.6%) more likely to receive public housing.

5.2 A Philadelphia Case Study

Historical accounts of public housing site selection have highlighted the political battles that surrounded the placement of projects in particular cities, for example, in Chicago ([Meyer-son and Banfield 1955](#)), Philadelphia ([Bauman 1994](#)), and Detroit ([Sugrue 2005](#)). These accounts have emphasized the conflict between public housing authorities and white working-class neighborhoods that resisted the construction of public housing projects in their communities. Here, I present a case study of site selection in Philadelphia, drawing on the historical treatment and maps from Bauman ([1987](#)). This case study is illustrative of the political dynamics that shaped site selection decisions in many cities ([Hunt 2005](#)).

Following the 1954 Housing Act, the Philadelphia Housing Authority proposed 21 public housing projects across the city. Upon announcement, many of these proposed projects faced significant backlash from local white communities, leading to their eventual cancellation. I identified these proposed Philadelphia public housing locations by scanning and georeferencing a historical map from Bauman ([1987](#)), shown in Figure 3. I georeferenced the historical map in QGIS, aligning it with a modern basemap using identifiable landmarks, such as major roads and rivers. Then, I located the proposed public housing sites on the georeferenced map and matched these points to 1950 census tracts. In total, I identify 12 neighborhoods with proposed-but-not-built public housing sites in Philadelphia.

⁷These results are consistent with similar exercises in Harris ([2025](#)) and Massey and Kanaiaupuni ([1993](#)).

I then compare the baseline characteristics of proposed-but-not-built project locations to those of actual public housing locations in Philadelphia. Table 3 presents the balance table, which shows that the initial characteristics of the proposed locations were very different from those of actual public housing locations. Most notably, these proposed-but-not-built project locations had very low initial Black population shares. They were also further from the central business district, less populated, and had lower unemployment rates. These differences illustrate the dynamics of site selection in one particular city, where whiter neighborhoods often resisted public housing construction. They also show that an identification strategy based on proposed-but-not-built locations would likely be invalid, as these locations likely do not represent a good counterfactual for actual public housing locations.

5.3 Did project demographics vary with neighborhood characteristics?

I now examine how the racial composition of the public housing projects varied with the initial neighborhood characteristics in which they were built. The key question is whether projects reinforced existing patterns of residential segregation or disrupted them. I focus on the share of Black residents in each project, using data from the 771 census tracts in my balanced sample for which I have available project-level racial composition data from the 1970s. Ideally, I would observe the racial composition of the projects at the time of construction, but this information is not systematically available. Still, to the extent that the racial composition of the projects was relatively stable over time, the 1970s data should provide a reasonable proxy for the initial demographics of the projects.

Formally, I regress the project Black share in each tract i in county c on the neighborhood characteristics measured in the decade before construction:

$$\text{Project Black Share}_{ic} = \gamma_c + \mathbf{X}'_{i,t-10}\boldsymbol{\beta} + \epsilon_{ic} \quad (2)$$

Whereas the regressions in Section 5.1 used 1940 covariates (or later urban policy exposure) for all projects, here I match each project to neighborhood characteristics from the decade immediately preceding its construction to better capture local conditions at the time of development. The results are shown in Table 4. Column (1) presents a parsimonious specification with only baseline Black share and median income, column (2) includes a fuller set of neighborhood characteristics, including rent, population, unemployment, distance to the CBD, and redlining status, while column (3) adds county fixed effects. I again include the distance from an interstate highway and urban renewal designation.

The core result is robust across specifications: public housing projects largely matched the racial composition of their surrounding neighborhoods, reinforcing rather than disrupting existing residential segregation patterns. Based on the coefficient in column (3), a 10 percentage point increase in baseline neighborhood Black share predicts a 2.7 percentage point increase in project Black share. Furthermore, projects built in poorer neighborhoods, even conditional on race, tended to be more heavily Black, as indicated by the negative coefficient on median income and positive coefficient on unemployment rate.

6 The Effect of Public Housing Construction on Neighborhoods

Having established in the previous section that public housing projects were systematically targeted towards poorer, minority neighborhoods, I now turn to estimating the effects of the projects on neighborhood change in subsequent decades.

6.1 Research Design

The central empirical challenge is to select appropriate counterfactual neighborhoods for those who received public housing. To do so, I employ a stacked matched difference-in-differences approach informed by the site selection results in Section 5.1. I proceed as follows.

First, I identify a donor pool of potential control tracts for matching. I define these as the set of census tracts that never received public housing during my analysis period and are not classified as nearby tracts, as described in Section 4. Excluding these nearby neighborhoods from the donor pool helps avoid concerns about spillover effects. In Section 6.4, I directly test the effects of the construction of the projects on these nearby neighborhoods.

Second, I match each treated neighborhood to a comparison neighborhood from this donor pool using propensity-score-based nearest neighbor matching with replacement. Critically, I perform this matching procedure separately for each treatment year. This ensures that treated and control neighborhoods are comparable in terms of their characteristics in the decades immediately before the construction of public housing.

The matching procedure works as follows. For neighborhoods treated in a given year, I match on the previous two decades of characteristics that predicted public housing placement, as identified in Section 5: total population, Black population share, median income, unemployment rate, and labor force participation rate. I also match on median rent, which was not

statistically significant in the fully saturated models but captures local housing market conditions. By matching on these variables over the prior two decades, I ensure that treated and control tracts followed similar trajectories before public housing construction. Additionally, I require an exact match on three dimensions: (1) whether the tract was redlined, (2) whether it was designated as an urban renewal tract, and (3) the county in which the tract was located. Exact matching on urban renewal ensures that the control neighborhoods were subject to the same urban renewal policies as the treated neighborhoods, thereby avoiding confounding the effects of the two policies. For each treated neighborhood, I select the control tract from the donor pool with the closest propensity score. Figure 4 shows absolute standard mean differences across pre-treatment characteristics for the treated neighborhoods and their matched controls, which show fairly good balance on all pre-treatment characteristics. Still, given the restrictiveness of my exact matching, the balance is imperfect, with absolute standard mean differences of above 0.1 for some covariates. I show in Section 6.5 that results are robust to alternative matching procedures that achieve better balance.

Third, I create a stacked analytic dataset in which each treated neighborhood and its matched control appear for the full panel of years from 1930 to 2000. Each treated neighborhood and its matched control are assigned a common treatment year (the year the treated neighborhood received public housing) and unique matched-pair identifiers. Note that control neighborhoods may appear multiple times in the stacked dataset if they serve as matches for multiple treated neighborhoods.

Finally, I implement a stacked difference-in-differences design that compares changes in outcomes over time between each treated neighborhood and its matched control. In particular, for tract i in matched pair m in time t , I estimate the following event-study specification:

$$y_{imt} = \alpha_{im} + \sum_{\tau \neq -10} \beta_{\tau} (D_{imt}^{\tau} \times \text{Treated}_i) + \delta_{mt} + \varepsilon_{imt} \quad (3)$$

where y_{imt} is the outcome of interest for tract i in matched pair m at time t , α_{im} are tract-by-matched pair fixed effects, $D_{imt}^{\tau} = \mathbb{1}[(t - T_{im}) = \tau]$ are indicators for event time τ relative to treatment year, Treated_i indicates whether tract i is a treated tract, and δ_{mt} are matched pair-by-year fixed effects. Tract-by-pair fixed effects control for time-invariant differences between each treated tract and its matched control. By including matched-pair-by-time fixed effects, I ensure that each treated tract is explicitly compared with its matched control at each point in time. The β_{τ} terms capture the effect of the arrival of public housing on outcomes in each treated tract relative to its matched control. Since all fixed effects are implemented within a sub-experiment defined by each matched pair, identification solely relies on within-matched-

pair comparisons.

This specification controls for a variety of potential confounders. Tract-by-pair fixed effects control for all time-invariant differences between the treated and control neighborhoods within the pair, including unobserved characteristics. Matched pair-by-time fixed effects control for any time-varying shocks that affect both treated and control neighborhoods within each matched pair, such as local economic shocks. In my baseline specification, in which I match by county, these fixed effects therefore not only control for county-level shocks, but also any local shocks that specifically affect similar types of neighborhoods within the same county.

This event-study set-up allows me to examine both pre-trends and dynamic treatment effects over time.⁸ Operationally, this specification is equivalent to a stacked difference-in-differences design in which each matched pair can be considered its own "sub-experiment" (Wing, Freedman, and Hollingsworth 2024). I weight each observation equally: since each treated unit is matched to a single control unit, one does not need to adjust for imbalances in the size of each sub-experiment through weighting as outlined in Wing, Freedman, and Hollingsworth (2024). By explicitly comparing matched pairs at each event time, this setup avoids econometric issues that have plagued the staggered adoption difference-in-differences setting (Callaway and Sant'Anna 2021). Standard errors are adjusted for spatial correlation following Conley 1999 within a radius of 2 kilometers, allowing for dependence between nearby tracts.

6.2 Validity of the Research Design

The key identification assumption is that, in the absence of public housing, the treated neighborhoods would have followed similar trends as their matched controls. I cannot directly test this assumption, but I find no evidence of systematic pre-treatment trends in the two decades prior to construction. Furthermore, funding for public housing was limited, and public housing authorities could not and did not build projects in every neighborhood that could have received it. As a result, neighborhoods that were initially similar to those that received projects ultimately did not receive projects, creating plausible counterfactuals. The benefit of matching to a neighborhood within the same county is that these control neighborhoods were subject to the same local political conditions and economic shocks. In Section 6.5, I show that results are robust to matching on control neighborhoods in different metropolitan areas.

⁸My balancing procedure ensures that I have at least one pre-trend estimate for each matched pair. Event time $t=30$ may not be observed for all matched pairs.

6.3 Effects on Treated Neighborhoods

Figure 5 presents the effects of public housing construction on the inverse hyperbolic sine of population by race. Following public housing construction, the total population in the public housing neighborhoods increased substantially, rising by approximately 15% in the immediate decade following construction, increasing to 16-17% in the following decades. This increase was driven by substantial increases in the total Black population, a 57.2% increase relative to the matched control. On average, we see little to no effect on the white population. These significant population increases reflect the influx of public residents themselves: Figure 6 shows results for the non-public housing population (the difference between the total and public housing populations) and shows a large average decline in the private population, both white and Black, following public housing construction.

Figure 7 shows that these population changes led to changes in the racial composition of the treated neighborhoods. Relative to the control neighborhoods, Black population shares increased by 2.9 percentage points in the first decade following construction, and continued to increase up to approximately 5.8 percentage points in the third decade after construction ($t=20$). The latter estimate represents a 20.9% increase in the Black population share relative to the baseline share of 27.8%.

Figure 8 shows the effects of public housing on median rent and income, showing broad declines in both. Median incomes fall sharply following public housing construction and decline further in the long run: median income falls by 9.5% in the first decade after construction, to 15.3% by the third decade. Median rents decline over time, with statistically insignificant declines at $t=0$ but reaching -10.1% by $t=20$.

6.4 Effects on nearby neighborhoods

A primary source of backlash against the mid-century public housing program has been the concern that it precipitated a broader urban decline and white flight, with negative spillovers on surrounding communities (Jackson 1985). Understanding these potential spillovers is crucial to assessing the program's overall impact. To test this, I estimate the geographic spillover effects of public housing on nearby neighborhoods.

As described in Section 4, I define the nearby neighborhoods as the set of census tracts that are contiguous to the treated neighborhoods and are within 1 km of a public housing project. Then, I apply the same nearest neighbor propensity score matching with replacement discussed in Section 6.1 to identify matched controls for these nearby neighborhoods. Balance

statistics for the nearby neighborhoods are shown in Figure 9, showing good balance on all pre-treatment characteristics, with only the distance from the central business district showing a standardized mean difference above 0.1.

I then estimate the treatment effects for these nearby neighborhoods using the same stacked difference-in-differences design as in Equation 3.

Figure 10 shows the effects of public housing construction on population by race in the nearby neighborhoods. In contrast to the treated neighborhoods, I find that the spillover neighborhoods experienced small population declines overall. I do not find evidence of substantial changes in the Black or white population in these nearby neighborhoods, suggesting that public housing construction did not, on average, precipitate large-scale white flight or racial transition in the nearby neighborhoods.

Figure 11 shows the effects of public housing construction on median rent and income in the nearby neighborhoods. I find small but statistically significant declines in median income in nearby neighborhoods, with median income falling by 2.7% in the first decade following construction and by 4.8% in the second decade. Effects in later years become statistically insignificant, though the point estimates remain negative. This may indicate some degree of economic decline or sorting in these nearby neighborhoods. However, I find no effect on median rent in these nearby neighborhoods, suggesting that the housing market effects of public housing construction were not widespread.

This evidence suggests that the geographic spillovers of public housing construction on nearby neighborhoods were relatively limited on average, and that the program's most pronounced effects were largely concentrated in the neighborhoods where the projects were built. These limited spillover effects challenge historical narratives that attributed broad urban decline to public housing (Jackson 1985). The concentrated nature of effects suggests that public housing's impact on urban segregation operated primarily through its site selection and direct effects on neighborhoods, rather than through widespread spillovers.

6.5 Robustness Checks

One might worry that the results are sensitive to the specific matching specification. In particular, one might be concerned that the exact matching criteria are too restrictive, resulting in imperfect nearest-neighbor matches. Indeed, the balance achieved in the main specifications above is imperfect, with some standardized mean differences above 0.1. I run several alternative matching specifications to test the robustness of these results. First, I run a specification that drops poor matches: in particular, I apply a caliper that excludes matches with a propen-

sity score difference of more than 0.2. Second, I loosen the exact-match restriction on county and instead require a match from a neighborhood within any other metropolitan area. This expands the donor pool, particularly for treated neighborhoods in smaller cities. A comparison of the results from these alternative specifications for the public housing neighborhoods is shown in Figure 12 and 13. Overall, the estimates are largely quite similar across these alternative specifications, both in sign and magnitude.

7 Mechanisms and Heterogeneity

In this section, I explore heterogeneity in the neighborhood effects of the public housing program. For simplicity, I show estimates at $t=20$, or 20 years after the first decade of public housing construction, which capture the program's long-run effects. There are several dimensions along which the effects of public housing may have varied.

First, the effects may have varied based on the initial neighborhood characteristics. Modern evidence on the construction of affordable housing projects through the Low Income Housing Tax Credit (LIHTC) program suggests that whether the new projects are a local amenity or a disamenity depends on neighborhood characteristics (Diamond and McQuade 2019). Furthermore, models of neighborhood tipping suggest that the initial racial composition of the neighborhood may influence the extent to which public housing construction precipitates white flight or overall racial transition (Schelling 1971; Card, Mas, and J. Rothstein 2008). Some historical accounts of public housing argue that public housing construction may have led to neighborhood racial transition through tipping dynamics (R. Rothstein 2017; Jackson 1985).

To test this tipping hypothesis, I examine how the effects of public housing varied across neighborhoods with different initial Black population shares. I divide treated and nearby neighborhoods into three groups based on their baseline Black population share measured 10 years before public housing construction: Neighborhoods that are almost entirely non-Black (less than 1% Black share), those with "medium" initial Black shares that I will consider as those in the tipping range (between 1% and 12%), and those with high initial Black shares (12% or higher). I choose 12% for the top of the "medium" range, as it corresponds to the tipping threshold estimated by Card, Mas, and J. Rothstein (2008) in 1970.

Figure 14 presents the results of this analysis at time $t=20$. I show estimates using raw population counts rather than log population, since percentage changes in Black population will mechanically be larger in neighborhoods with small initial Black populations. I find significant differences in the effects of public housing on neighborhood racial composition, depending on

the initial Black population share. Treated neighborhoods that were almost entirely non-Black and those in the "tipping range" saw sizable increases in Black population in response to public housing construction. In contrast, those with high initial Black shares saw no change in Black population, and if anything, some increase in white population. Similarly, nearby neighborhoods in the "tipping range" also saw sizable declines in white population, potentially suggesting some degree of white flight in response to public housing construction for neighborhoods around this range.

I also test whether the effects of the projects varied depending on when they were built. Comparing the effects of the projects built in the 1940s and 1950s to those built after 1960 may highlight how the changing political and social context surrounding public housing influenced its neighborhood effects. Figure 15 shows the results of this analysis at $t=20$. I find that, particularly for population and racial composition, the effects differed substantially based on when the projects were built. In particular, I see evidence of substantial long-run Black population increase in public housing neighborhoods, and long-run white population exit in nearby neighborhoods for projects built in the early period, but not for those built later. I am still exploring the mechanisms behind this result. One possibility is that this reflects the changing racial composition of American cities over this period and reflects the same racial tipping mechanisms as in Figure 14: For earlier projects, the Black population in the average neighborhood was much lower.

Finally, I test whether these effects differed in neighborhoods that were also affected by the urban renewal program. This analysis both highlights potential interactions between the two programs and addresses concerns that my baseline estimates may be conflated with the effects of urban renewal. Figure 16 presents the results in $t=20$, separately estimating Equation 3 for neighborhoods that were urban renewal tracts and those that were not. I find null effects on median income and population by race in neighborhoods that were also urban renewal tracts, suggesting that the impact of urban renewal may have outweighed any effects of public housing. In contrast, in neighborhoods not targeted by urban renewal, I find similar effects as in the main analysis.

8 Did Public Housing Create Low-Opportunity Neighborhoods

I have shown that public housing had significant and persistent effects on neighborhood population, racial and economic composition, and housing markets. I next ask whether these changes translated to differences in long-run neighborhood opportunity for children who grew

up in these neighborhoods.

I explore this question by merging tract-level data from the Opportunity Atlas (Chetty et al. 2018). These data include measures of long-run upward mobility and incarceration for children born from 1978 to 1983 whose parents were at the 25th percentile of the national income distribution. I will refer to these children as “low-income children”. In particular, I use tract-level measures of the mean household income rank in adulthood in 2014-2015 and the share of these children who were incarcerated as of April 1st, 2010, at the 2010 Census tract level. I concord these data to 1950 tract boundaries using my tract crosswalks, along with population counts of relevant children in each tract from the Opportunity Atlas data. I then match these data to the matched pairs of public housing tracts, nearby tracts, and control tracts used in Section 6.

I then estimate the following regression model separately for public housing tracts and nearby tracts:

$$Y_{im} = \beta \text{Treated}_i + X'_{i,1970} \gamma + \mu_m + \epsilon_{im} \quad (4)$$

where Y_i denotes the Opportunity Atlas outcome of interest, either the mean income rank or the incarceration rate for low-income children, in tract i , μ_m denotes a matched-pair fixed effect, and $X_{i,1970}$ is a vector of tract characteristics in 1970 (Black share, median income, total population, unemployment rate, and median rent). The variable Treated_i is an indicator for whether tract i is a public housing tract (or a nearby tract). The coefficient β captures the average difference in outcome between each treated tract and its matched control tract, conditional on 1970 neighborhood characteristics.

Tables 5 and 6 present results for public housing tracts and nearby tracts, respectively. I estimate specifications both without controls (Columns 1 and 3) and with controls for 1970 neighborhood characteristics (Columns 2 and 4).

Children who lived in public housing tracts experienced significantly worse outcomes: a 1.7 percentage point lower income rank and a 0.5 percentage point higher incarceration rate compared to matched controls (Table 5, Columns 1 and 3). About half of this effect persists even after controlling for 1970 neighborhood characteristics, such as Black share and median income (Columns 2 and 4), suggesting that public housing had effects beyond simply changing observable demographics.

By contrast, the apparent spillover effects on nearby tracts appear to reflect selection rather than true spillovers. While nearby tracts initially show worse outcomes (Table 6, Columns 1 and 3), these differences completely disappear when controlling for 1970 neighborhood characteristics (Columns 2 and 4). These results imply that neighborhoods closer to public housing

experienced worse outcomes simply because they were poorer and more Black by 1970, not because proximity to public housing directly reduced opportunity. The adverse effects of public housing on upward mobility were therefore geographically concentrated within immediate project tracts themselves.

Taken together, these results suggest that public housing shaped neighborhood opportunity through two channels: by altering observable neighborhood composition, and to a lesser extent, through additional effects of the projects themselves. However, one should interpret these results cautiously. The matched-pair fixed effects controls for earlier neighborhood differences, but we cannot observe pre-treatment mobility outcomes. Thus, we should not necessarily interpret these estimates as causal estimates of public housing on upward mobility.

9 Conclusion

This paper has examined the long-run neighborhood effects of the mid-20th-century U.S. public housing program. Using a newly constructed national dataset of more than 7,000 projects built between 1935 and 1973, linked to a consistent panel of tract-level census data spanning 1930–2010, I documented two central findings. First, public housing projects were systematically targeted toward neighborhoods that were initially poorer, more populated, disproportionately Black, and often redlined or slated for urban renewal. These patterns confirm the program’s slum-clearance origins and the racialized politics of site selection. Second, public housing construction had significant and persistent effects on neighborhood trajectories: treated tracts experienced long-run increases in Black population shares and sustained declines in incomes and rents, while spillover effects on nearby neighborhoods were more limited. Importantly, I find evidence consistent with racial tipping dynamics: neighborhoods with moderate baseline Black shares experienced substantial white population outflows following public housing construction, while neighborhoods with higher initial Black shares showed more muted responses.

Taken together, these results suggest that public housing not only reflected but also reinforced existing patterns of segregation and disinvestment, contributing quantitative evidence to longstanding debates about the role of federal policy in creating residential segregation ([R. Rothstein 2017](#); [Trounstein 2018](#); [T. D. Logan and Parman 2025](#)). Rather than catalyzing neighborhood improvement as envisioned in the Housing Act of 1949, the program contributed to the concentration of poverty and the persistence of racial segregation in American cities. The historical patterns I document have clear implications for contemporary affordable hous-

ing policy. Modern reforms to public housing have emphasized smaller-scale interventions, mixed-income developments, and designs that blend into existing neighborhoods rather than large, segregated projects. My findings help explain the rationale behind these reforms: large concentrations of subsidized housing in neighborhoods undergoing racial transition accelerated segregation and demographic change.

One crucial caveat is that my results do not necessarily imply that public housing failed to benefit individual residents. For many low-income residents, public housing may have provided better housing quality, stability, and affordability than available private-market alternatives. In ongoing work, I am linking individuals to public housing projects using full-count Census data to study the effects on project residents and their neighbors. This individual-level analysis will complement the neighborhood-level findings presented here by directly measuring the benefits and costs experienced by public housing residents themselves, providing a more complete assessment of the program's legacy.

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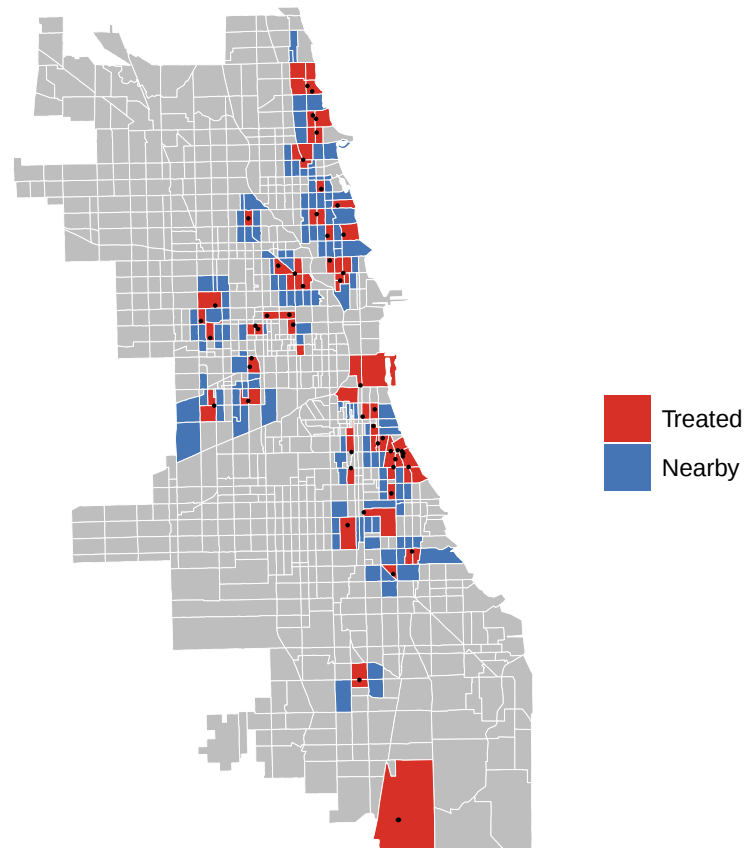
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Figures and Tables

Figure 1: Public Housing Projects and Spillover Areas: Chicago



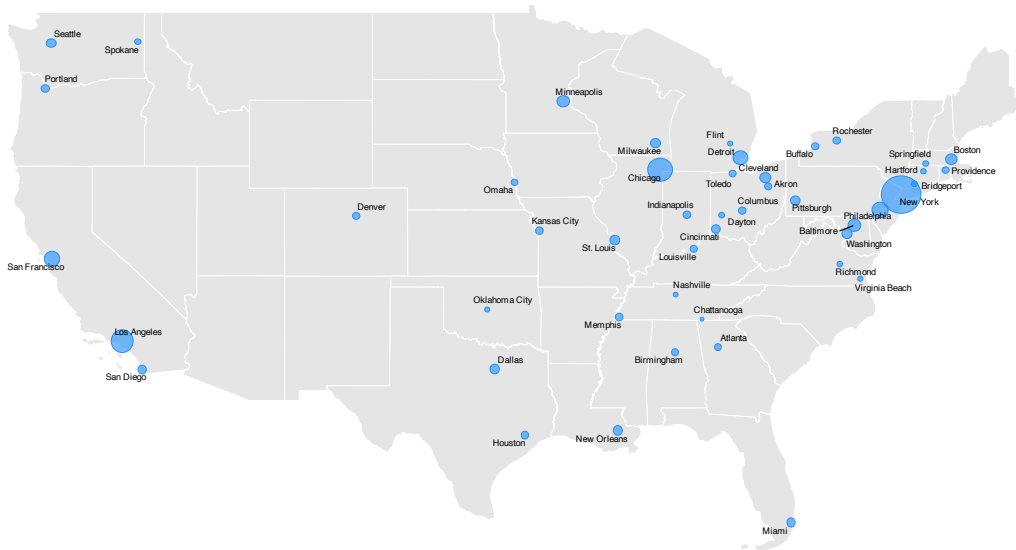
Note: This map shows census tracts in Chicago containing public housing projects built between 1941 and 1973 (red) and nearby spillover areas (blue). Spillover areas are defined as census tracts that share a border with a treated tract and are within 1 kilometer of the nearest public housing project.

Table 1: Sample Attrition

Step	Tracts	Pop. 1940 (M)	CBSAs	Treated Tracts	Projects	Units
Original sample	12,062	38.1	60	1,399	1,540	516,084
Balanced on years (1930-2010)	9,086	34.1	50	1,148	1,207	466,303
Complete variables	8,596	33	50	1,122	1,179	451,982
Exclude treatments ≤ 1940	8,487	32.4	50	1,013	1,069	395,698
Drop CBSAs <30 tracts	8,430	32.1	47	998	1,041	391,507
Drop population outliers	6,506	27.5	47	822	814	300,964

Notes: This table shows the impact of sample restrictions on the number of census tracts, 1940 population (in millions), CBSAs, treated tracts, public housing projects, and housing units.

Figure 2: Geographic Coverage of CBSAs in Analysis Sample



Note: This map shows the 45 Core-Based Statistical Areas (CBSAs) included in the balanced panel analysis. Shaded areas represent metropolitan areas that meet the sample selection criteria: CBSAs with at least one public housing project built between 1941 and 1973 and complete census tract data for all study years (1930-1990).

Table 2: Site Selection: Predicting Public Housing Placement from 1940 Neighborhood Characteristics

	(1)	(2)	(3)	(4)
Black Share	0.210*** (0.041)	0.189*** (0.041)	0.183*** (0.044)	0.175*** (0.040)
Asinh Total Population	0.039*** (0.010)	0.040*** (0.009)	0.043*** (0.010)	0.037*** (0.009)
Asinh Median Income	-0.108*** (0.020)	-0.081*** (0.020)	-0.077*** (0.020)	-0.068*** (0.020)
Asinh Median Rent	-0.076*** (0.018)	-0.031 (0.028)	-0.022 (0.030)	-0.026 (0.028)
Pct Graduated HS		0.090 (0.060)	0.097 (0.064)	0.060 (0.060)
Unemployment Rate		0.856*** (0.180)	0.855*** (0.185)	0.810*** (0.177)
LFP Rate		-0.351*** (0.128)	-0.295** (0.132)	-0.353*** (0.126)
Redlined (HOLC)		0.026 (0.017)	0.027 (0.017)	0.023 (0.017)
Asinh Dist. from CBD		0.001 (0.003)	0.001 (0.003)	0.001 (0.003)
CBD Indicator		-0.019 (0.031)	-0.026 (0.033)	-0.028 (0.032)
Share Needing Major Repairs			0.108 (0.077)	
Asinh Dist. to Highway				0.007 (0.009)
Urban Renewal Area				0.088*** (0.024)
Num.Obs.	6506	6506	6169	6506
R2	0.111	0.119	0.119	0.125
R2 Adj.	0.103	0.110	0.110	0.116
County fixed effects	Yes	Yes	Yes	Yes

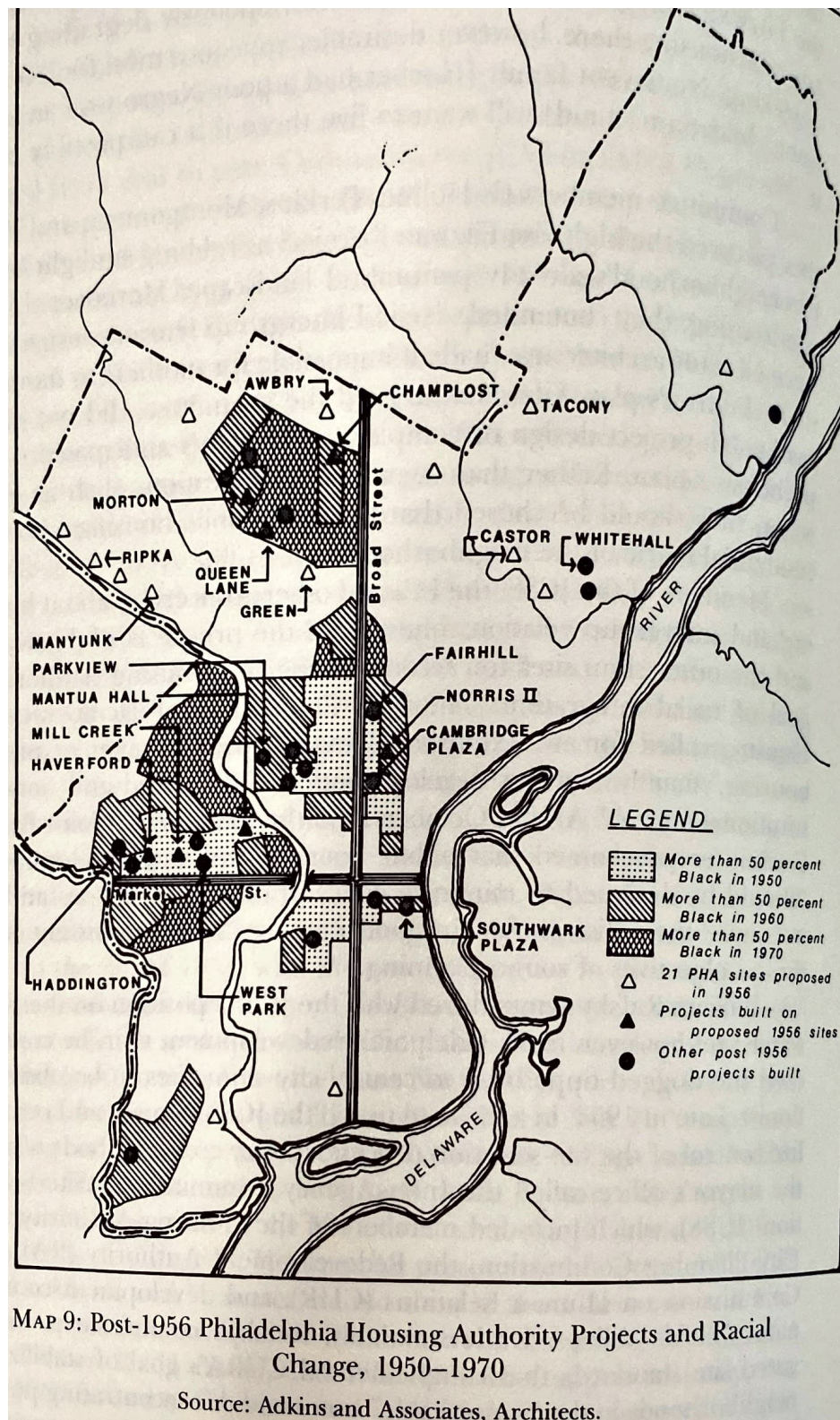
Notes: This table reports results from linear probability models estimating the relationship between 1940 neighborhood characteristics and the probability that a census tract received a public housing project between 1941 and 1973. The dependent variable equals 1 if a tract ever received a project and 0 otherwise. All specifications include county fixed effects, and standard errors are adjusted for spatial correlation following Conley (1999) within a 2-kilometer radius. Key predictors include demographic, socioeconomic, housing-market, and urban-structure characteristics measured in 1940, as well as indicators for HOLC redlining, urban renewal, and proximity to interstate highways. Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table 3: 1940 Neighborhood Characteristics: Proposed vs Actual Public Housing Locations in Philadelphia

Variable	Actual Public Housing	Proposed Only	Std. Diff.
Black Share	0.28 (0.29)	0.01 (0.02)	1.310***
Log Total Population	9.16 (1.27)	7.98 (1.78)	0.762**
Log Median Income	3.47 (0.30)	3.52 (0.28)	0.148
Unemployment Rate	0.21 (0.10)	0.14 (0.05)	0.897**
Log Median Rent	6.41 (0.32)	6.45 (0.40)	0.102
Log Distance from CBD	8.31 (2.04)	9.75 (0.26)	0.992**
Redlined (HOLC)	0.40 (0.50)	0.08 (0.29)	0.791**
Urban Renewal Area	0.34 (0.48)	0.17 (0.39)	0.398

Notes: This table compares baseline (1940) neighborhood characteristics between proposed-but-not-built public housing sites identified from a 1956 Philadelphia map (Bauman (1987)) and actually-built public housing sites in Philadelphia from the main analysis dataset. The sample includes 12 proposed-only locations and 47 actual public housing locations built between 1941 and 1973. This comparison provides evidence on whether proposed sites that were never built had baseline characteristics similar to those of sites that were actually built, which helps assess whether political or other factors, independent of baseline neighborhood characteristics, determined which proposals were realized. The first two columns show means with standard deviations in parentheses. The Std. Diff. column shows standardized mean differences. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Historical Map of Proposed and Actual Philadelphia Public Housing Sites, 1956



Note: This map shows the distribution of proposed and actual public housing sites in Philadelphia. Proposed-but-not-built sites serve as placebo treatments in our analysis.

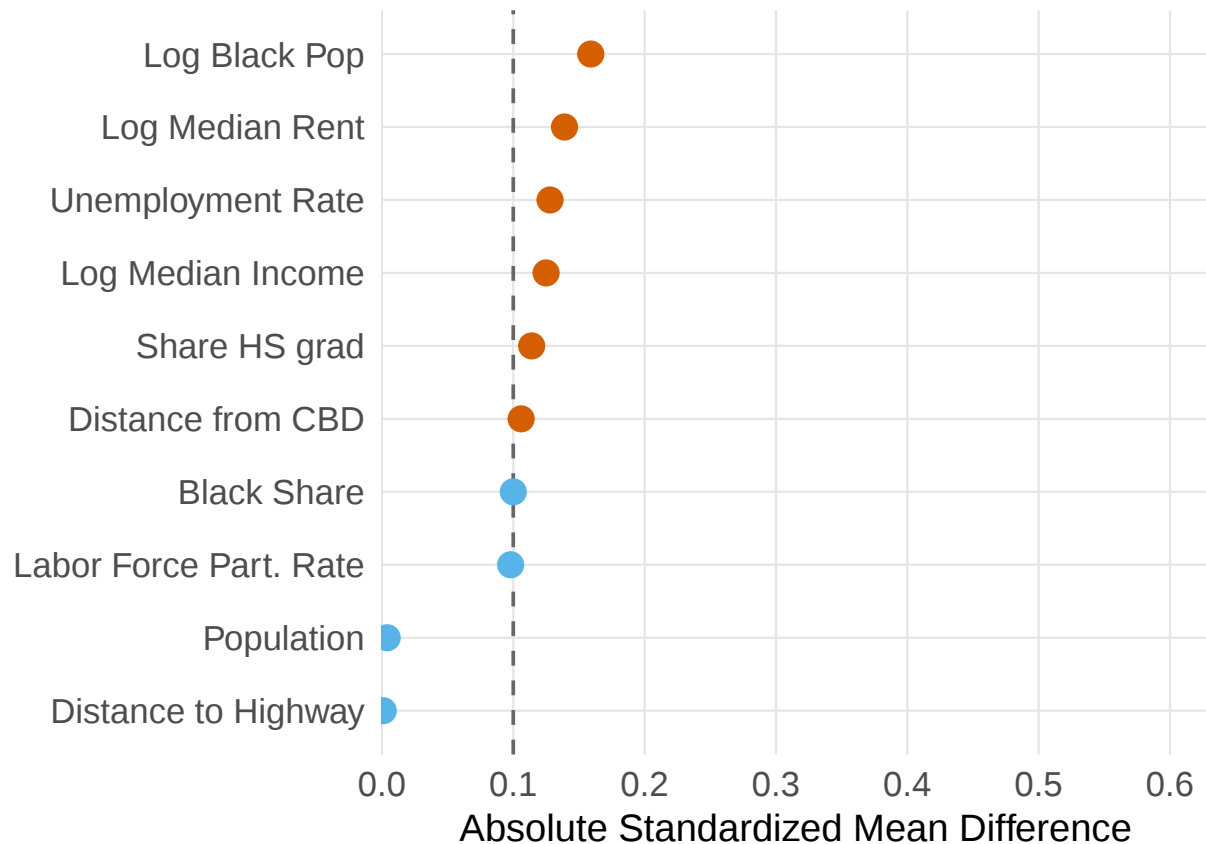
Source: Bauman (1987), georeferenced by author.

Table 4: Project Demographics: Predicting Racial Composition of Public Housing from Baseline Neighborhood Characteristics

	(1)	(2)	(3)
(Intercept)	1.554*** (0.172)	1.774*** (0.338)	
Black Share	0.435*** (0.062)	0.441*** (0.070)	0.228*** (0.059)
Asinh Median Income	-0.292*** (0.042)	-0.253*** (0.056)	-0.229*** (0.058)
Asinh Median Rent		-0.149** (0.071)	-0.098 (0.061)
Asinh Population		0.045* (0.025)	0.052*** (0.019)
Unemployment Rate		-0.188 (0.182)	0.309** (0.125)
LFP Rate		0.242 (0.238)	0.401* (0.238)
Asinh Dist. to CBD		0.011* (0.006)	0.019*** (0.005)
CBD		-0.075 (0.071)	-0.115* (0.066)
Asinh Dist. to Highway		-0.011 (0.032)	-0.045 (0.034)
Redlined (HOLC)		-0.032 (0.054)	0.024 (0.050)
Urban Renewal Area		-0.009 (0.045)	0.016 (0.030)
N	759	759	759
R ²	0.356	0.384	0.581
Adj. R ²	0.354	0.375	0.544
County FE	No	No	Yes

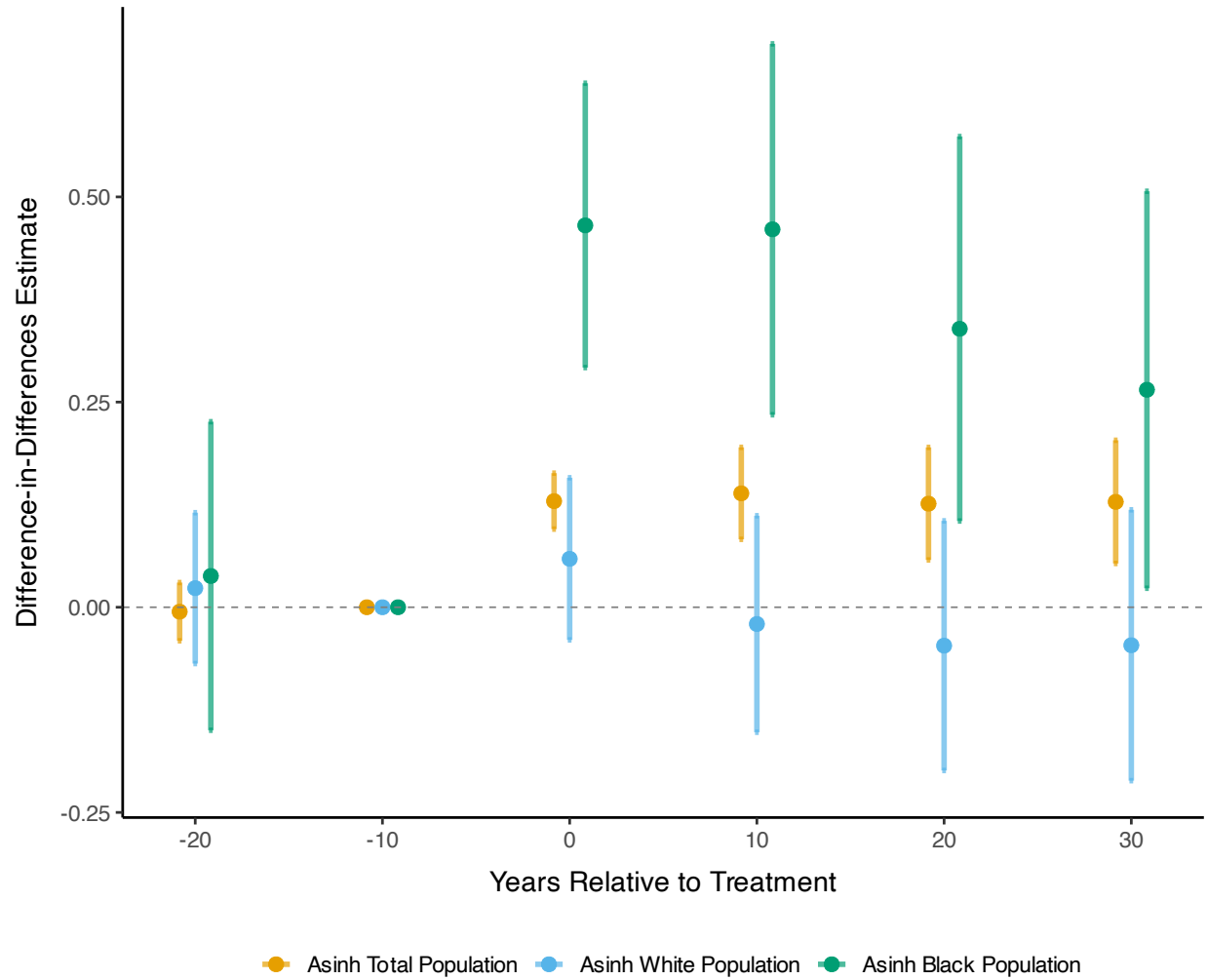
Notes: This table reports OLS regressions estimating the relationship between baseline neighborhood characteristics (measured in the decade preceding project construction, $t - 10$) and the racial composition of public housing projects in the 1970s. The sample includes only census tracts that received public housing between 1941 and 1973. The dependent variable is the Black population share within the public housing project, measured in the 1970s from administrative data (1977 Picture of Subsidized Households for most projects, 1971 data for NYC, and 1973 annual reports for Chicago). Column (1) includes baseline Black share and income only. Column (2) adds additional neighborhood controls. Column (3) adds county fixed effects. Standard errors are clustered at the county level. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 4: Pre-period Balance: Treated Neighborhoods vs Matched Controls



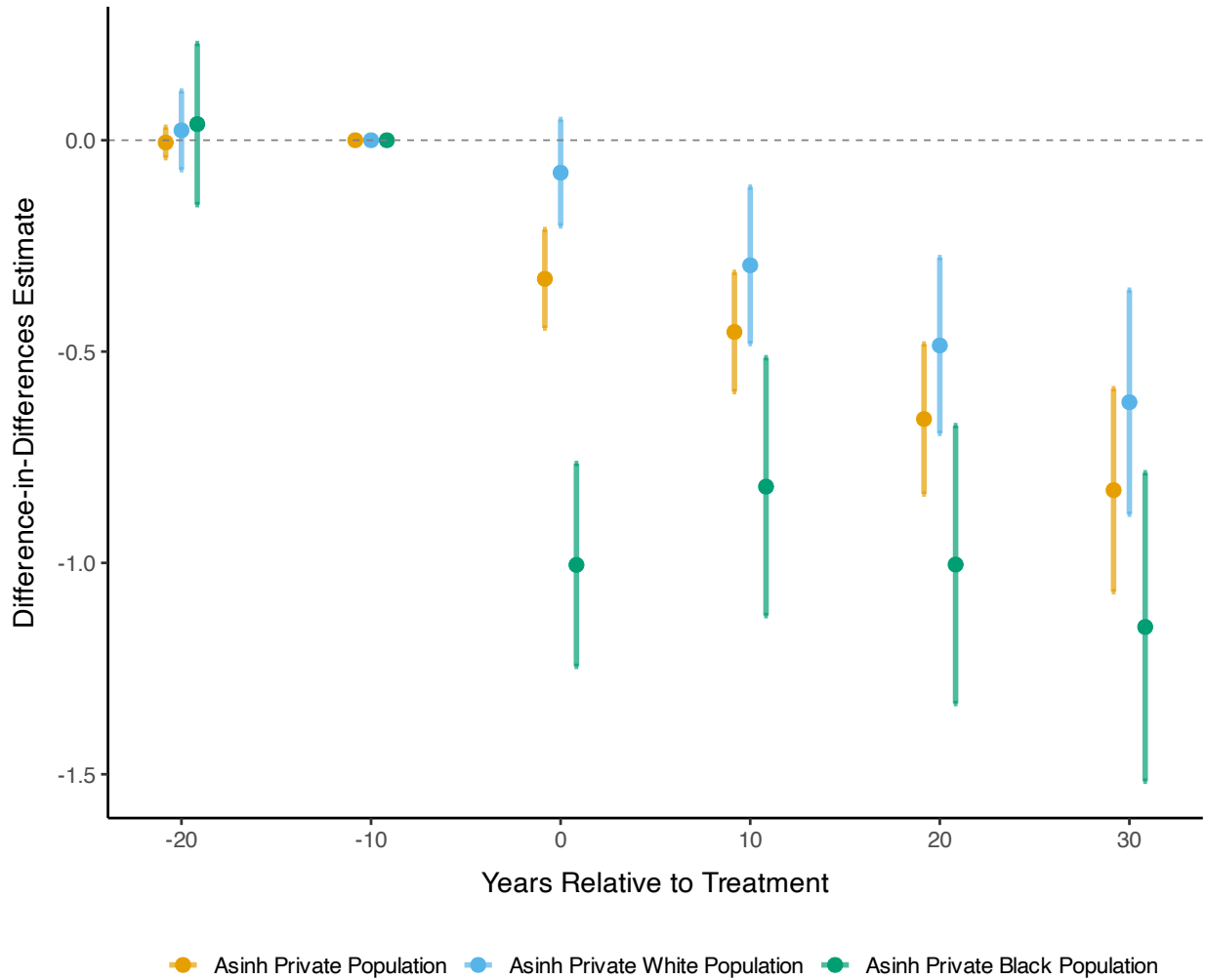
Note: This figure displays standardized mean differences (SMD) between treated neighborhoods and matched controls across key baseline covariates in the pre-period. Each point represents the SMD for the given year relative to public housing construction. Successful matching is indicated by SMDs close to zero. The reference period is 10 years before the construction decade (event time = -10). The vertical dotted line indicates the timing of public housing construction.

Figure 5: Population and Racial Composition Effects in Treated Neighborhoods



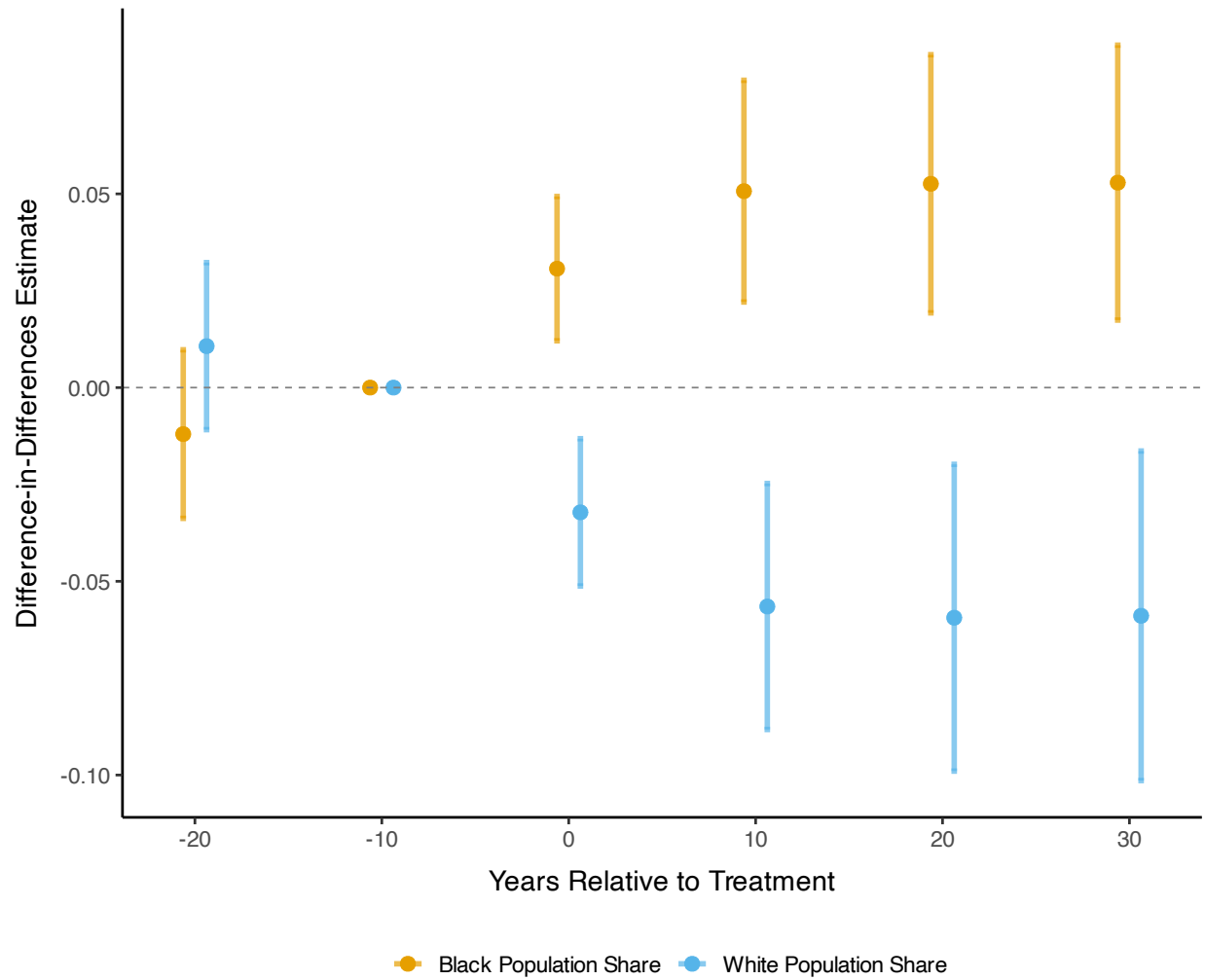
Note: This figure displays event study estimates of public housing effects on the inverse hyperbolic sine of total population, Black population, and white population in treated neighborhoods compared to matched controls. Each line represents the difference-in-differences estimate for the given year relative to public housing construction. The reference period is 10 years before the construction decade (event time = -10). The vertical dotted line indicates the timing of public housing construction.

Figure 6: Effects on Estimated Private Population, Treated Neighborhoods



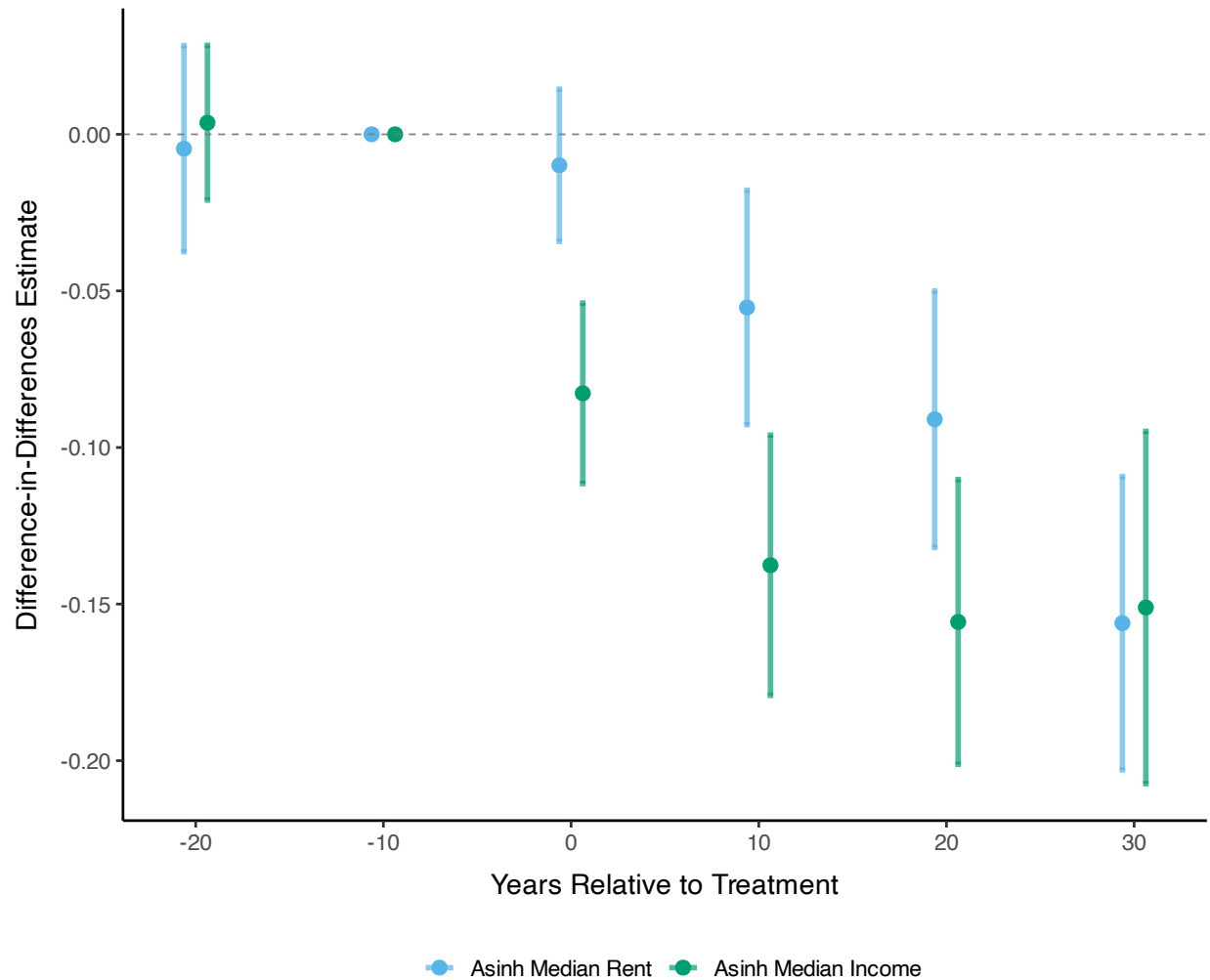
Note: This figure shows event study estimates of public housing effects on the inverse hyperbolic sine of private population by race in treated neighborhoods compared to matched controls. Private population is estimated by subtracting public housing residents from the total tract population. Public housing population estimates from 1970s administrative data are set to zero before project completion and held constant at the 1970s level from the treatment year onward. Each line represents the difference-in-differences estimate for the given year relative to public housing construction. The reference period is 10 years before the construction decade (event time = -10).

Figure 7: Effects on Racial Composition Shares in Treated Neighborhoods



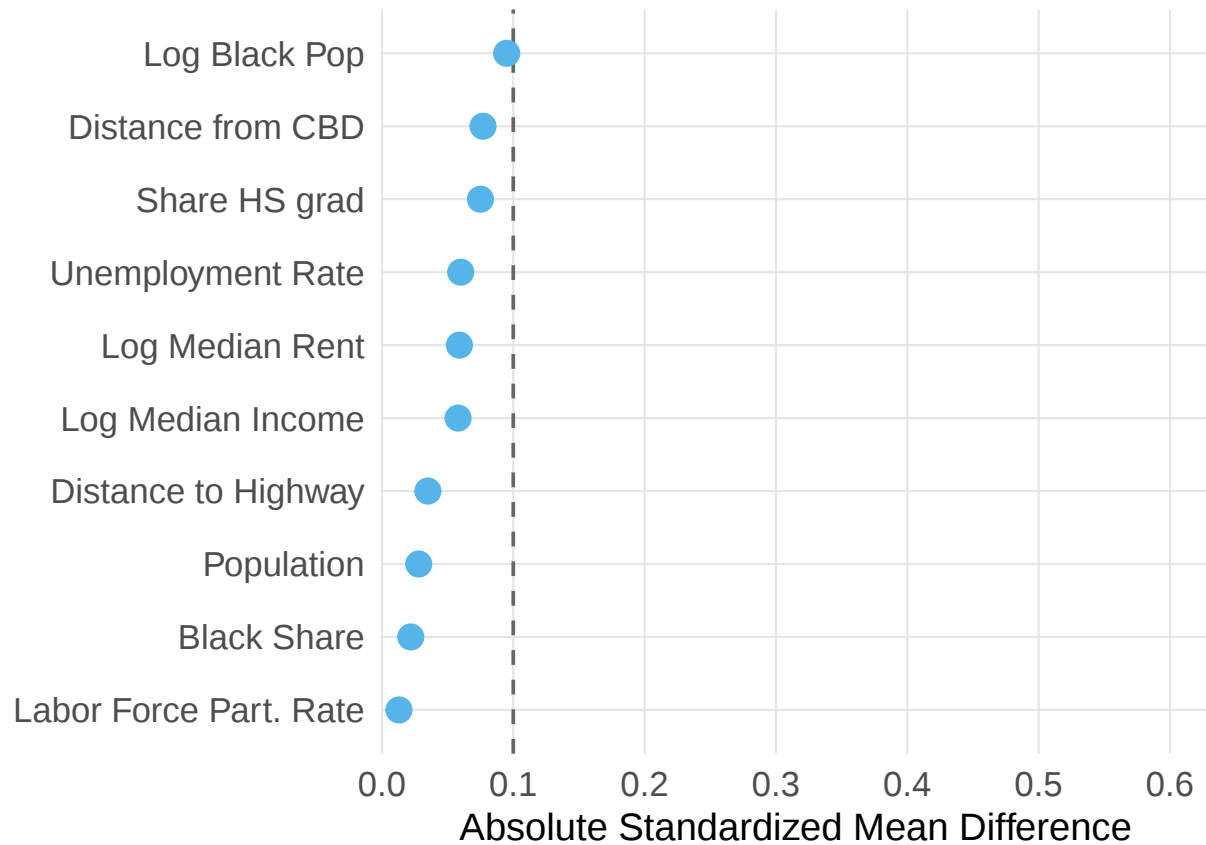
Note: This figure shows event study estimates of public housing effects on Black population share and white population share in treated neighborhoods compared to matched controls. Each line represents the difference-in-differences estimate for the given year relative to public housing construction. The reference period is 10 years before the construction decade (event time = -10).

Figure 8: Economic and Housing Effects in Treated Neighborhoods



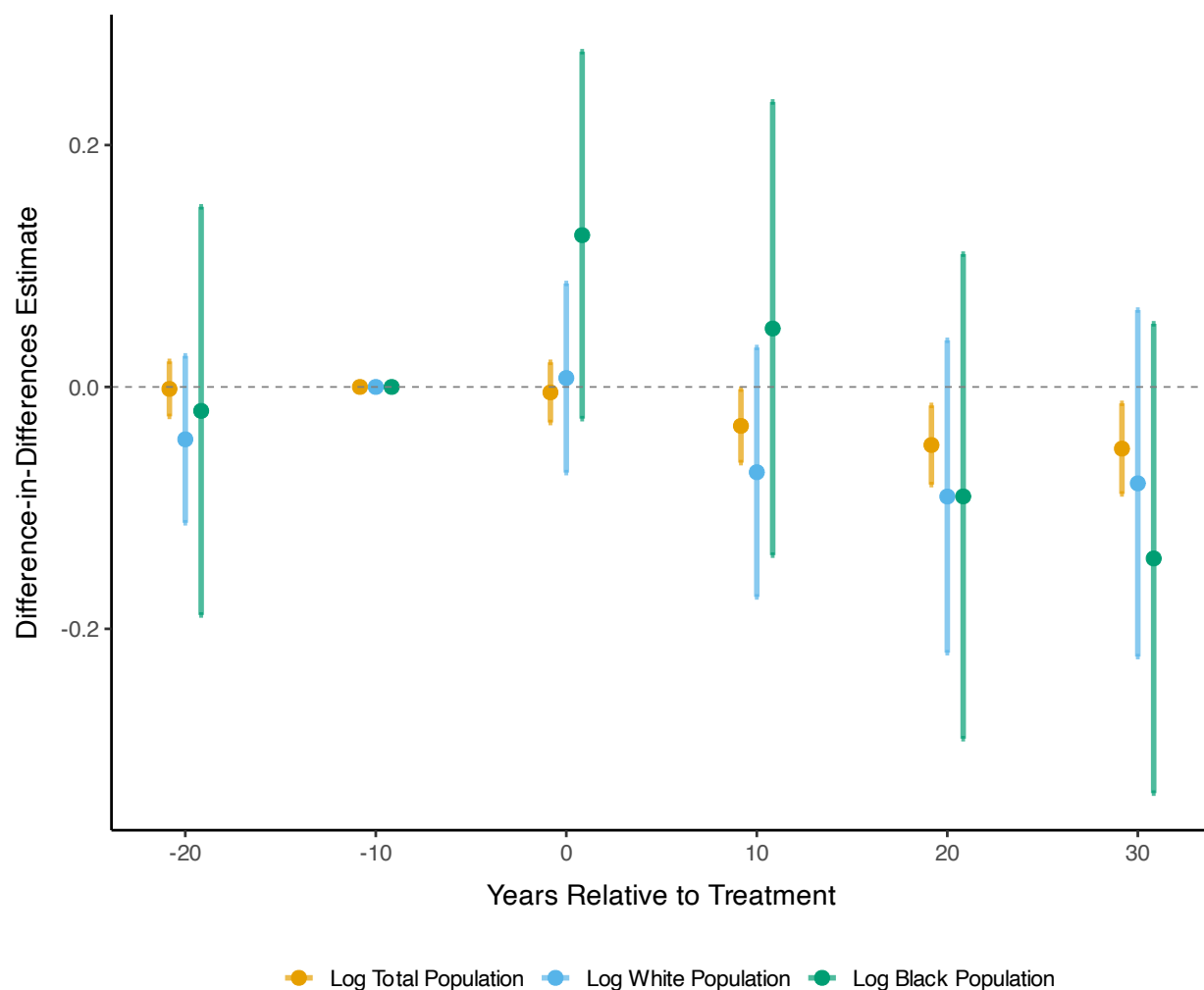
Note: This figure displays event study estimates of public housing effects on the inverse hyperbolic sine of median rent and median household income in treated neighborhoods compared to matched controls. Each line represents the difference-in-differences estimate for the given year relative to public housing construction. The reference period is 10 years before the construction decade (event time = -10).

Figure 9: Pre-period Balance: Nearby Neighborhoods vs Matched Controls



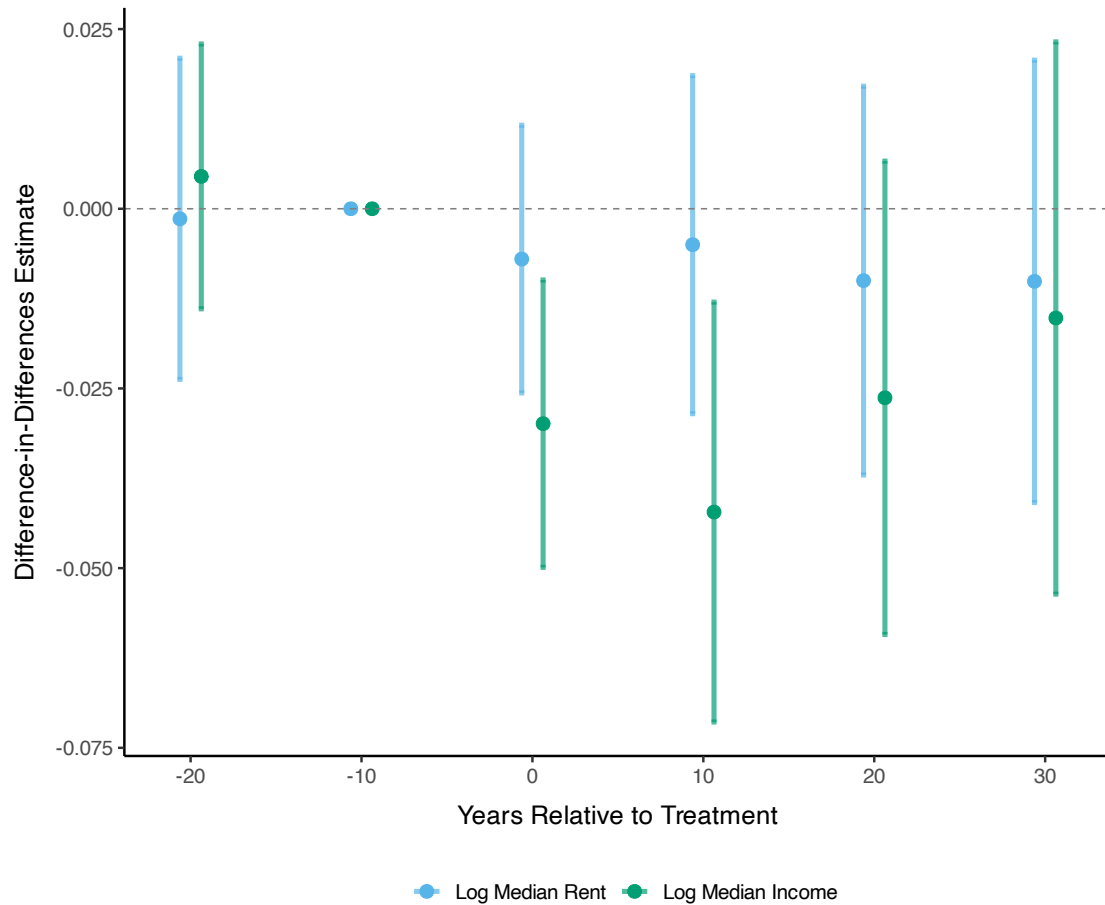
Note: This figure displays standardized mean differences (SMD) between nearby neighborhoods (those that share a border with treated tracts and are within 1km of the nearest public housing project) and matched controls across key baseline covariates in the pre-period. Each point represents the SMD for the given year relative to nearby public housing construction. The reference period is 10 years before the construction decade (event time = -10).

Figure 10: Spillover Effects: Population and Racial Composition in Nearby Neighborhoods



Note: This figure displays event study estimates of public housing spillover effects on the inverse hyperbolic sine of total population, Black population, and white population in nearby neighborhoods (those that share a border with treated tracts and are within 1km of the nearest public housing project) compared to matched controls. Each line represents the difference-in-differences estimate for the given year relative to nearby public housing construction. The reference period is 10 years before the construction decade (event time = -10). The vertical dotted line indicates the timing of public housing construction.

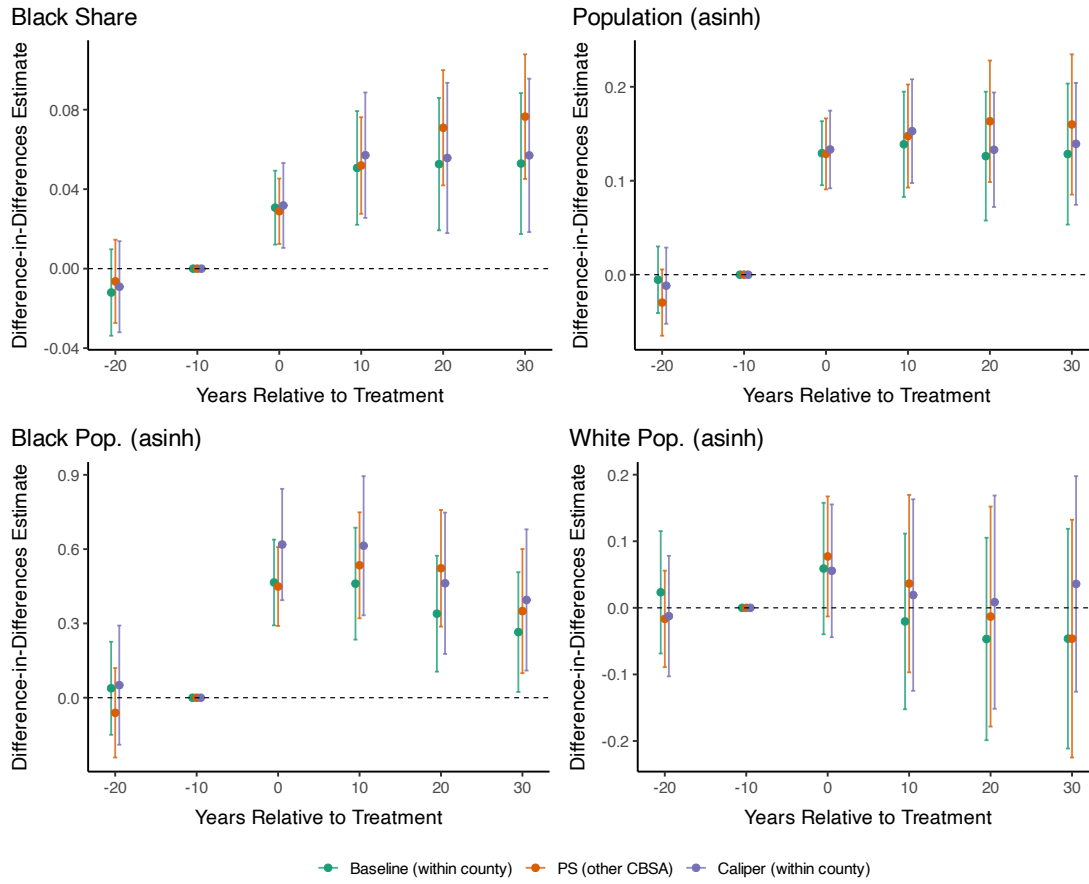
Figure 11: Spillover Effects: Economic and Housing Outcomes in Nearby Neighborhoods



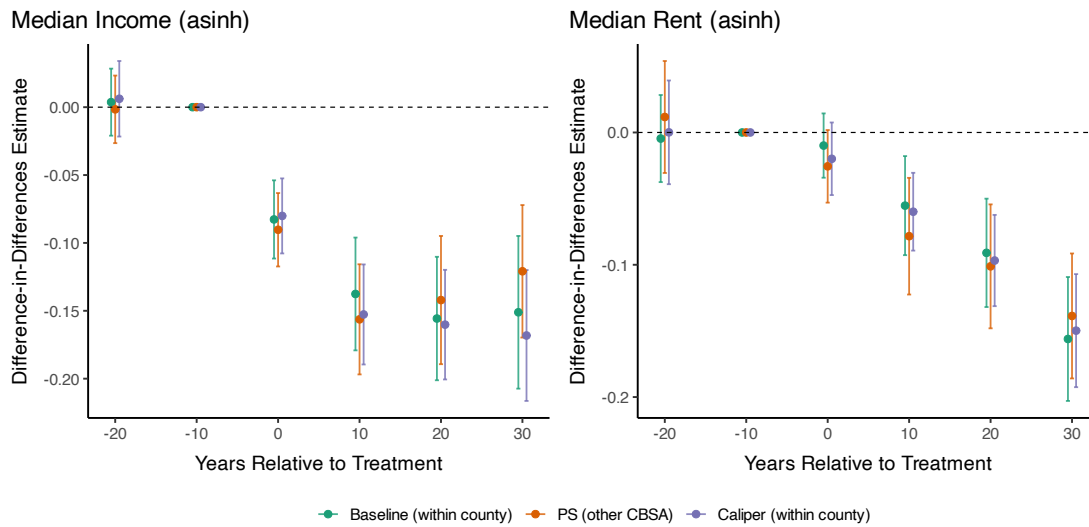
Note: This figure displays event study estimates of public housing spillover effects on the inverse hyperbolic sine of median rent and median household income in nearby neighborhoods (those that share a border with treated tracts and are within 1km of the nearest public housing project) compared to matched controls. Each line represents the difference-in-differences estimate for the given year relative to nearby public housing construction. The reference period is 10 years before the construction decade (event time = -10). The vertical dotted line indicates the timing of public housing construction.

Figure 12: Alternative Matching Specifications: Treated neighborhoods

(a) Demographics



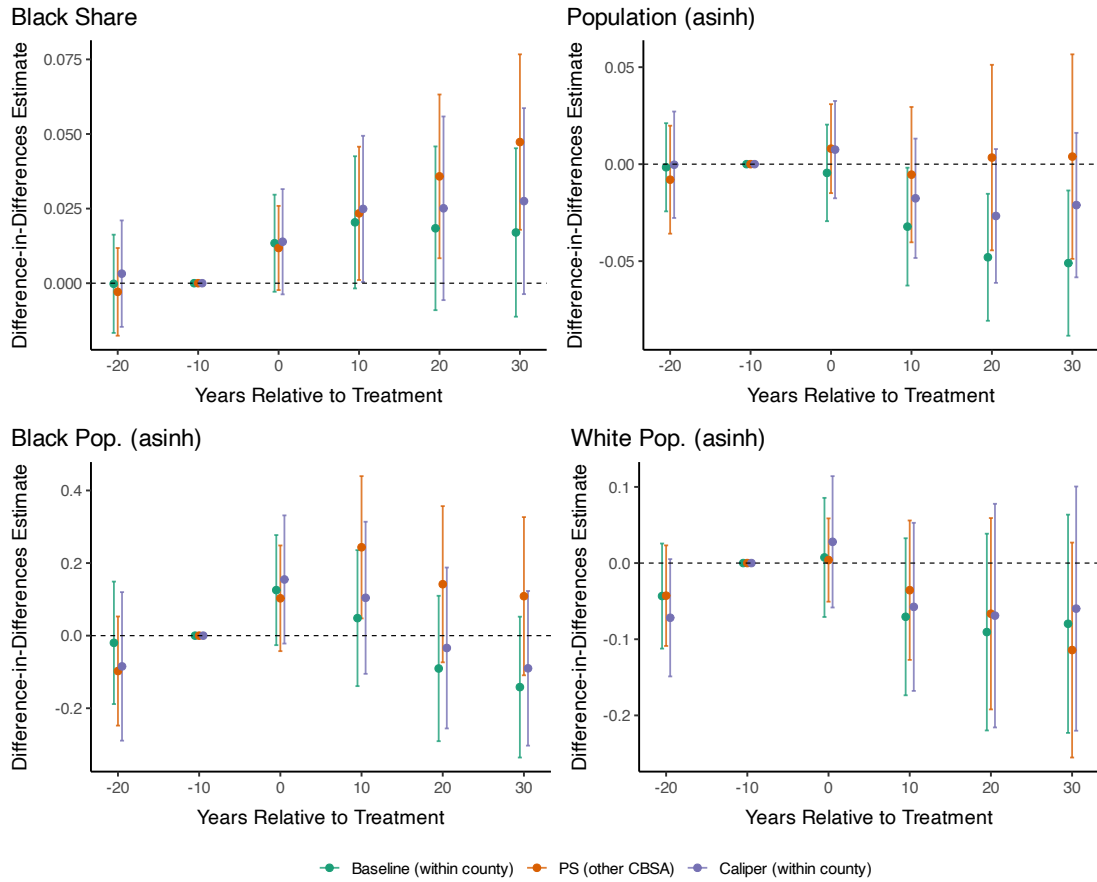
(b) Income & Rent



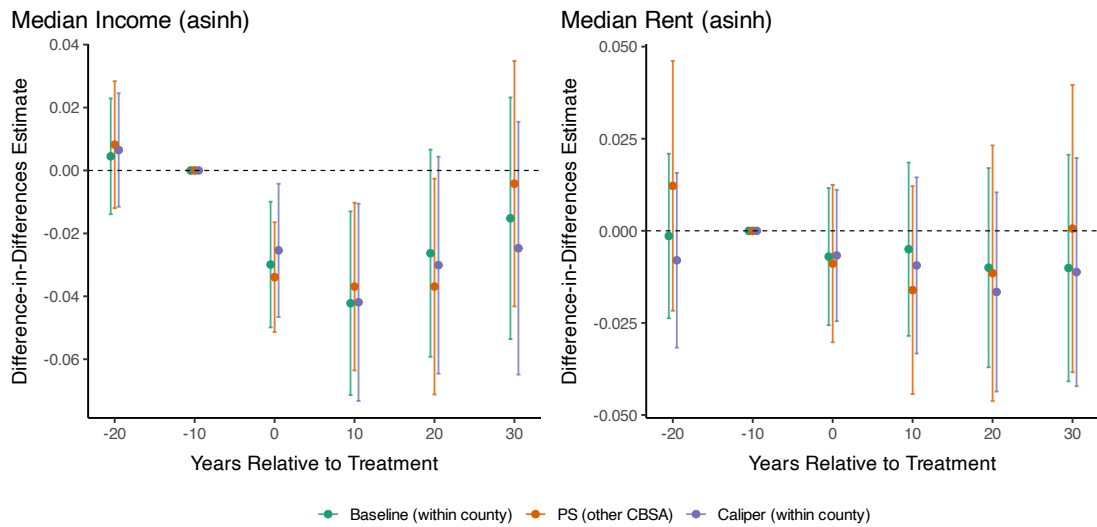
Note: Event study estimates across three matching specifications. Baseline: exact match on county, redlining, urban renewal. PS (other CBSA): cross-metro matching (antiexact on CBSA), exact on redlining/urban renewal. Caliper: baseline plus 0.2 SD caliper. All use 1:1 nearest-neighbor with replacement.

Figure 13: Alternative Matching Specifications: Nearby neighborhoods

(a) Demographics

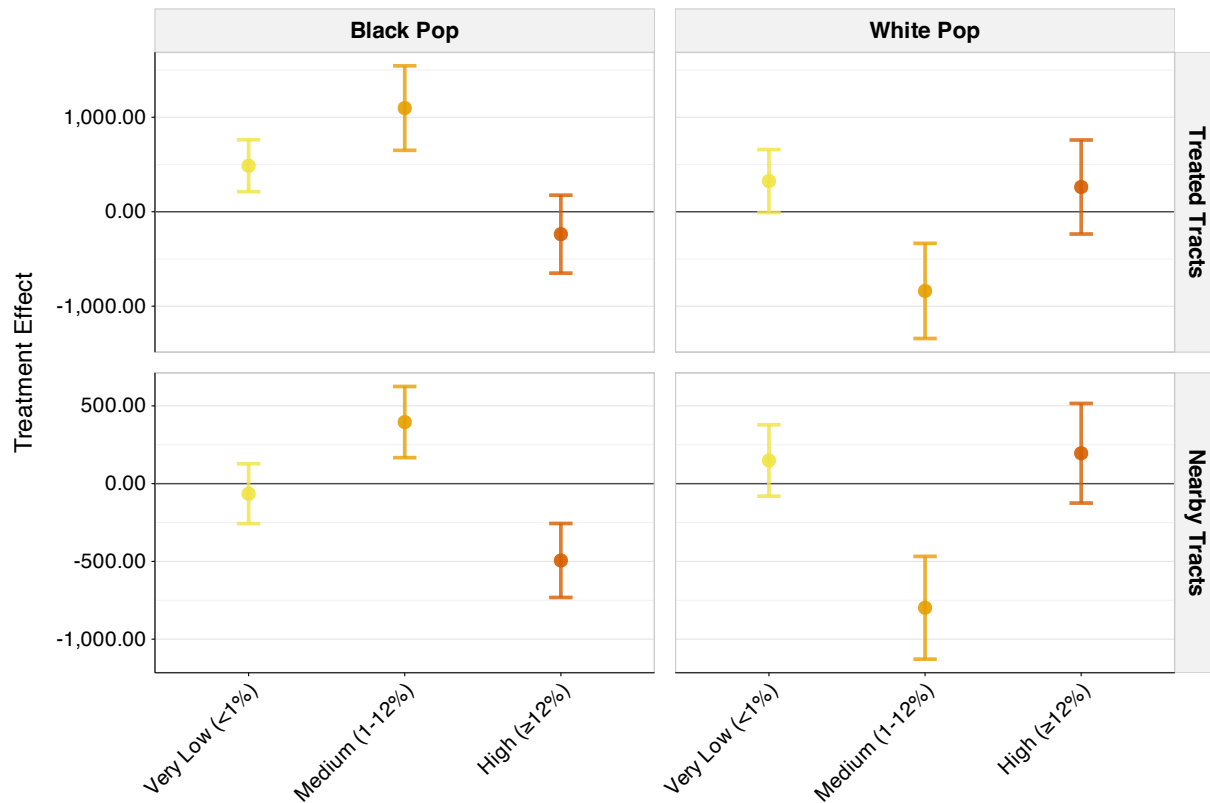


(b) Income & Rent



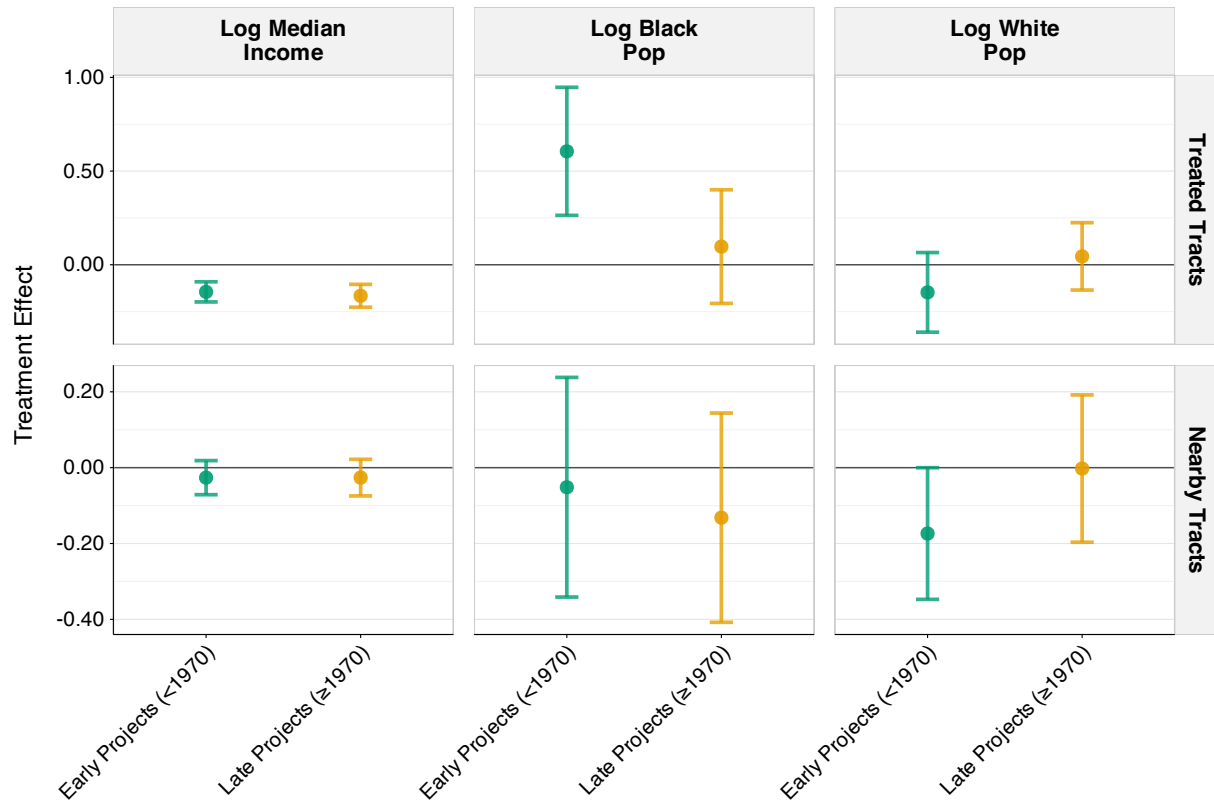
Note: Event study estimates for nearby neighborhoods (border treated tracts, within 1km of projects) across three matching specifications. Baseline: exact match on county, redlining, urban renewal. PS (other CBSA): cross-metro matching, exact on redlining/urban renewal. Caliper: baseline plus 0.2 SD caliper. All use 1:1 nearest-neighbor with replacement.

Figure 14: Heterogeneity by Baseline Black Population Share



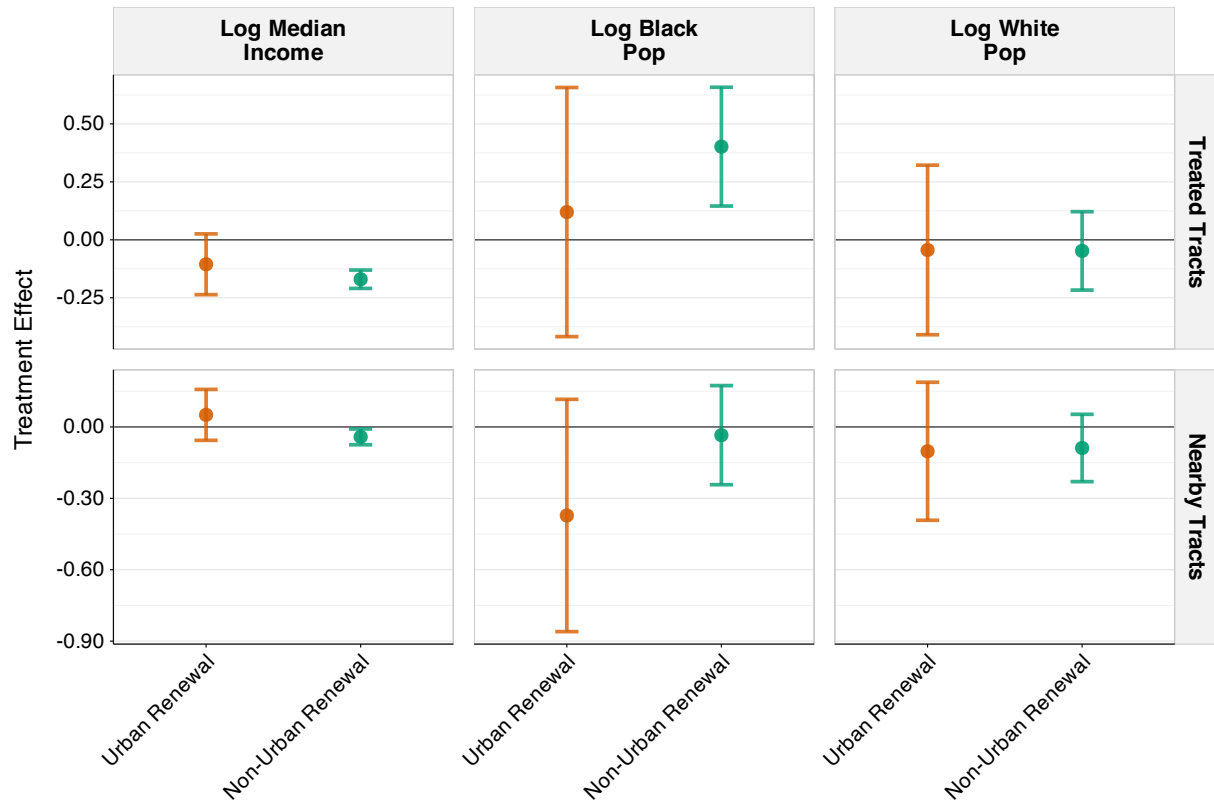
Note: This figure displays heterogeneous treatment effects at t=20 (20 years post-construction) by baseline Black population share. Neighborhoods are divided into three groups: low Black share (<1%), medium Black share (1-12%, the tipping range from Card, Mas, and J. Rothstein (2008)), and high Black share (≥12%). Results are shown separately for treated neighborhoods (left panels) and nearby neighborhoods (right panels). Population estimates use raw counts rather than inverse hyperbolic sine transformations.

Figure 15: Heterogeneity by Construction Decade



Note: This figure displays heterogeneous treatment effects at $t=20$ (20 years post-construction) by construction timing. Projects are divided into early period (built before 1960) and late period (built 1960 or later). Results are shown separately for treated neighborhoods (left panels) and nearby neighborhoods (right panels).

Figure 16: Heterogeneity by Urban Renewal Status



Note: This figure displays heterogeneous treatment effects at $t=20$ (20 years post-construction) by whether the neighborhood was also affected by urban renewal. Urban renewal tracts are defined as those with more than 5% of their area overlapping with an urban renewal project boundary. Results are shown separately for treated neighborhoods (left panels) and nearby neighborhoods (right panels).

Table 5: Opportunity Atlas Outcomes: Public Housing Tracts vs Matched Controls

	Mobility		Incarceration	
	No Controls	1970 Controls	No Controls	1970 Controls
Treated	-0.018*** (0.002)	-0.008*** (0.002)	0.006*** (0.001)	0.003*** (0.001)
Num.Obs.	1540	1540	1540	1540
R2	0.758	0.854	0.660	0.728

Notes: This table reports results from OLS estimation of Equation 4 for public housing tracts. Columns 1-2 report results for the mean income rank in adulthood in 2014-2015 for low-income children born between 1978 and 1983. Columns 3-4 report results for the share of these children who were incarcerated as of April 1st, 2010. Columns 2 and 4 control for 1970 neighborhood characteristics: Black share, median income, total population, unemployment rate, and median rent. All specifications include matched-pair fixed effects. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Opportunity Atlas Outcomes: Nearby Tracts vs Matched Controls

	Mobility		Incarceration	
	No Controls	1970 Controls	No Controls	1970 Controls
Nearby	-0.006*** (0.002)	-0.001 (0.002)	0.002** (0.001)	0.000 (0.001)
Num.Obs.	2756	2756	2756	2756
R2	0.756	0.836	0.680	0.745

Notes: This table reports results from OLS estimation of Equation 4 for tracts within 1km of public housing. Columns 1-2 report results for the mean income rank in adulthood in 2014-2015 for low-income children born between 1978 and 1983. Columns 3-4 report results for the share of these children who were incarcerated as of April 1st, 2010. Columns 2 and 4 control for 1970 neighborhood characteristics: Black share, median income, total population, unemployment rate, and median rent. All specifications include matched-pair fixed effects. Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Public Housing Data Construction

A.1 Estimating Public Housing Project Populations

This section describes the methodology for estimating the population of each public housing project using the 1977 Picture of Subsidized Households (PIC77) dataset. The PIC77 dataset contains household-level demographic information for public housing residents, including the number of households in the project, racial composition, household size, income, and age structure. However, it does not provide direct population counts.

To estimate total, white, and Black populations for each project p , I multiply the number of households for group r by the average household size:

$$\text{Population}_{pr} = \text{Households}_{pr} \times \text{Average Household Size}_p.$$

One challenge is that approximately 42% of projects lack household size data. To address this, I impute missing values using two categorical variables that explain substantial variation in household composition: (i) *elderly designation* (all elderly, some elderly, or none) and (ii) *racial composition* (simplified into six categories: all white, all Black, all other race, mixed, no white, and other). For each elderly designation $\times$ racial composition cell, I calculate the mean household size across all projects with non-missing data. Missing household size values are then imputed using the corresponding cell mean. This imputation strategy leverages the fact that elderly projects have systematically smaller household sizes, and household size varies by the racial composition of the project.

This approach assumes that average household size is constant across racial groups within each project. While household size may vary by race in the general population, this assumption is reasonable within public housing projects where unit size allocations are determined by family size rather than race, and eligibility criteria are applied uniformly across racial groups.

To validate the imputation strategy, I conducted a hold-out test where 20% of projects with observed household size were randomly set to missing and then imputed using the cell means calculated from the remaining 80%. The imputation has an R^2 of 0.46 and is essentially unbiased, with a mean error of -0.01 persons.

B Census Tract Data Harmonization

This section describes the geographic harmonization of Census tract-level data to consistent boundaries. My baseline analysis harmonizes all tract-level data from 1930 to 2010 to 1950 Census tract boundaries using an area reweighting approach following Eckert et al. (2020).

I proceed as follows. First, for each source year, I spatially intersect census tract shapefiles with 1950 tract boundaries to identify overlapping areas. Next, for each source tract i , I calculate weights as the proportion of the tract's area that falls within each 1950 tract j :

$$w_{ij} = \frac{\text{Area}(\text{Tract}_i \cap \text{Tract}_{1950,j})}{\text{Area}(\text{Tract}_i)}$$

Weights are normalized to sum to one for each source tract to account for minor geometric inconsistencies.

Finally, I use these weights to estimate count and median variables in the 1950 tract geography. This proceeds slightly differently for count-based variables and median-based variables.

For each 1950 tract j , count variables such as population, housing units, and employment are harmonized by simply summing the weighted contributions from all overlapping source tracts:

$$X_j = \sum_{i \in I_j} w_{ij} \cdot X_i$$

where X_i represents the count variable in source tract i and I_j is the set of all source tracts overlapping with 1950 target tract j . This method assumes that outcome variables are uniformly distributed within each source tract, which may introduce measurement error in areas where populations are spatially concentrated within tract boundaries. For instance, if a source tract contains a spatially concentrated Black population but overlaps evenly with multiple 1950 tracts, the method would allocate residents uniformly across those tracts, potentially attenuating spatial estimates of segregation.

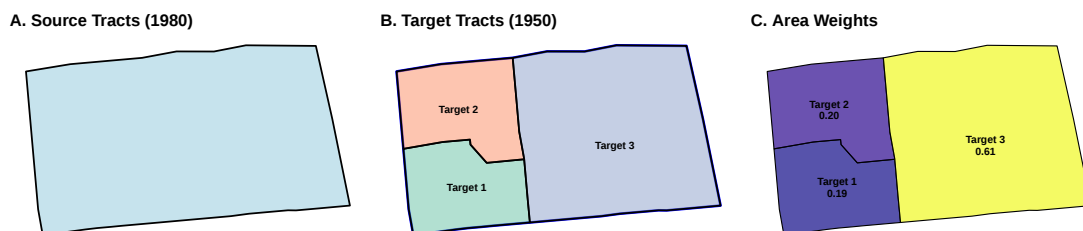
For median variables such as median income, median rent, and median home value, tract-level estimates are calculated as weighted averages across source tracts:

$$\tilde{X}_j = \frac{\sum_{i \in I_j} w_{ij} \cdot \tilde{X}_i}{\sum_{i \in I_j} w_{ij}}$$

This weighted-average approach is mathematically equivalent to using target-tract area shares, as recommended by Eckert et al. (2020) for intensive variables, because the denominator renormalizes contributions to sum to one for each target tract. This approach is applied to median

family income (1980, 1990, 2000), median contract rent (1940, 1980, 1990, 2000), and median home value (1940, 1980, 1990, 2000).

(a) Split Pattern: One 1980 tract $\rightarrow$ Multiple 1950 tracts



(b) Merge Pattern: Multiple 1980 tracts $\rightarrow$ One 1950 tract

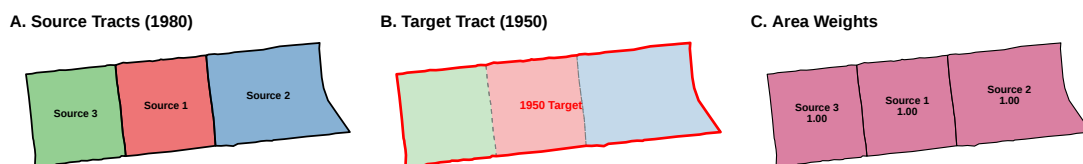


Figure 17: Census Tract Harmonization Examples

Note: These figures demonstrate the area-reweighting methodology for harmonizing 1980 census tracts to 1950 boundaries in Chicago. Panel (a) shows a split pattern where one 1980 source tract is divided across multiple 1950 target tracts, with weights representing the proportion of area allocated to each target. Panel (b) shows a merge pattern where multiple 1980 source tracts contribute to a single 1950 target tract.

C Robustness Checks: Alternative Matching Specifications and Standard Errors

This appendix presents full event study coefficient tables for alternative matching specifications discussed in the main text.

C.1 Alternative Matching Specifications

Here, I present the full set of coefficients from two alternative specifications: First, I present results using the baseline matching specification but adding a caliper restriction of 0.2 standard deviations of the propensity score to drop poor matches. Second, I run a specification matching without CBSA exact matching, which allows controls from different metropolitan areas.

Table 7: Caliper Matching: Treated Neighborhoods, Population and Demographics (Conley SEs)

Event Time	Log Black Pop	Log Total Pop	Log White Pop	Black Share
t = -20	0.051 (0.123)	-0.012 (0.021)	-0.012 (0.046)	-0.009 (0.012)
t = +0	0.619*** (0.115)	0.133*** (0.021)	0.056 (0.051)	0.032*** (0.011)
t = +10	0.614*** (0.143)	0.153*** (0.028)	0.019 (0.073)	0.057*** (0.016)
t = +20	0.462*** (0.146)	0.133*** (0.031)	0.009 (0.082)	0.056*** (0.019)
t = +30	0.395*** (0.145)	0.139*** (0.033)	0.036 (0.083)	0.057*** (0.020)

Notes: Event study coefficients for treated neighborhoods using caliper matching with propensity score caliper restriction. Outcomes include asinh(Black Pop), asinh(Total Pop), asinh(White Pop), and Black Share. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table 8: Caliper Matching: Treated Neighborhoods, Economic and Housing Outcomes (Conley SEs)

Event Time	Log Median Income	Log Median Rent	HS Grad Rate	Unemp Rate
t = -20	0.006 (0.014)	0.000 (0.020)	0.004 (0.005)	-0.003 (0.003)
t = +0	-0.080*** (0.014)	-0.020 (0.014)	-0.006 (0.005)	0.009*** (0.004)
t = +10	-0.153*** (0.019)	-0.060*** (0.015)	-0.017*** (0.006)	0.006* (0.003)
t = +20	-0.160*** (0.021)	-0.097*** (0.018)	-0.026*** (0.008)	0.009** (0.004)
t = +30	-0.168*** (0.025)	-0.150*** (0.022)	-0.028*** (0.009)	0.013*** (0.005)

Notes: Event study coefficients for treated neighborhoods using caliper matching with propensity score caliper restriction. Outcomes include asinh(Median Income), asinh(Median Rent), HS Grad Rate, and Unemp Rate. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table 9: Caliper Matching: Spillover Neighborhoods, Population and Demographics (Conley SEs)

Event Time	Log Black Pop	Log Total Pop	Log White Pop	Black Share
t = -20	-0.085 (0.104)	-0.000 (0.014)	-0.072* (0.039)	0.003 (0.009)
t = +0	0.155* (0.090)	0.007 (0.013)	0.028 (0.044)	0.014 (0.009)
t = +10	0.104 (0.107)	-0.018 (0.016)	-0.058 (0.056)	0.025** (0.013)
t = +20	-0.034 (0.113)	-0.027 (0.018)	-0.069 (0.075)	0.025 (0.016)
t = +30	-0.090 (0.109)	-0.021 (0.019)	-0.060 (0.082)	0.028* (0.016)

Notes: Event study coefficients for spillover neighborhoods (adjacent to treated tracts) using caliper matching with propensity score caliper restriction. Outcomes include $\text{asinh}(\text{Black Pop})$, $\text{asinh}(\text{Total Pop})$, $\text{asinh}(\text{White Pop})$, and Black Share. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 10: Caliper Matching: Spillover Neighborhoods, Economic and Housing Outcomes (Conley SEs)

Event Time	Log Median Income	Log Median Rent	HS Grad Rate	Unemp Rate
t = -20	0.006 (0.009)	-0.008 (0.012)	0.004 (0.004)	-0.002 (0.003)
t = +0	-0.025** (0.011)	-0.007 (0.009)	-0.002 (0.003)	-0.002 (0.002)
t = +10	-0.042*** (0.016)	-0.009 (0.012)	-0.002 (0.006)	-0.002 (0.003)
t = +20	-0.030* (0.018)	-0.017 (0.014)	-0.002 (0.007)	-0.003 (0.003)
t = +30	-0.025 (0.021)	-0.011 (0.016)	-0.001 (0.008)	0.002 (0.004)

Notes: Event study coefficients for spillover neighborhoods (adjacent to treated tracts) using caliper matching with propensity score caliper restriction. Outcomes include $\text{asinh}(\text{Median Income})$, $\text{asinh}(\text{Median Rent})$, HS Grad Rate, and Unemp Rate. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: No CBSA Restriction: Treated Neighborhoods, Population and Demographics (Conley SEs)

Event Time	Log Black Pop	Log Total Pop	Log White Pop	Black Share
t = -20	-0.061 (0.092)	-0.030* (0.018)	-0.017 (0.037)	-0.006 (0.011)
t = +0	0.449*** (0.081)	0.129*** (0.019)	0.077* (0.046)	0.029*** (0.008)
t = +10	0.535*** (0.109)	0.148*** (0.028)	0.036 (0.068)	0.052*** (0.012)
t = +20	0.523*** (0.120)	0.163*** (0.033)	-0.013 (0.084)	0.071*** (0.015)
t = +30	0.350*** (0.128)	0.160*** (0.038)	-0.046 (0.091)	0.076*** (0.016)

Notes: Event study coefficients for treated neighborhoods using propensity score matching without CBSA exact matching restriction, allowing controls from different metropolitan areas. Outcomes include $\text{asinh}(\text{Black Pop})$, $\text{asinh}(\text{Total Pop})$, $\text{asinh}(\text{White Pop})$, and Black Share. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 12: No CBSA Restriction: Treated Neighborhoods, Economic and Housing Outcomes (Conley SEs)

Event Time	Log Median Income	Log Median Rent	HS Grad Rate	Unemp Rate
t = -20	-0.002 (0.013)	0.012 (0.022)	0.001 (0.004)	0.005 (0.007)
t = +0	-0.090*** (0.014)	-0.026* (0.014)	-0.004 (0.005)	0.011 (0.006)
t = +10	-0.156*** (0.021)	-0.078*** (0.022)	-0.012* (0.007)	0.004 (0.006)
t = +20	-0.142*** (0.024)	-0.101*** (0.024)	-0.009 (0.008)	0.010 (0.007)
t = +30	-0.121*** (0.025)	-0.139*** (0.024)	-0.015* (0.009)	0.017** (0.007)

Notes: Event study coefficients for treated neighborhoods using propensity score matching without CBSA exact matching restriction, allowing controls from different metropolitan areas. Outcomes include $\text{asinh}(\text{Median Income})$, $\text{asinh}(\text{Median Rent})$, HS Grad Rate, and Unemp Rate. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 13: No CBSA Restriction: Spillover Neighborhoods, Population and Demographics (Conley SEs)

Event Time	Log Black Pop	Log Total Pop	Log White Pop	Black Share
t = -20	-0.098 (0.077)	-0.008 (0.014)	-0.043 (0.034)	-0.003 (0.007)
t = +0	0.103 (0.074)	0.008 (0.012)	0.004 (0.028)	0.012 (0.007)
t = +10	0.243** (0.100)	-0.005 (0.018)	-0.036 (0.047)	0.023** (0.011)
t = +20	0.142 (0.110)	0.003 (0.024)	-0.067 (0.064)	0.036** (0.014)
t = +30	0.109 (0.111)	0.004 (0.027)	-0.114 (0.072)	0.047*** (0.015)

Notes: Event study coefficients for spillover neighborhoods (adjacent to treated tracts) using propensity score matching without CBSA exact matching restriction, allowing controls from different metropolitan areas. Outcomes include asinh(Black Pop), asinh(Total Pop), asinh(White Pop), and Black Share. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table 14: No CBSA Restriction: Spillover Neighborhoods, Economic and Housing Outcomes (Conley SEs)

Event Time	Log Median Income	Log Median Rent	HS Grad Rate	Unemp Rate
t = -20	0.008 (0.010)	0.012 (0.017)	0.000 (0.004)	-0.004 (0.005)
t = +0	-0.034*** (0.009)	-0.009 (0.011)	-0.000 (0.004)	-0.006 (0.005)
t = +10	-0.037*** (0.014)	-0.016 (0.014)	0.003 (0.005)	-0.008* (0.005)
t = +20	-0.037** (0.018)	-0.011 (0.018)	0.005 (0.007)	-0.010* (0.005)
t = +30	-0.004 (0.020)	0.001 (0.020)	0.006 (0.009)	-0.005 (0.005)

Notes: Event study coefficients for spillover neighborhoods (adjacent to treated tracts) using propensity score matching without CBSA exact matching restriction, allowing controls from different metropolitan areas. Outcomes include asinh(Median Income), asinh(Median Rent), HS Grad Rate, and Unemp Rate. Standard errors account for spatial correlation following Conley (1999) with 2km cutoff. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

C.2 Baseline Specification: Tract-Level Clustered Standard Errors

This section presents event study coefficients for the baseline matching specification using standard errors clustered at the census tract level, rather than using Conley (1999) spatially-correlated standard errors used in the main text.

Table 15: Baseline Matching: Treated Neighborhoods, Population and Demographics (Tract-Clustered SEs)

Event Time	Log Black Pop	Log Total Pop	Log White Pop	Black Share
t = -20	0.038 (0.086)	-0.005 (0.017)	0.023 (0.036)	-0.012 (0.009)
t = +0	0.465*** (0.080)	0.129*** (0.016)	0.059 (0.039)	0.031*** (0.008)
t = +10	0.461*** (0.099)	0.139*** (0.022)	-0.020 (0.051)	0.051*** (0.011)
t = +20	0.339*** (0.103)	0.126*** (0.026)	-0.047 (0.059)	0.053*** (0.013)
t = +30	0.265** (0.105)	0.128*** (0.028)	-0.046 (0.065)	0.053*** (0.014)

Notes: Event study coefficients for treated neighborhoods using baseline propensity score matching with exact matching on CBSA and redlining status. Outcomes include asinh(Black Pop), asinh(Total Pop), asinh(White Pop), and Black Share. Standard errors are clustered at the census tract level. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table 16: Baseline Matching: Treated Neighborhoods, Economic and Housing Outcomes (Tract-Clustered SEs)

Event Time	Log Median Income	Log Median Rent	HS Grad Rate	Unemp Rate
t = -20	0.004 (0.011)	-0.005 (0.015)	0.007* (0.004)	-0.003 (0.002)
t = +0	-0.083*** (0.012)	-0.010 (0.011)	-0.008** (0.004)	0.005 (0.003)
t = +10	-0.138*** (0.015)	-0.055*** (0.014)	-0.015*** (0.005)	0.001 (0.003)
t = +20	-0.156*** (0.018)	-0.091*** (0.017)	-0.031*** (0.006)	0.005 (0.004)
t = +30	-0.151*** (0.021)	-0.156*** (0.019)	-0.036*** (0.007)	0.009** (0.004)

Notes: Event study coefficients for treated neighborhoods using baseline propensity score matching with exact matching on CBSA and redlining status. Outcomes include asinh(Median Income), asinh(Median Rent), HS Grad Rate, and Unemp Rate. Standard errors are clustered at the census tract level. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: *p<0.1, **p<0.05, ***p<0.01.

Table 17: Baseline Matching: Spillover Neighborhoods, Population and Demographics (Tract-Clustered SEs)

Event Time	Log Black Pop	Log Total Pop	Log White Pop	Black Share
t = -20	-0.020 (0.069)	-0.002 (0.011)	-0.043 (0.028)	-0.000 (0.006)
t = +0	0.126** (0.064)	-0.004 (0.011)	0.007 (0.029)	0.013** (0.006)
t = +10	0.048 (0.075)	-0.032** (0.015)	-0.070* (0.041)	0.020** (0.009)
t = +20	-0.090 (0.080)	-0.048*** (0.016)	-0.091* (0.049)	0.018* (0.011)
t = +30	-0.142* (0.080)	-0.051*** (0.018)	-0.080 (0.055)	0.017 (0.011)

Notes: Event study coefficients for spillover neighborhoods (adjacent to treated tracts) using baseline propensity score matching with exact matching on CBSA and redlining status. Outcomes include $\text{asinh}(\text{Black Pop})$, $\text{asinh}(\text{Total Pop})$, $\text{asinh}(\text{White Pop})$, and Black Share. Standard errors are clustered at the census tract level. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 18: Baseline Matching: Spillover Neighborhoods, Economic and Housing Outcomes (Tract-Clustered SEs)

Event Time	Log Median Income	Log Median Rent	HS Grad Rate	Unemp Rate
t = -20	0.004 (0.008)	-0.001 (0.010)	0.005 (0.003)	-0.002 (0.002)
t = +0	-0.030*** (0.009)	-0.007 (0.008)	-0.003 (0.003)	-0.003 (0.002)
t = +10	-0.042*** (0.012)	-0.005 (0.009)	-0.002 (0.004)	-0.004** (0.002)
t = +20	-0.026** (0.013)	-0.010 (0.011)	-0.002 (0.005)	-0.004* (0.003)
t = +30	-0.015 (0.015)	-0.010 (0.012)	-0.003 (0.005)	0.001 (0.003)

Notes: Event study coefficients for spillover neighborhoods (adjacent to treated tracts) using baseline propensity score matching with exact matching on CBSA and redlining status. Outcomes include $\text{asinh}(\text{Median Income})$, $\text{asinh}(\text{Median Rent})$, HS Grad Rate, and Unemp Rate. Standard errors are clustered at the census tract level. Reference period is 10 years before public housing construction (event time = -10). Statistical significance is denoted by: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

D Chicago High-Resolution Land Value Analysis

This appendix presents a supplementary spatial analysis of the effects of public housing on land values in Chicago using a high-resolution panel of land values. I use a dataset from Ahlfeldt and McMillen (2018) that provides nearly-decadal data on land values at a 300×300 foot grid-cell level from 1913 to 2010, sourced from *Olcott’s Land Values - Blue Book of Chicago*. While my baseline analysis estimates the effects of public housing on Census self-reported rents, assessed land values may provide additional insights into the economic impacts of these projects. Furthermore, the spatial granularity of these data is particularly well-suited for a spatial difference-in-differences design using concentric rings around project locations.

D.1 Empirical Strategy

I estimate the effects of public housing on land values using a stacked spatial difference-in-differences design following Blanco and Neri (2025), leveraging the high-resolution grid-cell data to analyze how public housing projects affected land values in concentric rings around project locations. As discussed in Section 5, the locations of public housing projects were determined by a combination of neighborhood characteristics. Here, given the spatial granularity of the data, I define the comparison points based on proximity to public housing project locations.

For each project, I compare the evolution of land values within a ring of a given radius around each project to those in a control ring farther away. I define treatment rings as 200m concentric buffers around each public housing project, and define the control rings as the ring of grid points 800-1000m away from the project. The data and estimation are organized in a stacked difference-in-differences framework with each public housing project treated as a separate “sub-experiment” (Wing, Freedman, and Hollingsworth 2024). The identifying assumption is that the trends in land values would have been similar between the treatment rings (0-800m) and the control rings (800-1000m) around each project in the absence of public housing construction.

I estimate the following event study at the grid cell i , public housing project g , and year t level:

$$\text{llv}_{igt} = \sum_{k \neq -1} \sum_{r \in R} \beta_{kr} \mathbf{1}[\text{event_time}_{it} = k] \times \mathbf{1}[\text{Ring}_{ig} = r] + \alpha_{gt} + \mu_{gr} + \gamma_{igt} + \epsilon_{igt} \quad (5)$$

where llv_{igt} is the log land value in grid cell i at time t around public housing project g , event_time_{it} is the event time relative to the project opening, and Ring_{ig} indicates the spatial

ring type (e.g., 0-200m, 200-400m, etc.). The coefficients β_{kr} capture the effects of public housing projects on land values at different event times k and across different spatial rings r . The term α_{gt} represents project-by-year fixed effects that ensure identification comes from comparing grid cells within each project over time, rather than across different projects. The term μ_{gr} represents project-by-ring fixed effects that control for baseline differences across spatial rings within each project. The term γ_{igt} includes additional controls—urban renewal status and proximity to an interstate highway—interacted with project identifiers to allow effects to vary flexibly within each project.

D.2 Results

Figure 18 presents the main results from the spatial land value analysis. The baseline specification shows that public housing projects, on average, had positive effects on local land values, particularly in the immediate vicinity of the projects. These effects mostly do not reach statistical significance, however.

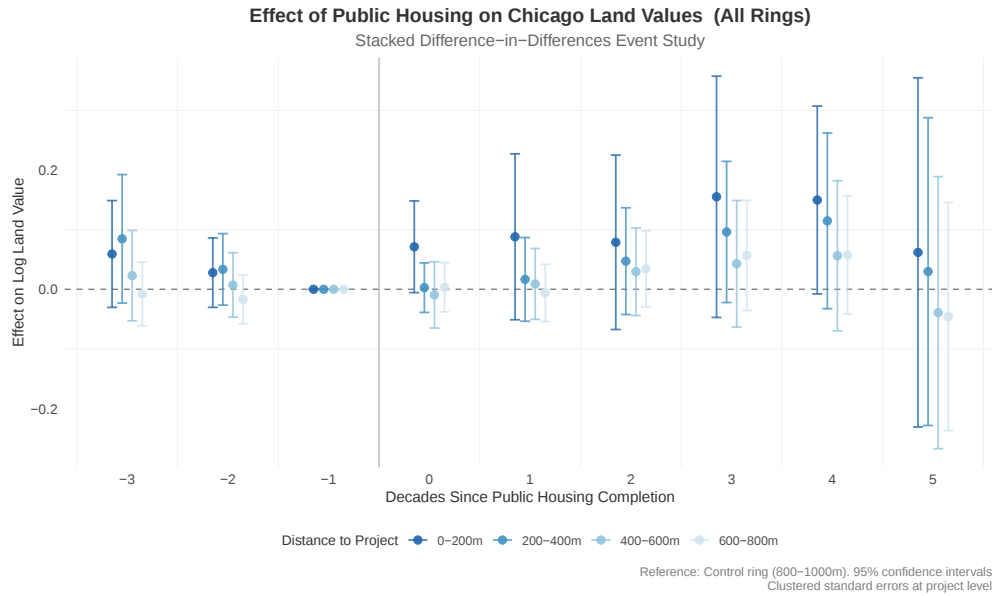


Figure 18: Effect of Public Housing on Chicago Land Values - Main Event Study

Figure 19 examines how these effects vary based on the size of the projects. I estimate the main specification separately for projects above the median project size (202 units) and those at or below it, comparing 55 smaller projects to 18 larger developments. This analysis reveals substantial heterogeneity in the impacts of neighborhood land values. In particular, I find some evidence that smaller projects seemed to have more persistent positive effects on

local land values.

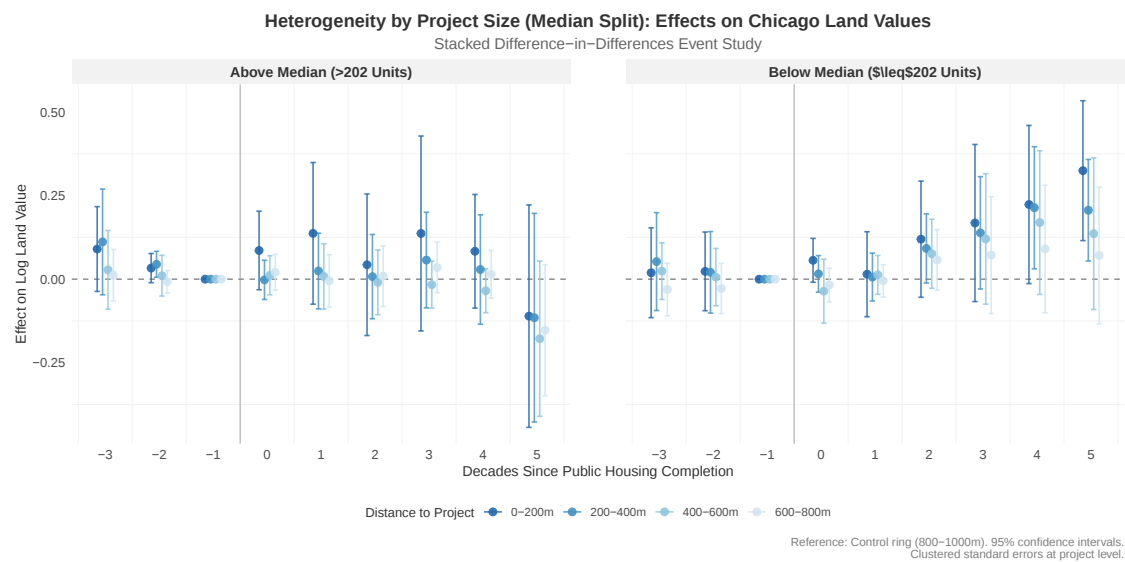


Figure 19: Heterogeneity by Project Size - Median Split Event Study